Quaternion engagements and terrains of knowledge (1858-1880):

A comparative social history of Peter Guthrie Tait and William Kingdon Clifford's uses of quaternions

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Declaration

I hereby declare that:

This thesis has been composed in its entirety by me (Josipa Petrunić) and that this work has not been submitted for any other degree or professional qualification.

Dedication

This thesis, as imperfect as it is, is dedicated to my mother, Katica Petrunić (née Stojkovic), and my father, Petar Petrunić. Both were raised in Communist Yugoslavia and both suffered severely as a result. I would not have had the opportunity to complete a doctoral degree had they not given up everything meaningful in their lives so that they could educate their children.

Remarks

I would like to thank my supervisor, Dr. John Henry, for his unfailing support throughout my doctoral studies. I would also like to thank my secondary supervisor, Dr. David Bloor, for introducing me to William Kingdon Clifford and quaternions in the first place. I would like to thank Dr. Steven Sturdy for helping me to clarify my thoughts on "terrains". And finally, I would like to thank my partner, Marino Iurillo, who, willingly or not, was forced to absorb much of the stress that accompanied this research.

Abstract

Historical studies of quaternion mathematics have usually placed Sir William Rowan Hamilton's "discovery" of quaternions within the context of the history of modern vector analysis. Exemplary of this technique is the seminal study *A History of Vector Analysis* by Michael Crowe (1967), in which Hamilton's development of quaternions is seen as an important precursor to the eventual development of contemporary vector calculus. Within Crowe's account, the reader also finds the story of two transitional figures: Peter Guthrie Tait (1831-1901) and William Kingdon Clifford (1845-1879). Tait is described as a propagator of Hamiltonian methods—someone who wrote about them more succinctly than did Hamilton, and someone who applied them to various topical problems in dynamics. Meanwhile, Clifford is described as a secondary, minor figure—a transitional character whose development of bi-quaternions figures not at all in Crowe's historiography.

This thesis redresses those categorizations by effectively "stopping the clock" at 1880, before the "modern" conception of vector analysis had emerged. Following a brief account of the state of British mathematics and science in the first half of the century (1800-1850), the present study focuses on the motivations behind Tait and Clifford's respective engagements with and uses of quaternion mathematics in the second half of the 19th-century.

Using the analytical metaphor of "terrains of knowledge" (which is inspired in part by the Wittgensteinian metaphor of language games, and the Strong Program account of finitism in scientific knowledge), I aim to describe the environments—philosophical, institutional, political, and religious—within which Tait and Clifford worked. By describing those "terrains of knowledge", the historian is able to explain why Tait and Clifford, two actors who lived in a similar time and similar place, engaged with the conceptual artifacts of "quaternions" in divergent ways.

In the case of Tait, the crucial "terrains of knowledge" to consider in identifying the conceptual environment requisite for him to have used quaternions in the manner that he did includes Cambridge and Belfast mathematics, the University of Edinburgh as an institution in flux (1840-1870), the "science of energy" (1850-1870), and Presbyterian politics and Tait's attack on secularism. In Clifford's case, the salient "terrains of knowledge" include the University of Cambridge and the morphing of symbolical algebra (1860-1870), non-Euclidean geometries in Britain, Clifford's Darwinism, and University College, London as a secularist urban educational institution.

When combined, these terrains constitute the varied intellectual environments within which each actor engaged with "quaternion" mathematics, and within which each actor found the resources needed to justify and render meaningful his respective view of that particular concept.

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Introduction

This is a story with two main characters: Peter Guthrie Tait (1831-1901) and William Kingdon Clifford (1845-1879). Born and raised in opposite parts of Britain in the first half of the 19^{th}-century, but trained in similar symbolical algebraic curricula at Cambridge University, both mathematicians engaged with curious 19^{th}-century mathematical entities known as quaternions. Their choice to do so was by no means evident. Their engagements with those entities were by no means unproblematic. And their respective views of one another's work were shaded by the vastly different conceptual resources that each actor drew upon. In Tait's case, those resources included Scottish conceptions of "energy" and Presbyterian morality. In Clifford's case, they included non-Euclidean geometries and Darwinist secularism.

Yet, the story that follows is not one of opposition, or even one of stark contrast. It is, rather, one of overlapping "terrains", in which Tait and Clifford are presented as two voyageurs traveling upon differing "terrains of knowledge" in their intellectual, social and personal navigations. They both encounter the artifact of "quaternion mathematics" and they both manipulate and use that artifact in distinct and idiosyncratic ways. Thus, this story has a simple plot, with a fairly simple conclusion, though its characters are vibrant and lively. Tait and Clifford were not in direct conflict in their respective developments of quaternions. Rather, they were operating upon differing "terrains of knowledge"—terrains that allowed each actor to define the worth of quaternions in varying symbolical, social and even moral terms.

Tait, Clifford and the history of science

In his review of Clifford's *The Common Sense of the Exact Sciences* (1885), Tait did not mince his words. He lauded his old friend Clifford and then proceeded to insinuate plagiarism. Tait wrote:

> Once more a characteristic record of the work of a most remarkable, but too brief, life lies before us. In rapidity of accurate thinking, even on abstruse matters, Clifford had

few equals; in clearness of exposition, on subjects which suited the peculiar bent of his genius and on which he could be persuaded to bestow sufficient attention, still fewer.

[However] the ease with which he mastered the more prominent features of a subject often led him to dispense with important steps which had been taken by some of his less agile concurrents. These steps, however, he was obliged to take when he was engaged in exposition; and he consequently gave them (of course in perfect good faith) without indicating that they were not his own. Thus especially in matters connected with the development of recent mathematical and kinematical methods, his statements were by no means satisfactory (from the historical point of view) to those who recognized, as their own, some of the best "nuggets" that shine here and there in his pages. His *kinematic* was, throughout, specially open to this objection: -- and it applies, though by no means to the same extent, to the present work (Tait 1885). [1]

Tait was at odds with his late colleague. Thinly veiling his accusation of plagiarism, he argued Clifford's development of bi-quaternions was fundamentally dependent upon his own work in quaternions.

Yet, unlike other cases of clear controversy in the history of science,[2] it would be hasty to argue that Tait's criticism of Clifford is indicative of their general interactions. The two had been on friendly terms for many years; they had even shared ideas over a brief correspondence in the 1870s. Without doubt, Tait and Clifford did not see eye-to-eye on matters of significant metaphysical, political and scientific importance. Their divergent philosophical views did not, however, always reflect antagonistic mathematical opinions. Consider for instance the fact that Tait concluded his account of *The Common Sense of the Exact Sciences* by acknowledging that, despite the apparent plagiarism,

Clifford could never have written in this vein. He would either have kept silent, or have blurted out the whole truth. Mystery and insinuation were not weapons of his, and should not be employed in connection with his name (Tait 1885).

So sincerely did Tait recognize Clifford's integrity in mathematical matters that he recommended Clifford as author of a proposed article on quaternions, which Arthur Cayley (1821-1895) had

[1] Tait admits that "the specially important and distinctive features of [Clifford's] work, viz. the homely, yet apt and often complete, illustrations of matters intrinsically difficult, are entirely due to the Author himself" (Tait 1885).

[2] See, for instance, Shapin and Schaffer's *Leviathan and the Air Pump* (1985), which documents the controversy between Hobbes and Boyle.

requested of Tait.[3] Notably, Tait's recommendation came *after* Clifford had already lambasted Tait's *Unseen Universe* (1875) in a public forum, saying it constituted little more than metaphysical propaganda for God-fearing ideologues. Despite Tait's antagonistic stance on Clifford's evolutionary theories and agnostic affiliations, he nonetheless recognized Clifford's worth as a mathematician and intellectual. Writing to the Reverend J. Paton (February 15th 1883), Tait stated,

> The naturalists, with the exception of Huxley, (who is really a great man), can easily be settled ... And in dealing with Huxley, the true ground to take is to state (what is perfectly certain, though he will not allow it), that Natural History has not got the length of even its Copernicus yet: -- far less its Galileo, its Kepler, or its Newton. The recent bosh about Darwin as the "Newton of Biology" simply shows those who utter it to be altogether ignorant of what Newton did! These people, even the best of them, have not yet got beyond what I once called "the beetle-hunting and the crab-catching stage"! ... My poor friend Clifford would have given you more trouble than all the host that is left. But then he was a man of true genius (Tait 1883).

Tait was a Presbyterian northern scientist; Clifford was an agnostic London mathematician. Their divergent professional and personal identities are important, but those attributes alone do not account for their varying approaches to quaternion mathematics throughout the 1860s and 1870s. In order to explain those respective mathematical practices, more complex account of the "terrains of knowledge" through which each actor navigated is required. By accounting for each actor's relative intellectual, conceptual, and linguistic environments, the historian is better placed to explain why Tait and Clifford differed in their uses of quaternions. As we will see, Tait's use of quaternions took place within a symbolical algebraic framework defined by the politics and practices of Scottish natural philosophy, as it was manifest in Edinburgh in the 1860s and 1870s. Meanwhile, Clifford's use of quaternions took place within a symbolical algebraic framework defined by the practices of non-Euclidean geometry and the evolutionary theories dominant at Cambridge and in London

[3] Cayley wrote to Tait (August 16th, 1877) asking, "Would it be any use to request you to undertake for next year the Report on Quaternions – if so, the Committee of Section A would be delighted to do so – if not, we thought of asking Clifford, who it seems is laboring on the subject. Please write or telegraph." On the back of the same note, Tait responded: "Clifford certainly, if he will accept the task which I found too personal" (Cayley 1877).

throughout the late-1860s and 1870s. Before delving into these contextual discussions further, however, the reader must understand what I mean by "terrains of knowledge".

"Terrains of knowledge" as an analytical metaphor

Since the 1960s, the history of science has increasingly sought to demonstrate the culturally situated nature of scientific theories. Following Thomas Kuhn's seminal *Structure of Scientific Revolutions* (1962), historians and sociologists of scientific knowledge have highlighted controversies in the production of scientific knowledge, as well as institutional and cultural foundations underpinning "normal science". Philosophers of *mathematics* have been slower to jump on the bandwagon. Linear histories of mathematics, such as Bell (1937), Klein (1968), and Boyer (1989), have included little, if any, account of the contextual settings within which mathematical actors carried out their craft. Admittedly, in recent years, a slew of historians have taken up the complex task of offering perspicacious cultural histories of mathematics, albeit with differing historical focal points. Richards (1988), Guicciardini (1989), Gray (1989), Durand-Richard (2001; 2007), Flament (2003), Warwick (2003), Crilly (2006), Parshall-Hunger (2006), and Mazzotti (2007) have all injected the history of mathematics with socially-aware accounts of mathematical productions ranging from the medieval period to the mid-20th century. The lucidity of their accounts justifies the methodologies they use, silencing debates over the use of detailed case studies as demonstrations of mathematics-in-the-making. In the words of Shapin and Schaffer (1985), it is now possible to forgo further philosophical and methodological debate and "just get on with it"—that is, get on with the task of writing social histories of mathematics.

Yet, one matter of methodological importance is still worth further clarification. The matter relates to the meaning of the word "context". It is now common, if not necessary, to talk about the "context" of a particular actor, a particular theorem, or a particular concept. Even established historians of mathematics (some of whom have not always invoked contextual methodologies in prior works) now regularly invoke claims of cultural context and social sensitivity. A case in point is

Ivor Grattan-Guinness's (2008) recent contribution against the long-standing claim of the 20th-century mathematician and quasi-Platonist, Eugene Wigner. In his 1960 article, "The unreasonable effectiveness of mathematics in the natural sciences," Wigner claimed that mathematics explains physical phenomena in an uncanny and inexplicable manner. He concluded that mathematics therefore describes necessarily true (though mysterious) *à priori* laws in the universe. In his opposition to Wigner's claims, Grattan-Guinness writes,

> When forming a problem and attempting to solve it, a scientist does not work in isolation: he is operating in various contexts, philosophical, cultural and technical, in some cases consciously recognized while in others intuitively or implicitly adopted. *Thinkers develop theories in the presence of other theories already available as well as by observations of the actual world, and can be influenced positively or maybe negatively by these theories ... it is the world of human theories that is anthropocentric, not the actual world* (Grattan-Guinness 2008, 3).

Grattan-Guinness contends mathematical knowledge, like other forms of knowledge, expresses the contexts within which it is generated.

Yet, herein lays the heart of a nagging methodological problem. What exactly constitutes "context"? By "context", does the historian mean *causal* factors that necessarily lead the practitioner to adopt certain theories or claims? Does the historian merely mean *contingent* factors that help a practitioner to adopt certain theories, but which are not necessary for the production of those claims (i.e. could the actor have come to make the same knowledge claims by relying upon other conceptual resources?). Or does the historian simply mean colorful detail that is only relevant when the scientist's knowledge claims are later deemed to be "wrong" or "false"? This last approach is that of the 20th-century sociologist Robert K. Merton, for whom context was important only really when the historian is forced to account for a scientist's false or incorrect claims (Merton 1973). The implication of Merton's use of "context" is that proper scientific thought progresses in a vacuum of intellectual and rational discourse, regardless of the socio-economic, politico-religious, philosophical or institutional affiliations of the practitioner who is producing the knowledge claim. From the Mertonian perspective, the identification of relevant "context" in the history of science indicates, at best, conditions that contingently pertain at the time of the development of a particular knowledge claim, and at worst, detail that is only relevant when the historian is explaining a Whiggishly determined "error" of judgment.

Over the past three decades, historians and sociologists of science have debunked the Mertonian understanding of scientific knowledge through the use of various case studies. They have shown that mystical and magical beliefs, which Merton and more traditional philosophers of science, such as Karl Popper, would have deemed to be "pseudo-scientific", were integral to the production of natural philosophical claims in the 16^{th}- and 17^{th}-centuries (see Koyré 1973; Henry 2001). They have shown that varying conceptions of "ether" in the 18^{th}- and 19^{th}-centuries embodied particular socio-religious viewpoints (Cantor and Hodge 1981). They have shown that sectarian affiliations and conceptions of divine knowledge defined the attitudes of British mathematicians towards non-Euclidean geometries in the 19^{th}-century (Richards 1988). And they have explored the socialization required to engage in experimental measurements of J-rays (Wynne 1976), N-rays (Nye 1980), parapsychological effects (Collins and Pinch 1979), free quarks (Pickering 1981), and gravitational radiation (Collins 2004). All those case studies point to the overriding conclusion that scientific knowledge is an expression of a practitioner's (or a group of practitioners') time and place set, their religious affiliations and beliefs, their institutional limitations, and their access to specialized discourse. "Context" is more than just peripheral detail. It is somehow constitutive of scientific knowledge.

Yet, as Shapin (1982) has also noted, many historical and sociological studies often fall short of constituting a full-blown "sociology of scientific knowledge". Historical and sociological studies may demonstrate the socially situated nature of scientific knowledge, but they often fail to explain *why* certain knowledge claims arise in the first place. Alongside their colleagues in the history of science, historians of mathematics have had to face similar concerns, as they have sought to demonstrate the socially situated nature of mathematical knowledge. Yet, all too often, historians of mathematics have stopped short of trying to explain *why* certain claims come about. And in those instances where the historian has tried to explain "why", he has often reverted to describing some sort of internal, logical driving force in mathematics itself. Hacking (1999), for instance, draws on the intuitionist school of J. E. L Brower in Holland in the early 20^{th}-century to argue that, yes, indeed, mathematical objects, theorems and proofs are produced *in time*. They are not *à priori* entities; they do not exist until they are created and used by human actors. "Constructivism" therefore holds in mathematics. Yet, according to Hacking, "constructivism" has a limit. Once a mathematical entity—be it theorem or proof—is created, the unfolding of any consequent arguments associated with it are driven by the internal logic of mathematical knowledge. Hacking's "why?" boils down to a logical impulse. Mathematicians are driven to make certain claims by the internal logic of the tools they are using.

Hacking's approach cannot be a satisfactory resolution. After all, it begs the question, why did the logical impulse come to be in the first place? One historian who has tried to answer these questions in terms of the history of quaternions is Andrew Pickering (1983; 1995). Pickering has argued that Hamilton's "discovery" of quaternions was a sociological phenomenon—one that can be best understood as a combination of "bridging", which is a "free" move, and "transcription", which is a "forced" move. This combination of moves is what Pickering labels the "mangle", and it accounts for Hamilton's ability to develop a non-commuting system of rotational operators known as "quaternions" (Pickering 1995, 120). Pickering explains that Hamilton was "free" to generate a model of the "unknown" from the "known"—to move from an already developed system of triplets towards an as-of-yet-undeveloped system of quaternions. For instance, Hamilton was able to work with triplets $(x + iy + jz)$ by assuming that they followed the same rules of algebra that applied to couples $(x + iy)$. Hamilton had been able to algebraically square couples. Thus, he chose to "bridge" the process to triplets and square them too.

However, the process of "bridging" does not tell the whole story. Pickering also identifies a subsequent process of "transcription". "Transcription" is the act of using a technique developed for another problem (i.e. squaring couples) to solve a new problem (i.e. squaring triplets). "Transcription" is a "forced" move in that it is motivated, and even necessitated, by the base mathematical model from which it is distilled. For instance, to make the squaring process work, Hamilton had to define $j^2 = -1$ in his triplet system, just as he had had to define $i^2 = -1$ in his couple system. In other words, Hamilton's "free moves" were not enough. He depended upon mathematically "forced moves" that "carried [his] extensions" to the point of developing workable equations that defined the based elements of quaternions (Pickering 1995, 131). For Pickering, therefore, it is the combination of "free" and "forced" actions that constitutes a cultural extension in mathematics. Wherever the pieces of the puzzle do not fit, an actor is able to freely "tinker with the various extensions in question" to generate partial solutions that are meaningful to him. However, that actor is also reliant upon the "force" of mathematical logic which drives him to certain conclusions. To be clear, Pickering is suggesting that, at certain points in Hamilton's investigations, the mathematician had no choice but to follow the established rules of algebra. As with Hacking, Pickering protects a place for the logical drive, or "forced" moves, necessitated by mathematical logic, even though he prefaces it with the "free" "tinkering" that describes freedom in mathematical modeling.

Problematically, Pickering nowhere explains where Hamilton's motivation to engage in such modeling comes from. Why couples? Why triplets? Why did Hamilton even try? Ultimately, the historian must explain "why" Hamilton wanted the system to work in the first place. What factors existent in Hamilton's life caused him to want to make the system work by any means necessary? This historian would contend that in so far as we understand Hamilton's definition of $j^2 = -1$ to be a tool he developed to help him complete the puzzle and make his triplet system *work*, then there was indeed a choice at play. There was no "forced" move necessitated by the logic of mathematics. Rather, there was a route chosen by Hamilton based on his calculation of a variety of factors. Contrary to Pickering's account, I would suggest Hamilton's desire to develop a meaningful symbolical system that had a direct geometrical analogue emanated from social norms existent outside the confines of his mathematical notebooks, where the specific acts of "bridging" and "transcribing" might have taken place. Pickering offers no account of those norms. In sum, Pickering's account is a description of "how" Hamilton made his claims, not "why" he made them.

My invocation of "terrains of knowledge" attempts to address this issue by identifying those factors that provided necessary resources for the production of unique knowledge claims. This does not provide a deterministic causal account (which is, in any case, an impossible task for the historian to carry out), but it does provide an account of the historically contingent factors required for knowledge claims to arise. In so doing, "terrains of knowledge" takes a step in the direction of fulfilling Shapin's (1982) request that sociologists and historians account for "why" certain knowledge claims arise, rather than just "how" they arise. I argue that "context" must be more than merely superficial detail invoked only when viewed retrospectively as the explanation for errors in judgment. In line with contemporary sociologists and historians of science, I consider "context" to be constitutive of an actor's knowledge claims.

One might think that this still leaves open the question of whether "context" is necessarily "causal" or merely "contingent". However, distinguishing between those two points is a more subtle exercise than simply avoiding the Mertonian approach, because it involves recognizing that context *is* causal. A "terrain" constitutes the supportive ground upon which an actor is dependent (i.e. the actor's conceptual resources) as well as the plethora of artifacts dispersed throughout the environment in which he finds himself acting. The actor can choose to move towards, or engage with, or even move away from particular artifacts appearing on a given terrain. The direction of the actor's movements indicates where it is that the actor locates his own interests. Thus, in describing an actor's navigations through a "terrain", the historian is engaged in the act of describing which interests motivate the actor to move in certain ways. The historian is also able, therefore, to identify why it is that an actor's actions and choices are meaningful (i.e. reasonable) to the actor himself. In other words, "terrains" are conceptual spaces that are infused with meaning; they allow actors to consider certain possibilities as reasonable and meaningful modes of action. They also contain the tools needed for the actor to engage in those possible courses of action. "Terrains" therefore describe "why" actors make the knowledge claims they do; and, in certain cases, they can also describe "how" the actors make those claims.

The various "terrains of knowledge" I identify for the purposes of analyzing Tait and Clifford's engagements with quaternions are those conceptual landscapes that Tait and Clifford navigated through as they made their respective claims. The "terrains" I highlight describe the environments within which Tait and Clifford felt their individual choices were meaningful. For instance, had Tait not studied at Cambridge or worked in Belfast where Thomas Andrews introduced him to Hamilton's quaternions as a valid research subject, had he not been transplanted to Edinburgh's world of natural philosophy alongside the symbolical algebraic mathematician, Philip Kelland, had he not been imbued with the language of "efficiency" and "work" as it appeared in thermodynamics, and had he not adopted particular Presbyterian accounts of the universe, he would not have been motivated to develop the specific claims about quaternions that he did, nor would he have seen those claims as meaningful in and of themselves. Similarly, had Clifford not studied mathematics at Cambridge in the 1860s, had he not adopted a belief in Victorian Darwinism, and had he not accessed the geometrical works of Riemann and the physiological works of Helmholtz while teaching at University College, London (where discussion of such nouveau concepts was allowed, if not openly encouraged), he would not have been motivated to produce the claims about quaternions that he did. In both Tait and Clifford's cases, the question "why" is answered by appealing to the complex conceptual topology that each actor navigated through and which provided him with the resources needed to effectuate those navigations and to deem them worthwhile. The question "how" is answered by an account (delivered in Chapter One) of the symbolical algebraic methodology put into place at Cambridge at the start of the 19th-century, and which was adopted more generally throughout the mid-century (identified here as the Peacockian "principle of the permanence of equivalent forms"). Combined with the varied "terrains" mentioned above, symbolical algebra and its varied mid-century manifestations allowed Tait and Clifford to engage with the artifact of quaternion mathematics from a plethora of directions and in historically unique ways.

Defined by geographical and academic location, and often overlaid by natural philosophical beliefs, a "terrain" is an analytical description of an actor's social placement at the time of a particular engagement. It allows for ebb and flow in an actor's interest in a particular artifact, and it helps the historian to identify those beliefs and institutional obligations that provided the actor with motivation to engage with the artifact in the first place. In other words, to explain "why" particular actors made the knowledge claims they did, the historian must appeal to the varied resources that existed as foundational terrains underlying each actor's movements.

How "terrains" relate to the Strong Program

Questions over "context" have led many historians and sociologists to develop differing analytical tools over the years, all of which have aimed to organize historical data in order to tell particular stories. In their seminal comparative work on Thomas Hobbes and Robert Boyle, Shapin and Schaffer (1985) have argued that "social context" is to be understood not only as the "wider society" but also "something else"—namely, the norms of the "scientific method" as crystallized in "forms of social organization", which constitute the "means of regulating social interaction within the scientific community." Those authors invoke Wittgenstein's notion of language games to identify certain ways of speaking, or "patterns of activity", that are normatively accepted. Those patterns (i.e. "language games") determine the bounds of social acceptability. Ultimately, they determine the degree to which any particular scientific claim can be deemed valid or justifiable. The authors "treat controversies over scientific methods as disputes over different patterns of doing things and of organizing men to practical ends" (Shapin and Schaffer 1985, 14). This is a useful starting point.

Researchers within the tradition of the Edinburgh Strong Program have built upon views such as those offered by Shapin and Schaffer to further specify the notion of "context". They have argued that "context" does not pre-determine scientific claims or acts, although it can create the space requisite for unique uses of scientific artifacts. On this view, each use of a scientific object (i.e. each knowledge claim) constitutes a new social act—one that must be understood as historically-shaped by contemporaneous norms, but which simultaneously changes the meaning of those norms (Barnes, Bloor and Henry 1996). The "strong" approach to historical reconstruction stipulates that scientific claims should be treated symmetrically as neither "right" nor "wrong". Rather, the analyst should start from the assumption that each claim is a reasonable instantiation of historically unique and contingent social factors (Barnes 1974; Bloor 1973; Bloor 1991). "The important point," Bloor (1999) argues, "is to separate the world from the actor's description of the world. It is the description that is the topic of enquiry, and the proposed separation is one of our resources." This approach narrows "context" to highlight those causal aspects of a practitioner's life that answer the question "why?"—"why" would an actor have sought to generate the particular knowledge claims he or she did?

In brief, the "strong program" theorists argue that accounts of scientific knowledge should include the following components: an account of "causality", understood to be the conditions that had to pertain for particular knowledge claims to have been made; a stance of "impartiality", understood to be a non-judgemental approach that avoids distinctions of "right" or "wrong"; and a respect for "symmetry", understood to be recognition of the fact that similar socio-cultural factors can give rise to either "successful" or "unsuccessful" scientific claims (Bloor 1991).[4] Identifying the causal element stipulated above constitutes the most difficult part of writing any social history of mathematics. The historian is helped, however, by the further clarification, offered by the Edinburgh sociologists, that "causality" can largely be identified as the "interests" at play in the generation of a particular

[4] The Strong Program also includes a fourth component labelled "self-reflexivity," which is understood to mean that any sociological account of scientific knowledge generation is itself open to analysis according to the three components mentioned above—namely, causality, impartiality and symmetry.

knowledge claim. As Barnes (1977) has put it, "All knowledge is actively produced by men with particular technical interests in particular contexts; its significance and its scope can never be generalised to the extent that no account is taken of those contexts and interests" (19). In other words, the process of producing a socially-informed historical analysis of mathematical acts requires that the historian ask the question: "What interests are at play in motivating an actor to make a particular knowledge claim?" Or, as this historian would prefer to put it, "Which terrains are supporting the actor's navigations and infusing those navigations with meaning?"

Far more complex than the Mertonian task of figuring out which explicit interests led a scientist or mathematician astray, identifying "interests" here involves the more subtle task of identifying those social factors that defined the realm of possibilities for any given historical actor and the discourses that infused those possibilities with meaningful justifications. What makes this difficult to do is that the most profound "interests" are those that have become so deeply institutionalized they have disappeared from view; yet, an actor who chooses to act against certain norms or "interests" would be deemed by his peers, his social grouping and even himself to be engaged in an act of self-destruction or failure. My invocation of "terrains" attempts to respond to the call for "causality" issued by the Strong Program by highlighting the foundational "interests" that motivated actors such as Tait and Clifford to navigate through particular conceptual and linguistic environments and to engage with artifacts that appeared within those terrains. In appealing to those differing terrains, the historian can thus understand why the actor produced the varying accounts of quaternions that he did, and why those productions were viewed by the actor himself as reasonable and meaningful courses of action to take.

Though neither Tait nor Clifford would have seen their lives in the discontinuous terms of easily distinguishable "terrains of knowledge", a "terrain" can nonetheless serve as an analytical tool that helps the historian generate a causal account of "why" actors make particular knowledge claims. It allows one to summarize a set of contingent resources that, wittingly or not, actors relied upon in the production of their claims. And though there may be other "terrains" in addition to those that I have highlighted in Tait and Clifford's case studies, I would contend that, in so far as one wants to explain those actors' uses of quaternions, there cannot be any *fewer.*

In sum, the manner in which a conceptual "terrain" causes a mathematician to make certain choices is akin to the manner in which a physical terrain causes a hiker to alter her gait. When "terrains of knowledge" are overlaid and mapped onto one another, their combined effect is that of a complex topological structure that allows for a symmetrical account of history—one devoid of moralistic judgements. The historian is able to see an actor becoming more or less motivated to generate a particular knowledge claim over time due to the influence of other, overlapping terrains. Tait's symbolical algebraic training and his Belfast-induced motivation to engage with Hamilton's quaternions became less of a causal influence on his choices from 1860 to 1865, when his engagements with thermodynamics contoured his intellectual terrains more profoundly, leading him away from the artifact of quaternion mathematics. That said, once in place, a "terrain" does not fully disappear either. The old terrain of "symbolical algebra" and the new terrain of mathematics at the University of Edinburgh overlapped to influence Tait's behavior in meaningful ways, leading him back to quaternions in the mid-1860s, though from a new and distinct direction—one which had been shaped by the contours of thermodynamics in intervening years.

"Terrains" also force the historian of mathematics to write about mathematics as a story populated by *people* doing things, as it is people who navigate through terrains. "Terrains" are inherently actor-dependent. They define an actor's socio-economic, political, religious, institutional, and educational experiences—all of which can provide resources for particular uses. But the central aspect of the metaphor is that *someone* is navigating *through* the terrain. Knowledge emerges as the product of those human navigations through varied environments, where particular objects (i.e. quaternions) are encountered as artifacts to be engaged with or abandoned. In other words, "terrains" cannot be used to explain the evolution or development of an idea. The history of ideas has no place in the world of "terrains", as "terrains" embed within them the notion that people, not objects, drive history.

We have at our disposal, then, an analytical tool that makes explicit the view that knowledge generation is open-ended and that the "progression" of a particular mathematical concept is not pre-determined by the actor's previous engagements. Particular knowledge claims arise from historically unique periods of overlapping concerns (i.e. "interests"). Like trekkers moving through a vast terrain, mathematical actors navigate through multiple layers of beliefs, obligations and structures as they use or abandon the conceptual artifacts they encounter. Therefore, "terrains of knowledge" can be invoked to offer comparative case studies. A description of the relative "terrains" that each actor navigates through can distinguish between the causal motivations underpinning particular knowledge claims among practitioners who may have operated in similar time periods but who nonetheless diverged in their uses of mathematical artifacts.

19th-century terrains of knowledge in the history of mathematics

Certain historical periods require the historian acknowledges some foundational "terrains" in order to produce meaningful explanations. For the historian studying 19th-century mathematics in Great Britain, fundamental "terrains" that are constitutive of any mathematician's claims include the nature of the actor's mathematical training (i.e. the decade and the school at which he or she studied mathematics), the professional institution(s) within which the actor developed his or her career, and the actor's explicit or implicit religious affiliations. Though these are not the only "terrains" relevant to an explanatory account of mathematics in the 19th-century (indeed, in Tait's case, I add a "terrain" of "energy science" and in Clifford's case I add a "terrain" of "non-Euclidean geometry"), they are the bare bones of any explanatory account of 19th-century mathematics.

Having thus outlined the concept of "terrains of knowledge" with regards to 19th-century mathematics, let us now proceed to an account of quaternion mathematics that invokes this analytical metaphor. In Chapter One, I offer an overview of the development of "symbolical algebra", and concomitant attempts to professionalize the discipline of mathematics and mathematical research in Britain, in the early decades of the 19th-century. In Chapter Two, I offer a brief segue between the rise of symbolical algebra in the 1810s and Hamilton's development of quaternions in the 1850s. By looking at Hamilton's self-described account of quaternions in his 1853 *Lectures on Quaternions,* we see that Hamilton's hope was to legitimize quaternions as a branch of symbolical algebra in order to gain credence for Irish science among practitioners of mathematics at Cambridge.

In Chapter Three, I offer a more detailed account of the various causal "terrains of knowledge" that Tait navigated through in his engagements with quaternions. Those terrains included Cambridge and Belfast mathematics in the 1850s, the University of Edinburgh and its mathematical and natural philosophical traditions, the "science of energy" and thermodynamics in Scotland in the 1860s, and Presbyterian political discourse in Scotland in the mid-19th century. In Chapter Four, I offer a similarly detailed account of the various causal "terrains of knowledge" through which Clifford navigated during his encounters with quaternions. Those terrains included Cambridge mathematics in the 1860s, non-Euclidean geometries as they were introduced in Britain in the same decade, Victorian Darwinism and its progressionist interpretation, and the University College, London as a secularist urban educational institution. By considering these differing terrains, the historian is well placed to suggest subtle reasons for which Tait produced the specific knowledge claims he did (i.e. that quaternions were admissible by the terms set out by Peacock's philosophy of symbolical algebra) , and that they were "efficient" in nature and therefore constituted a more moral form of mathematics), and to contrast these with reasons with Clifford's rather different knowledge claims (i.e. that quaternions were symbolical in nature, but that they were destined to offer geometrically meaningful claims that would advance the physical sciences and which would advance the status of human evolution more generally). As we will see, Hamilton, Tait, and Clifford's respective engagements with quaternions and their respective conceptual productions can be explained by appealing to the "terrains of knowledge" that each actor navigated. We start, then, at the opening of the 19th-century in Scotland and Cambridge, where mathematicians began to adopt symbolical algebraic notation in increasingly explicit ways.

Chapter One: Symbolical Algebra—the foundations

Introduction

It is cliché to argue that the early decades of the 19^{th}-century were a time of great upheaval and social change. After all, what time period in the history of humanity has not been a time of great upheaval and social change? Yet, important variations in social order, religious affiliation, and political organization were taking hold of Great Britain and Ireland throughout the 1800s. Those variations included the restructuring of university administration, the reformation of secondary and post-secondary curricula, and the emergence of new affiliations between industrial and academic communities. One important development to come out of these varied transformations was the introduction of continental approaches to the calculus—specifically, the introduction of Leibnizian differential notation and, in later years, the institutionalization of a philosophy of "symbolical algebra" which hearkened an overarching focus on *operations* in mathematics rather than magnitudes or quantities.

This section provides the reader with an overview of that symbolical algebraic tradition as it emerged in Britain in the first half of the 19^{th}-century. The aim is to identify key elements within that tradition, including the Analytical Society's deistic approach to knowledge, George Peacock's (1791-1858) "principle of the permanence of equivalent forms", mathematical reformers' institutionalization of symbolical algebra in the Cambridge Tripos examination system, and the wider move toward professionalizing scientific and mathematical disciplines throughout the 1830s.

Before beginning that discussion, it is important to note that in referring to a "tradition", or even a "philosophy", of "symbolical algebra" I do not mean to suggest there was a homogenous set of algebraic claims which actors such as Hamilton, Tait and Clifford adopted and abided by. The contrary was the case. "Symbolical algebra" meant many things to many people. It was introduced, used and developed in a plethora of ways in Scotland and England. And it was that mixed bag of symbolical claims and conceptualizations that provided youthful mathematicians at Cambridge with a vast realm of possibilities—possibilities that would come to constitute more specific "terrains" in the lives of Hamilton, Tait and Clifford in later years. Specifically, the British tradition of symbolical algebra in the first half of the 19th-century established a methodology in mathematical analysis—one that motivated young mathematicians to engage in research that would not have otherwise been condoned. Had a Newtonian tradition in synthetic or geometrically-focused research maintained its predominance at Cambridge, or at other British institutions of higher learning in the first half of the 19th-century, it would have been impossible for Tait and Clifford to make the quaternion claims they did. In addition, it was due to the deistic approach to mathematical knowledge advocated by early symbolical algebraists that mathematics, as a discipline, was opened up to the sons of industrialists and religious dissenters, thereby establishing a world of professional research in the mid-century, which was distinct from the world of gentlemanly science and privileged access to expert discourse that had defined scientific and mathematical engagement in Britain over the course of the previous two centuries.

A country in flux

Over the course of the 19th-century, Britain experienced a massive migration in human labour. Surging labour demands associated with newly emerging industries in the country's urban capitals and port centres resulted in a large-scale demographic shift. In 1801, the official census indicated that only one-third of the English population was "urban", while only one in ten urbanites lived in a city of 100,000 inhabitants or more. A century later, the 1901 census indicated that three-quarters of the English population were "urban", and one in three urban denizens lived in a city of 100,000 inhabitants or more (Kearns and Withers 2007, 1). The foetal form of a fledgling British welfare state developed within this milieu. As cities grew and populations became increasingly difficult to micro-manage, the three to five generations of British citizens who lived to witness the 19th-century unfold also witnessed the slow emergence of a system that sought to publicly manage the country's urban-dwelling and sexually-active populations (Driver 1989).

Growth in the bureaucratic macro-management of the country's population went hand-in-hand with the centralization of Britain's political institutions. While pre-industrial Britain had been governed by a "conglomeration of highly localized societies," ruled by magistracies and staffed by members of the local landowning class, industrialized Britain was moving towards increased parliamentary representation, catalyzing the decay of aristocratic nexuses of power (Williams 1994; Kearns 2007). A centralized, London-based parliamentary authority soon began to rear its nationalist head to ward off what it perceived to be the pernicious influences of Napoleonic France. But by the 1830s, the serious social, economic, and health outcomes associated with the country's rapid urbanization and industrialization became the stuff of home-grown dissent (Rees 2001; Southall 1996). Reformists and lobbyists targeted Britain's national institutions, identifying them as the root of domestic ill. Widespread grievances with Westminster manifested themselves in the *People's Charter* (1838). From 1838 to 1848, the Chartist Movement issued a litany of reformist demands, many of which sought to mobilize communities and improve upon local parliamentary representation (Searby 1977).[5]

As those socio-economic and political developments unfolded, they shaped the complicated milieus within which symbolical algebra gained a foothold. The youthful mathematicians who spearheaded a period of mathematical controversy at Cambridge in the 1810s were diverse in their political and social standings. Their engagements with philosophical, metaphysical and mathematical systems also varied. Indeed, the advocates of "symbolical algebra" invoked richly evolving hybrids of Lockean empiricism, Cartesian rationalism, and Berkelean idealism.[6] Perhaps not surprisingly historical efforts

[5] The ownership and control of local militias, which few protesting town mobs could effectively resist, belonged to élite ruling groups, including aristocrats and land-owners (Southall 1996, 178).

[6] The 18th-century interpretation of Locke that prevailed in France and in Britain was one that largely ignored the emphasis on mathematical-as-theological knowledge. In his *Essay Concerning Human Understanding* (1690), which includes the chapter "Of our Knowledge of the Existence of a God," Locke discusses three tiers of knowledge: the first is "immediate"

to narrowly categorize those mathematicians and their respective symbolical algebraic practices have often failed to provide meaningful historical accounts. Indeed, the story of the introduction of analytical techniques in Britain—including the adoption of Leibniz's $\frac{dy}{dx}$ notation for differentials (versus Newton's $\dot{x}$ dot-notation for fluxions)—has been told and retold by a plethora of historians. Boyer (1949), Dubbey (1977), and Novy (1973) have all described how the young Cambridge mathematicians Charles Babbage (1791-1871), John Frederick Herschel (1792-1871), and Peacock launched initial efforts to introduce "symbolical" or "continental" algebra during their student days at Cambridge, in large part through the formation of the Analytical Society in 1812. Novy (1973) has argued that later-century actors such as Augustus De Morgan (1806-1871), Duncan Farquharson Gregory (1813-44), George Boole (1815-1844), Hamilton, Arthur Cayley (1821-1895) and James Joseph Sylvester (1814-1897) also served as seminal "symbolical algebraists" given that they, too, influenced the country's approach to formal mathematics.[7]

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which follows the tradition of Descartes (the only sort of knowledge of this kind is of ourselves); the second is divine knowledge of God, which also requires individual effort; the third is empirical, which "we can have only by *sensation*" (Richards 1992, 51). This latter empirical category is the focus of Locke's *Essay*, and it becomes the focus of Lockean empiricism as practiced and advocated by his supporters in later decades and centuries.

Locke, however, had been keen to emphasize humans can acquire theological knowledge, too. One representation of such knowledge is found in mathematical certainty. Theological knowledge is "certain and evident truth," which is "equal to mathematical certainty". This sort of certain knowledge transcends the probable nature of natural philosophical or empirical knowledge. In Locke's view, such knowledge is deeply personal as it is the result of personal reflection. It is, therefore, impossible to communicate between people. In his chapter "On Probability," Locke claims, "In the demonstration of [a proposition] a man perceives the certain, immutable connexion there is of equality between the three angles of a triangle, and those intermediate ones which are made use of to show their equality to two right ones; and so, by an intuitive knowledge of the agreement or disagreement of the intermediate ideas in each step of the progress, the whole series is continued with an evidence, which clearly shows the agreement or disagreement of those three angles in equality to two right ones: and thus he has certain knowledge that it is so" (Richards 1992, 52).

The Lockean program served as a foundational resource for the early Analytical Society members who used it to defend their own republican-influenced hopes for curricular reforms.

[7] In his account of French mathematics in the 18th-century, the historian Ivor Grattan-Guinness (1990) has organized 19th-century mathematicians according to their research outputs. Mathematical practitioners from France and Britain fall into three "thinking styles," Grattan-Guinness claims. They include the geometrical, the algebraical, and the analytical, where the "analysts" include those practitioners who employed, to some degree, the techniques associated with limits (Grattan-Guinness 1990, 55-57). Grattan-Guinness recognizes that such categorizations are 20th-century constructions that fail to resonate with the actions of the actors they are meant to be describing. Not all historians are as explicit as Grattan-Guinness in recognizing the fluidity of such terms. In his detailed account of Newtonian mathematics in Britain throughout the 18th and early-19th centuries, Niccolò Guicciardini (1989) has claimed that a significant division existed between the development of continental techniques in Dublin and similar developments in Cambridge. While the Dublin group emphasized the teaching of "applied mathematics (mechanics, physical astronomy, optics)," the Cambridge group "was definitely purist-algebraist" (Guicciardini 1989, 124). It is not evident where Guicciardini locates the origin of those terminological distinctions, given that actors such as Peacock, Babbage and Herschel *did not* refer to themselves as "purist-algebraists." They did refer to themselves as "algebraists" and "analysts," but certainly not as "purists" or "formalists."

Yet, such a large list of mathematical actors, whose combined lifetimes spanned no fewer than 10 decades, suggests an inappropriate degree of generalization. What Britain experienced from 1810 onwards was not the introduction of one particular philosophy of mathematics to which a small cluster of identifiable mathematicians gravitated. Rather, what Britain experienced was the introduction of various new philosophies of mathematics, all of which drew metaphorical strength from contemporaneous socio-political transformations, and many of which resulted in distinct mathematical practices. Historians, such as Walter F. Cannon (1964), have therefore chosen to identify a loose "network of algebraists" that emerged in the 19^{th}-century, rather than point to any specific "school" or definitive tradition. Cannon has emphasized the malleability and imprecision of the boundaries defining members within that "network". Indeed, as we will see, early century symbolical algebraists differed significantly in orientation and practice from one another throughout the period spanning 1800 to 1850.

Calculus and Symbolical Algebra in Britain

By the mid-1700s, British natural philosophers and mathematicians had widely adopted Newtonian notation for fluxional calculus. They had interpreted the fluxion, as well as the Leibnizian differential, in terms of Newton's dual notions of limits and ultimate ratios, thereby bypassing the problematical concept of the "infinitesimal" as a measuring stick of incremental change (Guicciardini 1989; 2003). The "infinitesimal" had become a particular sticking point in British discussions, especially given Bishop Berkeley's (1685-1783) popular debunking of the concept. Berkeley had argued that because the infinitesimal could not be perceived, it was an unjustifiable metaphysical construction—an imagined entity construed by mathematicians to serve their own fanciful interests.

Yet, Berkeley was not representative of all 18^{th}-century British mathematicians. The calculus, as it had been developed and propagated by the Bernoulli clan and Leonard Euler (1707-1783), continued to attract a good number of British practitioners throughout the century. Mathematical actors, such as Edmund Stone (1695-1768) whose *Method of Fluxions* (1730) constituted a translation of

L'Hospital's *Analyse des Infiniment Petits* (1696), elaborated upon integral techniques (or the "inverse method of fluxions") as a symbolical extension of the calculus. Edinburgh's professor of mathematics, Colin Maclaurin (1692-1746), also refused to reject outright all aspects of the Leibnizian project. Representative of the Scottish Enlightenment, Maclaurin insisted upon the diffusion of both fluxional and differential calculus at universities in Scotland.

A degree of isolation did, however, come to reign over British mathematics in the last decades of the 1700s, due in no small part to the popularity of Berkeley's criticisms as well as widespread pedagogical hand-wringing over the proper place of the calculus within British school curricula. As Joan Richards (1988) has highlighted, primary, secondary and post-secondary teachers worried over the degree to which emerging techniques in analysis threatened the time-honoured conservative standards of "absoluteness" and divine truth that were manifest in the undeniable world of axiomatic Euclidean geometry. Symbolical techniques introduced concepts, such as negative and imaginary numbers, which had no clear empirical analogues. They fostered a sense of conventionality and arbitrariness in mathematical practice. Most importantly, however, the 1780s brought with it an avoidance of all things that whiffed of French revolutionary sentiment or, later, Republican zeal. Not surprisingly, the number of British actors willing to openly adopt continental notation dropped off, and those willing to laud accounts of Newtonianism and the fluxional method rose to take their place.

Yet, despite the metaphysical, institutional and even political barriers to the wider introduction of Leibnizian calculus in Britain, a number of key British and especially Scottish actors bridged the emerging divide between Europe and the Empire. Robert Woodhouse (1773-1827), a Cambridge don, defended the use of negative and imaginary numbers, as well as the use of "analytical" symbols (e.g. complex numbers) at a time when anti-French political sentiment was rife, if not virulent (Enros 1979). In his *Principles of Analytical Calculation* (1803), Woodhouse contended symbolical manipulations gained their "rational legitimacy" from "the rules governing [the] manipulation of the signs rather than [from the] ideas behind the signs" (Pycior 1984, 434). An arithmetical operational symbol such as the equal sign could be given a more general definition so as to allow for the manipulation of negative and imaginary quantities. Woodhouse argued,

> In their simplest meaning, the symbols $+ - \times$ designate additions, subtractions, multiplications, to be made on the supposition that the characters connected by these symbols can be resolved into units; and on this supposition, the first rules for transposition and multiplication are demonstrated; but subsequently to the extension of the rules, by which equations of no direct meaning and symbols incapable of being arithmetically computed are introduced, these symbols take more extensive signification: thus, $a \pm b\sqrt{-1} + c \pm 2b\sqrt{-1}$ is put $a + c + 3b\sqrt{-1}$ where the symbols $b\sqrt{-1}$, $2b\sqrt{-1}$ are connected together, in the same manner, as the signs of real quantities are, that is, of quantities that admit numerical computation: again $\left(a + b\sqrt{-1}\right) \times \left(c + d\sqrt{-1}\right) = ac + ad\sqrt{-1} + cd\sqrt{-1} - bd$, where the connecting sign $\times$ indicates an operation to be performed: what that operation is, we know from having previously established its nature, in those cases where the symbols employed were supposed to represent collections of units (Becher 1980, 391).

Woodhouse's claim was that because algebra can represent operations independent of empirical content, it offers a superior mode of thinking—one based upon the ability of mathematicians to render abstract those concepts that had been previously tethered to empirical reality.[8] Woodhouse's contemporaries, including the mathematician, John Toplis, at Cambridge (later Nottingham), Charles Hutton, at the Royal Military Academy in Woolwich, and James Ivory, at the Royal Military College in Marlow (later Sandhurst), also worked to generate greater respect for symbolical analysis in the post-Eulerian world (Craik 2000).

Yet, it was within the Scottish context that debates surrounding the metaphysical and pedagogical value of algebraic analysis gained vibrancy and profundity in the first two decades of the 19th-century. As an outgrowth of the Scottish Enlightenment, questions over the nature of mathematical knowledge had long been delineated by the epistemological skepticism of David Hume (Olson 1971). Nearly all Scottish philosophers of the 18th-century shared Hume's view to some degree, even

[8] Woodhouse's particular goal in this discussion was to address the question of convergence versus divergence in a series—an investigation later taken up by Augustin Cauchy in the 1820s.

though Scots such as Thomas Reid (1710-1796) and Dugald Stewart (1753-1828) advocated for a "Philosophy of Common Sense" and ultimately identified themselves as opponents to Hume. Reid and Stewart were deeply interested in the place of mathematics within the theory of knowledge. In Reid's first philosophical publication, "An Essay on Quantity" (1748), the Scotsman argued the content of "quantity" stems from the empirical observation of discrete entities in space. Yet, unlike Hume, who had accepted the ultimate fallibility of empirical knowledge, Common Sense philosophers emphasized the role of abstraction in mathematical reasoning as a means of preserving the *à priori*. Reid explained that because human senses are fallible, all knowledge (including mathematical knowledge) is the result of *faulty* sensory perception. Thus, geometers can never be certain about their judgments concerning the pure shape of an object. To gain truth, geometers must extend their thoughts beyond empirical observation; they must engage in that special capacity for thought known as "abstraction" (i.e. the innate ability to sever mathematical objects from empirical reality). It is for this reason that Reid, and later Stewart, maintained that mathematical knowledge deals with special classes of objects. Mathematical objects can be "conceived [of] without regard for their existence" even though the initial idea for the objects might have emerged from sensory experiences (Reid 1863, 77).

In the first decades of the 19th-century, Stewart propagated Reid's ideas in his *Elements of the Philosophy of the Human Mind* (1813)—a text that would come to later impress the young Babbage so much so that its main thesis reappeared in the latter's unpublished *Essays on the Philosophy of Analysis*. In his *Elements*, Stewart argued that although both geometry and algebra play a role in developing the reasoning faculties, algebra is the superior of the two. He wrote,

> [Just] as the decision of a judge must necessarily be impartial, when he is only acquainted with the relations in which the parties stand to each other, and when their names are supplied by letters of the alphabet ... so in every process of reasoning, the conclusion we form is most likely to be logically just, when the attention is confined solely to signs; and when the imagination does not present to it those individual objects which may warp the judgment by casual associations (Stewart 1814, 172-173).

The legitimacy of algebraic claims stemmed from their internal coherence, Stewart claimed. A logical derivation from starting principles to valid conclusions is evidence that symbolical algebra contains within it rational authority and universal legitimacy.

Stewart was not alone among 19th-century Scotsmen in supporting the adoption of this sort of symbolical algebraic philosophy. Though John Playfair (1748-1819) is best remembered for his popularization of James Hutton's theory of geology (in which he argued heat is the primary causal agent of physical transformations of the Earth), he was also deeply involved in mathematical research and teaching. Playfair believed in the principles of the French Enlightenment and its concomitant idealistic mandate to create a "universal language" (i.e. a classless, Republican language) in which all actors and practitioners could operate seamlessly. Playfair's most widely disseminated publication was *The Elements of Geometry, Containing the First Six Books of Euclid, with Two Books on the Geometry of Solids* (1795),[9] in which he defined the concept of "analysis" and defended it over and above any simple adherence to synthetic notions. In his first edition, the word "addition" is replaced with a "+" sign and the word "equal" is replaced with an " = " sign (Ackerberg-Hastings 2002). Though this constitutes only a basic introduction to symbolical notation, Playfair was writing for a student audience for whom even such basic symbolical usage would have been entirely new.

By the second edition of his book (published in 1806), Playfair had publicized his Republican political sentiments more clearly. Concomitantly, his use of symbolical notation emerged with greater prominence. Playfair ventured to introduce continentally-inspired symbolical notation throughout Book II of his text, explaining that:

> In the Second book ... some algebraic signs have been introduced, for the sake of representing more readily the addition and subtraction of the rectangles on which the demonstrations depend. The use of such symbolical writing, in translating from an original, where no symbols are used, cannot, I think, be regarded as an unwarrantable

[9] As Ackerberg-Hastings (2002) has noted, the result was that at least 13,000 British university and secondary students in the 19th-century learned their basic geometry through Playfair's textbook.

> liberty; for, if by that means the translation is not made into English, it is made into that universal language so much sought after in all the sciences, but destined, it would seem, only for the mathematical (Playfair 1806, v).

Composed by a liberal Whig, who supported the French Revolution (despite the fall of Napoleon), Playfair's text was as much a political treatise as it was a mathematical one. He acknowledged as much when, in addition to his book revisions, he launched a course in analysis for 3rd-year mathematics students in which he provided his class with the first English account of Laplace's *Mécanique Céleste*, arguing in favour of abandoning the English adherence to synthetic techniques altogether.

When Playfair was transferred to the university chair of natural philosophy, his successor, John Leslie (1766-1832), discontinued Playfair's continentally-inspired 3rd-year algebra course. Leslie was neither well-versed in continental methods nor was he supportive of the ideals of the French Revolution. He opted, instead, to teach one course in Newtonian fluxions every two years. In the preface to his *Elements of Geometry* (1809), he declared, "The analytical investigations of the Greek geometers are indeed models of simplicity, clearness and unrivalled elegance." Furthermore,

> It is a matter of deep regret, that Algebra, of the Modern Analysis, from the mechanical facility of its operations, has contributed, especially on the Continent, to vitiate the taste and destroy the proper relish for the strictness and purity so conspicuous in the ancient method of demonstration (Craik 2000, 139).

Leslie favoured a mathematical curriculum predominantly based upon geometrical methods.

Leslie's opposition to symbolical algebra was soon replaced, however, by explicit support for continental mathematicians as expressed by William Wallace (1768-1843), who was appointed to the mathematics chair after Leslie took over Playfair's position (Playfair having passed away by this point).[10] Wallace had been professor of mathematics at the Royal Military College prior to gaining his post in Edinburgh, and in that post he had authored articles on "fluxions" and "analysis" that were presented in the continental *d*-notation. Wallace had also published a translation of Legendre's

[10] Charles Babbage had also applied for the post; but he was denied by the long-running tradition of appointing Scots to Scottish chairs (Morrell 1997). In 1838, the Cambridge-trained mathematician, Philip Kelland, became the first English-educated Englishman to be elected to a Scottish mathematics chair.

memoir in Leybourn's Mathematical Repository (first in 1809 and then again in 1814). And he had made his continental sympathies explicit in an article oddly entitled "Fluxions" in the Edinburgh Encyclopaedia (1815), in which he presented a thorough overview of continental techniques in the calculus, advocating for their wider adoption throughout Britain. Wallace issued a series of encyclopaedic articles and popular journal contributions that indicated his very liberal attitude towards the wider adoption of algebraic analysis at the university level (Craik 1999).

The Analytical Society

Thus, the "analytical revolution" that occurred at Cambridge in the 1810s was not as unprecedented a "revolution" in English mathematics as has often been argued. Woodhouse, Playfair, Stewart and Wallace—along with other early-century mathematicians, such as John Brinkley (1763-1835) and Bartholomew Lloyd (1772-1837) in Dublin and William Spence (1777-1815) in Greenock—all advocated the use of "French" or continental analysis up to a decade before the origination of the Analytical Society. Yet, the group of actors that emerged in Cambridge in the 1810s was different from their Scottish and other national contemporaries in two important ways. Firstly, the Cambridge group was concentrated in a specific geographical location—Cambridge. Secondly, it formulated a specific goal—to publish and disseminate a symbolical analytical text that could be used by examiners who were setting questions on Cambridge's highly-regimented Tripos examination.

The world within which Peacock, Babbage and Herschel operated was a peculiar one. It was industrializing. It was urbanizing. Its inhabitants were engaged in debates over the relative value and worth of educational reform and they were struggling with the neo-conservative mores that characterized post-Napoleonic life in Britain. One of the most identifiable characteristics of that period was the religious framework within which British university life operated. A salient aspect of the Industrial Revolution (variously defined as having spanned from 1760 to 1830) was the adoption of critical attitudes towards the Anglican Church's monopolistic determination of proper knowledge. Concerns over the role of the Church in university governance resulted in parliamentary calls for universities, such as Oxford and Cambridge, to demonstrate their relevance in the new industrial era. In brief, those universities were forced to justify their irrefragable positions as the producers of Britain's moral and governing élite.

Note that the overarching objective of many British universities in the 18th-century had been to form right-thinking people—i.e. to mould those people who would supply a constant flow of staff to the professional clergy and government. Cambridge has often been characterized as the first of the British universities to "shed the last vestiges of the scholastic academic order, which had its origins in the high middle ages" and to adopt a curriculum that placed mathematics at the core of education. That process of "modernization" departed significantly from the "staid theological curriculum of Oxford" (Gascoigne 1984b, 1). It would be hasty to conclude, however, that the early 19th-century saw Cambridge move suddenly away from forming religiously-trained students. On the contrary, as with Oxford, Cambridge remained a large-scale seminary well into the mid-19th century. But unlike Oxford, it did so via mathematical rather than theological curricula. Cambridge's Latitudinarian tradition, which had coloured its religious discourse on campus throughout the 18th- century, meant that the Church viewed natural philosophy as part of its corpus of claims. Cambridge's latitudinarians held that mathematics and natural philosophy did not run contrary to the presence of God. Rather, the subjects served to simultaneously expose students to metaphysical truth. Thus, by the mid-18th century, the "iron grip of Whig clerical patronage" had taken firm hold of Cambridge, and sectarian divisions had given way to a newly unified curriculum based upon Newtonian "synthesis" in mathematics and Lockean empiricism in natural philosophy. By the late-18th century, that new curricular focus had become the "unchallenged basis" of Cambridge's undergraduate education, hardening into "a new orthodoxy almost as settled and complacent as the scholastic curriculum" (Gascoigne 1984b, 24).

Heading into the 19th-century, the link at Cambridge between Church (albeit Low Church) and mathematical knowledge was strong. By the 1810s, therefore, Peacock, Babbage, and Herschel found themselves advocating for a particular set of mathematical beliefs within a deeply religious environment. Consider the controversy that arose at Cambridge in the same decade over the role of the Bible in social life—specifically, over the manner in which that book was to be used and interpreted. In the early 1810s, posters and placards plastered the town's walls asking inhabitants whether they thought the Bible should be read with the Book of Common Prayer or on its own. A student-led attempt to form an independent Bible Society resulted in the formation of a group of undergraduates who contended the Bible ought to be distributed on its own, with no guidebook. On the other hand, Herbert Marsh (1757-1839), Margaret Professor of Divinity at Cambridge, argued the Bible should not be read without the prayer book, because people tended to misinterpret scripture. The Book of Common Prayer served as a prophylactic against the heresy that the "unlearned and unstable with which England now swarms" were apt to engage (Marsh 1811, 10). Isaac Milner (1750-1820), Lucasian Professor of Mathematics, clashed with Marsh over the reasonability of such claims. Milner took the position that the Bible's distribution would be a good thing, whether it was assisted or not. He also contended that Marsh's position was overly skeptical regarding the individual capacity of people to reason rightly (Ashworth 1996). That controversy highlighted the degree to which individual interpretation—whether in scriptural or mathematical matters—was still deemed to be illegitimate by powerful actors operating within the Cambridge framework. Analytical Society members expressed their own views on these matters by contending that an individual should have the right to generate mathematical interpretations without the intervening interference of any governing authority—i.e. Newtonians, university administrators or professors. Symbolical algebra provided a means, they argued, for individuals to engage with mathematical truth on their own.

Within these varied terrains of religious-industrial life, Peacock, Babbage and Herschel set to work to produce the Analytical Society's first publication. Not surprisingly, their writings were rife with both religious and industrial motifs. For instance, the Society's first publication, issued in 1813, was eventually titled *Memoirs of the Analytical Society*, despite Babbage's ironic recommendation that it be entitled "The Principles of pure D-ism in opposition to the Dot-age of the University" in order to reflect the mathematicians' French-inspired deistic approach to knowledge acquisition. In addition, the *Memoirs* (written by Babbage and Herschel, along with a preface authored by Babbage on the history of analysis in Europe and Britain), made it clear that the Society's intention was to built upon the Lockean tradition in empiricism (Woodhouse 1999). The algebraists argued knowledge is firstly derived from empirical sensory perception. Thus, all people have access to it. But, they contended, it can be abstracted and rendered universal through a process of rationalization, which allows latent relations between objects (or "ideas") to be discovered.

Thus, Babbage and Herschel identified three reasons why "analytics" would be a powerful tool for the Empire to adopt. Firstly, unlike classical geometrical techniques, symbolical algebra allowed for the definition of new entities not constrained by evident empirical content. Embracing such mathematical possibilities would have the practical effect of disburdening "the memory of all the load of the previous steps, and at the same time, affords it a considerable assistance in retaining the result" (Analytical Society 1813, 39). Secondly, symbolical algebra was more productive than geometrical definitions and proofs, as it made possible the consideration of problems that, when stated in common language or geometrical form, were impossible to solve due to their complexity and lengthiness. The authors argued,

> By separating the difficulties of a question, [analysis] overcomes those which appear almost insuperable when combined, or at least, [in] reducing each to its least terms, leaves them as the acknowledged landmarks of its progress, open to approach on all sides, should ulterior discovery furnish any rational hope of their removal. Meanwhile ... simple relations are found to exist between the most refractory functions, and even when the difficulties themselves prove invincible, their nature at least becomes thoroughly understood, and a means of evading them almost universally pointed out (Analytical Society 1813, ii-iii).

Thirdly, symbolical algebra offered a new definition of mathematical "truth". The shift to a "symbolical" approach to algebra meant that mathematical objects could be manipulated, and their relations justified, in terms of internally-defined equivalencies (Durand-Richard 2001). This was not an attack on *à priori* truth, the Society members contended. Nor was it an attack on God—the provider of irrefragable, eternal truths. Rather, as Babbage and Herschel explained, symbolical analysis guaranteed a new form of logical certainty, which emerged from the recognition of universal operations at play between conventionally established entities. In the eyes of the Analytical Society, symbolical algebra was both productive and religiously pious.

As a case in point, the algebraists offered an account of the Taylor theorem, otherwise known as the "theory of the development of functions" (Analytical Society 1813, iv). In the *Memoirs,* Babbage and Herschel talked of having to "reimport the exotic" in outlining the historical progression of the theorem that "immortalized the name of Brook Taylor." Taylor had initially developed his theorem using a method "not remarkable for its rigor." Joseph-Louis Lagrange (1736-1813) had recast Taylor's power series in his *Théorie des Fonctions Analytiques* (1797). In that work, the Frenchman had independently demonstrated Taylor's theorem by applying it to various branches of differential calculus without recourse to any notion of limits, infinitesimals or velocities. Meanwhile, Louis François Arbogast (1759-1803) had presented his own account of the theorem in a manuscript presented to the Academy of Sciences in 1789. Lagrange's algebraic calculus was considered to be the antithesis of Newton's fluxions—more so, even, than Leibniz's infinitesimals or D'Alembert's limits. The British algebraists viewed Lagrange's method as operating free of the conceptual tethers (namely, infinitesimals) strangle-holding common algebra and other approaches to the calculus (Fisch 1994, 249). Lagrange and Arbogast reinvented Taylor's series and had "established it as the true basis of the differential calculus" (Analytical Society 1813, iv). French mathematicians had initiated what Society members considered to be the "greatest revolution which has yet taken place in analytical science" (Analytical Society 1813, iv-v).

In the hands of Lagrange and Arbogast, differential calculus had improved upon Taylor's original contribution by making manifest the latent symbolical relations within Taylor series. So highly did the young Cambridge algebraists esteem the symbolical version of Taylor's theorem, and Lagrange and Arbogast's development of the "differential", that they excised Lacroix's d'Alembertian account of limits in their *Memoirs*, and replaced it with an account of Taylor's theorem according to Lagrange. Readers of the Society's first *Memoir* were urged to adopt Lagrange's "derived" functions, in which successive coefficients in the expansion of a function of a Taylor series defined successive derivatives of the function itself. With this symbolical algebraic account in hand, Society members viewed themselves as having established the predominance of symbolical thinking, which had as its aim the development of simple and efficacious tools that could productively advance mathematical research through the revelation of new symbolical relations, while at the same time preserving certainty in its logical form.

Babbage and Herschel offered more examples to justify their belief in the power of this symbolical approach. They discussed the usefulness of tabulating integral tables, which allowed for complicated partial differential equations to be calculated more efficiently. Such efforts had already effected results through the "labours" of Fagnani, Euler, Landen and Legendre, all of whom played a role in reducing complicated integrals, such as $\int \frac{Pdx}{R}$, where P is a rational and integral function of x, and R is a quadratic radical of the form $\sqrt{(\alpha + \beta x + \gamma x^2 + \delta x^3 + \varepsilon x^4)}$, to three species of problems. In other words, "a variety of integral formulae have, by dint of indefatigable research on all hands, been reduced to the evaluation of [a few] functions," the Society authors concluded (Analytical Society 1813, vii). They also highlighted the simplification resulting from the development of integral tables, which could be used to solve difficult integrals composed of "transcendants". In the hands of European practitioners, the Society members argued, those analytical techniques had achieved a sophisticated level of expediency and technical precision (Analytical Society 1813, vi). As Babbage and Herschel concluded,

> It is this connection with fresher sources, which can restore fertility to subjects apparently the most exhausted, and which cannot be too earnestly recommended to those who wish to enlarge the limits of analysis. The fire of improvement, however

> dormant, and seemingly extinct, may yet break forth at the contact of some external flame. The history of mathematics affords too many instances of the most distant principles coming into play on the most unexpected occasions, to allow of our ever despairing of success in such enquiries ... [To date] the multitude of different methods and artifices, which for the most part lead only to the same results, and whose power is limited by the same points of difficulty, is at length grown into a very serious evil. Our continental neighbours seem sensible of this ... digesting various points into a systematic form. But there is still much to be done in this line. That man would render a most invaluable service to science, who would undertake the labour of reducing into a reasonable compass the whole essential part of analysis, with its applications, curtailing its superfluous luxuriance, rejecting its artificial difficulties, and giving connection and unity to its scattered members (Analytical Society 1813, xxi-xxii).

According to the *Memoirs,* symbolical algebra was productive, efficient and capable of unifying previously disparate mathematical and natural philosophical concepts.

Problematically, the lack of public support for such work in Britain had delayed the development of similar advances locally. Babbage and Herschel invoked two case studies to legitimate their doomsday view. The laborious work of Christian Kramp (1760-1826), who had calculated a table of values for the integral $\int \varepsilon^{-x^n} \cdot dx$ in the case of $n = 2$, and the work of the Scottish manufacturer (and leisure mathematician), William Spence (1777-1815), who had offered a table of integral values for the "logarithmic transcendants" (defined by Spence as $L^n(\text{l} \pm x) = \int_0^x \frac{L^{n-1}(\text{l} \pm x)dx}{x}$, where $L^{\text{l}} = \ln x$), had largely gone unnoticed.[11] The Analytical Society members concluded that Spence's work had "display[ed] considerable ingenuity and a depth of reading rarely to be met with among the mathematical writers of this country." Yet, "there is little hope of seeing...similar efforts" in the future, as the mathematician who chooses to engage in such work would find "his labours will, at all events, meet with little remuneration" (Analytical Society 1813, vii-viii).

French Revolutionary motifs among the Analytical Society members

Despite the restoration of the monarchy in France, anti-French sentiment still filled the halls of Cambridge's lofty colleges throughout the 1810s. Dissent was not tolerated well. The works of

[11] Guicciardini notes that Spence tabulated results for the functions L^2 and L^3, and applied them to the integrals: $\int FL^{\text{l}}(V)dx, \int FL^2dx$,where $F = X/(a + bx \pm x^2)^{\frac{1}{2}}$ and X and V are rational functions (Guicciardini 1989, 106).

Lagrange, along with those of Pierre Simon Laplace (1749-1827) and Sylvestre Lacroix (1765-1843), were considered the tainted products of the French Revolution. Because Lagrange's treatises on the calculus were written in response to the needs of the Republican educational system, a rejection of Lagrange was a rejection of the Napoleonic system itself (Grattan-Guinness 1990). Representative of a young generation of quasi-revolutionary supporters, Peacock, Babbage and Herschel openly adopted the works of the French revolutionary authors. Their choice to do so was representative of similar choices being made more broadly by the sons of the country's middle-class industrialists, who were flooding the university's lecture halls. While the bulk of Cambridge's student population still hailed from the families of landed British aristocracy, the bulk of its Wranglers hailed overwhelmingly from Britain's emergent middle class of professionals, some of whom sympathized with the Republican ideals of egalitarianism. Those middle-class sons were a minority at Cambridge, but they were radical in their political views.

Prior to entering Cambridge, Herschel, for instance, had travelled to France with his parents where he had met Napoleon. In an 1812 letter to Babbage, Herschel called his friend "citizen" in the French style. In a letter to Whittaker in 1813, he disparaged the failed state of Britain, lauding the idea of a sweeping revolutionary make-over. Herschel wrote,

> Look at our overgrown *metropolis* and the kindred *tumours* of the second magnitude which have arisen in every limb of this diseased and corrupted body each tainting the little circle around it, each daily increasing in extent and foulness – The nation is apoplectic. – Choked with its own population – Overloaded with its useless manufactures and decayed commerce – The sin has been crying, and the expiation must be vast, and sweeping and satisfactory (Becher 1995, 410).

As the middle-class son of a court astronomer, Herschel was an unapologetic critic of the Church. Though he eventually performed his duties in the Senate House Examination in 1813 (an initial part of which questioned students on their religious beliefs), to finish Senior Wrangler, he was explicit in his rejection of the Church's authority in the university's governance structure. Herschel argued that humans possess the rational capacity to organize empirical data so as to discover abstract and universal truths that lay beyond the temporally observable world of phenomena. It is, therefore, the duty of humans to be makers of their own destinies.[12]

The ardently critical Babbage, meanwhile, was the middle-class son of a banker. Early in his university career, Babbage transferred from Trinity College to Peterhouse, as he had refused to ordain as a minister—a prerequisite for Trinity fellowships. Babbage's move to Peterhouse did him little good, however. His religious views led him to fail the initial Acts of the Senate House exam.[13] Babbage ultimately graduated with an ordinary degree, having been denied the right to write the formal Tripos.[14] Sympathizing as he did with French deism, Babbage viewed God as a distant actor

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[12] Herschel made his religious views patently clear in his *Preliminary Discourse on the Study of Natural Philosophy* (1830)—a text in which he linked the notions of divine knowledge to considerations of universal, harmonizing truth (i.e. operational relationships) in mathematics and natural philosophy. Herschel argued that, in carrying out his investigations, the mathematician and natural philosopher finds: "The study of one [science] prepares him to understand and appreciate another, refinement follows on refinement, wonder on wonder, till his faculties become bewildered in admiration, and his intellect falls back on itself in utter helplessness of arriving at an end. [Thus] he feels himself capable of entering only very imperfectly into these recesses of his own bosom, and analyzing the operations of his mind—in this as in all other things, in short, 'a *being darkly wise*'; seeing that all the longest life and most vigorous intellect can given him power to discover ... serves only to place him on the very frontier of knowledge, and afford a distant glimpse of boundless realms beyond" (Herschel 1987, 4-6). Furthermore, "There is something in the contemplation of general laws which powerfully persuades ... to commit ourselves unreservedly to their disposal; while the observation of the calm, energetic regularity of nature, the immense scale of her operations, and the certainty with which her ends are attained, tends, irresistibly, to tranquilize and re-assure the mind, and render it less accessible to repining, selfish, and turbulent emotions. And this it does, not by debasing our nature into weak compliances and abject submission to circumstances, but by filling us, as from an inward spring ... ; by showing us our strength and innate dignity, and by calling upon us for the exercise of those powers and faculties by which we are susceptible of the comprehension of so much greatness, and which form, as it were, a link between ourselves and the best and noblest benefactors of our species, with whom we hold communion in thoughts and participate in discoveries which have raised them above their fellow mortals, and brought them near to their Creator" (Herschel 1987, 16-17).

[13] The Acts were the pre-classification system the university used to place students into differing groups for the final days of Tripos examination. The Acts were composed of two mathematical questions and one moral question. The entire event was moderated by a Master of Arts. In Babbage's case, the M.A. was the Reverend Thomas Jephson, a devout clergyman. In his response to the moral question put to him, Babbage apparently sought to argue the thesis that God was material. Thomas Greenwood, a fellow member of the Analytical Society, and son of a muslin manufacturer, later recounted: "Jephson's Piety received such a violent shock that 'Descendas' [fail] thundered from his lips ... All Peterhouse was in an uproar, when the direful news came that their crack man had got a descendas" (Becher 1995, 408).

[14] Note that Babbage was eventually brought back into the Cambridge fold in 1827, when he was made Lucasian Professor. The position was made available as a result of George Airy's move from the post to the Plumian Chair following Woodhouse's death. Babbage's appointment to the famed Lucasian chair was, however, not the result of his own doing. It was, rather, the direct result of Peacock and Herschel's machinations. By the late-1820s, those two actors had established an increasing degree of political control within university affairs. They conspired to organize support for Babbage's election while he was in Italy with his wife. Wilkes (1990) has offered an insightful account of how it is that Babbage, with his radical views in politics, and his vehement stance on mathematical philosophy, was also snobbish and ungrateful in his personal behaviour—this latter character flaw being the product of Babbage's upper middle-class upbringing, which had allowed him to live free from financial concern as an undergraduate, despite having no aristocratic links. Babbage had found himself in a dire financial state following the abrupt end of his university career. As a result, he had begun to apply to various posts in the hopes of gaining employment to satisfy his working father, though he was unsuccessful in most of his efforts. As election for the Lucasian chair neared, Whewell, among others, suspected Babbage would fail to succeed—his religious views were still too outlandish for wide acceptance at the university. At the

uninvolved in the daily operations of the world. The adoption of *d*-notation, or "D'ism", in Babbage's pun, was the young man's way of defending his view that humans ought to forge their own destinies.

By contrast with his more radical colleagues, Peacock's religious attitudes lay within the framework of the established theism of the university. The son of a poor clergyman and school master, Peacock had been awarded a sizarship to attend Trinity College. Soon afterward, he was awarded a college scholarship for the needy sons of clergy. Having graduated Second Wrangler and first Smith's prizeman in 1813, he was then awarded a Trinity fellowship, after which he spent a decade as college tutor. Peacock often operated as the voice of sober second thought within the Algebraic Society, when it first started to function. When the Society's 1813 *Memoirs* failed to sell, due in large part to its explicitly pro-French and anti-fluxionist stance, Peacock encouraged its members to issue a second publication that was more conciliatory towards the university's established views. He argued that explicit attacks on the fluxionists should not be included in the second book, so as to avoid having the text labeled pro-French or anti-British (Becher 1995). The Society's second publication—a translation of Lacroix's book—was well-thought out. The translation contained a number of significant alterations to the original, offering a definition of limits that was more to the liking of the university's traditional tutors. Peacock's calculations proved profitable; two hundred copies of the Lacroix translation were sold in Cambridge within a month of the book's publication.

The successful publication of the Lacroix translation also opened up a channel for further attempts to include continental techniques within the university's mainstream curriculum. Admittedly, it was a temporal and unstable channel—one that could have closed up had it not been for continued institutional efforts made by Peacock and his supporters throughout the 1820s. Nonetheless, it allowed Peacock to make the seemingly audacious move as an 1817 Tripos "moderator" (i.e.

same time, as a sign, perhaps, of middle-class wealth, Babbage neither resided in Cambridge nor lectured as he was expected to do as Lucasian Chair (Wilkes 1990, 210-212).

examiner) to pose questions using Leibniz's *d*-notation—a controversial act that generated voluble criticism. As Peacock later told a colleague,

> I assure you that I shall never cease to exert myself to the utmost in the cause of reform, and that I will never decline any office which may increase my power to effect it. I am nearly certain of being nominated to the office of Moderator in the year 1818-1819, and as I am an examiner in virtue of my office, for the next year I shall pursue a course even more decided than thitherto, since I shall feel that men have been prepared for the change, and will then be enabled to have acquired a better system by the publication of improved elementary books. I have considerable influence as a lecturer, and I will not neglect it. It is by silent perseverance only that we can hope to reduce the many-headed monster of prejudice and make the University answer her character as the loving mother of good learning and science (Macfarlane 1916, 4).

Peacock's bold language should be qualified. There was such great opposition to his initial attempts to introduce *d*-notation that Peacock himself feared he had achieved nothing but vitriolic opposition. His friends concurred. In a letter to Herschel in March 1817, Whewell wrote:

> You have I suppose seen Peacock's examination papers. They have made a considerable outcry here and I have not much hope that he will be moderator again. I do not think he took precisely the right way to introduce the true faith. He has stripped his analysis of its applications and turned it naked among them. Of course all the prudery of the University is up and shocked at the indecency of the spectacle. The cry is 'not enough philosophy' (Wilkes 1990, 213).

However, due to the fact that the symbolical algebraic network had by then convinced sufficient numbers of college tutors of the worth of modifying the university's curriculum, Peacock was—contrary to all expectations—again appointed to the position of Moderator in the 1819 session. He capitalized on the opportunity by posing his d-notation questions again.

Importantly, Peacock's second attempt to introduce d-notation on the Tripos was soon followed by supporting publications. Whewell's textbooks in mechanics displayed continental symbolism and George Airy's (1801-1892) textbooks in optics and astronomy did likewise.[15] Both authors helped to

[15] By 1819, William Whewell (1794-1866) had already disappointed his analytical colleagues by advocating for a more traditional approach to mathematical research in his *Elementary Treatise on Mechanics* (1819). Natural philosophy should determine, first, the structure of the universe through the inductive observation of empirical phenomena. Secondly, mathematics ought to be applied as a tool in natural philosophy and thus rendered subordinate to it. Whewell's view was that French mathematicians had become overly attached to, "the forms and processes of pure analysis, which they have cultivated with such signal success [and which] had given them disrelish for the more physical and inductive part of the reasoning, and made them comparatively indifferent to the manner in which they arrive at that

institutionalize symbolical analysis at Cambridge in the years immediately following the 1817 and 1819 exams. Peacock also issued his own *Collection of Examples of the Applications of the Differential and Integral Calculus* (1820), including contributions by Herschel. His hope was that his text would become a Cambridge coaching book used in training students for future Tripos exams.[16]

The Tripos as an institution

The role of the Tripos exam in this process of institutionalization deserves special mention. By 1780, unofficial changes to the structure of The Senate House Examination (the official name of the Tripos) had placed the exam at the centre of Cambridge undergraduate degrees. By the 19th-century, the "hotly-contested order of precedence"—i.e. the student ranking of performance on the exam—was posted at the end of the exam week and students fought for top place. The exam became a written test in 1828. Past exams and solution guides were then published and used as training tools (Warwick 2003, 115).[17] Private mathematics coaches increased in importance as the rank of Senior Wrangler became, almost exclusively, the domain of those trained by established Cambridge tutors such as William Hopkins (1793-1866). Thus, by the 1830s, the Senate House Examination had come to embody two widely-accepted opinions evident in early-Victorian British society. The first was a view propagated by former Analytical Society members, which held that examinations constituted the best means of judging talent in a society still weighed down by the economic barriers that kept talented people at the social periphery of British life. Efforts to move away from aristocratic and religious authorities could be promoted by competitive systems in which accolades would be bestowed upon those who merited kudos rather than inherited it.

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part of the subject where the machinery of analysis begins to work. Hence those principles which mechanics *must* borrow of experiment are often made to depend on abstract reasoning and artificial definitions: or introduced as self-evident, with some slight notice as to their agreement with matters of fact" (Whewell 1819, vi).

[16] It is important to keep in mind that rise of Lagrangian-inspired calculus on the Tripos in the years following 1819 was an *addition* to, not a complete usurpation of, what had gone before. On the 1820 exam, for instance, students were still asked to "Explain by short examples the method of exhaustions, of indivisibles, and of prime and ultimate ratios." By 1830, questions still appeared on the exam asking students to "Define the Differential Coefficient, (1) according to the method of limits; (2) according to La Grange's method. [And] Show that both methods lead to the same result" (Wright 1831). Symbolical algebra was an add-on rather than a revolutionary overthrow of previous content.

[17] Prior to 1827, students were required to respond to orally-dictated questions—the oral diction being an outgrowth of the traditional Senate House Examination disputations, in which the topics were relayed to candidates verbally.

The second opinion was embodied in the views of Britain's rising industrial class more generally. It held that competition and free enterprise were inherently good. Cambridge University administrators played to this widespread industrial belief, arguing that, with its competitive ethos, Cambridge could produce the Empire's new leaders and innovators. The Tripos became an entrenched institution within British society, serving as an emblem of meritocracy and progress. Indeed, by the end of the 1820s, the yearly ritual of the Tripos exam had already become a cultural rite of passage for many young Cantabrigians and a cultural standard of excellence for Britons more generally. It also came to serve as the model for British civil service examinations following federal reforms in the 1850s, and it became the model for university mathematics exams issued by universities established throughout the colonial empire and the United States. By 1873, the famed Cambridge coach and textbook writer, Isaac Todhunter (1820-1884), claimed, "Ours is an age of examinations, and the University of Cambridge may claim the merit of originating this characteristic of the period." Todhunter went so far as to argue that the Tripos was "the model for rigour, justice and importance, of a long succession of institutions of a similar kind which have since been constructed" (Gascoigne 1984a, 547).

The rise of the Tripos must be considered in light of Cambridge's internal political battles. By the turn of the century, the university was under severe pressure to reform the jealously guarded autonomy that each college had developed over its respective curriculum. The Tripos was a mechanism by which that autonomy could be degraded. It was the only university-wide examination that all students seeking to graduate with honors had to complete; it was the institution through which reforming administrators at the university-level sought to wrest power away from college fiefdoms. The examination allowed the university's administration—as small and as weak as it was—to maintain ultimate control over the dissemination of degrees. The more important the university-wide Tripos became, the less important individual college rankings and curricula became (Warwick 2003). Political efforts to centralize power hardly meant that colleges suffered an absolute castration of their abilities to dominate the examination process. As all Masters of Arts were eligible to examine students, examiners from differing colleges could query and scrutinize top performers from rival colleges in order to bolster their own. The phenomenon was particularly virulent at St. John's and Trinity, both of which were colleges that tried to rival the growing reputation of the university's Tripos by issuing college-based exams with internal rankings. Yet, under the pressure of a mid-century parliamentary inquiry into the status of education at Cambridge, the university also became the poster-child for curricular reform across the country. Though the colleges continued to possess resources that rendered the central university a miniature financial foe, it was the university that ultimately provided the "lustre of a Senior Wranglership" (Gascoigne 1984a, 557). By the 1830s, that "lustre" referred to mathematical prominence. A top Wrangler was a top symbolical algebraist.

Alongside the institutionalization of symbolical algebra on the Tripos, another output of the Analytical Society (which stopped being a "Society" only a few years after its establishment, as members graduated and moved on to fellowship careers) was the formation of the Cambridge Philosophical Society in 1819. The geologist, Adam Sedgwick (1785-1873), and the botanist, John Henslow (1796-1861), conceived of the Society during a tour of the Isle of Wight. It was meant to be a corresponding society devoted to a broad repertoire of emerging research. The Society's objectives were defined by the mandate to promote "scientific enquiries" and facilitate "the communication of facts connected with the advancement of Philosophy and Natural History" (Hall 1969, 7). By the end of the 1820s, 170 Fellows and £300 in private investment had flowed into the group. Though the Society stated its goal in broad terms—i.e. as the advancement of all forms of natural philosophical inquiry—the Philosophical Society became a conduit for primarily mathematical research. Of the papers published in 1820 (the Society's first year of collaboration), eight of 11 contributions (published in Volume I, Part I, of the *Transactions of the Cambridge Philosophical Society*) were written by Herschel, Babbage, Whewell, Sedgwick or John Willis Clarke (1833-1910), the Cambridge professor of anatomy (Cannon 1964, 72). Mathematical discourse predominated. Furthermore, the secretaries of the group up to 1850 included Sedgwick, Henslow, Peacock, Whewell, the university teacher and architectural historian Robert Willis (1800-1875), and Hopkins—three of whom (Peacock, Whewell and Hopkins) advanced symbolical algebra as a primary methodology for research within the pages of the Society's journal. That journal soon became a vehicle for the dissemination and propagation of a "Cambridge School" of analytical mathematics. The Cambridge Philosophical Society thus served to institutionalize symbolical algebraic research as a model and standard for mathematical engagement into the mid-century.

Professionalizing science and mathematics in the 1830s

The rise in prominence of the Tripos, and the establishment of the Cambridge Philosophical Society, was not enough to convince everyone that Britain was now at the forefront of mathematical research. When asked during his visit to Cambridge in 1842 to name the greatest living English mathematician, Carl Jacob Jacobi (1804-1851) infamously responded, "There is none" (Macfarlane 1916, 3). That Jacobi's comments were overly cynical, there is little doubt. But that British mathematics had failed to gain notice on the international stage well into the 1840s, there is some justification. Jacobi's statement summed up what had been a growing sentiment among numerous practitioners for years. As a nation, Britain was perceived to be scientific decline. The "declinist" debate over the status of mathematics and natural philosophy gave rise to two specific concerns. The first related to the question of Britain's place in the world. The second related to the role of continental mathematics in Britain's hoped for revivification.

In a series of articles starting in 1804 in the *Edinburgh Review,* Playfair famously denounced the "decline" of British mathematics and natural philosophy. He argued British mathematicians had to acquaint themselves with mathematics from the continent (i.e. Laplacian mathematics) if they hoped to make any worthy achievements in the future (Horsley 1804). The views propounded by later-century "declinists" in the 1830s shared some of Playfair's sentiments, and even drew on his concerns. But, the declinists of the 1830s were a breed of their own. Their concerns were shaped by the fact that analysis and symbolical algebra had, by that time, come to enjoy a happy home at Cambridge. Yet, international recognition of this fact was nowhere to be found. Much blame for this seeming lack of British exceptionalism on the global mathematical scene was directed at the Royal Society. Its mismanagement and poor governance were considered by actors such as Babbage to be the root of decrepitude in British mathematics. In his *Reflections on the Decline of Science in England* (1830), Babbage, by then Lucasian Professor of Mathematics, invoked industrial metaphors to highlight the lack of efficiency and productivity engendered in the structure of the Royal Society. Babbage argued the outmoded philosophical body had severed Britain from the productive world and impeded the advancement and progress of natural philosophy more generally. He wrote,

> The progress of knowledge convinced the world that the system of the division of labour and of co-operation was as applicable to science as it had been found available for the improvement of manufacturers. The want of competition in science produced effects similar to those which the same cause gives birth to in the arts. The cultivators of botany were the first to feel that the range of knowledge embraced by the Royal Society was too comprehensive to admit of sufficient attention to their favourite subject, and they established the Linnean Society. After many years, a new science arose, and the Geological Society was produced. At another and more recent epoch, the friends of astronomy, urged by the wants of their science, united to establish the Astronomical Society. Each of these bodies found that the attention devoted to their science by the parent establishment was insufficient for their warrants, and each in succession experienced from the Royal Society the most determined opposition (Babbage 1989, 21).

In a more sarcastic tone, Babbage continued,

> Instituted by the most enlightened philosophers, solely for the promotion of the natural sciences, that learned body justly conceived that nothing could be more likely to render these young institutions permanently successful than discouragement and opposition at their commencement. Finding their first attempts so eminently successful, they redoubled the severity of their persecution, and the result was commensurate with their exertions, and surpassed even their wildest anticipations (Babbage 1989, 21).

The Society was closed, elitist, and reluctant to advance British natural philosophy or mathematics. It required reform at its most fundamental level if it were to remain relevant.

Babbage offered a series of recommendations for such reform, including changes to the Society's publications standards. He argued the Society should include *all* data in its published experiments, as opposed to simply publishing the most "accurate" findings. He recommended it change its experimental and methodological "principles" so as to oust "fraudsters," "hoaxes" and "cooks"—i.e. those individuals who, of "a hundred observations," could pick out "fifteen or twenty which will do for serving up." The Society had encouraged and protected poor scientific practices. In so doing it, it had excluded those individuals who had contributed most to the scientific advancements of the day: botanists, astronomers, medical experimenters, and mathematicians (Babbage 1989, 91).

The lack of any other nationally-supported scientific research program led to gloomy assessments of Britain's technical competencies among other practitioners, too. When, in 1828, the government abruptly abolished its Board of Longitude, one of the few state-funded natural philosophical agencies (in which Herschel had served as commissioner) to produce new research, a sense of abandonment came to prevail within certain research communities. In response, provincially-based reformers mobilized to launch a new nation-wide research network to counter-act the lack of research support flowing from federal coffers. Local philosophical groups, including the Manchester Literary and Philosophical Society, the Bristol Philosophical Institution, and the Yorkshire Philosophical Society worked to establish a research network that resided outside the confines of any one particular university. Mobilized by the energetic Scottish inventor and natural philosopher, David Brewster (1781-1868), Herschel, Whewell, Babbage and Forbes soon engaged in a flurry of letter writing that resulted in the first meeting of the British Association for the Advancement of Science (BAAS) in York, hosted by the Yorkshire Philosophical Society (Morrell and Thackray 1981).

To be sure, the location of the first meeting was not acceptable to all of London's intellectual élite. Many of Britain's savants protested the idea that a newly-established British scientific institution was to be born in a remote part of the country. The significant absence of enthusiasm from all corners of the disciplinary spectrum, and the physical absence of major figures from Cambridge, Oxford, and London, demonstrated the first meeting of the BAAS was a fledgling attempt to unify a heterogeneous group of "reformers".[18] Babbage, Whewell and Herschel, for instance, all expressed varying degrees of doubt regarding the form and organization of the Society. The BAAS's second meeting was thus moved to London to bring established actors into the fold. Meetings were thereafter organized at locations centring on major academic sites including Cambridge, Oxford, Edinburgh and Dublin. Those meetings led to a series of membership expansions that drew in local gentry and "gentlemen of science". The BAAS grew to become a cultural institution of significance by the end of the decade (Cannon 1978; Howarth 1922; Williams 1961).

George Peacock's symbolical algebraic philosophy

Within this milieu of professionalizing science, Peacock's efforts to institutionalize symbolical algebra stand out as particularly noteworthy. As a primary disseminator of the new symbolical algebraic philosophy, Peacock's role in the advancement of symbolical techniques in both teaching and research should not be understated.[19] By 1814, Peacock (Second Wrangler, 1809) had been elected to a fellowship at Trinity College. He served as college lecturer and later tutor. As a fellow, Peacock engaged in various activities aimed at the improvement of education, including a restructuring of Cambridge's administrative and examination system. When Peacock first posed questions in continental notation on the 1817 exam, he rang a warning bell to students, coaches and university administrators indicating that continental mathematics was no longer the stuff of mere peripheral interest or extra-curricular activity. By the 1820s, the Tripos, and the culture of "coaching" at Cambridge that had emerged around it, meant that questions set on the Tripos exam began to establish the standard for curriculum design (Warwick 2003).

[18] Of the three Cambridge participants present, only one was well-known outside of Cambridge. It was James Willliam Geldart (1785 – 1876), professor of law (Morrell and Thackray 1981, 85). The London contingent included only a small group of geologists, led by Roderick *Murchison*, president of the Geological Society the following year, the zoologists *John Gould* (1804-1881) and James Rennie (1787-1867) (Morrell and Thackray 1981, 87).

[19] Peacock's social involvements were another defining aspect of his life. As later Dean of Ely, he advocated for improved sanitation in the region's waterworks and a widening of access to basic education for children of the working poor (Pycior 1981, 25).

As with many of his contemporaries, however, Peacock was woefully aware of the implications that poorly planned curricula could have had on the youth of the day. Questions concerning mathematical methodology could not be distinguished from questions concerning moral education. Curriculum reform necessarily implied pedagogical reform, which implied a change to the standards and norms of Cambridge's "moral" and "liberal" education. Those norms were what had distinguished Cambridge and other British universities for centuries; preserving a place for them figured prominently in Peacock's deliberations over the proper method of introducing students to symbolical algebra. In his entry-level textbook, *Treatise on Algebra* (1830), Peacock's central question was not whether symbolical algebra was useful, but rather *how* and *when* it was right to introduce the new concepts to students.

As a young academic, Peacock had been attracted to symbolical algebra due to his interest in the problem of negative numbers and imaginary "quantities". His response to the problem of imaginary numbers had been to argue that the "symbolical algebraist"—i.e. the mathematician who was interested in using arithmetical operations (such as addition, subtraction, multiplication and division) in conjunction with symbols that represent unknowns—was free to play around with operations as he wished (Pycior 1976; 1981). The mathematician was free to assign arbitrary rules, invoke new meanings and construct innovative symbols to explore symbolical relationships without having to concern himself with physical reality. Peacock maintained that analysis is fundamentally conventional in nature, though not temporal. The symbols being operated upon do not need to represent identifiable, temporal quantities to gain their legitimacy. Peacock argued the value of algebraic results is to be found in the operations used, not in the quantities produced (Durand-Richard 2001). For Peacock, algebra was a logical endeavour—one that highlighted the abstract and universal nature of symbolical "equivalences". Not surprisingly, Peacock avoided geometrical representations of negative or imaginary numbers, as such representations were temporal and particular. In his view, they were irrelevant to the universalizing project embodied in symbolical analysis.

In his *Treatise*, Peacock clarified the matter further by establishing distinct boundaries around particular types of mathematical operations. He first identified "symbolical algebra" as the third step in a three-step process of Lockean abstraction. The first step involved arithmetic reasoning; the second step involved algebraic reasoning; the third step involved symbolical reasoning, whereby concerns over the physical meaning of symbols and their rules of manipulation faded away entirely. Peacock argued that arithmetical algebra corresponds to natural language and existent entities, while symbolical algebra marks an abrupt change in the style and content of reasoning. In symbolical reasoning, the mathematician focuses upon the operations at hand. The operations carry the procedure through and the meaningfulness of the results obtained is based entirely upon the logic of the operations used. The mathematician could abandon all concerns over the empirical significance of particular mathematical objects. The primary aim of the symbolical algebraist was to explore operational and relational truths.

In his *Treatise*, Peacock attacked the views of older mathematicians such as William Frend (1757-1841), a former second Wrangler, and Francis Maseres (1731-1824), Fellow of Clare College—two of the most vocal opponents of the use of the negative and imaginary numbers. In the opinions of Frend and Maseres, mathematics involved the manipulation of *quantities*. Quantities (or magnitudes) could not be negative or "imaginary" as no existent referents could ever be imagined for such mathematical objects. As Frend had written in his *Principles of Algebra* (1796),

> A number may be greater or less than another number; it may be added to, taken from, multiplied into, or divided by, another number; but in other respects it is very intractable; though the whole world should be destroyed, one will be one, and three will be three, and no art whatever can change their nature. You may put a mark before one, which it will obey; it submits to be taken away from a number greater than itself, but to attempt to take it away from a number less than itself is ridiculous. Yet this is attempted by algebraists who talk of a number less than nothing; of multiplying a negative number into a negative number and thus producing a positive number; of a number being

> imaginary. Hence they talk of two roots to every equation of the second order, and the learner is to try which will succeed in a given equation; they talk of solving an equation which requires two impossible roots to make it soluble; they can find out some impossible numbers which being multiplied together produce unity. This is all jargon, at which common sense recoils; but from its having been once adopted, like many other figments, it finds the most strenuous supporters among those who love to take things upon trust and hate the colour of a serious thought (Frend 1796-9, 10).

In his *Treatise,* Peacock addressed Frend directly, aligning himself with early-century advocates of symbolical mathematics, including Playfair, Woodhouse, and the former professor of rhetoric at the University of Edinburgh, William Greenfield.[20] Peacock argued the science of algebra had to distinguish between "arithmetical algebra" and "symbolical algebra". The distinction pivoted on the question of which entities could be dealt with in each respective branch of mathematical practice. In arithmetical algebra, the standard rules of arithmetic hold and the symbols representing unknown values are directly related to meaningful, existent quantities (i.e. "x" can stand in for apples or geometrical lengths). In symbolical algebra, a sort of "freedom" unknown to the traditional field of arithmetical algebra holds sway. The entities involved can include negative or imaginary "quantities", even though new rules become requisite for combining such entities. In those cases, the interpretation of the outcomes of symbolical algebraic operations can still be linked to meaningful processes in natural philosophy, though such linkages are not *necessary* to justify the processes in and of themselves.

In the preface to his second edition of the treatise, the two-volume *Treatise on Algebra* (1842/1845), Peacock acknowledged that his 1830 distinction between arithmetical and symbolical algebra had not had the effect on teachers of elementary algebra that he had hoped for. This was largely due to the imperfect development of the distinction he had advocated, he concluded. Thus, in the first volume, *Arithmetical Algebra* (1842), and the second volume, *Symbolical Algebra* (1845), he more profoundly highlighted the radical break he had intended to define a decade earlier. Peacock's aim in

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[20] For a detailed account of the role that these mathematicians played in defending the use of negative and imaginary "quantities" in the late-18th and early 19th-centuries see (Rice 2001).

the 1840s was to clearly identify symbolical algebra as a universalizing agent. He re-emphasized the epistemological and procedural differences between arithmetical and symbolical algebra, and he underlined the fact that symbolical algebra is dependent upon empirically justifiable operations in arithmetic, though not bound to the original empirical content of that arithmetic. In arithmetical algebra, he wrote, symbols *always* represent numbers, and the operations to which arithmetical symbols are "submitted" are the same as those operations used in common arithmetic. Thus, the signs - and $+$ denote the operations of addition and subtraction "in their ordinary meaning." In other words,

> ... in expressions such as $a + b$ we must suppose a and b to be quantities of the same kind; in others, like $a - b$, we must suppose a greater than b and therefore homogeneous with it; in products and quotients, like ab and $\frac{a}{b}$ we must suppose the multiplier and divisor to be abstract numbers; all results whatsoever, including negative quantities, which are not strictly deducible as legitimate conclusions from the definitions of the several operations must be rejected as impossible, or as foreign to the science (Peacock 1845, 1).

In arithmetical algebra, every symbol has a direct representative in terms of digits (i.e. positive, integer numbers). Furthermore, each combination or operation within arithmetical algebra results in another identifiable integer number. If the result is not a positive integer, then the operation itself and the symbols are "foreign" to the science.

In symbolical algebra, on the other hand, the rules of arithmetical algebra apply, but without the concomitant restrictions. Indeed, symbolical algebra "removes altogether [the] restrictions" of arithmetic. Peacock contended,

> Symbolical subtraction differs from the same operation in arithmetical algebra in being possible for all relations of value of the symbols or expressions employed. All the results of arithmetical algebra which are deduced by the application of its rules, and which are general in form though particular in value, are results likewise of symbolical algebra where they are general in value as well as in form; thus the product of a^m and a^n which is a^{m+n} when m and n are whole numbers and therefore general in form though particular in value, will be their product likewise when m and n are general in value as well as in form; the series for $(a + b)^n$ determined by the principles of arithmetical

> algebra when n is any whole number, *if it be exhibited in a general form, without reference to a final term,* may be shown upon the same principle to the equivalent series for $(a + b)^n$ when n is general both in form and value (Peacock 1842, vi).

For Peacock, the universality and generality of symbolical algebra constitutes its strength as a method. In focusing on the irrelevance of the precise quantities of things represented by the symbols, Peacock forced an ideological point upon his readers. Analysis includes the abstract manipulation of unknowns. The manipulation and the expansion of mathematics can take place without regard for physical reality. Operations are what analysts consider regardless of the existence of the mathematical objects being related. As Peacock concluded,

> In the further development of this science, we shall continue to be guided by the same principle, making the results of defined operations, or the rules for forming them, the basis of the corresponding operations and results in Symbolical Algebra, and also of the interpretation of the meaning which must be given to them, whenever such interpretation is practicable (Peacock 1845, 59).

The principle underlying this discussion is what Peacock termed the "principle of the permanence of equivalent forms." The "principle of the permanence of equivalent forms" is the basis of Peacockian symbolical algebra. It can be stated as follows:

> Whatever algebraic forms are equivalent, when the symbols are general in form but specific in value, will be equivalent likewise when the symbols are general in value as well as in form (Peacock 1845, 59).

Thus, from Peacock's point of view, what remains constant when transitioning from an arithmetical to a symbolical mode of thinking is reliance upon the *operations* in question. If the operations used are well-defined in arithmetic, then they remain well (and legitimately) defined in symbolical algebra, despite the fact that the entities they operate upon may or may not "exist" in the symbolical algebraic case. Peacock made sure to appease elementary teachers and geometrically-inclined professors by qualifying the "freedom" of symbolical analysis. It is, he noted, a freedom dependent upon the empirical truths of arithmetical algebra from which it is originally derived. Yet, Peacock also emphasized the special universalizing nature of abstract symbolical thought. In so doing, he legitimated the professional actions of those mathematicians who had chosen to engage in analytical practices and who had abandoned geometrical proofs as the standard of reasonability.

Critical responses to Peacock's "principle"

The "principle of the permanence of equivalent forms" set off a philosophical debate within varied professional circles over the nature of mathematical reasoning. Three brief case studies suffice to characterize the responses that flourished in the wake of Peacock's definition of the "principle". The first is that of De Morgan; the second is that of Charles Dodgson (1832-1898) (also known as Lewis Carroll); and the third is that of George Boole (1779-1848). De Morgan's approach to mathematical knowledge stemmed from an explicitly non-sectarian perspective. In the Tripos of 1827, De Morgan graduated Fourth Wrangler. He never proceeded to the M.A. degree because of his religious objections to the examination process. Having left Cambridge, he initially pursued legal studies, and was only coaxed back into mathematical research when the London University (later University College) opened in 1828. De Morgan was a Dissenter by upbringing. When he was offered the London University's first Professorship of Mathematics, he accepted the post based upon his support for the institution's non-sectarian stance (as well as its proposal to not base its studies on Cambridge-style examinations) (Richards 1992, 55). That institutional support changed in 1866, when De Morgan accused the university's council of refusing a candidate for the chair of Logic and Mental Philosophy due to the candidate's religious credentials. As a direct result of the council's disregard for De Morgan's concerns, De Morgan resigned from his post following nearly 40 years of service (Ranyard 1871, 409).

Throughout that period, De Morgan's attitude toward symbolical algebra shifted in a number of important ways. Historians have been divided over whether De Morgan was a proto-"formalist", because of his initial activities with the early agitators in symbolical algebra (Nagel 1935), or whether he was an opponent of the new philosophy of mathematics, given his later equivocations on the usefulness of symbolical algebra (Richards 1980). Pycior (1983) has captured De Morgan's varying moods best in categorizing his work into three stages. The first stage begins in the late-1820s, when De Morgan defended the traditionalist, British view that mathematical knowledge constitutes irrevocable *à priori* truths that are self-evident in nature. Negative and imaginary quantities posed something of a problem for De Morgan. His equivocation on the topic led him to adopt an ambiguous position with regards to symbolical algebra. In his "Introductory Lecture Delivered at the Opening of the Mathematical Classes in the University of London" (1828), and in his *The Study and Difficulties of Mathematics* (1831), De Morgan viewed mathematical knowledge as self-evident and rational. The presence of negative and imaginary numbers, which go against the self-evidence of mathematical truth, could be explained away only by an appeal to particular empirical case studies. Negatives, for instance, could be associated with debts or loses of income. De Morgan thus worked to generate meaningful accounts of mathematical entities such as $-a, a^{-x}$, and $\sqrt{-a}$ (Pycior 1983).

Peacock's 1830 *Treatise* changed De Morgan's views on these matters. The *Treatise* provided De Morgan with the resources required to shape a formidable response to his earlier concern over the status of particular mathematical entities. Thus, in his second "stage" of engagement with symbolical algebra, De Morgan used Peacock's philosophical division between arithmetic and symbolical to furnish a means of teaching foundational lessons in morality and logic, while also supporting the move towards advanced algebraic and analytical techniques. In his 1835 review of Peacock's *Treatise*, De Morgan argued that mathematics is a revisable field of study. Its principles are not axiomatic. Rather, they are based upon investigation and trial-and-error. De Morgan published his review in the *Quarterly Journal of Education,* a journal set up by the Society for the Diffusion of Useful Knowledge (published between 1831 and 1835). In it, he defended the idea that symbolical algebra was not a science based upon identifiable magnitudes or geometrical entities. Rather, it was based upon the investigation of operations and relationships (De Morgan 1835). De Morgan had even begun to advocate for a level of mathematical generalization that went beyond what Peacock had proposed.

Recall that, for Peacock, arithmetical algebra (i.e. the science of "quantity" and "magnitude") still formed the basis of mathematical extension and abstraction. For Peacock, the laws of arithmetic dictated the laws of algebra. For De Morgan, the rules of arithmetic constituted no limit to the laws of algebra. Algebra could invoke entirely new rules in order to investigate new relationships and operational procedures. De Morgan's criticism of Peacock went to the point of arguing there is no clear link between arithmetic and algebra at all. Rather, algebra should be seen as entirely independent of arithmetic and subject to *differing* rules of operation. Peacock's principle could be done away with (De Morgan 1835, 300-304). Of particular importance is De Morgan's claim that basic rules in arithmetic, such as commutative and associative principles, might not be needed in certain algebraic systems. In problems of dynamics, for instance, descriptions of rotation might invoke symbolical rules whereby combining $(+)$ and $(+)$ or $(-)$ and $(-)$ produces a $(-)$ result, or whereby combining $(+)$ and $(-)$ or $(-)$ and $(+)$ produces a result $(+)$ result, contrary to common arithmetic. He claimed,

> The hypotheses, the meaning of the symbols, however laid down, are in our own power: subject only to the great rule of all search after truth, that nothing is to be asserted as a conclusion, more than is actually contained in the premises (De Morgan 1835, 99).

For De Morgan, the speculative aspect of symbolical algebra was now relevant to a liberal education as it taught students to engage in "hypothetical reasoning".

By the late-1830s, however, De Morgan had faced difficulties in teaching this approach to his entry-level students at UCL. He thus began to devote himself to the elaboration of a third "stage" of engagement with symbolical algebra, in which he focused on developing the physical and geometrical analogues to symbolic forms and operations. Whewell's diatribes against the immoral nature of teaching young students symbolical algebra also caused De Morgan a great deal of concern.[21] He thus came to qualify his previous accounts by adopting an equivocating attitude. De

[21] By the 1830s, Whewell had distinguished between advocacy of permanence in "liberal" education and the materialism and temporality inherent in analytical and algebraic thinking. In his *Thoughts on the Study of Mathematics, as a Part of a Liberal Education* (1835), he contended that geometrical reasoning was superior from an educational point of view to

Morgan never repeated his wholehearted endorsement of the investigative and hypothetical nature of symbolical algebra, but neither did he abandon his endeavour to develop the field further. Rather, De Morgan encouraged students to seek out greater meaning in "applied mathematics" and he disseminated his newly pragmatic views through a series of lectures delivered from 1839 to 1844 to his students at UCL. Entitled "On the Foundation of Algebra," his lectures maintained that algebra could be divided into two branches: the technical (i.e. symbolical algebraic) and the logical. Technical algebra is not a "science", De Morgan argued; it is an exploratory "Chinese puzzle" and it is sometimes even "useless" (De Morgan 1842, 289). On the other hand, "logical" algebra (also called "double" and "complete") constitutes meaningful science.

In his *Trigonometry and Double Algebra* (1849), De Morgan developed his so-called "double algebra" further, by defining it as the algebra of imaginary numbers. The adjective "double" stemmed from the fact that such algebras involved two components: length and direction. De Morgan stated his goal as,

> The construction of Algebra upon a basis which will enable us to give a meaning to every symbol and *combination of symbols* before it is used, and consequently to dispense, first, with all unintelligible combinations, secondly, with all search after interpretation of combinations subsequent to their first appearance (De Morgan 1849, 89).

In his later works, De Morgan appears to have cut ties with Peacock's "principle of the permanence of equivalent forms." This is perhaps not surprising given that De Morgan was struggling within an institution that relied heavily upon the teaching of applied mathematics in order to produce technically-trained men for the urban world of London.

De Morgan's criticisms of Peacock's "principle" stemmed largely stems from his pragmatic desire to reform the educational framework within which he had to teach applied mathematics. Charles Dodgson, on the other hand, was a more abstract critic of Peacock's. Dodgson emphasized the truth

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that of algebraic reasoning, because it required "more thinking" than the mere application of memorized operational rules (Whewell 1835, 42). Whewell was also concerned that the labour-saving mechanisms of algebraic reasoning took away from the human toil and labour that Christians ought to perform throughout their lifetimes.

of pre-established logical rules, and he sought to petrify the educational system according to the methodologies favoured by those university practitioners dominant at the end of the 18^{th}-century. Dodgson's response was a fundamentally conservative one. The Oxford mathematician and logician possessed a well-informed opinion of algebra. He learned about Peacock's 1830 *Treatise* through Bartholomew Price, a fellow at Pembroke College and Dodgson's former mathematics tutor (Pycior 1984, 161). Price leaned toward an interpretation of mathematical meaning that underscored geometrically-defined referents. Dodgson's main criticism of Peacock's system emerged in the 1850s in his correspondences with Mary Dodgson, in which he expressed skepticism over the logical nature of extending words such as "multiplication" from particular known quantities to general unknown variables. Dodgson's sentiments were also expressed in Alice's behaviour in Lewis Carroll's storybook publications. Operating within a tradition in mathematical humour (a common pastime; both Frend and De Morgan had published satirical and humorist accounts of mathematical problems in non-technical publications, such as the *Athenaeum* and *The Lady's Diary: or the Woman's Almanac*), Dodgson issued his reactionary views through the silly musings of his now-famous Alice character. In *Alice in Wonderland,* the Mad Hatter argues it is impossible to subtract something from nothing. And while at the tea party, when the March Hare asks Alice whether she would like more tea, Alice responds by saying she has "had nothing yet [and] so ... can't take more" (Pycior 1984, 164). Dodgson's popularly-reflected criticisms indicate the degree to which Peacock's philosophy of generalized symbolical analysis, while popular among practitioners at Cambridge, nonetheless evoked controversy beyond the borders of that institution.

As De Morgan wavered and Dodgson attacked, Peacock did receive wholehearted support from a group of young Cambridge students who had studied for the Tripos throughout the 1820s and 1830s. Peacock's work was crucial to their development of a specific field of symbolical analysis as advocated by Duncan Gregory (1813-1844), and which became known as the "calculus of operations". As a young Cambridge graduate, Gregory had become interested in *operations* as the fundamental component of algebraic thinking through his exposure to the Cambridge Philosophical Society. In his article, "On the real nature of symbolical algebra" (1840), Gregory defined symbolical algebra as the science that explores combinations of operations that are not defined by their nature (i.e. what they are or what they do) but rather by the laws to which they are subject (i.e. the rules that govern their combinations) (Gregory 1840). Gregory also related symbolical algebra to geometric interpretability. Although in arithmetic a and $+a$ are isomorphic, in geometry they might not be. Therefore, a might indicate a simple magnitude or quantity, while $+a$ might indicate a magnitude and a direction. Gregory contended that mathematicians could free themselves from "prejudice" if they avoided symbols that already had definite meanings associated with them, so that they could push even further with the generalization of operational equivalences.

Based on Gregory's Peacockian-inspired work, a group of thinkers coalesced to form a nascent group of symbolical logicians in Britain in the early-1840s. Spearheaded by the British mathematician and logician George Boole (1815-1864), research in symbolical logic emerged as a mid-century manifestation of Peacock's "principle of the equivalence of permanent forms." Richard Whately, author of *Elements of Logic* (1826) and George Bentham, author of *An Outline of a New System of Logic* (1827) had already established logic as a domain worthy of study in Britain, in particular at Oxford. But it was through the work of Boole that symbolical algebra began to shape debates over the formation of a new *kind* of logic—one inspired not by scholastic syllogism, but rather by abstract symbols, the analysis of operations, and the formalization of operational rules. In his pamphlet, *The Mathematical Analysis of Logic* (1847), Boole assigned a primary role to "operation" including a clear dedication to both Gregory and Peacock. For Boole, the symbolical algebraic philosophy of Peacock and Gregory had hearkened a new era in mathematical and logical freedom—one in which "true Calculus" could be characterized by its "method". As Boole argued,

> The expression of magnitude, or of operations upon magnitude, has been—the express object for which the symbols of Analysis have been invented, and for which their laws have been investigated. Thus the abstractions of the modern Analysis, not less than the

> ostensive diagrams of the ancient Geometry, have encouraged the notion, that Mathematics are essentially, as well as actually, the Science of Magnitude ... This conclusion is by no means necessary. If every existing interpretation is shewn to involve the idea of magnitude, it is only by induction that we can assert that no other interpretation is possible. And it may be doubted whether our experience is sufficient to render such an induction legitimate. The history of pure Analysis is, it may be said, too recent to permit us to set limits to the extent of its applications. Should we grant to the inference a high degree of probability, we might still, and with reason, maintain the sufficiency of the definition to which the principle already stated would lead us. We might justly assign it as the definitive character of a true Calculus, that it is a method resting upon the employment of Symbols, whose laws of combination are known and general, and whose results admit of a consistent interpretation. That, to the existing forms of Analysis a quantitative interpretation is assigned, is the result of the circumstances by which those forms were determined, and is not to be construed into a universal condition of Analysis. It is upon the foundation of this general principle, that I propose to establish the Calculus of Logic, and that I claim for it a place among the acknowledged forms of Mathematical Analysis, regardless that in its object and in its instruments it must at present stand alone (Boole 1847, 4).

Mathematics is not dependent upon number, quantity, or magnitude, Boole argued. Rather, it is a conventional and logical language.

Boole's follower, William Stanley Jevons (1835-1882) institutionalized that view more profoundly in the second half of the century (Durand-Richard 1991). Boole and Jevons's joint pursuit of a Peacockian-inspired formal language of logic in mid-Victorian Britain did eventually lead to a backlash. Neo-empiricists, inspired by Victorian interpretations of Humean skepticism, were disturbed by Boole's newly proposed system of universal logic. John Stuart Mill (1806-1872), Alexander Bain (1818-1903), and Clifford himself reacted against the establishment of a completely distinct and empirically unfettered domain of absolute truth statements (for an account of the German context, within which similar debates arose see Peckhaus (1999)). Thus, Peacock's "principle of the permanence of equivalent forms" generated both deep-rooted opposition as well as hearty support throughout the remainder of the 19th-century. While Peacock's legacy lies in influencing particular developments in symbolical analysis and formal logic, through supporters such as Gregory, Cayley Boole and Jevons, it also lays in influencing the reactive responses of empirical geometers and epistemological critics, such as Mill and Clifford, the latter of whom appealed to evolutionary theories as a reason for why operational truths were, contrary to Peacock's claim, temporal in nature rather than universal.

Conclusion

Symbolical algebra was discussed, debated and institutionalized in various forms throughout the 19th-century. For many mathematical actors in Britain, some version of "symbolical algebra" came to constitute part of the "terrains of knowledge" through which they navigated in their Victorian practices. Governed by practitioners at the University of Cambridge, and standardized by the norms of the Cambridge Philosophical Society and the *Cambridge Mathematical Journal*, symbolical algebra came to serve as a standard by which Hamilton judged his own work on quaternions in the 1850s. It also came to define a particular curriculum, which had excised all experimental and "unfounded" sciences, for Tait in the same decade. And it came to include the works of mid-century symbolical algebraists, such as Cayley, for Clifford in the 1860s.

In brief, symbolical algebra came to constitute both a methodological approach towards mathematical practice, in that it required mathematics to be succinct, symbolical, and unconcerned by empirical or geometrical meaning, as well as a metaphysical view about the nature of mathematical equivalences as universally true. Mathematics became a matter of operational procedure rather than a matter of magnitude measurement. Conventional in nature, and expressed controversially in Peacock's "principle of the permanence of equivalent forms," the symbolical algebraic "philosophy" created the basis for the specific, mid-century "terrains" that would manifest themselves in Tait and Clifford's lives, and which would imbue their navigations with meaning.

We turn now to an account of Hamilton's efforts in the 1850s to recast his mathematical research, and his quaternions in particular, as a manifestation of Britain's symbolical algebraic tradition.

Chapter Two: Hamilton's mid-century quaternions as symbolical algebra

Introduction

Hamilton's quaternion system formed an important artifact that both Tait and Clifford encountered in their mathematical engagements in the second half of the 19^{th}-century. Volumes of material have been written on Hamilton's quaternion developments. Consider Crowe (1967), Hankins (1977; 1980), and more recently Schlote (1997), Flament (2003), and Lewis (2005). The aim here is to diverge somewhat from those accounts by not placing Hamilton *at the beginning* of a tradition in vector analysis. Rather, if one stops the clock at 1853, it is obvious that Hamilton was an actor who was firmly situated within a tradition that was already decades-old—a tradition that he responded to in order to establish himself as a dominant mathematician within the wider British context. Hamilton navigated through the terrains of Protestant science in Ireland as well as Peacockian-inspired symbolical algebra in Britain, as he sought to develop, disseminate, and legitimate his quaternion mathematics by situating his work within a realm of Cantabrigian practices. In so doing, he sought to subtly redefine symbolical algebra as a craft that allowed for the symbolical representation of intuitive, universal truths.

In the 1840s, Hamilton identified his work exploring "couples" and "triplets" as having been geometrically motivated—that is, as having been motivated by geometrical considerations related to directed magnitudes. However, Hamilton's "Preface" to his 1853 *Lectures on Quaternions* openly declares an allegiance to a version of symbolical algebra in which geometrical analogues to symbolical operations are no longer necessary. The institutionalization of symbolical algebra in Cambridge's Tripos system, its propagation within the Cambridge Philosophical Society, and its dominance in mathematical research presented at the British Association for the Advancement of Science created an environment through which Hamilton found himself having to navigate in the 1850s. Unable to deny the predominance of Peacock's "principle of the permanence of equivalent forms," Hamilton sought to recast his own work in light of that algebraist's claims. His choice to do so was motivated by his need to develop and publish original research so as to satisfy his administrators at Trinity University, Dublin, and by his desire to gain grander recognition for both Irish science in general, and his mathematical work in particular, among the emerging class of symbolical algebraic practitioners then dominant at Cambridge. "Irish science" and "symbolical algebra in the mid-century" constituted two of the "terrains of knowledge" that must be described in order to explain why Hamilton chose to present his quaternions in the manner that he did in his 1853 "Preface".

Protestant science in Ireland

One of Hamilton's most memorable contributions to British science as Royal Astronomer of Ireland and Professor of Astronomy at Trinity College was his fêted "prediction" in 1832 of canonical refraction using Fresnel's wave theory of light. Despite its laudatory reviews, canonical refraction was not that important of a phenomenon. It added little value to studies in optics, and it was not significantly discernable from other similar developments in the field at the time. The Cambridge physicist, Gabriel Stokes (1819–1903), argued that the prediction of canonical refraction did not even justify a necessary belief in *Fresnel's* wave theory of light. Hamilton's prediction could have been garnered by mathematically analyzing the corpuscular theory of light (Hankins 1980, 95). In a letter to Herschel, Hamilton admitted his finding did not "essentially require the adoption of either of the two great theories of light in preference to the other." Hamilton's prediction did, however, earn him special status as someone who proved the ability of abstract mathematical reasoning to predict accurate physical events. As Whewell exclaimed in his opening address to the BAAS in 1833, "In the way of such prophecies, few things have been more remarkable than the prediction [of canonical refraction]" (Attis 1997, 19).

Hamilton had long believed that certain and absolute knowledge was not empirically induced, but rather deduced from *à priori* starting principles. For Hamilton, the Fresnel wave prediction constituted an early justification of his Idealist outlook. Yet, Hamilton's motivations in pursuing Fresnel's wave theory were broader and more political than a simple pursuit to justify a German-inspired Idealist philosophy. Hamilton chose to work on the particular topic of Fresnel's wave theory because he hoped it would justify and legitimate Irish science in the eyes of English mathematicians at Cambridge. Hamilton's engagement with the Protestant Ascendancy in Ireland also meant that he was constantly engaged in using natural philosophy (or "science") as an intellectual weapon to defend the independently productive nature of the Irish Protestant population. It was this primary concern that motivated Hamilton's research in an area of optics that was relatively unimportant, but which served to situate him within the right school of thought, at least in so far as Cambridge mathematics was concerned.

Wave theories of light were largely promulgated and supported by Cambridge mathematicians and natural philosophers, including Herschel, George Airy (1801-1892) and Humphrey Lloyd, for whom the wave theory "represented an ideal of a mathematical, predictive science in which abstract theories are confirmed by experiment" (Attis 1997, 22). Dublin-based scientists followed the program initiated by those Cambridge mathematicians. At the opposite end of the theoretical spectrum were those advocates of corpuscular theories of light, who were largely located at Scottish Presbyterian institutions. They included Sir David Brewster (1781-1868), the physicist, mathematician and natural philosopher, and Henry Brougham (1778-1868), the Scottish lawyer and later British parliamentarian. By the mid-century, appeals to English and Cantabrigian scientific traditions were deeply interwoven with the an Irish Protestant intelligentsia's keen desire to institutionalize its scientific outputs and thus help to enshrine Protestant governing control over Ireland more generally. Hamilton's engagement with Fresnel's wave theory was representative of that religious allegiance. Many of the corpuscular theorists of the north, including Brewster and Brougham, were religious Dissenters with profound ties to the Whig party. They advocated for Benthamite utilitarianism in both politics and education. Proponents of the wave theory, on the other hand, were predominantly Tory. The notion of utilitarianism was anathema to their élite university-based knowledge. And although not all Cambridge mathematicians were Tories (indeed, many actors within the early symbolical algebraic network were openly Whig or Whig-sympathizers), the Whig politics of Cambridge were different in content from the Whig politics of Scotland. Recall that young members of the Cambridge symbolical network had supported the democratization of access to mathematical knowledge. After all, symbolical algebra was meant to expose mathematical inquiry to the industrial middle-classes, allowing mathematicians to gaze upon universal relationships without the mediating effort of any particular religious or institutional authority. Yet, Peacock was by no means a Dissenter, and Babbage and Herschel's French-inspired republican efforts in British mathematics constituted a project aimed at advancing the English middle-class and disempowering the English aristocracy. The young Cambridge analysts did not equate their London-centric middle-class goals with the aims of the emerging, industrial middle-classes of the Clydeside and Edinburgh. Recall, for instance, that several English algebraists failed to openly support the BAAS in its opening years due to its overly provincial character and the locations of its meetings (see Chapter One). In addition, although De Morgan joined the Society for the Diffusion of Useful Knowledge, even he argued that because a university education was meant to teach students rationality and morality, it was not meant for all segments of the population (Phillips 2005; De Morgan 1830). Certainly, many of the symbolical algebraists aimed for an opening up of access to education, but few expected their efforts to succeed in delivering wide-ranging social services, such as free university education, which were explicit goals for northern Whigs, such as Brougham and Brewster (Marsh 2003; Davie 1964).

Consider also the differences evident in pedagogical philosophy between reformist Cantabrigians in the 1830s and their contemporaneous northern Whig colleagues. At Cambridge, Adam Sedgwick (1785-1873), a political Whig and reformer, opposed religious tests. In his *Discourse on the Studies of* *the University* (1833)[22], he argued inductive mathematical philosophy could help to reveal the mind of God and point towards his creative acts. His views on these matters were in line with the empirical views of the Glaswegian mathematician, James Thomson (1786–1849), father of William Thomson (later Lord Kelvin). However, the northerner and southerner diverged significantly with regards to the final objective of those inductive studies. For Sedgwick, the primary aim of university education was not to promote social equality. It was, rather, to improve upon the rational and moral capacities of youth. Sedgwick claimed,

> Studies of this kind [do] not merely contain their own intellectual reward, but give the mind a habit of abstraction, most difficult to acquire by ordinary means, and a power of concentration of inestimable value in the business of life (Sedgwick 1834, 11).

By referring to the "power of concentration" in the "business of life", Sedgwick did not have in mind the entrepreneurial business of life. In fact, he was opposed to any utilitarian approaches to pedagogy or educational reform. Sedgwick opposed, for instance, the efforts of the Society for the Diffusion of Useful Knowledge, which had been founded in London in 1826 by a group of Whig supporters led—not surprisingly—by Lord Brougham. That society had close ties to UCL and to various mechanics institutes in London. Its aim was to provide cheap versions of scientific textbooks to those students who were unable to afford a formal education. Such "societies" did not promulgate the sort of élite educational ethos that a clergyman such as Sedgwick supported (Smith and Wise 1989).

For Sedgwick, as for Whewell (who was, by then, a much more conservative Cambridge proponent of mathematical education), the "business of life" referred to the proper moral and intellectual edification of young, middle-class men.[23] Mathematical training was part of that "liberal" education.

[22] This text was first delivered as a sermon in Trinity College Chapel in 1832.

[23] It is important to underscore the fact that the moral view of a Cambridge education extended beyond the particular beliefs of Whewell and Sedgwick. Writing home to his father in 1841, the young William Thomson deprecatingly recounted his first "classical lecture" at St. Peter's College: "Today we had the first classical lecture, or rather the introduction to the classical lectures. The lecture (by Freeman, who is transplanted from Trinity) was a rather curious one. He told us good deal about the university's ideas of education, as opposed to the modern diffusion-of-useful-knowledge-Societies' ideas; that the idea of education was not as a collection of useful facts, so much as of a training and strengthening of the mind. That now we have in a manner given our consent to her dogma (not expressed in any

Thus, despite the stated aims of some of the early members of the symbolical algebraic network, for whom symbolical thinking and mathematical speculation served as a metaphor for industrial efficiency, the mathematical curriculum at Cambridge in the 1830s still aimed to elevate a select group of English men, rather than a cross-section of industrializing British society. In addition, for moralizers, such as Sedgwick and Whewell, the Cambridge curriculum, and its embodied mathematical reforms, produced moral men who were trained to submit to proper authority structures. In the north, on the other hand, latitudinarian reformers, such as James Thomson, pursued broader educational objectives. For Thomson, university education was an inseparable part of industrial and urban life. Its aim was to alleviate poverty, decrease suffering and counteract toil. This utilitarian aspect of mathematical training was manifest in Thomson's Glaswegian classroom throughout the 1830s. As Smith and Wise have concluded, "A gulf indeed existed in the 1830s between the Cambridge disdain for economic man and the interest of the industrial entrepreneurs of Clydeside" (Smith and Wise 1989, 61).

Along with his Cantabrigian colleagues, and in opposition to many Scots at the time, Hamilton shared a strong distaste for utilitarianism and its underlying political message of democratization (Graves 1882). Hamilton's engagement with Fresnel's wave theory must not be viewed, therefore, as the simple outgrowth of his "mathematical" interests. Rather, "Hamilton's compromise" must been seen in the "context of Anglo-Irish relations" in which the Irishman desired "to prove the Irish capable of great scientific accomplishments." As one historian has argued, Irish Protestants largely ignored the culture of the "native" Irish, and followed cultural standards emanating from England. "They felt their ascendency over the Catholics was cultural as well as political," and Trinity College, Dublin was the main institution through which that cultural and intellectual ascendency was expressed. For people like Hamilton, an "entire way of life depended on Protestant Ascendency in

decree or book, but indicated in her system) and so we should give it a fair trial by being good boys and attending lectures punctually. That the ultimate object of the lectures was not altogether for the examination, and not merely for annoyance (though perhaps that might be its immediate object) but to carry out the university's idea of education, and to ensure at least one hour a day being spent properly and according to her ideas" (Smith and Wise 1989, 58).

Ireland, and their intellectual role was to support that ascendency by keeping English culture and Anglican theology alive" (Attis 1997, 28).

Hamilton's efforts to legitimate Irish scientific contributions worked. By the mid-1830s he was lauded throughout England by Whewell and Babbage—the latter of whom used Hamilton's "prediction" that a single ray of light could be refracted into a cone of light within a biaxial crystal to argue that symbolical analysis can predict empirical phenomena (Babbage 1989). Hamilton was knighted by the Lord Lieutenant at the BAAS meeting in Dublin in 1835, and he was awarded the Royal Medal of the Royal Society for "discoveries in Optics, and particularly that of Canonical Refraction" (Attis 1997, 19). Meanwhile, Hamilton continued his engagements with religious projects in Ireland. From 1837 to 1846, he served as president of the Royal Irish Academy, during which time he sought to develop wide-ranging projects related to Irish history and culture (Lewis 2005, 461). Hamilton's efforts to develop an algebraic system of couples, followed by his efforts to develop a system of triplets, must be understood as the product of his navigations through a terrain in which Irish Protestantism had to prove its worth to a Cambridge-dominated English audience.

As a corollary to his Protestant-minded mathematical projects, Hamilton's adherence to "Kantianism" is a subject that has generated no small amount of historical and philosophical speculation (see Hankins 1977; Flament 2003). Studies that have explored this aspect of Hamilton's work have all too often ignored the political hues of Hamilton's metaphysics. Hamilton's Kantianism related to his emerging identity as a "Cambridge"-style analyst in important ways. Kantianism had become influential in certain social circles in England throughout the 1820s. German Idealism had become the focal point for discussions in the Apostles Club at Cambridge over the course of the following decade.[24] Flowing through the conduit of Samuel Coleridge (1772-1834), who, though not

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[24] A flourishing of interest in the relationship between mathematics, science and theology blossomed in the formation of German idealist social gatherings in Britain. Aided by the socializing and publicizing efforts of Madame de Stael and Thomas Carlyle (1795-1881), German Idealism became the subject of conversation among an élite group of English literati. Though wide-spread interest was temporal, the German Idealist trend did help to generate a more permanent

a member of the Apostles Club was one of the Club's key experts on Kant, Kant's *à priori* synthetic account of knowledge manifested itself in Hamilton's algebraic system of couples, which Hamilton labeled the "Science of Pure Time." For Hamilton, Kantianism was a means of making contact with powerful social and political actors in England. Later, it would serve as a flexible and open belief structure that provided him with a foundation upon which he could reconcile the symbolical algebraic approach then favored in Cambridge with his own geometrical work on "couples".

Hamilton as an algebraist of a particular sort

Authors such as Nagel (1935) and Novy (1973) have traditionally argued that Hamilton rejected symbolical algebra from the outset, while his contemporaries, including De Morgan and Gregory, adopted it in unique ways. In so far as Hamilton did reject symbolical algebra, he was not alone. Recall from Chapter One that the actuary and religious critic, William Frend, had been a long-time opponent of symbolical methods. Frend viewed the Cambridge network of practitioners as a group of mathematicians intent on *attacking* liberal thought. Hamilton was also joined in oppositional ranks by the young Cambridge Wrangler, and later Queen's College fellow, Osborne Reynolds, who thought the methods of symbolical reasoning institutionalized a bizarre and unjustifiable form of metaphysics. Reynolds even went so far as to publish an anonymously authored text, entitled

following among a select group of Cantabrigian and Oxfordian students. Undergraduates and fellows at Trinity College organized in the 1820s to teach and discuss German Idealism, contemporary poetry and modern science within a Christian context (Cannon 1964). The upshot of those gatherings was the creation and formalization of the "Apostles Club"—a discussion group that included an eclectic range of practitioners, such as Frederick Denison Maurice (1805–1872), the Church of England clergyman mostly remembered for his later "Christian Socialism," Richard Trench (1807–1886), the Church of Ireland archbishop of Dublin (mostly remembered for his active involvement in reforms within the Irish Church); John Kemble (1807–1857), the Benthamite, Millian, and later Cambridge linguist; John Sterling (1806–1844), the writer and poet and Alfred Tennyson (1809–1892), the Queen's poet laureate in later years. Among those Apostles, as with the bulk of Trinity's students, religious crises erupted not over "the Real Presence or Apostolic Succession, but over the application of Neibuhr's anti-mythical methods to the Bible and to Christian tradition generally," as one historian has argued (Cannon 1964, 78). "Interest in all things German" radiated from Trinity in the 1830s. One publisher even issued a student's cram-book on Idealism "so strong was the need 'to Niebuhrize,' as it was called" (Cannon 1964, 77). Though Niebuhr had actually been considered a "conservative" in his native country, his readers at Cambridge managed to reshape his works into a symbol of liberalism and reform. Apart from Niebuhr, other German Idealists were also fêted by this select group of British élite. Those philosophers included Immanuel Kant (1724-1804) and Frederick Schelling (1775-1854). Interest in German philosophy also extended to include Friedrich Schleiermacher (1768-1834), as well as the poets Johann Wolfgang von Goethe (1749-1832) and Johann Schiller (1759-1805). Those philosophers enjoyed a period of deep revival at Cambridge, as the Apostles sought to apply the Germans' views to current mathematical and natural philosophical developments. The symbolical algebraists' efforts jibed at times with those of the Apostles Club (some of whose members crossed over).

Strictures on Certain Parts of Peacock's Algebra (1837), in which he attacked Peacock for his ambivalence and vagueness with regards to symbols themselves (Pycior 1982). Reynolds contended that Peacock had defended two different definitions of "symbols". The first stated that symbols could be entirely "arbitrary"; the second stated that symbols represented "nothing at all". Maintaining both definitions was a contradictory position to find oneself in, Reynolds argued, because "nothing at all" could not also represent "anything at all," even if that "anything" had been arbitrarily chosen. Reynolds denied that symbolical algebra made any sense given that "nothing at all" could not be operated upon, and, if the symbols in question were representative of "anything at all" (rather than of "nothing"), then the burden of proof was on the mathematician to demonstrate empirically that similar operations on an infinite number of different types of things necessarily produced similar effects (Reynolds 1837).[25]

Historians have been right to highlight that Hamilton joined the oppositional ranks of people such as Reynolds, though this author would argue that alliance was short-lived and highly qualified. While it is the case that Hamilton initially rejected "symbolical algebra" (as advocated in Peacock's original *Treatise*), his views shifted by the late-1840s. In fact, if we give any credit to Hamilton's account of his own mathematical journey as it evolved from the mid-1840s to his 1853 publication of *Lectures on Quaternions*, we must view him as something of a converted believer in, or at least a converted *user* of, symbolical algebra. To understand why this is the case, let us return to Hamilton's earlier engagements with Coleridge's Kantian-inspired metaphysics. The degree to which Hamilton's Kantian predispositions shaped his later mathematics has been recounted by authors such as Crowe (1967), Hankins (1977), Bloor (1981), and Winterbourne (1982).[26] Those historical studies have often focused on the fact that Hamilton was relatively non-productive between 1838 and 1842. Due in

[25] Reynolds was no peripheral radical, either. His criticisms were recollected positively by the eventual editor of the *American Journal of Mathematics,* James Joseph Sylvester (1814-1897), who noted that Peacock had delayed his second edition of the *Algebra* in light of Reynold's insightful criticisms (Pycior 1982, 406-407).

[26] Crowe (1967) has argued that Kantianism was a legitimacy tool rather than a constitutive element of Hamiltonian mathematics. On the other hand, Hankins (1976) has claimed that Hamilton's paper on conjugate functions (i.e. ordered pairs of numbers) is the product of a long-running interest in both the representativeness of imaginary numbers and in the determination of a general expression for a logarithm of a complex number taken to complex base.

part to the illness of his wife, and due in part to his ensuing alcoholic dependencies, Hamilton produced little mathematical research over that four-year period. He did, however, manage to find the time to produce a series of tracts on philosophical matters. He wrote about "triads" of philosophical concern, where the "triad" of particular concern to himself was that of Will, Mind, and Life—a conceptualization he first located in Coleridge's writing. Hamilton's later development of quaternions was influenced by this Kantian-Coleridgean outlook, which suggested to him a mathematical discourse of "triads" (or "triplets") and which motivated his hunt for algebraic triplets (i.e. a system of three ordered numbers represented by triple coordinates x, y, z) (Hankins 1977).

Even more importantly, however, is the fact that, by the 1830s, Hamilton had focused his energies upon elaborating the notion of Kant's *à priori* synthetic category of knowledge to legitimate his own use of negative numbers. Negative numbers, Hamilton argued, were to be understood as analogous to points on a timeline, which could occur before (negatively) or after (positively) one another. Hamilton argued that imaginary numbers could, therefore, be represented as ordered couples (a_1, a_2) in *time*. Hamilton was so convinced of the metaphysical meaningfulness of this claim that he wrote in a notebook (dated 1831),

> In all Mathematical Science we consider and compare relations. In algebra the relations which we first consider and compare, are relations between successive states of some changing thing or thought. And numbers are the names or nouns of algebra; marks or signs, by which one of these successive states may be remembered and distinguished from another ... *Relations between successive thoughts thus viewed as successive states of one more general and changing thought, are the primary relations of algebra* ... For with Time and Space we connect all continuous change, and by symbols of Time and Space we reason on and realize progression. Our marks of temporal and local site, our *then* and *there*, are at once signs and instruments of that transformation by which thoughts become things, and spirit puts on body, and the act and passion of mind seem clothed with an outward existence, and we behold ourselves from afar. And such a transformation there is when, in Algebra, we contemplate the change of our own thoughts as if it were the progression of some foreign thing, and introduce Numbers as the marks or signs to denote place in the progression (Hankins 1976, 336).

Such strong comments suggest Hamilton was viewing his mathematics from a particular metaphysical perspective and that his mathematical endeavors—as with his other activities—were an expression of that worldview.

This characterization of intuitionist mathematics helped Hamilton identify with Thomas Carlyle's account of German Idealism, which held that all matter is subordinate to "mind" (Bloor 1981).[27] Carlyle's view had been that anti-religious arguments favoring materialistic perspectives of the universe crumbled apart when forced to carry the weight of Idealism's main claim—namely, that all matter is *mind* (Bloor 1981, 208). On an Idealist reading, Carlyle argued, only theology provides ultimate answers to questions such as "Why"? Inductive, sensory perceptions are necessarily subordinate to the rational laws of categorization produced by the mind. Carlyle's specific vision of Idealism relied upon the existence of *à priori* natural laws. It considered natural law to be derived from pure Reason (*Vernunft*) rather than from empirical Understanding (*Verstehen*).

Publicized through the works of the Tory "propagandist" Samuel Coleridge, Carlyle's Idealist epistemology was used in the early part of the century to criticize the French Revolution and its sympathizers. Coleridge thought that revolution was an instantiation of human *understanding* usurping "the name of reason" (Bloor 1981, 210). Similarly, he criticized commercial interests in Britain's industrializing centers for having ripped apart the social fabric of Britain. Idealist epistemology rectified the situation, he claimed, by encouraging a new synthesis of opposing forces. Coleridge thought aristocratic interests of the past ought to come together with the needs of industrialists in a synthesis that would create a new national class of "clerisy". Coleridge envisioned that "clerisy" to be the "essential element of a rightly constituted nation," which would balance out the interests of each class in order to produce a transcendent order of harmony (Bloor 1981, 213). Highly elitist and deeply class-based, Coleridge's German Idealism had an effect on the young Irish Astronomer Royal. Working within a Protestant institution that constantly felt the pressure to justify

[27] Carlyle (1795-1881) was the author of an article on Idealism in the *Foreign Review* (1829), which Hamilton vigorously annotated.

its elite position within Ireland's divisive and hierarchal polity, Hamilton chose to engage with an ideological construct that had gained prominence among important social commentators and poets, such as Carlyle, and which could also be put to good use in defending the intellectual superiority of Protestants in Ireland.

Note that Carlyle's Idealism did not fit all of the natural philosophical or metaphysical beliefs that Hamilton had developed. Indeed, Hamilton often found that he had to redefine certain aspects of the Idealist project in order to advance his own natural philosophical beliefs. For example, Hamilton held fast to a belief in Rudjer Boscovich's (1711-1787) theory of point atomism. Boscovich, a Jesuit mathematician and fellow of the Royal Society, had presented a picture of the "atom" in his 1758 *Theoria Philosophiae Naturalis* in which "massiness" and impenetrability were eliminated and replaced with "a kinematical inertial property" such that "matter is composed of discrete, indivisible points without extension or mass" (Kargon 1965, 138). In the Boscovichan worldview, matter is non-existent—it is a mere manifestation of God's will. What truly exists are the motivating forces God grants the points, which set them into motion in the first place. In a letter to Coleridge in 1832, Hamilton acknowledged that Coleridge had explicitly rejected *all* brands of *atomism* as inherently materialistic, including the Boscovichan view. However, Hamilton had long agreed with Boscovich and, in the Jesuit's defense, he argued the problem lay in the actual meaning of "atomism". Because the undulatory theory of light was, in fact, fundamentally atomistic (in Hamilton's view), there did exist a harmonious means of bringing together Boscovich's atomism with Coleridge's Idealism. Hamilton wrote to his poet friend,

> Do I then at all express a possible view, or am I talking nonsense, when I say that I regard a certain atomistic theory as having a subjective truth, and as being a fit medium between our understanding and certain phenomena: although objectively, and in the truth of things, the powers attributed to atoms belong not to them but to God? The atomistic theory of which I speak is nearly that of Boscovich, and consists in representing all phenomena of motion as produced by the action of localized energies of attraction or repulsion, each energy having a centre in space; and this centre, which is supposed to be a mathematical point, without any figure or dimension, being called an

> *atom* instead of a point, merely to mark its conceived possession of, or connexion with, physical properties and relations (Graves 1882, 593).

The word "atom" in Boscovichan atomism did *not* refer to a materialistic entity that existed apart from the mind of God, Hamilton claimed. Rather, it referred to a linguistic tool that could help scientists conceptualize physical events. God's superior role in the movement of the universe was preserved by such a theoretical outlook. Thus, a Boscovichan scientific worldview did not usurp Reason in favor of materialist Understanding.

Throughout his career, Hamilton found many means of reshaping his scientific beliefs in order to fit particular metaphysical constructs and vice versa. Indeed, Hamilton would come to reshape his quaternion mathematics to fit the philosophical outlook of the symbolical algebraists, who he perceived as dominant actors at Cambridge by the 1850s.

Hamilton's initial derision of symbolical algebra

Following Hamilton's first meeting in 1832 with Peacock, Herschel, Babbage and Airy, the Irish scientist wrote to his poet friend, Aubrey Thomas de Vere (1814–1902), that the Cambridge mathematicians were "winning to themselves mansions above the earth, though beneath the highest heavens." At the time, Hamilton felt that symbolical algebra fell short of the standards set out by Idealism's account of Reason in that symbolical relationships failed to point to necessary *à priori* entities. The only actor within the Cambridge group that Hamilton could see himself engaging with was Whewell—an actor who had already begun to diverge politically and philosophically from his Cambridge colleagues. Whewell had begun to adopt a more explicitly Tory view of what he thought a "liberal" (read moral) and geometrically-founded education ought to be.

But apart from philosophical affinity to Whewell, there were also a number of other institutional reasons for why Hamilton might have felt justified in siding with Whewell against the more radical symbolical algebraists of the day. Despite Peacock's *Treatise* (1830) and his *Report* to the BAAS (1833), Hamilton might have viewed Peacock's symbolical algebra as peripheral to the central concerns of many Cambridge mathematicians. By the mid-1830s, for instance, Peacock was engaged in vociferous debate with Whewell over the construction of a scientific laboratory at Cambridge. Peacock had lobbied the university for the construction of such a laboratory in 1829; by 1832 it had been partially started. Construction was stalled, however, due to Whewell's vehement opposition (for an account of Whewell's stance on empiricism see Strong (1955)). Whewell had also begun to oppose certain aspects of Peacock's symbolical algebraic reforms, lambasting the unregulated introduction of symbolical methods on the Tripos exam. Peacock, meanwhile, was facing institutional limitations to the promulgation of his analytical works at the level of university lectures. Despite having been appointed Lowdean professor, Peacock was unable to attract large audiences to his mathematical lectures. In fact, many of his lectures were delivered to empty-seated auditoria (Becher 2004). Thus, although Peacock had written a seminal textbook in symbolical analysis, it might have appeared to an external observer that he was not able to effectively disseminate his ideas on campus.

In that same decade, Peacock also found himself walking a political tightrope. In the 1820s, recall, he had had to compromise with more conservative elements at the university in order to avoid a painful backlash against his unauthorized introduction of the d-notation on the Tripos. As the adoption of continental analysis in coaching rooms increased, a reactionary backlash emerged again. Peacock had to acquiesce to some of the university's administrative demands to preserve broad support and goodwill for the continental techniques that had gained popularity among recent graduates. Thus, by the mid-1830s, Peacock had had to distance himself from the more radical proponents of the second generation of symbolical algebra, i.e. those practitioners who were emerging as prominent actors within the Cambridge Philosophical Society. Peacock devoted himself to curricular reforms that highlighted the physical applications of algebra—an approach more in line with Whewell's own view of what constituted an acceptable curriculum (Warwick 2003). Peacock was thus able to stave off the reactionary opposition to analytical questions on the Senate House

Exam. Meanwhile, Whewell had by then become a priest, professor of mineralogy and explicit Tory supporter. He was also deeply and publicly involved in the operations of the BAAS, where his aim was to reshape that nascent organization so as to better reflect the hierarchal and gentlemanly culture of science that he envisioned as proper to natural philosophy (Robson and Cannon 1964). Ironically, Whewell wished to advance the sort of elitist culture that Brougham and Brewster had initially sought to undo in their establishment of the organization in the first place. Whewell worked to get himself appointed as vice-president of the BAAS in 1832. He became the local secretary in Cambridge for the annual meeting in 1833, and, in 1837, he served as vice-president once more.[28]

Hamilton's perception of Cambridge mathematics might well have been colored by the seeming importance of Whewell as a rising star within the BAAS and the seeming acquiescence of Peacock with regards to reactionary curricular demands.[29] In addition, Whewell's widely-received *History of the Inductive Sciences* (1837) and his *Philosophy of the Inductive Sciences* (1840)—both of which maintained that inductive science is the instantiation of absolute laws of nature that indicate the presence of a Creator—helped to re-establish a role for theist rationalist beliefs (Yeo 2003). Those theistic views were not unlike Hamilton's own. Hamilton had explicitly related the existence of *à priori* necessary truths to the presence of an Almighty Being. It is perhaps not surprising then that throughout the 1830s Hamilton deemed Whewell to be representative of the direction in which British mathematics and natural philosophy were headed.

Whewell's social position would have also helped to feed into Hamilton's analysis of the situation. Throughout the 1820s, Whewell had labored to climb up England's social ladder. He increasingly moved in aristocratic circles and involved himself with former aristocratic Cambridge graduates, such as the Earl Fitzwilliam (1786-1857) and the second marquees of Northampton (1790-1851). He cemented his augmented social status in 1841 by marrying Cordelia Marshall, who he had been introduced to through the Woodsworth family. That family connection was especially useful, as

[28] Whewell later became president in 1841.
[29] Whewell was professor of mineralogy at Cambridge in the 1830s; he later became professor of moral philosophy in 1841.

Cordelia's eldest sister, Mary, was married to Lord Monteagle—an aristocrat who helped to define Whewell's later Tory-affiliations (Todhunter 1876). Whewell's movements within aristocratic circles in the 1830s and 1840s would not have gone unnoticed by Hamilton, whose own political sympathies were opposed to any reordering of English-inspired class hierarchies in Ireland.

In the meantime, major proponents of the symbolical approach, including Babbage and Herschel, were heavily occupied in duties elsewhere, adding to Hamilton's sense that the symbolical algebraic movement was *not* an established component of Cambridge's research future. In 1834, for instance, Herschel sailed off to the Cape of Good Hope where he remained for four years, retrieving astronomical data and fighting for better pay and housing conditions from the state-funded observatory (Crowe 2004). Babbage, for his part, was non-resident at Cambridge, despite his status as the Lucasian Chair from 1828 to 1839. He also refused to offer any lectures. His engagements lay elsewhere, as he was deeply involved in the construction of his proposed calculating machine—a project that had become nationalized in 1830, but which collapsed in 1834 following recriminating arguments with Joseph Clement, Babbage's master-engineer (Schaffer 1994).

Hamilton's lack of engagement with the more radical members of the symbolical network of reformers in the 1830s should not, therefore, be seen as the simple result of his metaphysical disagreements with those practitioners. Rather, it is probable that Hamilton perceived the symbolical algebraic program to be peripheral or at least not foundational to the future of Cambridge mathematics and natural philosophy. However, in deeming Whewell's more conservative mathematical views to be a better commodity upon which to hedge his bets, Hamilton was misled. Firstly, although Peacock's lectures *were* poorly attended, he was not alone in experiencing non-existent audiences for university lectures. The fact that students found college and university lectures to be useless in preparing for the Tripos was well-known. The shifting pedagogical environment at Cambridge favored private coaching over university lectures as a means of providing students with the bulk of their daily learning and examination preparation. Although there had been "private" tutors present at the university since the late-18th century, by the middle of the 19th-century, private tutors (or "coaches") had become a staple part of an undergraduate's life, and a necessary part of any successful wrangler's education. Undergraduates hoping to finish in top wrangler positions relied upon their coaches to gain the tacit knowledge associated with emerging techniques in analysis and algebra. Coaches, such as Hopkins, advocated and used techniques in symbolical algebra in their classrooms, and had no qualms about passing on those techniques to their students, knowing full well that the content of the Tripos was largely determined outside the formal structure of university lectures (Warwick 2003). Contrary, therefore, to reactionary sentiments that viewed symbolical algebra as improper material for consumption by undergraduate students, the coaching system that had embraced such techniques was, in fact, producing qualified undergraduate mathematicians.

Students of the symbolical algebraic curriculum had even started to publish in professional journals—a phenomenon that Whewell famously lamented over, saying it was proof mathematics had failed to instill in students a proper respect for authority.[30] One of Peacock's Scottish students, Duncan Gregory, along with another Scot, Archibald Smith, and an English student, Samuel Greatheed, jointly established the *Cambridge Mathematical Journal* in 1837. The *CMJ* soon became Britain's preeminent mathematics journal, reinforcing the presence of higher algebra and analysis on the Tripos (Becher 1984; Crilly 2004). Not surprisingly, symbolical algebraic questions set on the Mathematical Tripos increased notably in both number and difficulty by 1840. The engine of mathematical production found in the university's coaching classrooms and on the pages of the *CMJ* powered the rise of a second generation of symbolical algebraists, including Gregory and his co-editors. This developing establishment effectively protected Cambridge's symbolical algebraic curriculum from reactionary undoing in the 1850s, when the Whig government of Lord John Russell (later Earl Russell) established a parliamentary commission to analyze the curriculum of Cambridge

[30] Whewell maintained a good liberal education ought not to "rouse [students] to speculate for themselves," which analysis was apparently doing (Warwick 2003, 99).

University and to standardize it according to other universities across the country. Peacock was appointed head of that commission, and one aspect of his job was to analyze the role that private coaches played in undermining traditional university authority. Though not opposed to the way in which coaching had allowed for the introduction of higher level analysis, Peacock was concerned about the lack of standards present in diverse coaching classrooms. Defending unregulated coaching practices, Hopkins testified that such teaching had, in fact, allowed the university to transform itself into a manufacturer of professional mathematicians, as evidenced by the success of the *CMJ*.

Peacock ultimately agreed with Hopkins. The confluence of Hopkins's Peelite Toryism with Peacock's Whig politics resulted in an interesting political crossover that severed Whewell from the mainstream of Cambridge science and mathematics. Peacock was able to institute a series of university-wide reforms that bolstered the position of university lecturers, and which provided professors with the ability and motivation to teach higher-level analysis in their lecture halls, thereby further entrenching symbolical analysis in the university's curriculum (Warwick 2003). Hamilton would not have missed this transformation in Peacock's political might, nor would he have missed indications of the deepening institutionalization of symbolical algebra at Cambridge. Thus, despite his former reservations, Hamilton began to recast his work in quaternion mathematics as an outgrowth of that very Peacockian-influenced analytical tradition that he had long derided.

Victorian accounts of symbolical algebra in Britain

In the year 1842, the Board of Overseers for Hamilton's Observatory demanded an account of the natural philosopher's astronomical and scientific achievements. Recall that the early-1840s had been witness to Hamilton's alcoholism and his wife's illness; his mathematical efforts had dropped off. Although he did manage to publish some tracts on the philosophical notion of "triads", the Board's demand for accountability focused on Hamilton's mathematical works. The Irish mathematician returned to his previous work on couples and triplets with a vigor largely defined by the Board's overbearing institutional watch. Facing the pressure to publish, Hamilton issued a nascent account of quaternions in 1843 in which he offered a four-termed expression to denote directed magnitudes in three-dimensional space. The four terms corresponded to three positional identifiers (a versor) on a three-dimensional axis, and one magnitude term (a scalar). Hamilton published accounts of this system in 18 installments in the *Philosophical Magazine* over the remainder of the decade, and in 10 installments in the *Cambridge and Dublin Mathematical Journal* (formerly Gregory's *CMJ*). The Board was satisfied, and Hamilton's job was secured.

However, Hamilton's publications went on to generate no small amount of debate between symbolical algebraists, including Cayley and De Morgan. That debate was partly the result of Hamilton's new philosophical gloss, which he added to his 1853 "Preface" to the *Lectures on Quaternions.* In that "Preface", Hamilton chose to justify his mathematical research in quaternions in a manner disjointed from his previous Kantian ideals. Now his justifications of quaternions invoked philosophical claims related to the worth of symbolical algebra as a methodology. Hamilton recounted how, when playing around with triplet systems, he had found the distributive principle was at times lost, while at other times many "absurd" pre-determined definitions were required to make the system work. Although arithmetical rules remained a guiding tool for him, Hamilton stated they could no longer constitute the absolute standard by which he was to proceed. Had they done so, then he would have had to give up hope of finding a successful solution altogether. Hamilton then cited the works of Peacock, Babbage and De Morgan. Notably, Hamilton especially aligned himself with De Morgan, who had, by then, expressed some reservations over the notion of a purely symbolical system with no physical referents. Yet, reflecting his newly modified, symbolical algebraic philosophy of mathematics, Hamilton hypothesized that mathematical entities, such "directed lines", "moments in time", "space-steps", and "time-steps" were fundamentally *operational* in nature. Realizing, perhaps, that this shift in view would open him up to severe criticism from those quarters in which he had previously sought refuge—namely, the loose network of Kantians upon whom he had earlier relied—Hamilton's "Preface" reads like an apologia in which he seeks to demonstrate that the formerly heretical views of the symbolical algebraists are, in fact, akin to his former Idealist beliefs.

The De Morgan- Hamilton relationship

Recall that upon reading Peacock's *Treatise* (1830), Hamilton had initially reacted negatively to symbolical algebra. In a letter to Peacock he noted,

> When I first read [the *Treatise*]... and indeed for a long time afterwards it seemed to me...that the author designed to reduce algebra to a mere system of symbols, and *nothing more*; an affair of pothooks and hangers, of black strokes upon white paper, to be made according to a fixed but arbitrary set of rules: and I refused, in my own mind, to give the high name of *Science* to the results of such a system (Graves 1885, 528).

In his 1837 essay, "Theory of Conjugate Functions, or Algebraic Couples; with a Preliminary and Elementary Essay on Algebra as the Science of Pure Time"—the first part of which was read to the Royal Irish Academy in 1833; the second part of which was read two years later—Hamilton offered a three-category classification system of algebraic analysis as a critical response to Peacock's view of symbolical algebra. The first category, Hamilton claimed, was the "practical", which viewed algebra as an instrument for applications; the second was the "philological", which viewed algebra as a conventional language with no necessary content; the third was the "theoretical", which viewed algebra as a means of helping "contemplation" in the search for meaningful mathematical relationships and objects. Of the practitioners operating in this field, Hamilton had written,

> The Practical person seeks a Rule which he may apply, the Philological person seeks a Formula which he may write, and the Theoretical person seeks a Theorem on which he may meditate. The felt imperfections of Algebra are of three answering kinds. The Practical Algebraist complains of imperfection when he finds his Instrument limited in power; when a rule, which he could happily apply to many cases, can be hardly or not at all applied by him to some new case; when it fails to enable him to do or to discover something else, in some other Art, or in some other Science, to which Algebra with him was but subordinate, and for the sake of which and not for its own sake, he studied Algebra. The Philological Algebraist complains of imperfection, when his Language presents him with an Anomaly; when he finds an Exception disturbs the simplicity of his Notation, or the symmetrical structure of his Syntax; when a Formula must be written with precaution, and a Symbolism is not universal. The Theoretical Algebraist complains of imperfection, when the clearness of his contemplation is obscured; when the Reasonings of his Science seem anywhere to oppose each other, or become in any part

too complex or too little valid for his belief to rest firmly upon them; or when, though trial may have taught him that a rule is useful, or that a formula gives true results, he cannot prove that rule, nor understand that formula: when he cannot rise to intuition from induction, or cannot look beyond the signs to the things signified (Hamilton 1967, 3).

Although no one algebraist "belongs *exclusively* to any *one* of three schools, so as to be only Practical, Philological, or only Theoretical," Hamilton wrote, Peacock's "arithmetical algebra" is best aligned with the "practical" and his "symbolical algebra" with the "philological".

Hamilton viewed his own work on couples as aligned with the "theoretical". The Irish mathematician recollected that his aim in developing "couples" had been to

> improve the *Science*, not the Art nor the Language of Algebra. The imperfections sought to be removed, are confusions of thought, and obscurities or errors or reasoning; not difficulties of application of an instrument, nor failures of symmetry in expression (Hamilton 1967, 4).

And in a tempered critique of the symbolical algebraic approach, Hamilton added:

> That confusions of thought, and errors of reasoning, still darken the beginnings of Algebra, is the earnest and just complaint of sober and thoughtful men, who in a spirit of love and honor have studied Algebraic Science, admiring, extending, and applying what has been already brought to light, and feeling all the beauty and consistence of many a remote deduction, from principles which yet remain obscure, and doubtful (Hamilton 1967, 4).

Hamilton's position in the 1830s was that of someone who felt ontological content had to exist for mathematical entities to be justifiable. That content had to be established *prior* to the generalized manipulation of the entities. Hamilton's overriding objective had been to establish a "science" of algebra that was:

> Strict, pure, and independent; deduced by valid reasonings from its own intuitive principles; and thus not less an object of *à priori* contemplation than Geometry, nor less distinct, in its own essence, from the Rules which it may teach or use, and from the Signs by which it may express its meaning (Hamilton 1967, 5).

Hamilton saw himself as offering the novel claim that Time and Algebra were *necessarily* connected (this being the "inductive" part of his work), and he put forward the view that "this notion or intuition of Time may be unfolded into an independent Pure Science"—i.e. that it constituted a new branch of mathematics, which could then be used in the resolution of specific scientific quandaries related to physical phenomena (this being the "deductive" part of his work).

In sum, Hamilton viewed his project as a unique synthesis of deductive and inductive processes. Through the deductive use of algebraic rules, a new "Science" could emerge that would help to shed light on the ways things *are* (i.e. progressions in time) rather than just comment on universally true symbolical relations that were referent to nothing existential.[31] Hamilton's claim that "the Science of Pure Time ... is co-extensive and identical with Algebra, so far as Algebra itself is a Science" (Hamilton 1967, 5) indicates that, in the 1830s, he viewed symbolical algebra as valid only in so far as it possessed inductive referents. The progression of time could not be severed from the algebraic rules describing it.

Hamilton also considered his work in the 1830s to be a modern extension of classical mathematics, which had long been based upon the notion of "*Continuous Progression*" or "Time". "It is the genius of Algebra to consider what it reasons on as *flowing*, as it was the genius of geometry to consider what it reasoned on as *fixed*," he wrote (Hamilton 1967, 5). Ancient geometers looked upon forms as "fixed"; it was Newton who, eventually, developed the alternative principle in algebra of looking upon forms, such as the tangent to a curve, as flowing and unfixed. Hamilton claims,

> The Newtonian Method of Tangents ... regards the curve and line not as *already* formed and fixed, but rather as *nascent*, or in a process of generation: and employs, as its primary conception, the thought of a *flowing point* (Hamilton 1967, 5).

The entire theory of fluxions depended upon the notion of Time, as did Lagrange's later "philological" attempt to reduce the Theory of Fluxions to a "systems of operations upon symbols,

[31] Hamilton's belief was that "the Intuition of TIME is such a rudiment." He stated, "This belief involves the three following components: First, that the notion of Time is connected with existing Algebra; Second, that this notion or intuition of Time may be unfolded into an independent Pure Science; and Third, that the Science of Pure Time, thus unfolded, is co-extensive and identical with Algebra, so far as Algebra itself is a Science. The first component judgment is the result of an induction; the second of a deduction; the third is the joint result of the deductive and inductive processes" (Hamilton 1967, 5).

analogous to the earliest symbolic operations of Algebra," despite the fact that Lagrange had attempted to "reject the notion of time as foreign to such a system" (Hamilton 1967, 6).

Hamilton's aim was to subsume the foundations of the "symbolical algebraic" approach under the heading of "Algebra as Time", thereby providing it with ontological content that its initial advocates had stripped away. Hamilton concluded that Lagrange,

> In one of his own most important researches in pure Algebra, (the investigation of limits between which the sum of any number of terms in Taylor's Series is comprised) ... employs the conception of *continuous progression* to show that a certain variable quantity may be made as small as can be desired (Hamilton 1967, 6).

Although Lagrange had considered algebra to be the science of functions, it is impossible to think of "functions" without thinking of "its essence as consisting in a *Law connecting Change with Change*," Hamilton argued. And, "where *Change* and *Progression* are, there is TIME ... The notion of Time is, therefore, inductively found to be connected with existing Algebra" (Hamilton 1967, 6). Hamilton viewed his algebraic research of the 1830s to be part of a long tradition in algebraic (and Kantian) thought—a tradition that had explicitly relied upon the notion of progression in time. Hamilton concluded that "The notion or intuition of ORDER IN TIME is not less but more deep-seated in the human mind, than the notion or intuition of ORDER IN SPACE" (Hamilton 1967, 7). In sum, "algebra" has ontological content, based upon the fact it emerges from the intuition of progression in time, which precedes even spatial intuition as a natural guiding principle for rational thought.

Recall that in the 1840s, Peacock produced his two newly revised texts on algebra (on *Arithmetical Algebra* in 1842 and *Symbolical Algebra* in 1845), in which he emphasized the fundamentally empirical nature of the normal rules of algebra, and in which he had demonstrated how symbolical algebra, as arbitrary and as meaningless as it might seem, was nonetheless anchored to arithmetical operational truths. The relationships revealed in symbolical algebra were universally true even though they were not necessarily related to things *in this world*. Peacock's books received important support from De Morgan, who had by then established himself as one of Britain's pre-eminent mathematicians. Yet, De Morgan had also read Hamilton's first paper on conjugate functions and algebra as "pure time." As a result, he modified some of his own views (see Chapter One) in order to present a new interpretation of symbolical algebra in a series of lectures entitled "On the Foundation of Algebra," delivered to his UCL classes between 1839 and 1844, in which he argued that logical algebra is "the science which investigates the method of giving meaning to the primary symbols and of interpreting all subsequent symbolic results" (De Morgan 1842, 173). De Morgan had, therefore, adopted a view similar to Hamilton's "theoretical" approach—namely, that entities operated upon should be defined prior to their usage. De Morgan developed that Hamiltonian-inspired view further in his *Trigonometry and Double Algebra* (1849), in which he stated,

> The object of this book is the construction of Algebra upon a basis which will enable us to give a meaning to every symbol *and combination of symbols* before it is used, and consequently to dispense, first, with all unintelligible combination, secondly, with all search after interpretation of combinations subsequently to their first appearance (De Morgan 1849, 89).

De Morgan's shift in allegiance away from pure Peacockian symbolism towards a Hamiltonian-influenced view in the 1840s meant that the London mathematician had become an advocate of a merged form of abstract mathematical reasoning—one in which universal symbolical relationships produced physical conceptualizations (Richards 1987).

On the flip side, however, in his 1835 review of Peacock's *Treatise*, De Morgan also noted the virtue of an approach that rendered mathematical analysis entirely abstract and relational. That virtue came from the fact that such an approach was anti-dogmatic. Symbolical algebra opened the door to the critical adoption of non-established and questionable entities and operations. In so doing, it constituted a form of thinking that reflected De Morgan's dissenting attitude toward epistemological, theological and pedagogical authorities.[32] In the preface to his *Lectures*, Hamilton notes that De Morgan had criticized him for being too *dogmatic* in his 1830s conjugate functions paper. De Morgan had accepted Hamilton's notion of "pure time" as a useful interpretative tool, but

[32] De Morgan's anti-dogmatic political stance was one of the main reasons for which he had been expelled from Cambridge during his student days, as he had refused to take the religious test required for a fellowship appointment.

to argue that *all* algebraic analysis could be reduced to "pure time" was unjustifiably "dogmatic" in De Morgan's view. As the London mathematician had written,

> A symbol may thus denote either magnitude [or] operation, by which magnitude is attained, or the conception of one extreme arrived at, the other having been the previous object of contemplation. The earlier algebraists most certainly dwelt on the first notion; $a + b$ is with them the result of an operation, in which the method of obtaining it is so completely forgotten, that the *result* $a + b$ is actually obtained by a distinct operation. It seems to me that Sir William Rowan Hamilton, in his very original and methodical memoir on algebra as the science of pure time, has adopted a view of the third kind. I cannot see why the whole paper might not be as easily applied to succession of points in a line, as to succession of epochs in time. Succession, that is to say *continuous* succession, might be made the fundamental conception in both cases; and if such were the author's intention in the use of the word *time*, I should be very glad to maintain after him that *one* of the explanations which suffice to convert technical into logical algebra, has been fully established in his memoir. But, if any thing more *physical* be intended by the distinguished author, and if some of his phrases are to be interpreted as of his asserting algebra to be *the* science of pure time, I should then cite him as an instance of the *dogmatism* already alluded to (De Morgan 1842, 176).

Hamilton responded to this review a decade later, by adopting a philosophical outlook reflecting some of De Morgan's criticisms, and which adopted certain aspects of Peacock's "principle of the permanence of equivalent forms."

In 1845, Hamilton met with Peacock again. He engaged in lengthy correspondences with him after their meeting. Unlike their early-1830s encounter, Hamilton no longer thought Peacock and his clan were "building castles in the sky." Rather, in 1846, he wrote to Peacock saying,

> My views respecting the nature, extent and importance of symbolic science may have approximated gradually to yours; and that approximation may be due chiefly to the influence of your writings and conversation (Ohrstrom 1985, 53).

Thus, we find Hamilton philosophically repositioning himself in the latter half of the 1840s. For example, he emphasizes the importance of the associative principle and the uniqueness of the "determinateness of division," the latter of which referred to the fact that "a quotient [is] never indeterminate or impossible, unless the constituents of division all vanish" (Ohrstrom 1985, 51)[33].

[33] This determinateness of division is based on Hamilton's understanding of the importance of the modulus of the quaternion, where the "law of the modulus" fulfills the condition that the modulus of a product be the product of the moduli, where the modulus of a quaternion $q = a + bi + cj + dk$ is $|q| = (a^2 + b^2 + c^2 + d^2)^{1/2}$. As Gray (1997)

The associative principle and the notion of determinate division had appeared as two fundamental properties in Peacock's 1842 *Treatise*.

Hamilton's repositioning—the Lectures on Quaternions (1853) as a symbolical project

Hamilton's *Lectures on* Quaternions (1853) runs to a lengthy 736 pages. In his preface (which is a mere 64 pages in length), he repackages and presents the history of quaternions as part of an ideological divide in the history of mathematics. He situates himself on one side of that divide—the symbolical algebraic side—and favors the creation of new mathematical techniques, new notations, and new rules. Concomitantly, he places himself within a uniquely British community of activist mathematicians who saw themselves as working at the forefront of research involving deistic mathematics (for a brief account of Hamilton's quaternions, see Appendix One).

Hamilton initiates his monolithic work with the claim that, like his Cambridge counterparts, he too is doing something new. He calls his mathematical analysis the "Method or Calculus of Quaternions". His preface offers an historical recounting of the generation (in his words "discovery") of this new method, and it aims to highlight the importance of quaternions to the future of British mathematics. Hamilton's objective here is to underscore the profundity of the quaternion concept and its fecundity for future research. By association, his aim is to underscore the profundity and fecundity of Irish mathematics in general. In so doing, he offers the suggestion that quaternion algebra could serve as the basis for a *new* symbolical algebra—one even more profound than Peacock's own.

Firstly, Hamilton identifies himself as having long been situated within a tradition of mathematical and philosophical thinkers who had struggled to come to terms with imaginary numbers. He writes,

> The difficulties which so many have felt in the doctrine of Negative and Imaginary Quantities in Algebra forced themselves long ago on my attention; and although I early formed some acquaintance with various views or suggestions that had been proposed

writes, this law "served as Hamilton's guarantee that the system of objects he constructed did not, for some hidden reason, self-destruct" (Gray 1997, 90).

> by eminent writers, for the purpose of removing or eluding those difficulties (such as the theory of direct and inverse quantities, and of indirectly correlative figures, the method of constructing imaginaries by lines drawn from one point with various directions in one plane, and the view which refers all to the mere play of algebraical operations, and to the properties of symbolical language), yet the whole subject still appeared to me to deserve additional inquiry, and to be susceptible of a more complete elucidation (Hamilton 1853, 1-2).

Hamilton agrees with those mathematicians who had argued that "negatives" and "imaginaries" are not "properly *quantities* at all." In the 1830s, he recounts, he had sided with those symbolical algebraists who had claimed that imaginary numbers were *operations* rather than quantities, though he felt that the approach failed to provide the operations in question with adequate meaning. Hamilton recalls that he had,

> still felt dissatisfied with any view which should not give to them, from the outset, a clear interpretation and *meaning*; and wished that this should be done, for the square roots of negatives, without introducing considerations *so expressly geometrical*, as those which involve the conception of an *angle* (Hamilton 1853, 2).

That dissatisfaction had initially led Hamilton to Kantianism. Hamilton recalls that in his 1837 conjugate functions paper, he had defined algebra as the "Science of Pure Time," in which algebra emerged as a symbolical manifestation of successive moments in temporal existence.

Algebra was no "mere Art, nor Language, nor *primarily* a Science of Quantity," he recounts. Rather, it was "the Science of Order in Progression." Although,

> the successive *states* of such a progression might (no doubt) be represented by *points upon a line*, yet I thought that their simple *successiveness* was better conceived by comparing them with *moments of time*, divested, however, of all reference to *cause* and *effect*; so that the "time" here considered might be said to be abstract, ideal, or *pure*, like that "space" which is the object of geometry (Hamilton 1853, 2).

Hamilton had argued couples could be used to represent directed lines and imaginary quantities. He was then

> encouraged to entertain and publish this view, by remembering some passages in Kant's Criticism of the Pure Reason, which appeared to justify the expectation that it should be

possible to construct, *à priori,* a Science of Time, as well as a Science of Space (Hamilton 1853, 2-3).

The "Science of Pure Time" was, in other words, the offspring of Hamilton's Kantian insight into the *à priori* nature of the world.[34] Hamilton aligned his early work on couples with the venerable metaphysical claims of that German philosopher, by saying he had approached mathematical knowledge from a particular "point of view as regards the first elements of *algebra*" (Hamilton 1853, 3).

However, by 1853, Hamilton acknowledges that in having pursued such an extreme version of Kantianism in his earlier algebra, he had unfairly dismissed Peacock's *Treatise on Algebra* (1830). His negative perception of Peacock's symbolical algebra, emerged from the fact that, from his Kantian view, the search for *à priori* truths required a constant appeal to that which was intuitively instantiated. In later years, Hamilton says, he came to realize that purely abstract symbolical manipulations, in which the rules of algebra are treated as malleable and flexible, could reveal important relationships between abstract mathematical objects—relationships not evident to the mathematician who relies solely on intuitions of temporal succession. The symbolical algebraic approach allowed, therefore, for the construction of new starting principles—principles from which the mathematician could develop entirely new mathematical systems, some of which might productively lead to new intuitive conceptions.

Recalling conjugate functions

In his preface, Hamilton recalls De Morgan's criticism of "dogmatism" in his 1837 conjugate functions papers. Hamilton responds to the criticism by saying he had not intended for that paper to be interpreted as claiming that "simple and elementary notations" should be been understood as "necessary." Rather, the claims he had proposed in that paper were merely "consistent among themselves, and preparatory to the study of the quaternions" (Hamilton 1853, 3). The focus on

[34] Hamilton made this claim despite the fact that he read Kant directly only in 1834, *after* he had published his first paper on Algebra as "pure time".

"internal consistence" here is indicative of Hamilton's effort to accommodate the concerns of the English algebraists for whom algebraic entities, such as "couples", ought to have been interpreted as open-ended in content, as opposed to being reliant upon any *one* necessary interpretation prior to their manipulation. In an effort to subtly redefine his past efforts, Hamilton presents his conjugate functions paper as having been a nascent form of symbolical analysis rather than the "theoretical" analysis that he had previously defined it to be. For instance, Hamilton recalls the first claim made in his conjugate functions, which stated,

> If the letters A and B were employed as *dates*, to denote any two *moments* of time, which might or might not be distinct, the case of the coincidence or *identity* of these two moments, or of *equivalence* of these two dates, was denoted by the equation,
>
> $$B = A;$$
>
> which symbolic assertion was thus interpreted as not involving any *original* reference to *quantity*, nor as expressing the result of any comparison between two *durations* as *measured.* It corresponded to the conception of simultaneity or *synchronism*; or, in simpler words, it represented the thought of the *present* in time. Of all possible answers to the general question, "*When*," the *simplest* is the answer, "Now," and it was the *attitude of mind*, assumed in the making of this answer, which (in the system here described) might be said to be originally symbolized by the *equation* above written (Hamilton 1853, 3-4).

An expression of "non-equivalence," such as $B > A$ or $B < A$ is not to be interpreted as referring to a specific quantity. Rather, it referred to a time sequence, or any sequence in general. "Subsequence" and "precedence" answer to

> thoughts of the *future* and the *past* in time; or as expressing, simply, the one that the moment B is conceived to be *later* than A, and the other that B is *earlier* than A: without *yet* introducing even the *conception* of a *measure,* to determine *how much later*, or how much earlier, one moment is than the other (Hamilton 1853, 4).

Having established a meaning for the basic symbols $=, >, \text{and} \ <$, Hamilton recalls proposing that to construct a "*science of pure time,*" the first use of the symbol (–) might be in the construction of a

> complex symbol $B - A$, to denote the *difference between two moments*, or the *ordinal relation* of the moment B to the moment A, whether that relation were one of identity or of diversity; and if the latter, then whether it were one of subsequence or of precedence, and in whatever degree (Hamilton 1853, 4).

In the study of such symbols, questions about quantity often emerge, he notes, because it is at this point that the mathematician usually wants to know about the "*degree* of such diversity"—a question that invokes the conception of "*duration*, as *quantity* in time" (Hamilton 1853, 2), where the full meaning of $B - A$ is not be known until the mathematician knows "*How long after*, or how long before, if at all, B is than A" (Hamilton 1853, 4).

Having emphasized the symbolical nature of his algebraic speculations at the time, Hamilton also recalls his metaphysical justifications for interpreting such symbols on the basis of a time sequence. He writes, "The contrast between the Future and the Past appears to be even earlier and more fundamental, in human thought, than that between the Great and the Little" (Hamilton 1853, 4). Unlike his 1837 paper, however, Hamilton uses his 1853 "Preface" to recast those past claims by emphasizing the generality of the sequences (be it in time or otherwise) that he had been speculating on. In his "Preface" Hamilton explains,

> After *comparing moments,* it was easy to proceed to *compare relations*; and in this view, by an *extension* of the recent signification of the sign $=$, it was used to denote *analogy* in time; or, more precisely, to express the *equivalence of two marks of one common ordinal relation*, between *two pairs* of moments (Hamilton 1853, 5).

The formula, $D - C = B - A$, can be interpreted to denote an "equality between two intervals in time," thus indicating a general relationship between symbolical entities used. D is to C as B is to A, where the equivalences at play indicate "identity or diversity," and where the "*quantity and quality* of such diversity" are only "taken into account" afterwards (Hamilton 1853, 5). In other words, Hamilton spells out a process by which symbolical equivalences are first established and their "quantity" or "quality" (i.e. their positivity or their negativity) are determined secondarily, after the analysis has been completed.

In his conjugate functions paper, such relationships had been subjected to various further transformations and combinations (including "inversion" and "alternation"), all of which he had interpreted in terms of moments (subsequent or precedent) in time. In his "Preface", however, Hamilton emphasizes that all those manipulations agree "with the received *rules* of algebra." Though, in his 1837 paper, Hamilton had maintained that algebra was "identical to" the concept of time, he does not repeat the claim in his 1853 "Preface". Rather, he insists the rules governing the algebra of "couples" mimic the rules governing normal algebra. For example, the "the contrasted formulae of inequalities of differences," such as

$$D - C > B - A, D - C < B - A,$$

can be interpreted to mean "D was *later, relatively* to C, than B to A; and the other that D was *relatively earlier*" (Hamilton 1853, 5). But Hamilton leaves open the possibility that other interpretations are also possible, suggesting that symbolical equivalence holds true for "anything at all," as Peacock had claimed. For example, in his account of the $(+)$ symbol, Hamilton says he had previously used the symbol as

> a mark of a combination between a symbol, such as the smaller Roman letter a, of a *step in time*, and the symbol, such as A, of the moment *from* which this *step* was conceived to be made, in order to form a complex symbol, a + A, recording this conception of transition, and denoting the moment (suppose B) *to* which the step was supposed to conduct (Hamilton 1853, 5).

In other words, the operation $(+)$ combined symbols representing (i.e. "time steps" and "space steps". In 1837, Hamilton had argued that any transition (or "step") symbolized using $(+)$ should be regarded as a "mental act" that could be conducted "*backwards* as *forwards* [as] in the progression of time." Hamilton claimed the expression, $\mathrm{B} = \mathrm{a} + \mathrm{A}$, denoted "the conception that the moment B might be *attained,* or mentally generated, by making (in thought) the step a from the moment A" (Hamilton 1853, 5-6). If the symbol B – A were considered to be an ordinal relation between two moments, the rules of arithmetic would then tell us that a "*step from one* [moment] *to another*" could be symbolically represented as, $\mathrm{B} - \mathrm{A} = \mathrm{a}$. The mathematician could even use the equation above as a symbol for "one common step," such that the identity $(\mathrm{B} - \mathrm{A}) + \mathrm{A} = \mathrm{B}$ determined the difference between two moments as a "relation." In his 1853 preface, however, Hamilton focuses on the "ordinal relations" at play—"ordinal relations" that need not be situated within a "Science of Pure Time." Rather, algebraic rules acting on unknown symbols produce universal relations even when no *à priori* intuition of time-based progression is assumed.

Shifting views of symbolical algebra

Hamilton admits that while it is true that in earlier decades he had believed meaning ought to precede manipulation, he no longer believed that to be the case. He still did not believe that symbolical algebra should be conceived of as purely linguistic or conventional, but he no longer believed all algebraic elements required predetermined definitions in order to produce powerful and meaningful results. Useful conclusions could emerge even when algebraic manipulation preceded definition. In a footnote, Hamilton identifies his intellectual benefactors in this conversion of opinion. His *Lectures* had been deeply inspired by the algebraic thinking then dominant in England. In having maintained previously a view "so little supported by scientific authority" (i.e. his view of algebra as the "science of pure time"), Hamilton states he had become well aware of his deficiencies as a symbolical algebraist. He writes,

> I am very willing to believe that (though not unused to calculation) I may have habitually attended [in the past] too little to the *symbolical* character of Algebra, as a Language, or organized system of *signs*: and too much (in proportion) to what I have been accustomed to consider its *scientific* character, as a Doctrine analogous to Geometry, through the Kantian parallelism between the *intuitions* of Time and Space. This is not a proper opportunity for seeking to do justice to the views of others, or to my own, on a subject of so great subtlety: especially since, in the *present* work, I have thought it convenient to adopt throughout a *geometrical basis,* for the exposition of the theory and calculus of the Quaternions. Yet I wish to state, that I do not despair of being able hereafter to shew that my own old views respecting Algebra, perhaps modified in some respects by subsequent thought and reading, are not fundamentally and irreconcilably opposed to the teaching of writers whom I so much respect as Drs. Ohm and Peacock (Hamilton 1853, 14).[35]

Hamilton continues,

[35] In referring to Ohm, Hamilton had in mind Ohm's *Versuch eines Vollkommen Consequenten Systems der Mathematik* (Berlin, 1829). With Peacock, he had in mind the *Treatise on Algebra* (1830), *Report on Certain Branches of Analysis* (1834),[35] *Arithmetical Algebra* (1842), and *Symbolical Algebra* (1845).

> I by no means dispute the possibility of constructing a consistent and useful system of algebraical calculations, by starting with the notion of *integer number*; unfolding that notion into its necessary consequences; expressing those consequences with the help of *symbols*, which are already general in *form*, although supposed at first to be limited in their signification, or *value*; and then, by *definition,* for the sake of *symbolic generality*, *removing the restrictions* which the original notion had imposed; and so resolving to *adopt*, as perfectly *general* in *calculation*, what had been only *proved* to be *true* for a certain subordinate and limited extent of *meaning*. Such seems to be, at least in part, the view taken by [Ohm and Peacock]: although Ohm appears to dwell more on the study of the *relations* between the fundamental *operations,* and Peacock more on the *permanence* of equivalent *forms* (Hamilton 1853, 15).

Thus, Hamilton recognizes that interpretation is subordinate to the universal relations that emerge through symbolical analysis.

Indeed, in his "Preface," Hamilton states he now accepts the legitimacy of symbolical algebra as a philosophical and methodological tool. Yet, in placating his Kantian colleagues, he also adds this does not necessitate an entire abandonment of his Kantian intuition. Hamilton writes,

> I confess that I do not find myself able to frame a distinct *conception of number,* without *some* reference to the thought of *time,* although this reference may be of a somewhat abstract and transcendental kind. I cannot fancy myself as *counting* any set of things, without first *ordering* them, and treating them as *successive:* however *arbitrary* and *mental* (or *subjective*) this assumed succession may be. And by consenting to *begin* with the abstract notion (or pure intuition) of TIME, as the *basis* of the exposition of those axioms and inferences which are to be expressed by the symbols of algebra, (although I grant that the commencing with the more familiar conception of *whole number* may be more convenient for purposes of elementary instruction), it still appears to me that an advantage would be gained: because the necessity for any merely *symbolical extension* of formulae would be at least considerably *postponed* thereby. In fact (as has been shewn above), *negatives* would then present themselves as easily and naturally as positives, though the fundamental contrast between the thoughts of *past* and *future,* used *here* as no mere *illustration* of a result otherwise and symbolically deduced, without any clear comprehension of its meaning, but as the very *ground* of the reasoning (Hamilton 1853, 15).

Hamilton's aim, therefore, is to go beyond Peacock's "principle" generalization and abstraction in order to make the algebraic rules governing the combinations of "time-steps" the foundations for a new symbolical algebra altogether. In supporting the symbolical program, Hamilton uses his 1853 "Preface" to usurp it to some degree, by arguing that the regular principles of arithmetical algebra, which Peacock had used as the basis for his symbolical algebra in 1845, were too limiting and that they ought to be replaced with the broader foundations formed by the algebra that governs "couples". Hamilton concludes, therefore, that Peacock's phraseology—which defines symbolical algebra the "science of suggestion"—should be reinterpreted more broadly, as a means of "build[ing] up afterwards a new structure of purely *symbolical generalization*." "If the *science of time* were adopted," he writes, "instead of merely Arithmetic, or (primarily) the doctrine of integer number," a grand new mathematical system could emerge (Hamilton 1853, 16).

From Hamilton's new perspective, symbolical algebra encourages a highly suggestive and open-ended system of symbolical operational analysis, which allows for the adoption of new sets of foundational guidelines. In Hamilton's mind, however, those new guidelines were his own. In building upon this programmatic call for action, Hamilton prefaces his account of quaternions by describing them as an instantiation of meaningful mathematics formed, in the first instance, by the establishment of new foundational rules in symbolical manipulation (Hamilton 1853). Hamilton writes,

> It will be perceived, by those who shall do me the honor to read this work with attention, that I have employed a *method of transition*, from *theorems proved* for the *particular* to *expressions assumed* for the *general*, which bears a very close *analogy* to the methods of Ohm and Peacock (Hamilton 1853, 16).

The equivalences generated through analysis justify the extension of couples into triplets and then into quaternions—the last of which, Hamilton believes, constitute the forefront of a new revolution in analytical thought.

Triplets to quaternions

Given that his work on couples had previously led to various "unsuccessful" extensions into "triplets," Hamilton re-emphasizes the superior benefits to be gained by adopting a methodology based upon the pursuit of symbolical equivalences. Hamilton starts his account of quaternion mathematics with a discussion of his past attempts to develop a triplet system. He recollects that he had initially attempted to compare two "triads," each of which represented three moments in time.

He had compared, for instance, three moments such as, B_1, B_2, B_3, with three other moments, A_1, A_2, A_3. His aim was to obtain a "triad of ordinal relations," or a "*triad of steps* in time," which he hoped would be represented by the following expression:

$$(B_1, B_2, B_3) - (A_1, A_2, A_3) = (B_1 - A_1, B_2 - A_2, B_3 - A_3).$$

In that set up, the "constituent steps" of the triad would be denoted as:

$$(B_1 - A_1) = a_1, (B_2 - A_2) = a_2 , (B_3 - A_3) = a_3,$$

so that,

$$(B_1, B_2, B_3) = (a_1, a_2, a_3) - (A_1, A_2, A_3).$$

Representing a triad of steps "symbolically" produced "in thought" another moment-triad. Hamilton initially succeeded in showing how the moment-triads could be added (as above), subtracted, multiplied and divided by other number-triads in a manner analogous to the same processes for couples. In so doing, three "distinct and independent unit-steps" emerged—a primary, a secondary and a tertiary (i.e. $1_1, 1_2, 1_3$). In addition, other "unit-numbers" also emerged, "each of which might *operate*, as a species of *factor*, or multiplier, on each of these three steps, or on their system" (Hamilton 1853, 18). Those unit-numbers were represented as by x_1, x_2, x_3. Fully written out in triad form, the unit-steps took the form of $(1,0,0), (0,1,0), (0,0,1)$, where a triad of steps could be represented as $r1_1 + s1_2 + t1_3$, where r, s, t represents three numerical coefficients (positive or negative).

Yet, although $1_1, 1_2, 1_3$ represent three "steps in time," and although triplet factors (m, n, p), "by which [the] step-triplet was to be multiplied, or *operated* upon," could be put into the analogous form $mx_1 + nx_2 + px_3$, a full triplet system still eluded Hamilton. The extension of his findings posed several problems, one of which was the unwieldiness of the resulting algebra. For instance, in explaining the distributive property of this proposed system, Hamilton found that nine products emerged from the combination of a number-triplet with a step-triplet. In proceeding to develop those nine step-triplets into nine trinomial expressions of the following form,

$$1_{f,g} = 1_{f,g,1}1_1 + 1_{f,g,2}1_2 + 1_{f,g,3}1_3,$$

Hamilton found that twenty-seven symbols of the form $1_{f,g,h}$ resulted, each of which served to represent a fixed numerical coefficient (i.e. a "constant of multiplication"). Problematically, as with the analogous coefficients he had used in multiplying couples, the twenty-seven coefficients of the triplet combinations had to be pre-defined in order to produce a "perfectly *definite step-triad*" in the multiplication of a number-triad by step-triad.

In addition to the unwieldiness of the system, Hamilton found various departures "from ordinary analogies of algebra," one of which included the fact that the "product of two triplets may vanish without either factor vanishing" (Hamilton 1853, 22). Though Hamilton had been prepared to widen the narrow limitations of legitimate algebraical operations in order to develop his system further, the various problems he encountered in combining two triplets led him to deem the resulting systems overly "arbitrary" and not worthy of further development.

The geometry of triplets

At about the same time as Hamilton was abandoning his pursuit of general sets of *n* moments, certain new "motivations"—namely, his reading of John Warren's *Treatise on the Geometrical Representation of the Square Roots of Negative Quantities* (1828)—deepened Hamilton's interest in pursuing a geometrical analysis of triplets. Hamilton states that he took up Warren's book in the hopes of advancing his "triplet" efforts. Thus, having faced the seemingly insurmountable algebraical "arbitrariness" associated with extending operations on couples to operations on triplets, Hamilton turned his efforts towards geometrical presentations of the same relations.

Note that Hamilton had encountered Argand's account of the geometrical presentation of imaginary numbers in Peacock's *Report on Certain Branches of Analysis* (1834). That encounter first led him to reconsider the addition and multiplication of directed lines in space. Previously, Hamilton had represented such addition and multiplication only in algebraic terms as "couples". On his reading of Warren, however, he turned to considering the addition of lines as the "composition of motions, or of forces, by drawing the diagonal of a parallelogram" (Hamilton 1853, 32-33). Hamilton tells his reader that various contributions to the study of $\sqrt{-1}$, including the work of Abbé Buée (1806), Benjamin Gompertz in his *The Principles and Applications of Imaginary Quantities, Book II, Derived from a Particular Case of Functional Projections* (1818), John Wallis's much older *Treatise of Algebra* (1695) and, most recently, De Morgan's *Trigonometry and Double Algebra* (1849), had provided him with "a perfectly clear interpretation" of the symbol $\sqrt{-1}$ with regards to operations on right lines in a given plane. In those accounts, $\sqrt{-1}$ denoted "a *second unit-line,* at right angles to that line which had been selected to represent positive unity" (Hamilton 1853, 33-34).

All of the attempts listed above, however, were limited to one plane. Of them, Hamilton wrote,

> When it was proposed to *leave the plane,* and to construct a system which should have *some general analogy* to the known system thus described, but should *extend to space*, then difficulties of a new character arose (Hamilton 1853, 34).

Hamilton recalls that, in the 1830s, he had tried to extend those accounts to points on the surface of a sphere, interpreting the points as "moments". He had failed to produce any useful results.[36] Yet, "among [his] papers," Hamilton contends, there were hints of another approach. In his 1853 "Preface", Hamilton claims to have rediscovered his own symbolical work from the 1830s. He writes that he had contemplated the fact that, "while *lines in space* might be *added* according to the same

[36] Charles Graves (1812–1899), the eventual bishop of Limerick and mathematician, had worked on an extension using two couples laying in perpendicular planes. In that account, a system could be generated in which "two new imaginaries", i and j, served particular roles. The symbol i caused a directed line in space to rotate through a $90°$ angle about the z-axis, while j caused "a line to revolve through an equal angle in its own vertical plane (that is, in the plane of the line and of z)". Graves had multiplied together the triplets $x + ij + jz, x' + iy' + jz'$ such that he obtained (in using a "peculiar" set of rules for multiplication) a third triplet of the form: $x'' + iy'' + jz''$. Graves noted that multiplication of triplets in this manner would not be a distributive operation, although it would be commutative (Hamilton 1853, 38). Graves's approach is of "historical" interest, Hamilton notes—presumably because Graves toyed with the notion of abandoning particular algebraic principles (such as the law of distribution) in order to develop a new system.

rule as in the plane, they might be *multiplied* by multiplying their lengths, and *adding* their polar angles" (Hamilton 1853, 39). When first toying with this idea, Hamilton had become aware of Warren's geometrical account of imaginary numbers, in which the author had used the parameters $x = r\cos\theta$ and $y = r\sin\theta$. Hamilton's aim was to extend Warren's approach to three dimensions by employing the coordinate transformations $x = r\cos\theta$, $y = r\sin\theta\cos\phi$, and $z = r\sin\theta\sin\phi$ in order to define the multiplication of lines in space. He found, however, that the approach did not satisfy the distributive principle, leading him to abandon his efforts once more.

By the early 1840s, Hamilton had been put under pressure by the Board of Governors at his university. Recall the Board had demanded evidence of Hamilton's professional research outputs. Though he does not mention that fact in his "Preface," he does note that his friend and colleague, the bishop Charles Graves (1812–1899), proffered an article in which Graves had sought to represent "usual imaginary quantities of algebra, *each by a corresponding unique point on the surface of a sphere*" that lay about the origin with a radius of 1. In his approach, Graves had expressed $r(\cos\theta + \sqrt{-1}\sin\theta)$ using the triplet (x, y, z), in which he assumed the triplet met the condition $x^2 + y^2 + z^2 = 1$. Graves also claimed rules obtained from such operations would be analogous to similar operations in the "more *general* case," where x, y, z are entirely independent of one another. Graves's idea was based upon a previous suggestion of De Morgan's that one could represent points on the surface of a sphere (Hamilton 1853). Hamilton adopted De Morgan's claims and he used Graves's paper as a basis upon which to develop his own triplet system, picking up the pieces of the disarrayed puzzle he had left unfinished in the late-1830s.

By 1843, Hamilton laid out his objective as that of finding an analogy between the algebraic expression of a triplet system and the study of lines in space. His aim was to "*retain* the *distributive* principle, with which some of his formerly conjectured systems had been inconsistent." At first, Hamilton supposed he "*could* preserve the *commutative* principle *also*, or the convertibility of the factors as to their *order*" (Hamilton 1853, 43). In place of his previous use of $\mathrm{x}_1, \mathrm{x}_2, \mathrm{x}_3$, Hamilton now employed the symbols of $1, i, j$, such that a numerical triplet could be expressed as $x + iy + jz$. The idea was to interpret x, y, z as three different axes, where the triplet denoted a line in space running through the coordinates. By analogy to couples, for which Hamilton had defined $i^2 = -1$, he defined $j^2 = -1$, for "triplets", which he "interpreted as answering to a rotation through two right angles in the plane of xz, [just] as $i^2 = -1$ had corresponded to such a rotation in the plane" (Hamilton 1853, 44). Based on those two definitions, and assuming that $ij = ji$, the multiplication of two triplets resulted in the following symbolical display:

$$(a + ib + jc)(x + iy + jz) = (ax - by - cz) + i(ay + bx) + j(az + cx) + ij(bz + cy).$$

The solution was still problematical, however, as the product ij lacked any meaning apart from its definition within this particular symbolical system. Hamilton did not "at once see what to do with [it]" (Hamilton 1853, 44). The theory of triplets, as he had symbolically defined it in previous years, required that ij be another triplet of the form $ij = a + i\beta + j\gamma$, where the coefficients of α, β, γ were constants. Thus, Hamilton recounts, he was initially at a loss as to how to adapt the new symbol *ij* so as to reflect "some *guiding geometrical analogies"* (Hamilton 1853, 44).

The mathematician recollects that his first successful attempt at understanding the above symbols occurred once he had more fully adopted the symbolical algebraic philosophy of Peacock and De Morgan. Assuming that b and c were proportional to y and z, whatever those symbols referred to (though, geometrically speaking, he noted their proportionality could refer to the fact the entities lay in the same plane), the last term of the product—namely, $ij(bz + cy)$—would disappear, and the product of two triplets would produce a three-termed result

$$ax - by - ca + i(ay + bx) + j(az + cx).$$

That result could then be interpreted as a line with factor lines defined by factor triplets—namely,

$$(a + ib + jc) \text{ and } (x + iy + jz).$$

Hamilton recalls he had been inclined to consider the product ij as equal to 0, since the term $ij(bz + cy)$ had disappeared from the overall product. However, when he considered the factor triplets as being co-planar, he found that b and c were proportional to yz and z. Thus,

$$bz - cy = 0.$$

This meant that the fourth term might well have vanished from the final product mentioned earlier not because $ij = 0$, but because $bz = cy$, and therefore the expansion of $ij(bz + cy)$ would equal 0.[37] The upshot of this latter consideration, he found, was that the following identity held:

$$ij = -ji.$$

In addition,

$$ij = +k, ji = -k,$$

where "k" was not defined.

Hamilton recalls the product of two triplets ought to have produced the following "quadrinomial" form:

$$(a + ib + jc)(x + iy + jz) = (ax - by - cz) + i(ay + bx) + j(az + cx) + k(bz - cy).$$

He observed the squares of the four coefficients of $1, i, j, kk$ did produce the following identity (in which no relation was assumed between a, b, c, x, y, z):

$$(a^2 + b^2 + c^2)(x^2 + y^2 + z^2) = (ax - by - cz)^2 + (ay + bx)^2 + (az + cx)^2 + (bz - cy)^2.$$

Based on those findings, Hamilton states that instead of seeking out triplets of the form $(a + ib + jc)$ or (a, b, c), he realized it would be more effective to use "quaternions" of the form $(a + ib + jc + kd)$ or (a, b, c, d), where the "triplet" arises as a particular instance (i.e. as an "imperfect form"

[37] Hamilton's rules for expanding such multiplications indicate the expansion is equivalent to the sum of $ib.jz$ and $jc.iy$, which is zero if the triplets are co-planar.

of quaternions).[38] The problem now was to define the symbol, k, which Hamilton had simply defined as "some new sort of unit operator." Not only did Hamilton have to find a symbolical definition for k but he also had to determine the meaning of the products that emerged in the symbolical combination of that symbol with the other component parts of the system—namely, ik, jk, ki, kj. In so doing, Hamilton recalls, "it seemed natural" that after having assumed $i^2 = j^2 = -1,\;\; ij = k$, and $ji = -k$, he ought to similarly assume $ki = -ik = -i^2j = +j$, and that $kj = ijk = j^2i = -i$. These assumptions seemed appropriate, he states, even though the definition of k^2 "was less obvious" and "for a while" he was "disposed to consider this square as equal to *positive* unity" (because $i^2j^2 = +1$). In his manipulations, however, Hamilton realized it would be more convenient to simply suppose "in consistency with the foregoing expressions for the products of i, j, k" that

$$k^2 = ijij = -iijj = -i^2j^2 = -(-1)(-1) = -1.$$

In sum, "all the fundamental assumptions for the *multiplication of two quaternions"* were completed by the early-1840s, he concludes. They can be summarized by the foundational rules of quaternions:

$$i^2 = j^2 = k^2 = -1; ij = -ji = k; jk = -kj = i; ki = -ik = j.$$

Consequently, the multiplication of two quaternions can be summarized as follows:

$$(a, b, c, d)(a^{'}, b^{'}, c^{'}, d^{'}) = (a^{''}, b^{''}, c^{''}, d^{''}),$$

or equally,

$$(a + ib + jc + kd)(a^{'} + ib^{'} + jc^{'} + kd^{'}s) = a^{''} + ib^{''} + jc^{''} + kd^{''},$$

assuming that the following four conditions are satisfied by the constituents of the quaternions being multiplied:

$$a^{''} = aa^{'} - bb^{'} - cc^{'} - dd^{'},$$

[38] It is result that emerges when kd does not exist.

$$b'' = (ab' + ba') + (cd' - dc'),$$

$$c'' = (ac' + ca') + (db' - bd'),$$

$$d'' = (ad' - da') + (bc' - cb').$$

Claiming to have been unaware of Euler's theorem regarding the sum of four squares, Hamilton writes that "on trial" he had come to discover the above expressions for a'', b'', c'', d'' had the following modular property:

$$a''^2 + b''^2 + c''^2 + d''^2 = (a^2 + b^2 + c^2 + d^2)(a'^2 + b'^2 + c'^2 + d'^2).$$

Thus, if a line is represented by a triplet of the form $ix + jy + kz$ (rather than the former version $x + iy + jz$), then the product of two lines in space could be represented by a quaternion. The geometrical significance of this construction is "very *simple.*" For example, take the following product of two lines in space (represented by triplets):

$$(ix + jy + kz)(ix' + jy' + kz') = w'' + ix'' + jy'' + kz'',$$

where the following conditions are met,

$$w'' = -xx' - yy' - zz',$$

$$x'' = yz' - zy', y'' = zx' - xz', z'' = xy' - yx'.$$

In this case, the w'' part of the product is entirely independent of ijk and represents the "*product of the lengths of the two-factor lines, multiplied by the cosine of the supplement of their inclination* to each other" (Hamilton 1853, 33).[39] This part of the quaternion—the independent w''—is the part of the quaternion system that Josiah Willard Gibbs (1839-1903) and Oliver Heaviside (1850-1925) later

[39] The other part of the quaternion in this example (namely, $w'' + ix'' + jy'' + kz''$) represents a line in space, which has a length equal to that of the product of the two lengths of the original lines, multiplied by the sine of their inclination relative to one another. This part of the quaternion also has direction. That direction is perpendicular to the plane in which the original factor lines lay; its direction of rotation depends upon the original direction of rotation about the positive k-axis from the positive semi-axis of i towards that of j.

deemed useless in their own development of vector analysis in the post-1880s era. For Hamilton, however, it was the *key* to the usefulness of quaternions, as it allowed for a combined discussion of scalar and vector "magnitudes" in one fell swoop.

In summarizing his development of the quaternion algebraic system, Hamilton writes,

> When the conception...had been so far unfolded and fixed in my mind, I felt that the *new instrument* for applying *calculation to geometry*, for which I had so long sought, was now, at least in part, attained" (Hamilton 1853, 33).

Hamilton recalls first presenting his quaternion definitions to the Royal Irish Academy at a council meeting in October 1843 and again, one month later, at the same institution's general meeting. At those meetings, Hamilton did not present all aspects of his quaternion mathematics; he avoided, for instance, any discussion of the non-commutative aspect of the system which, required better "metaphysical (or *à priori*)" justifications than he had proposed previously (Hamilton 1853, 33). In the *Lectures*, however, he provides that justification by arguing it stems from the fact "no one direction in space is to be regarded as eminent above another" (Hamilton 1853, 33). In other words, the direction of two lines multiplied together is not determined uniquely unless the directions of the lines are predetermined. In such cases, if the two lines in question lie along a similar axis (i.e. they are "co-axal"), then their product follows the rules of arithmetical algebra. If α and β are the two lines in question, then $\alpha\beta - \beta\alpha = 0$. But if the two lines are not rectangular (i.e. perpendicular) then $\alpha\beta + \beta\alpha = 0$.

Hamilton concludes his algebraic account of quaternions by offering one last definition of the rotating operators. The quaternion is best understood as a *quotient* of two directed lines in three dimensional space—i.e. the product of a symbolical operation. In a footnote, Hamilton indicates he had first developed the definition of a quaternion as a "geometrical quotient" in a series of papers published in the *Cambridge and Dublin Mathematical Journal* in the late-1840s. In those papers, he had attempted to build upon the philosophy of Peacock's *Symbolical Algebra* (1845) in extending it to a category he termed "Symbolical Geometry". He wanted to "allow a more prominent influence to the general *laws of symbolical language,*" and to this extent he had "sought to imitate the *Symbolical Algebra* of Dr. Peacock." In his interpretation of the quaternion as a quotient (represented as $b \div a$), Hamilton emphasizes the role that "operation" plays in producing a meaningful solution. The "fundamental property is ... conceived to be, that by *operating*, as a *multiplier* (or at least in a way *analogous* to multiplication), on the *divisor-line*, [a quaternion] *produces* (or generates) the *dividend-line*, b." Thus, it satisfies the following identity:

$$(b \div a) \times a = b.$$

This involves an extended analogy to multiplication from normal arithmetic, given that it implies an operation that affects both length and direction, and also because it does not commute. The term "quaternion" is particularly fitting, therefore, because the comparison of the lengths of two directed lines is contained in one number (or ratio) and the comparison of their directions is contained in a three-termed triplet, where one term denotes the angle between the two lines, while the two remaining terms denote the "aspect of the plane of that angle, or the *direction* of the AXIS of the positive rotation in that plane, *from the* divisor-line (a) to the dividend-line (b)" (Hamilton 1853, 33).

Throughout the remainder of his *Lectures,* Hamilton offers various geometrical and "physical" applications that had been suggested to him by Herschel over the course of the previous decade. He extends particular thanks Cayley, De Morgan, William Fishburn Donkin (1814–1869), the Graves brothers, and William Spottiswoode (1825-1883), whose works had inspired his development of quaternions. Hamilton notes also that it was De Morgan's speculations in 1844 of an "algebra of the n^{th} character" that had encouraged his own speculations on sets of moments in space (Hamilton 1853, 33). Indeed, throughout his *Lectures,* the influence of the symbolical algebraists is manifest. In the first chapters of the book, Hamilton discusses the "general views respecting the *four signs*, $+ - \times \div$" and the "addition and subtraction of lines corresponding to the composition and decomposition of vections, or motions; line plus line, and line minus line" in great detail. In Chapter II, he introduces a definition of the quaternion as a quotient (between two directed lines in space), where the $\div$ sign is described as taking on a new and "extended" meaning to indicate both a change in length and *direction*. In the same chapter, Hamilton introduces his terminology of "tensors" and "scalars", where a "tensor" refers to the magnitude of direction in a directed line, while a "scalar" refers to the "*reals* of ordinary algebra." In Chapter Three, Hamilton explains how the product of a scalar and a vector (that is, the product of a number and a directed line) is itself a directed line with a particular length. The symbols ix, jy, kz are presented as three rectangular lines that can be mapped onto the Cartesian system of co-ordinate space in three dimensions, by indicating a line from the origin $(0,0,0)$ to a point (x, y, z). In Chapter Ten, Hamilton offers an elaboration of the non-commutative character of the composition of "versions", where a version operation results in a change in angle towards a particular hand (i.e. around a particular axis) within a particular plane. The symbols $i, j, \text{and } k$ are then denoted to be vector-units; they are further defined as "quadrantal versors" (or "semi-versors"), and each is said to be represented by the square root of negative one, $\sqrt{-1}$.

Conclusion

Hamilton's "Preface" reflects some of the varied terrains he was navigating through as he produced and legitimated his quaternion mathematics in the 1840s and 1850s. Specifically, I have highlighted here the motivations he drew from the terrain identified as "Irish Science", where he played a role in promoting Protestant-led scientific developments, as well as the terrain identified as "symbolical algebra in the mid-century", which motivated Hamilton to respond to the dominance of Peacockian-influenced analytical practices in England. Thus, the "Preface" should not be read as a direct account of how Hamilton actually developed his quaternions. For example, though he recounts much trial-and-error in his efforts, there is little doubt that there was much *more* trial-and-error than Hamilton admits. But more importantly, Hamilton suggests his process of proceeding—i.e. the methodologies he used in those trial-and-error efforts—were largely symbolical algebraic. That is to say, they were driven by a focus on the operations governing symbolical manipulations, rather than considerations of the geometrical behavior of vectors. This was not truly the case. As part of Hamilton's trial-and-error period of investigation in the 1840s, he attempted many geometrical resolutions to the problem of squaring a triplet (Graves 1882; 1885). Hamilton's unwillingness to elaborate fully upon those previous efforts in his "Preface" is a bold indication of the degree to which he felt that, in order to gain legitimacy in the eyes of English symbolical algebraists, he had to present his work as the production of analytical manipulations that sought to determine operational equivalences between the components of quaternions. In part hoping to satisfy his Irish funders, and in part hoping to gain credence with English algebraists, Hamilton crafted an account of his mathematical works that he hoped would achieve both. His admittance that conceptions of "pure time" still weighed upon him indicates, however, that Hamilton viewed his conceptions as potentially extending beyond Peacock's principle of abstraction. Somewhat like De Morgan during his "second" stage of engagement with Peacock's "principle," Hamilton's aim, as stated in his "Preface," was to gain notice for having pushed beyond the boundaries of Peacockian symbolical algebra, by adopting a version of symbolical geometry that possessed underlying notions of time-based sequences.

By 1858, with the help and encouragement of Tait, Hamilton also began to conceive of the possibility of his quaternions serving as a Tripos exam subject. Tait raised the possibility of quaternions forming part of the developing corpus of symbolical algebraic material being institutionalized within the élite confines of Cambridge's coaching classrooms.[40] We turn then to a more detailed account of the "terrains of knowledge" that Tait navigated through as he engaged with quaternions in order to

[40] Hamilton's work in quaternions has been analyzed from a multiplicity of normative perspectives. Altmann (2005), for instance, argues that Hamilton got his own account of quaternions "wrong." Altmann contends that Hamilton confusedly treated both axial rotations and rotations through an angle as though they were the same thing. In looking to highlight Hamilton's misdemeanor, Altmann offers an account of a quaternion-like approach proposed by another actor, Olinde Rodrigues (1795-1851). Rodrigues had also developed a four-termed rotational system. Rodrigues's history is beyond the parameters of this study, as are analyses of whether or not Hamilton got it "wrong." In fact, the latter consideration is not a concern for social historians, as the focus for such historical analysis should be on determining what it is that Hamilton saw himself as doing, as opposed to normatively determining whether he was "right" or "wrong" in acting as he chose to.

understand why he chose to present quaternions in a manner that differed from Hamilton in his 1853 “Preface.”

Chapter Three: Peter Guthrie Tait and quaternion engagements, 1858-1880

Introduction

On August 19, 1858, Peter Guthrie Tait (1831-1901)—then professor of mathematics in Queen's College Belfast—wrote his first letter to Sir William Rowan Hamilton. He recalled:

> I attacked your volume on Quaternions immediately after its appearance, and easily mastered the first 6 lectures—but the portions I was most desirous of understanding, viz. the physical applications of the method, have given me very considerable trouble; and, but for your offered assistance, I am afraid I should have had to relinquish all hopes of using Quaternions as an instrument in investigation, on account of the time I should have had to spend in acquiring a sufficient knowledge of them.
>
> I have all along preferred mixed, to pure mathematics, and since I left Cambridge where the former are comparatively little attended to, have been busy at the Theories of Heat, Electricity &c. Your remarkable formula for
>
> $$\left\{\left(\frac{d}{dx}\right)^2 + \left(\frac{d}{dy}\right)^2 + \left(\frac{d}{dz}\right)^2\right\},$$
>
> as the square of a vector form, and various analogous ones with quaternion operators, appear to me to offer the very instrument I seek for some general investigations on Potentials, and it is therefore almost entirely on the subject of "Differentials of Quaternions" that I shall trespass on your kindness (Wilkins 2005, 2).[41]

The focus on the differentials of quaternions, and their physical applications to theories of heat, electricity, and vortex motion would come to serve as defining themes in Tait's later quaternion publications, the most widely-received of which appeared after Hamilton's death in 1866.

At the time of his initial communication with the Irish mathematician, Tait had only just begun to establish a professional identity. He was an able mathematician trained in symbolical analysis at Cambridge. Yet, he was also a mathematician who had chosen to pursue a career in "mixed mathematics" (later known as "applied mathematics"). Tait's career choices were not arbitrary. They

[41] There is a discrepancy with regards to the equation cited in this letter. Knott (1911) quotes the same equation as does Wilkins, but he writes it as: $\frac{\partial^2}{\partial x^2}+\frac{\partial^2}{\partial y^2}+\frac{\partial^2}{\partial z^2}$. Knott's (1911) form is a correct representation of what Hamilton intended, although I have kept Wilkins' version for the sake of citing the edited text properly.

were the result of the specific terrains of knowledge he found himself navigating through from the 1850s onwards.

Though Tait was born in Dalkeith (on April 28th, 1831), his mother moved the family (including Tait and his three siblings) to Edinburgh following their father's death in 1837. In Edinburgh, John Ronaldson, Tait's uncle, was able to help raise the children. Ronaldson was a banker—a middle class tradesperson working within Edinburgh's increasingly affluent urban community in the mid-century. He knew well how important social mobility was for a fatherless and penniless boy like his nephew. Thus, Tait's education and training began early. Ronaldson served as the first conduit through which the young Tait was able to access contemporary scientific discourse and instrumentation. The amateur natural philosopher took his nephew on geological walks, engaged him in telescope studies of the stars and planets, and exposed him to newly developed techniques in photography. Ronaldson sent Tait to the Edinburgh Academy, where the latter studied classics for four years with the popular headmaster, James Cumming, and mathematics with James Gloag (1795-1870). Included among his friends at Edinburgh Academy were Lewis Campbell (1830-1908), later a renowned classics scholar, and James Clerk Maxwell (1831-1879). Though both of those youngsters were ahead of Tait by one year, they trailed him in the Edinburgh Academical Club Prize for mathematics in 1846 (Knott 1911).[42]

Having graduated from the Edinburgh Academy, Tait studied for one semester at the University of Edinburgh in 1848, where he was first exposed to symbolical algebra in the natural philosophy courses of James D. Forbes (1809-1868). Tait's more regimented exposure to mathematics also began in Edinburgh, under the tutelage of Philip Kelland (1808-1879). Kelland had graduated from Cambridge in the 1830s, during the rise of the first generation of symbolical algebraists. He prepared Tait for some of the symbolical techniques the young student would later encounter at Cambridge. When Tait did finally enter Cambridge in 1848, he was subjected to the same style of Tripos

[42] Conversely, Tait came in third, behind Campbell and then Maxwell for the classics prize. In 1847, Maxwell beat Tait, coming first (Knott 1911, 4-5).

cramming as were his fellow students. By 1852, the year of Tait's graduation, there was no mention of Hamiltonian quaternions, or any vectorial systems for that matter, on the Tripos. Not surprisingly, when Tait picked up the *Lectures on Quaternions* (1853), published a year after his graduation, he soon discontinued his reading and left the bulk of the book unread, likely concluding it to be of little relative worth to any wrangler budding with the hope of becoming a professional mathematician in Britain. Thus, despite Hamilton's efforts in his 1853 preface to paint himself in the image of symbolical algebra, à la De Morgan and Peacock, his system remained unwieldy, massive and, most importantly, irrelevant to Tripos-oriented students and recent graduates. In the years immediately following his graduation, Tait viewed quaternions and Hamilton's *Lectures* as professional dead-ends.

It was not until 1858, after Tait had already spent some months working in the laboratory of Thomas Andrews (1813-1885), a chemist at Queen's College Belfast that Tait chose to re-engage with Hamilton's treatise on quaternions. When he did so, however, he was treading upon socio-intellectual terrain that differed significantly from that of Cambridge's mathematical milieu. Cambridge had provided Tait with the navigational tools of symbolical analysis; but Belfast provided him with the varied conceptual tools of natural philosophy. Those natural philosophical tools helped Tait to diversify his early mathematical training, imbuing him with the motivation to use mathematical analysis in the service of natural philosophy. And encouraged by Andrews, who lauded Irish science, Tait began to view Hamilton's non-commutative symbolical algebra as a potential tool in the further development of Irish and, later, Scottish science.

Cambridge and Belfast were not the only intellectual terrains relevant to Tait's navigations. As with any actor living in any cultural setting, Tait's life was composed of an infinite number of terrains of knowledge, although only four will be discussed here. The most salient terrains to note include, Cambridge and Belfast's mathematics in the 1850s; mathematical traditions at the University of Edinburgh in the 1860s; industrial Scotland and its lucrative "science of energy"; and Presbyterian politics and its sectarian divisiveness in the mid-century. Each of those terrains overlapped with one another to generate a complex environment within which Tait's natural response was to use symbolical algebraic analysis in tackling both mathematical and natural philosophical problems, and to identify and justify his analytical practices by appealing to claims of "efficiency"—claims that he had learned to make due to his navigations through Scottish Presbyterianism and northern theories of thermodynamics. The aim of this section will be to paint a picture of Tait's diverse intellectual topology as it developed and shaped his approach to, and engagement with, those cultural-historical artifacts known as "quaternions". In so doing, we will see how his quaternion outputs were the product of the unique terrains that Tait navigated through, and which provided him with the motivation to use quaternions in meaningful ways.

An overview of Tait's productions, 1858-1880

During his tenure at Cambridge, Tait began to develop his publication profile by co-authoring a book aimed at the typical Tripos student. It was a text replete with practice problems and exam-style solutions. Entitled *A Treatise on Dynamics of a Particle,* and published in 1856 with W.L. Steele as co-author, the text was in its nascent phases while Tait was still an undergraduate. He continued to work on it throughout his employment as a fellow at Peterhouse College for two years thereafter. Given Tait's position within the milieu of Cambridge coaching, it is not surprising that the *Treatise*—his first major publication—was styled along the lines of a typical Cambridge coaching text. It contained little discussion of the foundational principles of natural philosophy or dynamics, both of which served as focal points for Tait's later career outputs. In addition, although Tait had already read through the first half of Hamilton's *Lectures* in 1853, his analytically-minded *Treatise* made no mention of Hamilton's alternative algebraic system or of its potential uses in dynamics. Evidently, the *Lectures* had made no impression upon the young mathematician by the end of his Cambridge career.

The introduction to Tait and Steele's *Treatise* indicates why this was. In that introduction, the philosophical stance the authors adopt is representative of their four years of Cambridge training. The educational reforms introduced by the previous generation of symbolical algebraists ensured that the young Tait emerged fully equipped with the belief in hand that abstract mathematical presentations could capture the essence of universal relations—not all of which required natural phenomenal or geometrical equivalents. Furthermore, those natural phenomena that were known to exist, such as motion, could be discussed analytically (i.e. in symbolical mathematical terms) without recourse to empirical or experimental knowledge. In the *Treatise,* Tait called this approach "purely geometrical," by which he meant that geometrical relations between particles in motion could be compared and manipulated using the basic rules of algebra. Tait and Steele thus claimed:

> Although the idea of some physical cause is necessary when we consider the effects produced on the motion of a particle, yet there are many properties of motion which may be arrived at without having recourse to anything but the abstract idea of motion itself. Such properties are to be regarded as purely geometrical, in contradistinction to others which depend on our experimental knowledge of the constitution of matter, and the idea of Force (Tait and Steele 1856, xi).

It is in this *Treatise* that one also finds Tait's first published definitions of important concepts such as "dynamics," "statics," "kinetics" and "kinematics"—definitions that demarcated the experiential from the analytical. The authors identified "Dynamics" as the science that investigates "the action of Force." Dynamics, they said, can be divided into two sub-categories: statics and kinetics. Statics focuses on the study of forces that "compel rest or ... prevent change of motion," while "kinetics" focuses on the study of forces that "produce or ... change motion." In kinetics, one must consider *both* the forces at play and their effects upon the bodies whose motion they change. Meanwhile, the field of inquiry that explores the characteristics of the forces that cause change (without worrying about the nature of the bodies being acted upon) includes "kinematics". This latter field was the domain of inquiry that the two young authors focused their efforts upon. Tait and Steele argued that "kinematics" included Newton's calculus of fluxions. The authors made sure, however, to subjugate

Newtonian mathematics to (what they considered to be) the superior technique of "differential calculus." In so doing, Tait and Steele distinguished between Newton's "fluxions," which did not invoke any notion of "limit" or "infinitesimal" (as understood in the post-Cauchy world), and "differential calculus", which did.

In then characterizing their book as one of "kinematics"—or "pure mathematics"—Tait and Steele added to the 19th-century institutionalization of the distinction between "pure" and "mixed" mathematics. Having played upon a vocabulary that was popular in Cambridge, the young authors effectively produced a book that became an immediate success. It proceeded to become one of the most popular teaching texts of the century. A second edition was produced a decade after the first; a seventh edition was issued by the end of the century. Yet, by the mid-1860s, Tait was already lamenting that he had ever produced such a book. In fact, in the preface to his second edition (published after Steele had died), Tait decried the dry, *a*-philosophical approach that he and Steele had unwittingly adopted as recently graduated authors. He declared the text to be a perfect indication of the sort of rote learning and poor insight that Cambridge students often relied upon, emphasizing the prominence of those students who "crammed" their way through the obstacle course of the Tripos exam and who, in Tait's view, often failed to understand the physical phenomena underlying mathematical manipulations.

In the preface to his 1865 edition, for instance, Tait wrote:

> I am glad of the opportunity, presented by the call for a second edition, to make reparation for many faults of the first ... the whole of the second Chapter has been rewritten, upon the basis of the corresponding portion of Thomson and Tait's *Natural Philosophy* which, though as yet unpublished, was printed off nearly two years ago (Tait and Steele 1865, viii).

Tait then tacitly criticized his former Cambridge-inspired "pure" mathematical approach in writing:

> To several important theorems more than one demonstration has been appended: with the object of exhibiting the use of the various processes by applying them to the

> deduction of results of real value, instead of to the solution of [Tripos] "Problems" of unquestionable absurdity (Tait and Steele 1865, ix).

And although Cambridge coaches, like Isaac Todhunter (1820-1884), had told Tait they liked the book well enough in its original form," Tait lamented the fact that support had

> prevented me from attempting a thorough alteration of style which I had contemplated, viz. to cease breaking up the subject into detached propositions—specially fitted for "writing out".[43] I retain my own opinion, however, that this is *not* the form in which such a treatise ought to be written; although there can be no doubt that it offers certain advantages to the student whose sole object in reading is to pass an examination (Tait and Steele 1865, ix).

In sum, Tait bemoaned the fact the *Treatise* was "intended to be merely an analytical one." He even guided his reader to locate "the full discussion and experimental demonstration of the elementary principles on which the analysis is founded" by referring "to works on Natural Philosophy; of which, so far as mere Abstract Dynamics is concerned, we have a more admirable example in [Newton's] *Principia*" (Tait and Steele 1865, ix). All of these lamenting regrets and natural philosophical concerns emerged *only* in the 1860s, however, well after Tait had become inculcated into the northern "science of energy" and its concomitant science of "thermodynamics". In 1858, there were no such laments. There was only a shifting socio-intellectual terrain, upon which Tait trod in his efforts to further define himself professionally.

That shifting terrain had allowed Tait to engage with the world of experimentation in Thomas Andrews's laboratory in Belfast; it had also allowed him to become immersed in the technically-minded world of engineering that had long motivated industrial practice in Belfast. Thus, although Tait's ties to symbolical algebra—and its abstract algebraic manipulations—still informed his research motivations at the end of the 1850s, he had also begun to experience a transformation in his professional and intellectual identities. Consider, for instance, the terms of reengagement that defined Tait's uptake of quaternions from 1858 to 1860. Tait's initial interest in corresponding with Hamilton was not to laud the latter's *algebra* as a worthy system in "pure mathematics"; it was,

[43] "Writing out" was the phrase used to refer to a student's ability to reproduce a set answer to typical Tripos questions.

rather, to speculate over Hermann von Helmholtz's recent account of vortices and the possibility of using quaternions to express vortex motion in space. Entitled "Ueber Integrale der hydrodynamischen Gleichungen, welche den Wirbelbewegungen entsprechen," and published in *Crelle's Journal* (July 1858), the article by Hermann von Helmholtz that initiated Tait's interest in quaternion mathematics explored the equations that represented motion in a "perfect fluid" (i.e. a fluid that experiences no friction and which cannot be compressed). To do so, Helmholtz invoked the following equations, which are known as "Euler's equations" (Epple 1998):

$$X - \frac{1}{h}\frac{dp}{dx} = \frac{du}{dt} + u\frac{du}{dx} + v\frac{du}{dy} + w\frac{du}{dz}$$

$$Y - \frac{1}{h}\frac{dp}{dy} = \frac{dv}{dt} + u\frac{dv}{dx} + v\frac{dv}{dy} + w\frac{dv}{dz}$$

$$Z - \frac{1}{h}\frac{dp}{dz} = \frac{dw}{dt} + u\frac{dw}{dx} + v\frac{dw}{dy} + w\frac{dw}{dz}$$

$$0 = \frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz}.$$

where u, v, w represent the velocity components of the fluid at any given point with coordinates x, y, z. The components of the external force are given by X, Y, Z, while h represents the density of the fluid and p its pressure. Helmholtz's objective was to analyze cases in which there is *no* velocity potential throughout the entire fluid.

Tait became engaged with Helmholtz's account because he was interested in the mechanical interpretation of "potential flows" (where "potential flow" indicates the existence of differing fluid velocities at differing points in a fluid). Helmholtz had used the notion of an infinitesimal in Euler's equations to argue that any instantaneous movement in an infinitely small portion of fluid could be decomposed into a translation, an expansion, and a compression in mutually orthogonal directions, along with a rotation about a particular axis, where the rotational axis would have the following cosine proportionals:

$$\frac{dv}{dz} - \frac{dw}{dy} =: \xi, \qquad \frac{dw}{dx} - \frac{du}{dz} =: \eta, \quad \frac{du}{dy} - \frac{dv}{dx} =: \zeta.$$

Helmholtz then argued the angular velocity of that infinitely small movement is proportional to the square root of $\xi^2 + \eta^2 + \zeta^2$. To describe these very specific infinitesimal rotations, Helmholtz introduced a new vocabulary, including "vortex lines" and "vortex tubes" (Epple 1998, 312-313).

That which most interested Tait was Helmholtz's decomposition of an infinitesimal motion in a fluid into a translation, a compression (or expansion), and a rotation about an axis. Having previously read parts of Hamilton's account of quaternions in 1853, which offered an account of scalar contraction (or expansion) of a directed line along three real axes, and a "vector" change in the line's angular direction, Tait saw an aesthetic resemblance between Helmholtz's speculations and Hamilton's quaternion symbolism. He wondered whether Hamilton's algebraic system could be used to rewrite Helmholtz's speculations in alternative mathematical notation. Indeed, Tait did eventually learn to use Hamilton's notation of i, j, k to represent the imaginary components of a given quaternion, and $V\alpha, S\alpha, \text{and } T\alpha$ to represent, respectively, the quaternion's versor, scalar and tensor parts. He also learned to use Hamilton's differential operator,

$$\nabla = i\frac{d}{dx} + j\frac{d}{dy} + k\frac{d}{dz},$$

to denote the velocity field of any fluid (u, v, w) as a "vector function."[44] Lastly, Tait would later come to conclude that differential operations on a "vector function" could be interpreted as the kinematical fluid flow in a velocity field and that this was proof that quaternionic accounts could intuitively represent physical events occurring within fluids (Epple 1998, 317-318).

[44] He would learn to show how local rotations could be determined by the direction and length of the vector part of Hamilton's differential operator, as applied to the velocity field.

According to at least one historian, it was this "amalgamation of quaternion analysis and flow thinking, made possible by Helmholtz's treatment of vortex motion, [that] served as the central motivation behind [Tait's] crusade for quaternion methods" (Epple 1998, 318). No doubt, Helmholtz's theory of vortex motion did initially spark Tait's memory and cause him to reconsider the aesthetic form of Hamilton's quaternions. However, Epple's (1998) claim that this constituted Tait's primary motivation for re-engaging with quaternions in a detailed manner is an overstatement. Tait's interest in quaternion presentations of vortex motion sparked his initial correspondence with Hamilton, but it was not the primary reason that Tait chose to engage with quaternions for a decade thereafter, especially as given the degree of opposition he faced from close colleagues, such as Thomson. Tait's work in vortex motion did not even constitute a major theme in his initial correspondences with Hamilton from 1858 to 1860. Rather, the topic that most vibrantly coloured Tait's correspondence with Hamilton was the quaternionic account of the equation for Fresnel's wave surface. This topic choice indicated that Tait's motivations for re-engagement with quaternions were more complex in nature than a mere desire to translate vortex motion into a new symbolical language. Given the fame associated with Hamilton's account of Fresnel's wave surface, and his linked "prediction" of canonical refraction, it is likely Tait saw quaternions as a candidate for original research and immediate publication. With Hamilton's help, the transformation of the equation for Fresnel's Wave Surface into quaternionic form constituted original and noteworthy research, and (as Tait hoped) it might benefit from Hamilton's previous scientific fame as associated with his canonical refraction "prediction" in the 1830s.

Thus, Tait's engagement with quaternions in the 1850s was the result of varied motivations—some of which were related to his symbolical mathematical background, some of which related to his budding new interests in natural philosophy, and some of which related to his interests as a developing young professional in need of recognized publications in order to establish a name for himself. In other words, Tait's terrains of knowledge were overlapping, thus providing him with several motivations for engaging further with quaternion mathematics.

Those terrains overlapped in even more complex ways throughout the 1860s, when Tait became professor of natural philosophy at the University of Edinburgh. During that decade, Tait published the *Sketch of Elementary Dynamics* (1863) with Thomson. Those two authors also produced their much-lauded account of science-to-date, *A Treatise on Natural Philosophy, Volume 1* (1867), and Tait followed up with his solo *Sketch of Thermodynamics* (1868). In addition, both Tait and Thomson—sensing, perhaps, the unpalatable nature of their voluminous *Treatise* (1867)—added to their thermodynamic roster by producing the more succinct account of "energy" in the *Elements of Natural Philosophy* (1873). That stream of thermodynamic literature, flowing as it did from the lecture halls and laboratory rooms in Edinburgh and Glasgow, indicates the degree to which Tait and Thomson were embedded within northern "energy" science, with its concomitant notions of "conservation" and "dissipation" (Wise and Smith 1989). Thus, although Tait's exploration of quaternions was initially motivated by his training in symbolical algebra, and his professionalizing objectives, his later "energy" science commitments came to motivate his engagements with quaternions in specific ways well into the 1870s.

From 1859 to 1865, Hamilton was engaged in completing a second book on quaternions, the end product of which was the opaque and poorly-received *Elements of Quaternions* (1866) (published one year after his death). In continuing with his attempts to legitimate his mathematical system within the symbolical algebraic framework, Hamilton's 1866 production did not stray far from the symbolical accounts of quaternions that the Irish mathematician had originally offered in his *Lectures* (1853). Thus, despite Tait's movements towards thermodynamics, Hamilton stayed deeply entrenched within his own Irish world of Cambridge-inspired "pure" mathematics. Indeed, when Tait's solely-authored account, *An Elementary Treatise on Quaternions* (1867), appeared one year later, its preface indicated not only how much the "Tait" of 1867 had come to differ from the "Tait" of 1858, but also how much the Tait of 1867 had come to diverge from Hamilton in general. Tait had become a natural philosopher who used quaternions to advance his natural philosophical agenda; Hamilton had remained a mathematician who used the underpinnings of symbolical algebra to justify his quaternion productions as respectable Irish science.

Throughout this transformative period in Tait's academic life, the Scotsman was also involved (as were most other academics at the time, wittingly or not) in the politico-religious events that pervaded Scotland's public institutions. As an example of his navigations through the world of mid-century Scottish Presbyterianism, consider Tait's co-authored biography of James D. Forbes—*Life and Letters of James David Forbes* (1873). Recall that Forbes had been elected to the post of Edinburgh's chair of natural philosophy following John Leslie's death in 1832. That appointment was largely considered to be the result of Forbes's Tory alignments. Forbes's competitor for the job was the radical reformist Whig David Brewster. Recall that Brewster had been intimately connected with Henry Brougham, the later Lord Chancellor and joint founder (along with Brewster) of the BAAS in the 1830s.[45] At the time of Forbes's appointment, the success of the BAAS was far from certain, and Brewster's alliance with Brougham was as much a liability as it was a resource. Prominent Tory advocates entrenched at the university throughout the early- to mid-19th century favoured Forbes's connections over Brewster's own. Thus, it was the conservative Forbes that served as Edinburgh's top natural philosopher from 1832 onwards (Horn 1967).

Given that Forbes was Tait's first instructor in natural philosophy, it is understandable that the Scottish mathematician harboured a sympathetic and nostalgic view of Forbes's life. Tait's Presbyterian and conservative political views further bolstered those sentiments, solidifying them into a lifelong respect for his former teacher. This held true despite the fact that Brewster was

[45] As a Dissenter, Brewster had found that most university appointments were cut off to him. With Brougham's support, he was eventually ensured a government pension later in life of £100 per annum (which increased in 1836 to £300 a year). It was this same connection that allowed Brewster to be later knighted and appointed to the principalship of the United Colleges of St Leonard and St Salvator at the University of St Andrews in 1838. Late in his life, in 1858, he was elected—again through Brougham's influential positioning—to the post of vice-chancellor of the University of Edinburgh, an office that he used to promote scientific experimentation in the arts curriculum (Morrison-Low 1984).

eventually elected vice-chancellor of Edinburgh university—a post that granted him considerable control over the nature and content of Edinburgh's undergraduate curriculum, including Tait's natural philosophy courses. Indeed, Tait would even come to agree with Brewster on important matters, including the promulgation of increased scientific experimentation within Scottish universities and the establishment of a laboratory at the University of Edinburgh. Yet, Tait never shared Brewster's dissenting political opinions. Rather, he clung to a long-standing conservativeness throughout his life, and his biography of Forbes serves as a poignant reminder of that fact. Tait's unwavering political stance aligned itself with that of Forbes—a Tory in politics and a devout Church of Scotland practitioners.

Those politico-religious views cannot be distinguished from Tait's mathematical and scientific outputs. In fact, Tait eventually issued what was his most controversial "scientific" publication in response to what he perceived to be the threat of irreligiousness in Britain, born in the form of Darwinism and agnosticism in the mid-1860s. *The Unseen Universe: or Physical Speculations on a Future State* (1875) was jointly authored by Tait and the Scottish natural philosopher Balfour Stewart (1828-1887). Originally published anonymously, *The Unseen Universe* constituted nothing less than a religious diatribe attacking Darwinist and secular accounts of archaeological history and the universe's origins. The book coincided with the publication of Tait's natural philosophical lectures, *Recent Advances in Physical Science* (1876), which further advanced a "scientific" account of the unapologetic metaphysics put forward in their 1875 book. Those efforts led both to ridicule and criticism by young secularist mathematicians and scientists, including Clifford. They also led to worried hand-wringing among established scientists, such as Thomson. Yet, Tait did not waver in his religious views or in his public expression of them. Near the end of the 1870s, he had become so confident in his religious-scientific stance—which included his overarching belief in the unidirectional dissipation of a (Divine) "potential" universe—that he published *Paradoxical Philosophy: A Sequel to the Unseen Universe* (1878), again with Stewart as co-author. That text was an extension of the claims made in 1875 and 1876, but this time with an explicit by-line and no attempt at anonymity. Tait was no longer interested in merely satisfying the needs of Tripos crammers or, what he perceived to be, misled mathematical purists. Rather, in a bold attempt to link his northern Presbyterianism to the "science of energy", Tait demonstrated he was interested in explaining the ontological structure of the entire universe. His aim was to engage in a grand metaphysical project—an objective that defined many of his mathematical navigations until the end of the century.[46]

Cambridge and Belfast mathematics

An account of the "terrain of knowledge" that was Cambridge in the early 1850s will help to explain why the young Tait was able to engage meaningfully with quaternions in the late 1850s. Tait entered Cambridge three years after Peacock had published his *Symbolical Algebra* (1845). At the time of Tait's entry, Cambridge had already experienced significant curricular reforms favouring symbolical algebra à la Peacock, Babbage and Herschel. More than ever, the university was being identified with its unique mathematical curriculum, especially because its mathematical degree provided the opportunity for social mobility among middle-class students. Although many Cambridge students aimed for a post within the clergy, mid-century mathematics students often had their sights set on new professional careers within academia. Students who graduated as "wranglers" could bet on a college fellowship or future employment within a university, technical institute, primary or secondary school.

Yet, although Cambridge's mathematical curriculum had managed to increasingly attract non-aristocratic students, the demographic composition of the university's *overall* student body had not actually changed when compared to the first half of the century. The sons of aristocrats still

[46] The brief account offered here is not a complete synopsis of Tait's publications from 1858 to 1880. In his moralistic dialogues, religious discourses, and mathematical researches, Tait proved to be a diverse and prolific writer. He issued a number of smaller articles in popular and specialist forums, related not only to quaternions and the "science of energy", but also to knots, golf ball dynamics, graph theory and meteorology. He also wrote a plethora of obituaries for fellow scientists and mathematicians.

constituted the institution's bread and butter (Warwick 2003). Thus, Tait's Cambridge was a school at an historical crossroads. It was moving towards an era in which symbolical analysis dominated the mathematical curriculum entirely and provided middle-class men opportunities for social mobility, while at the same time relying on its ancient socio-demographic roots for legitimacy and economic stability.

The world of "wrangler" mathematics that Tait entered into, and which he became representative of, was largely dominated by young men emerging from industrial, middle-class, and Anglican (or Presbyterian) families. Between 1830 and 1860, students entering Cambridge remained overwhelmingly representative of the class of Briton who could claim an "upper class" (i.e. landed gentry) gentleman as a father (Becher 1984). Conversely, the majority of *wranglers* (i.e. those studying for an honours bachelor degree in mathematics) hailed predominantly from those families in which the father was a "professional", i.e. a businessmen, a banker, or a lawyer. In fact, only 7.7 per cent of wranglers' fathers were "titled or gentry." Cambridge's wranglers were, therefore, disproportionately representative of the country's rising industrial class.[47] In addition, they overwhelmingly represented *urban*, rather than rural, settings. Of the 220 wranglers for whom educational histories are available, the City of London and its surrounding counties produced a disproportionately high number (more than 45 per cent of the 260 English wranglers of the period) (Becher 1984, 100-101).[48]

One area where wranglers and regular students did overlap was in their religious creed. Apart from the well-known scandals involving the non-graduation of Babbage, and the departure of De Morgan, on religious grounds, only 15 wranglers from 1830 to 1860 were identified as "non-Anglican", and five of those were Presbyterian (the only religious group permitted to take an honours degree without swearing faith to Anglicanism) (Becher 1984, 104). As a Scottish Presbyterian, Tait never

[47] It was also disproportionately industrial in origin when compared to the country's top public schools. In Britain, 67 per cent of students at top public schools could boast aristocratic lineages (Becher 1984).

[48] Many of those wranglers had attended urban universities, such as King's College (25 wranglers) and University College London (10 wranglers), prior to matriculating at Cambridge.

faced the type of religious discrimination reserved for faith-based minorities, such as the Jewish James Joseph Sylvester (1814–1897), who was denied a degree based on his faith.[49] Still, for religious majorities and minorities alike, a high rank on the Tripos constituted the best means of socio-economic advancement.[50] Tait's case was no different. Throughout his undergraduate career, Tait's choices as to which subjects he devoted most time to were shaped by his overarching need to pursue those opportunities that would conduce to social and economic security. As Knott (1911) has pointed out, Tait could not rely upon a wealthy family or any other form of independent financial security once he completed his studies. In entering the halls of Peterhouse in 1848, Tait found himself enmeshed in an educational matrix in which the prestige of top status on the Tripos constituted a primary means of propelling oneself from financial insecurity into a respected professional post.

The world of Cambridge mathematics that Tait entered into was also shifting one. The 1840s had witnessed repeat protests by Whewell and others, who argued against private coaching, which they viewed as undermining the liberal and moral nature of a Cambridge degree. Whewell contended a Cambridge degree ought to encourage students to intuit underlying physical structures, rather than merely regurgitate well-accepted solutions to analytical and symbolical problems. Whewell also argued the unregulated and rapid rise of analysis, and the crammed nature of private tutoring, forced students to skip important steps in physical intuition. On Whewell's view, Cambridge's Tripos-oriented culture was guilty of churning out a flock of standardized students who reproduced

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[49] The removal of religious testing as a credential for an honours degree did not occur in Cambridge until 1858, six years *after* Tait's graduation. Sylvester had attended the University of London in 1828 at the age of 14 to prepare for entry to Cambridge; he subsequently graduated 2nd wrangler in 1837, though he was denied his honours degree because of Jewish faith. Following the repeal of the Test Acts, Sylvester was eventually awarded an honours degree by Cambridge in 1872 (Hunger Parshall 2006).

[50] The range of career choices for former wranglers was large. Of the 212 wranglers (out of 296), for whom later occupation could be determined, the total number of different career paths was 25, including academe, law, business, politics and the military (though the latter three were less popular than might be expected given the parentage of the students). Academe was, by far, the most-often pursued career path, indicating that students did not enroll in Cambridge in order to carry on the family business. Rather, nearly half (or 43 per cent of the top ten wranglers of each year from 1830 to 1860) sought careers in academe (including "higher", "lower" and "other" academic institutions), of which about 60 per cent of those in lower education (where higher-level mathematics was not usually taught) had dual careers in the clergy. Another 27 per cent in higher education (university and public schools, for example, where higher mathematics and science was a part of the main curriculum), also held dual career posts in the clergy.

memorized solutions quickly but who did not grasp the underlying foundations of their solutions. Recall that Peacock had similarly worried about the *extent* of private tutoring at the university. But his concerns centred more specifically on the issue of regulation, or lack thereof, which he felt allowed for too much variation in the standards of coaching (Warwick 2003, 101). Over the course of the following decade, a compromise was struck between the university's administrators and its critics, including Whewell and Peacock. The university accepted Whewell's suggestion that it should divide the curriculum into two parts: "permanent" and "progressive", where the former related to matters of geometry, mechanics, optics and elementary calculus (i.e. "permanent" in the sense of having been practiced for many decades), and the latter related to nouveau topics in higher-level symbolical algebra and analysis, which included analytical and celestial mechanics. However, contrary to Whewell's hopes private coaching would continue. In addition, new regulations formulated in 1846 by a special university syndicate (and implemented in 1848, the year of Tait's entry) extended the examination period to eight days (previously six), where the first three days of the exam were to be devoted to elementary subjects in mathematics, and the remaining five days were to be devoted to honours topics, testing those branches of higher level mathematical analysis that distinguished wranglers from ordinary students.

During this reform period, some of Peacock's suggestions with regards to coaching were also adopted. On Peacock's recommendation, the Board of Mathematical Studies was established—its role being to govern and standardize the content of the Tripos. Ironically, it had been the old *ad hoc* process of coaching and examining students that had initially allowed Peacock to introduce continental d-notation questions in the 1817 exam (despite the fact that such continental techniques were—at the time—of interest only to a small group of students). The newly created Board was given the task of abolishing topics that it found to be overly speculative and too experimental. The problematical topics in question included electricity, magnetism, and heat—all of which lacked "any axiomatic principles whatsoever," the Board argued (Warwick 2003, 102). The result was that Peacock legislatively enshrined an institution-wide emphasis on symbolical analysis and kinematics, rather than allowing the curriculum to absorb or reproduce any developments in experimental and natural philosophy—a move that defined Cambridge mathematical degrees until well into the 1870s (i.e. when the Cavendish laboratory was finally, though controversially, established). The Board recommended that, given the high number of subjects being introduced and examined *ad hoc*, in previous years, "... the mathematical theories of electricity, magnetism, and heat should not be admitted as subjects of examination."[51] In the following year, the Board further recommended "the omission of elliptic integrals, Laplace's coefficients, capillary attraction, the figure of the earth considered as heterogeneous, &c, besides certain limitations of the questions in lunar and planetary theory." As President of the Mathematical Society, J.W. Glaisher, later recounted,

> In making these recommendations, the Board expressed their opinion that they were only giving definite form to what had become the practice in the examination, and were only putting before the candidates such results as they might themselves have deduced by the study of the Senate House papers of the last few years (Glaisher 1886, 20).

Those combined curricular reforms meant that, by the time Tait entered, Cambridge's curriculum had become highly homogenized.[52] Tait entered Peterhouse just as the Board's content reforms were being implemented. The subjects that would later constitute the core of his scientific career—namely, heat, magnetism, and electricity—had been excised from the curriculum, accounting therefore for Tait's early focus on "pure mathematics" and "kinematics" in the *Treatise on Dynamics of a Particle* (1856).

[51] As Glaisher explains, "The introduction of a new subject had been generally preceded by the publication of a work, by a Cambridge mathematician, in which it was treated in a manner adapted to the examination" (Glaisher 1886, 19).

[52] Only in 1865 would the Board recommend any slight modification to its reformist rules, stipulating the inclusion of Laplace's coefficients and the "figure of the earth, considered as heterogeneous." Otherwise, as Glaisher recounts, the exam manifested a fairly homogenous set of analytical questions between 1848 and the late-1870s. Both Peacock and Hopkins were satisfied with aspects of the Board's efforts. It created for the first time a regular syllabus material that could be used to teach standard Cambridge students, as well as its top achievers. Whewell remained unconvinced and perturbed by the amount of higher-level analysis still present in the examination syllabus. He alone felt the university's curriculum should focus on the skills of the lowest achiever, such that even the least competent student could see the mind of God at work in the design of the universe (Warwick 2003 94-96, 102).

The second generation of Cambridge symbolical algebra

By the early 1840s, as Peacock was composing and publishing his second editions of the *Treatise on Algebra* (1842-1845), emerging around him was a community of "Whig mathematicians of the second generation" (Smith and Wise 1989). Among the "older generation", Lagrange's technique of using the Taylor series expansion to represent derivatives had been popular; it was now falling out of favour. There was a push to include the notion of a "limit", although Cauchy's "formal" approach (which introduced epsilon-delta notation) did not fully satisfy Cambridge mathematicians, either. Thus, the newly emerging approach to limits still relied upon a lingering sense of infinitesimals—i.e. quantities that got infinitely smaller and smaller. This approach defined the derivative, say $\frac{dy}{dx}$, or $\frac{df(x)}{dx}$, as the limit of the ratio $\frac{f(x+h)-f(x)}{h}$, where "h" approaches zero. However, that approach had raised its own problems. For instance, the limit did not exist if the ratio became infinite as h approached zero. Thus, the burden of proof was on the mathematicians using the technique to show that the ratio remained finite as $h \to 0$.

Whewell's resolution to the issue was to advocate a more traditional "intuition" of a "limit," which he expressed in his *Doctrine of Limits* (1838). In 1842, Augustus De Morgan published a slightly different account of limits in his *Differential and Integral Calculus*, where he reintroduced the notion of a limit as "*ultimate ratios,*" arguing they required no aid "whatsoever from the theory of series, or algebraical expansions" (De Morgan 1842, 3). De Morgan's aim was not to return to a traditional notion of the "intuition" of limits as Whewell had done, but rather to edify students by making more explicit the physical processes occurring in the computation of a limit. De Morgan contended that students not well-versed in higher analysis required help in formulating the concept of a limit in physical or geometrical terms. The epistemological benefit of his approach, he said, was that it would encourage better mathematical "reasoning" overall. In his textbook, De Morgan applied the notion of limits to problems in mechanics and geometry, which would have resonated well with his UCL students.[53]

A decade later, James Thomson (senior) followed De Morgan's lead and published a second edition to his *Introduction to the Differential and Integral Calculus* (1848) for use by his Glaswegian students. In that text, Thomson identified De Morgan as one of the "latest and best writers" to have "made the Method of Limits, or, what is virtually equivalent, the Infinitesimal Method, the basis of their treatises" (Smith and Wise 1989, 170). Thomson's appreciation for De Morgan came largely through the conduit of his son, William, who had by then become an active participant in the second generation of symbolical algebraists at Cambridge. Those students considered Whewell to be an obstacle to the further advancement of mathematics. In contradistinction to Whewell's insistence on an "intuitive" approach to limits, young analysts such as Gregory and Thomson praised De Morgan's approach. Thomson expressed his praise in letters home, in which he also applauded Peacock's symbolical algebraic philosophy more generally (Smith and Wise 1989, 171). As a student at Cambridge during the height of these mid-decade debates over the nature of symbolical algebra, the young Thomson became a believer in De Morgan's style of thinking. Thomson followed De Morgan in accepting the general analytical faith that symbolical reasoning expressed universal truths and that those truths are not reliant upon empirical content for their justification. However, like De Morgan, Thomson also noted that such truths are largely irrelevant to most daily happenings. They are not applicable to industry or machines. They do not produce usable results. Upon emerging from Cambridge, Thomson was, in many ways, a De Morgian—a student inspired by Peacock's symbolical algebra, but De Morgan's specific interpretation it.

In responding to the apparent student-led rejections of his ideas, Whewell complained in 1845 to Herschel that "the most active students [are] being encouraged to study rather the latest improvements, contained in memoirs, journals and pamphlets, than the standard works of

[53] De Morgan's textbook was written for the Society for the Diffusion of Useful Knowledge, as well as for his students at UCL.

mathematical literature." Whewell likely had in mind Gregory and Smith's *Cambridge Mathematical Journal*, given that the *CMJ* represented the youngish group of reformist mathematicians (including Thomson) among whom Peacock and De Morgan had become leaders in symbolical analysis (Crilly 2004). Among, this second generation of analysts, Whig politics and educational reform had fused with industrial concerns in more than just the metaphorical ways in which Babbage and Herschel had previously used manufacturing metaphors to serve as images of the symbolical mind at work. Here now was a group of young mathematicians whose aim it was to become *professional* mathematicians, and who did not oppose working within an industrialized context. Students such as Archibald Smith (1813-1872), R.L. Ellis (1817-1859), Thomson and Tait all came from, or had connections to, Scotland or Ireland—the industrial heartlands of the British Empire. This group of "northern Whigs and their commercially oriented peers" transformed the pursuits of gentlemanly science more broadly in Britain, rendering possible "a regular profession of mathematics" (Smith and Wise 1989, 176).

Two key features characterized this new generation of professionalizing mathematician. They were the hunt for "power" and the desire for "efficiency". These overriding objectives can be indentified in the mathematical and scientific pursuits of the actors in question. These principles were manifest in mathematics, for instance, by stipulating that the highest principles to be attained in mathematical research were generality of expression (power), simplicity of technique (efficiency), and utility of results (industrial applicability). The "symmetrical method" in mathematics involved expressing the equations of analytic geometry in a manner such that figures could be described in space without reference to particular axes. Rather than expressing the equation of straight lines as $y = kx + b$, one could express the general line as $mx + ny = c$, so that the "coordinates appear symmetrically and the equation has the same form for all orientations of the axes." Smith had applied this in his 1835 method in simplifying a derivation that Ampère had given of Fresnel's wave surface. Whereas Ampère's derivation had taken 32 pages, Smith was able to extract the wave surface in symmetrical form in two pages of analysis. It was a method lauded and publicized in subsequent issues of the *CMJ,* which stipulated that submissions had to be short, to the point, and entirely symbolical. In effect, Smith's solution had set the new standard for mathematical "efficiency" (for an account of the CMJ's standards see Crilly (2004); for an account of its place within mathematical literature more generally see Bartle (2005)).

Young actors such as Gregory, Sylvester, Boole and Cayley played a foundational role in promoting this succinct, symbolical approach at Cambridge, in particular by providing more generalized accounts of differentiation and integration, thereby extending the methodology advocated by Peacock and De Morgan in the second half of the century. The nascent stages of this symbolization of differentiation and integration is indicated in a letter from William Thomson to his father a few months after his arrival to Cambridge in 1841. In that letter, Thomson writes,

> On Thursday I got an examination paper in algebra from Hopkins. Almost every question in it was on the principles of algebra, and seemed to inculcate most of Peacock's views, with which I am beginning to agree. Mr Cookson had lent me Peacock, and so I was able to answer most of the questions, I hope in the way he wished. I have been calling two or three times on Gregory, and he seems to take quite the same view of the principles of algebra. He has been doing a great deal in finding the values of definite integrals, in a very curious way, by the separation of symbols. He has given one specimen of the method, which I believe is his own discovery in the Math. Journal, and others in his "Examples." He is not however quite clear about the principles of it yet (Smith and Wise 1989, 181).

The "separation of symbols" technique mentioned in Thomson's letter can be explained as follows. In differential calculus, the symbol $\frac{dy}{dx}$ had indicated the operation of differentiation. That symbol had not been used previously to indicate an entity that could itself be operated upon. Gregory, however, chose to treat $\frac{dy}{dx}$ as though it were a magnitude that could be operated upon. Gregory rewrote linear differential equations as:

$$\frac{d^2y}{dx^2} + \frac{dy}{dx} + ay = g(x),$$

in the form:

$$\left(\frac{d^2}{dx^2} + \frac{d}{dx} + a\right) y = g(x).$$

Gregory treated the "y" contained in the symbols for differential operations as though it were a quantity that could be manipulated using the regular rules of distribution. This meant the entire bracketed part in the latter equation could be treated as one operation acting on y, which could be symbolized as $f\left(\frac{d}{dx}\right)$, which could then be treated as a linear function of the operation $\frac{d}{dx}$. In Gregory's symbolical terminology:

$$f\left(\frac{d}{dx}\right) y = g(x),$$

where the inverse equation is,

$$y = f^{-1} g(x).$$

The latter equation could then be used to solve the differential equation for y, or to integrate it, if f^{-1} represented a meaningful inverse operation in calculus (analogously to how it represented an inverse quantity in normal algebra). In sum, Gregory's "separation of symbols" method meant that f^{-1} could represent a linear function of the operation of integration just as f could represent a linear function of differentiation (Smith and Wise 1989, 181).

Gregory worked to link algebra and calculus at an abstract level. He claimed the two approaches did not differ based on the unit of analysis (in algebra "quantity" was manipulated; in calculus "operations" were manipulated), because all "quantity" could be represented as "operations." Quantities such as a and x could be regarded as operations on unity, i.e. $a(1)$ and $x(1)$. Gregory ripped a page out of Peacock's text and applied it to problems of differentiation and integration in a manner that had not been advocated for before (at least not in Britain), but for which the philosophical resources of "symbolical algebra" provided justification. Consider, for example, Gregory's claim that $a(x)$ and $a^n x$ constituted the operations of a performed on x and a performed

n times on x, respectively. For Gregory, the theorems of algebra are only true if the rules of combination of operations are also true. This was a reapplication of Peacock's "principle of the permanence of equivalent forms" extended to include operational symbols. Gregory argued, "Whatever is proved of the latter [algebraic] symbols, from their known laws of combination, must be equally true of all other symbols which are subject to the same laws of combination" (Smith and Wise 1989, 182). The regular rules of algebraic operations performed on symbols assumed to represent quantity held also for symbols assumed to represent operations in differentiation and integration. If the following rules held in normal algebra, namely:

$a^m a^n(x) = a^{m+n}(x)$ index rule;

$a[b(x)] = b[a(x)]$ commutative rule; and,

$a(x) + a(y) = a(x + y)$ distributive rule,

then the following rules are also true in a newly extended form of algebra. That is:

$d^m d^n(x) = d^{m+n}(x)$;

$\frac{d}{dx}\left[\frac{d}{dy}(z)\right] = \frac{d}{dy}\left[\frac{d}{dx}(z)\right]$;

$d(x) + d(y) = d(x + y)$.

In sum, if a particular procedure or operation holds in normal algebra, it also holds in symbolical representations of differential equations (Smith and Wise 1989, 182).

By relying upon the Peacockian tradition in symbolical algebra, Gregory and his colleagues were able to extend basic operations to complicated and, at times, seemingly meaningless symbolical expression. The "terrain of knowledge" that is described by Cambridge mathematics in the mid-century included those new mathematical practices. As Gregory wrote in 1841, interpretation with regards to empirical reality was an important part of science, but not a necessary part of symbolical algebra:

> Algebra takes cognizance only of the laws of combination of the symbols, and not of their meaning—in the eye of that science the symbol and the operation are identical. When we turn to the interpretation of our results, we must of course consider the meanings of the symbols—but such interpretation is out of the province of Algebra, and belongs to the science, the operations of which are symbolized (Gregory 1841, 2).

Thomson's practices at the time also indicate how strongly the symbolical algebraic terrain of knowledge that was Cambridge mathematics contoured the engagements of budding mathematicians across the country. Having read Gregory's paper, "On the Integration of linear differential equations with constant coefficients," (published in the *Cambridge Mathematical Journal*), the 17-year old Thomson furnished his own contribution to symbolical analysis just before arriving to Cambridge in 1840. That paper (his third submission to the journal), "On the uniform motion of heat in homogenous solid bodies, and its connection with the mathematical theory of electricity," was eventually published in the *CMJ* in 1843. In it, Thomson argued that if a problem in one domain of physics could be presented in the same mathematical form as a problem in any other domain of physics, then the solution determined for the first problem must also hold in the second. It was in this manner that Thomson linked "heat" to "electricity".[54] Thomson's use of analogy in natural philosophy drew on Gregory's use of analogy in algebra, where mathematical operations are analogous across differing sets of mathematical symbols, although Thomson did diverge from Cambridge's mathematicians in applying the method to physical operations across differing sets of physical entities, his inculcation into the new generation of Cambridge symbolical algebra had equipped him with the tools and motivation required to translate natural philosophical concepts into algebraic form throughout the remainder of his life.

[54] For an account of Thomson's use of analogies, see Roche (2008).

The successful Scotsman

Tait and the young W.J. Steele (Tait's later co-author) both arrived at Peterhouse in 1848. Both were identified early on as potential high wranglers. The expectation was that Steele would prevail as Senior Wrangler. In his personal notebooks, Tait recounted his arduous preparation for the Tripos. From December 1851 to January 1852, he devoted five to six and a half hours per day revising. On January 8th, he began to study again after a "Brief Respite from Torment" behind him. During the following eight days, his study time ranged between five-and-a-half to seven-and-a-half hours each day. On the last day of preparation, January 19th, he wrote across the page of his diary, in large capital letters "L'ENFER!", indicating the beginning of the exam period (Knott 1911, 8).

Despite the hell Tait felt himself to be experiencing, his studying paid off. He ranked first among his colleagues, with his winning achievement recounted in friendly terms by J.D. Hamilton Dickson in the *Magazine of the Peterhouse Sexcentenary Club for the Michaelmas Term* (1902). Dickson wrote,

> How the old gyp's face used to light up as he told the story of that January morning when the Tripos list was read. One gyp was in the Senate House to hear the list, and as soon as Steele's name came out as Senior Wrangler he was to rush out and make a signal by stretching out his arms like a big T; another gyp near the 'Bull' was to repeat the signal; and a third at the College gate was to rush in with the news. When that list was read and Tait's name came first the gyp nearly collapsed, but hearing Steele's name next he recovered, and noting on that Peterhouse was first, rushed out, made the signal, and fled with all speed to College to correct the pardonable error he had telegraphed (Knott 1911, 9).

Placing first on the Tripos was a social achievement as much as it was a mathematical one. When Tait discovered the result, he telegraphed home: "Tait Senior, Steele second, tell Gloag." The message was curt, but meaningful. James Gloag, Tait's former mathematics teacher, received it promptly, and his elated reception was documented in the *Chronicles of the Cumming Club*:

> When intelligence reached the Academy of the great event, Gloag was "raised" and out of himself with excitement. "Have ye hard the news aboot Tait?" he asked of everybody he met, M— among others. "No," answered M—, "he's got a Bishopric, I suppose, or something of that sort." "No, Sir, it's not Archibald Cam'ell Tait it's Peter Guthrie Tait, a vary different parson—Senior Wrangler, Sir," and off he went to spread the news (Knott 1911, 9).

On January 31st, Tait also wrote to Sir Doyle Money Shaw, president of the Cumming Club (a group of former students of James Cumming, master of Edinburgh Academy), describing his rank in poetic excitement:

> My dear Doyle,
>
> I'm all in a flutter
> I scarcely can utter, &c., as
> the song has it:--
> I AM SENIOR WRANGLER!
> Tell it to the Cumming Club—&c.
> &c. and believe me
> yours very sincerely,
> PETER GUTHRIE TAIT, B.A. (Knott 1911, 9)

Tait had reason to boast and his former classmates had reason to celebrate.[55] Among the top 300 wranglers from 1830 to 1860, only 20 were or Scottish origin and at least one of those would come *after* Tait (namely, James Clerk Maxwell, 2nd Wrangler, 1854) (Becher 1984).

Tait's personal euphoria would not last long, however. For the next two years, he worked as a college fellow at Peterhouse College, where he found himself shoved back into the Tripos system, now with the aim of training others to perform as he had on the examination. Though Tait might not have resented the experience with as much disgust as he would later claim to have, he did recall his job had become that of "eagerly scanning examination papers of former years, and mysteriously

[55] In recognition of his grand achievement, the Cumming Club organized a special meeting in Tait's honour. The club's *Chronicles* reported that Gloag and the Academy Master felt Tait deserved to have a "banquet specially designed to do him worship." Indeed, the fever of support was so strong that traditional rules were broken and energetic festivities ensued, unlike any the club had previously experienced. As Knott recounts: "For once the exclusive rule of the Club was broken through, and invitations scattered with a lavish hand amongst those—and they were many—who beyond the limits of the Class, held kindly memories of Tait and of the Academy.... It was a high occasion for them all. Gloag could hardly divest himself of the idea that he was the hero of the occasion, such credit did he take to himself... Festive conversation was at fullest swing—that is to say, many talkers, few listeners—when suddenly the scene of revelry was broken in upon by an ominous 'boom.' Tongues were still for a moment, but only for a moment. Then once again, clearer, deadlier than before, the "boom" is heard above the clatter of tongues. In a moment the mystery is solved. The President, Doyle Shaw, ever active for good, or evil, from his end of the table as it approached the gallery, had observed peeping over the edge of this gallery, at an inviting angle, the rim of a big drum. Straightway the idea arose that by well directed vertical fire this tempting object might be reached. The first orange discharged hit the mark unobserved by the company, but the second "boom" discovered all. The idea was hailed as a brilliant one that only needed development. The entire dessert, organs and apples, was soon expended. Then the thought occurred to Doyle Money Shaw to improve upon his original idea. While the practice was still going on he managed cleverly to 'swarm' up one of the pillars with the intention of capturing the big drum. But on arriving at the spot and with shout of ecstasy he announced to those below that the entire band instruments were there. Without a moment's loss of time these were handed down, and from hand to hand; and nothing would serve these festive spirits but the "Conquering Hero" in Tait's honour" (Knott 1911, 10).

finding out the peculiarities of the Moderators and Examiners under whose hands their pupils are doomed to pass" and then spending his life "discovering which pages of a text-book a man ought to read and which will not be likely to 'pay'" (Knott 1991, 11). Tait remained critical of the Tripos system for years after his graduation, despite pandering to it in his early publications. In speaking to his Edinburgh students a decade after having entered Cambridge, Tait stated,

> The value of any portion as an intellectual exercise is never thought of; the all-important question is—Is it likely to be set? I speak with no horror of or aversion to such men; I was one of them myself, and thought it perfectly natural, as they all do. But I hope such a system may never be introduced here (Knott 1911, 11).

Tait's 1860s recollections are somewhat unfair to the worth of the exam, however. Without doubt, the Tripos had come to represent issues at the forefront of mathematical research at Cambridge and across Europe. New and unexpected questions also often led to fruitful research among graduates. The *CMJ,* for instance, regularly published short articles with proposed solutions to past Tripos problems. Those answer solutions often gave rise to other new publications. Near the end of the 19th-century, Edward Routh (one of the university's famed "coaches") recounted how Hopkins's technique of "infinitesimal impulses"—a notion that Hopkins had used to clarify difficult problems in dynamics to his students—was eventually "published" in the form of a Tripos question in 1853, set by one of Hopkins's former pupils. Following that examination, Arthur Cayley (Senior Wrangler, 1842), Tait and Steele all published solutions to differing Tripos problems using the same technique (Warwick 2003, 157-158). The intimate relationship between mathematics as it appeared on the Tripos and active research as it played itself out on the pages of expert publications, such as the *CMJ,* helped to create a newly emergent culture of Tripos-inspired professional research at Cambridge in the mid-century (Warwick 2003). It was this practice-ladenness of Cambridge's "pure" Tripos mathematics of the 1840s and 1850s that first allowed Tait to use quaternions in (what he felt were) meaningful ways, as he entered his first professional post in 1856.

A Belfast story

As Tait finished his youthful schooling in Edinburgh and then approached the mid-point of his university training at Cambridge, Ireland was being weakened by starvation and emaciated by famine. Political cries for deep legislative reform emerged as part of the multifarious reactions to the country's undeniable socio-economic degradation. In broad terms, Ireland's population could be characterized as having been composed of three groups: politically-dispossessed Catholic peasants (many of whom lived as tenants on Anglo-Irish land, and many of whom operated within the socio-financial confines of a subsistence economy); politically dominant and wealthy Anglicans; and a small group of Presbyterians, which peppered the Irish landscape and which mediated between Catholics and Protestants in an undefined mid-way citizenship of quasi-subjugation and quasi-empowerment.[56]

Over the course of the 19th-century, the bulk of the island "continued in a state of commercial and industrial backwardness," although the North of Ireland began to approximate Scotland's Clydeside—one of the great workshops of the British Empire (Smith and Wise 1989, 8). By 1800, Belfast had transformed itself into a major commercial and industrial centre, replete with factories and political ideals to accompany its transformation. By the 1820s, the city had opened the world's first cross-channel steamer service to the Clyde, fuelling the entire country's population boom. By the 1840s, the population of Belfast had ballooned to over 70 000 people, compared to a mere 20 000 in 1800, rendering it Ireland's biggest and most prosperous town. The principles of

[56] Though small in numbers, the Irish Presbyterians formed well-organized dissenting groups of faithful practitioners. At times, Presbyterians constituted a cohesive political force, establishing venerable Presbyterian institutions that were used to lobby government throughout the 17th and 18th centuries. These three groups navigated around one another, sometimes forming useful partnerships, while at other times clashing in economically and politically-motivated violence (Smith and Wise 1989, 4-5). In this tripartite division, political struggles over public institutions often took on religious overtones, recasting the politics of power into the politics of faith and allegiance. In 1791, a new non-denominational political force also emerged, colouring the landscape in unprecedented ways. The Society of United Irishmen coalesced with the objective of uniting disparate religious worshippers across the country. That group's aim was to establish legislatively-supported religious toleration, ending thereby the legal inequity existent between practitioners of differing faiths. Highly popular for a brief time, the "united" Irish front was eventually defeated in 1798 by the Act of Union between Great Britain and Ireland in 1801. The Union established an Anglo-Irish (Protestant) political partnership. Although it failed to initiate a socio-cultural homogenization across the country, it did mitigate some of the radical zeal that had been evident in the north of the country throughout much of the 18th-century. As Presbyterians increasingly moved away from supporting the United Irish cause, and moved towards supporting the Anglo-Irish accord in the hopes of gaining increased political representation, fewer "united" Irishmen gained prominence in public life.

"improvement" and "enlightenment" coloured the pages of the region's newspapers. In 1806, those ideals materialized in the form of public educational institutions that aimed to redress the region's past sectarian divisions, by running non-denominational programming. One such institution was the Belfast Academical Institution, founded in part by William Drennan (1754-1820), a former United Irishman, who had agitated for Catholic emancipation. That institution accepted latitudinarian-minded academics, such as the senior James Thomson, who became professor of mathematics from 1814 onwards.[57]

Industrialization was not, however, a panacea for all internecine politicking and heartache in Ireland. By the late-1810s, the Academical Institution lost its government grants due to the overtly religious views of its headmasters. Though those grants were reinstated in 1828, the institution continued facing pressure from radical branches of the Presbyterian community, which had long opposed non-sectarian education. That meant some institutions in Northern Ireland were being pressured to move away from the country's hard-earned reputation for latitudinarianism and non-sectarianism, and towards renewed religious divisiveness. Despite the denouement of radical politics in Ireland in the late 18th-century—a phenomenon that had informed the liberal and latitudinarian views of

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[57] The Belfast Academical Institution formed its curriculum with the aim of satisfying the needs of Belfast's newly industrializing environment as well as its diverse religious needs. Though Drennan's school still produced ministers for various sects of religious practice, it also provided widely-popular lectures in arithmetic and geography. In response to the demand for technical topics, James Thomson also published textbook, *A Treatise on Arithmetic in Theory and Practice* (1819), in which he emphasized "Mercantile Arithmetic." Questions on navigation, insurance, land, commodities and manufactures featured prominently (Smith and Wise 1989, 15). It was within this context of latitudinarian appeal and industrialization that Thomson became an early and enthusiastic supporter of the introduction of continental techniques to Ireland, representative—as he felt they were—of France's revolutionary and republican democracy. Writing an anonymously authored review in the *Belfast Magazine* (1825) on the state of science in Scotland and Ireland, Thomson had argued: "Since the days of Newton ... British mathematicians have been far surpassed in several branches of science by their neighbours on the continent. This has been particularly the case in the higher and more difficult parts of pure mathematics, and in physical astronomy; in which the Bernoullis, Clairault [sic], D'Alembert, Euler, Lagrange and Laplace, have made such discoveries as will form monuments of glory, not only to themselves, but to Mankind. These great men pursued the path pointed out by Newton, and explored the mechanism of the universe, with such masterly power, and such distinguished success, as must ever be considered among the most glorious triumphs of mankind ... While ... the men of Science in Britain were wasting their time and talents, some in restoring the ancient geometry of Greece, and some in following servilely and implicitly the *manner* in which Newton presented his investigations, without being actuated by the *spirit* by which he was directed in his researches." Thomson further contended: "To follow, at the present day, the modes of investigation employed by the original discoverers ... to the exclusion of the new aids of science, is as absurd as it would be to reject the use of the steam-engine as a prime mover for machinery" (Smith and Wise 1989, 17-18). Recall that Babbage had previously argued productive modes of manufacturing were manifest in automated machines just as symbolical and analytical modes of thinking were manifest in the mind's power to efficiently abstract. A decade after Babbage had made his argument, Thomson linked continental mathematics to the progressive development of the country's industrial ethos. The Academical Institution where Thomson taught for over a decade served as a microcosmic instantiation of Ireland's grander industrial development.

actors such as Thomson—a certain degree of re-radicalization occurred in the mid-19th century. Ireland was increasingly caught up in vehement debates over the extension of the franchise to all religious parties and the establishment of equal legal status for all its religious practitioners.[58] In fact, it was due to these renewed religious divisions that James Thomson eventually found the academic environment of Glasgow more conducive to his latitudinarian outlook, leading him to relocate there with his family in the 1830s following his wife's death (Smith and Wise 1989, 12).

The Ireland that Thomson left, however, was similar to the Ireland that Tait would encounter upon arriving to Belfast in 1854, two decades later. Having left the staid political environment of Cambridge, Tait arrived in a city and a country where long-reigning instability, internecine battle and explicitly propagandized religious vitriol had characterized the bulk of recent history. It was a colourful, passionate and influential environment for any impressionable person to walk into. And while the Thomsons had left in disgust, seeking a less divisive realm of existence, Tait stayed on in Belfast for six years, absorbing the local culture of unapologetic religious expression and explicit religious diatribe—an experience that explains, to some degree, why it is that Tait felt comfortable publicly expounding upon his own religious-scientific views in later years, even though his colleague William Thomson did not.

Tait at Queen's College

Of the most salient aspects of Tait's work in Belfast, the first was his collaboration with the chemist Professor of Chemistry, Thomas Andrews (1813-1885), which taught him how to become an experimental natural philosopher. The second was his engagement with Hamilton's calculus of

[58] The provocative political spirit of its varied ethno-religious communities embodied itself in the 1840s in a group of Ulster liberals, who formed into the Ulster Constitutional Association. The Association had as its mandate: "To obtain for Ireland an equalization of all rights, franchises, and benefits with Britain; and the closest possible assimilation of laws and institutions of both countries; to the end, that, by complete incorporation, the system of imperial legislation may be rendered permanently beneficial to the interests of the United Kingdom, and thus strengthen and perpetuate a connection advantageous, if maintained on such principles, to the people of both countries, and essential to the preservation of this great empire in its commanding position among the nations of the earth" (Kennedy 1949, 242). Throughout the remainder of the decade, the Ulster Constitutional Association issued calls for reform to local government, pushed for "extended federalism" with parliaments in England, Scotland and Ireland, and encouraged local political elections and financing (Kennedy 1949, 246).

quaternions, which allowed him to publish unique mathematical research at the outset of his professional career. By the time Tait arrived at Queen's College in 1858, Andrews had been appointed Vice-President of the university. Andrews had studied at Trinity College, Dublin in the early 1830s at the height of Hamilton's fame over the prediction of canonical refraction, (he had gained distinctions in both classics and science). He went on to study at the University of Edinburgh, gaining his MD before returning to Belfast to initiate his medical practice and teach chemistry. After taking up the Vice Presidency at Queen's College, he became known to the scientific world as the author of papers on the subjects of voltaic action and heat of combination (Harden 2004). An Irish patriot in politics, though not an explicitly nationalist one, Andrews was also a socially-active Irishman who had volunteered to help feed impoverished sufferers during the famine. As an academic, he associated Hamilton's name with Irish renewal. In his support of Tait's quaternion engagements, there is a sense in which Andrews sought to encourage Irish productions that could propel the scientific reputation of Ireland forward following the socio-political strife the country had recently experienced.[59]

When Tait initially mused about picking up the *Lectures on Quaternions* (1853), it was Andrews who supported his efforts to do so. It was also Andrews who wrote to Hamilton with a letter of introduction for the young laboratory acolyte. Indeed, Andrews created a fertile terrain upon which Tait initiated and maintained his correspondence with Hamilton in the late-1850s. In an early letter to Hamilton, dated November 22nd, 1858, (composed only five days after Tait had received his first response from the Irish mathematician), Tait wrote he was unlikely to show anyone in Ireland or England his initial quaternionic research given that "none of my intimate friends are engaged in the study of Quaternions ... save as regards my answering Dr. Andrews, who is much interested in my progress, and asks me now and then about it" (Wilkins 2005, 15). The fertile terrain provided by

[59] Little academic work has been carried in the case of Thomas Andrews. Tait and A. C. Brown's co-authored *The Scientific Papers of the Late Thomas Andrews* (1889) constitutes a useful starting point, although little historical analysis exists surrounding Andrews's role in the history of Irish science.

Andrews's mathematical encouragements allowed Tait to engage with quaternions within the loosely-bound world of Belfast's natural philosophy and mathematics curriculum.

Tait's initial engagement with quaternions can be understood by appealing to his overlapping "terrains" of knowledge—namely, Cambridge's symbolical algebra and Belfast's experimental, natural philosophy. Recall that mathematical topics such as electricity, heat and magnetism had played no role in Tait's specific education at Cambridge, as they had been excised from the curriculum just prior to his arrival. Furthermore, there had been no experimental education to speak of at Cambridge (the Cavendish laboratory being decades away from established). However, by virtue of Andrews's mentorship in Belfast, Tait was able to learn about experimental science through his informal laboratory apprenticeship from 1854 to 1860. It was an apprenticeship that led to formal publications as joint papers with Andrews on the liquefaction of ozone gas—publications that helped Tait launch and establish his professional career as a natural philosopher. As Tait's biographer later recalled,

> Tait gave efficient aid, more particularly in the calculations involved, and in the construction of much of the apparatus used. He proved such an apt pupil in the art of glass blowing that ere long Andrews gave that part of the manipulation over to his eager and energetic companion. Tait used to speak with intense admiration of the extreme care and patience with which Andrews carried out all his researches. Each difficulty or discrepancy as it arose had to be disposed of before progress could be reported and the investigation advanced a stage. At times indeed, the patient care of the skilled experimenter must have chafed somewhat the brilliant young mathematician, ever eager to get to the heart of things; but no amount of argument or theorising on Tait's part could move the master from the steady tenor of his way. Years after when Andrews in his failing health visited Edinburgh Physical Laboratory to inspect a set of his own apparatus for the liquefaction of gases it was at once a privilege and an inspiration to witness the deep affection and admiration with which Tait regarded his whilom colleague (Knott 1911, 13).[60]

[60] In a reminiscent letter to Andrews's wife following the death of her husband in 1885, Tait make clear his long-standing debt to Andrews. He wrote: "It does not become me to speak of the irreparable loss which you and your family have suffered. But it may bring some consolation to you to be assured that there are many, in many lands, whose sympathies are sincerely with you; --and who lament, with you, the loss of a great man and a good man. For my own part, I feel that I cannot adequately express my obligation to him whether as instructor or example. I have always regarded it as one of the most important determining factors in my own life (private as well as scientific) and one for which I cannot be sufficiently thankful, that my appointment to the Queen's College at the age of 23 brought me for six years into almost daily association with such a friend" (Knott 1911, 13).

In addition to Andrews's laboratory skills, Tait's other Belfast colleagues included a variety of people involved in an array of natural philosophical endeavours. Wyville Thomson was an experimental scientist who would later go on to lead the Challenger Expedition. And James Thomson, the brother of William Thomson, was an engineer who had long-focused his efforts on engine dynamics (Rowlinson 2003). Much in contrast to his encounters with mathematicians and symbolical algebraists at Cambridge, Tait's Belfast experiences included a coterie of professional colleagues who inculcated him into the world of industrially-motivated laboratory and experimental research. The importance of those figures cannot be overstated. Tait credited Andrews with the skills he gained in experimentation, which later qualified him to open up, equip and run a laboratory in Edinburgh in the 1860s. In addition, working with the coterie of friends mentioned above, Tait was exposed to the world of instrumentation. On a trip to Paris in 1855, for instance, he visited the city's Exposition, where he studied the scientific objects and instruments on display. In an enthusiastic letter sent to Andrews on September 21st, 1855, Tait stated he had begun to identify as an "experimentalist" and that he felt that his role in Andrew's laboratory, and in Belfast's world of science, had become a central one.[61] Thus, while in Belfast, Tait was encouraged to lecture on "mixed" mathematics. His "mixed" lectures constituted supplements to Professor John Stevelly's lectures in Natural Philosophy, which Tait ran as voluntary classes for Honours students. He offered advanced accounts of dynamics that allowed him to "escape from the comparative dreariness of Pure mathematics into the satisfying realities of Applied" (Knott 1911, 12).

Yet, it would be a misrepresentation of Tait's navigations to assume that Cambridge had become a distant memory. On the contrary, Tait continued to offer his students additional tutorial sessions in

[61] In greater detail, Tait wrote to Andrews: "I have made attempts to see Ruhmkorff, Soleil, and Tyndall. The former was out of the way, Soleil was in Glasgow, and I believe so was Tyndall. I extracted from the woman in Soleil's shop all the information they could give about the Saccharimeter. I saw the instrument, pr. 260 fr., and bought a description of it and its use by Moigno. I found and examined all the electromagnetic apparatus in the Exposition, and it was my decided opinion that an instrument in Ruhmkorff's stall called "Appareil de Faraday" was the very thing for us ... I hope you agree with me in the matter of the apparatus for Faraday's experiments. The only objection that I could see to it is that possibly it might not be powerful enough; but of that you will be a much better judge. Not far from Ruhmkorff's there is a collection of clockwork and along with it a small machine for exhibiting the permanence of the plane of rotation. I have not seen the gyroscope itself—this machine seemed to me not only comparatively useless, but even dangerous" (Knott 1911, 64).

"pure" mathematics, preparing them for higher-level analysis, even though no exam similar to the Tripos existed in Ireland. He formed small groups outside of regular lecture hours and performed as any good Cambridge coach should, by assigning Tripos-style exercise problems to his students on a regular basis. Rather than experiencing a radical break with the past, Tait's socio-intellectual terrains of knowledge had become layered. He was now working within multiple conceptual environments rather than turning explicitly away from any one in particular. As Tait engaged with quaternions more profoundly, his mathematical practice remained analytical in nature, though his interpretations of symbolical processes began to invoke natural philosophical metaphors and justifications.

Quaternion engagements, 1858-1860

In the early months of 1858, Tait read Helmholtz's article on vortex motion. After obtaining Andrews's support, Tait wrote to Hamilton stating his interest in pursuing the matter further using quaternion algebra. However, he also mentioned he felt uncomfortable with many aspects of Hamilton's algebraic analysis. Over the course of their ensuing correspondence, Tait and Hamilton negotiated over the meaning of quaternions; they also pursued the particular problem of translating the equation of Fresnel's wave surface into quaternionic form; they discussed the potential usefulness of the algebraic system with regards to a variety of other natural scientific concerns (including heat and magnetism); and they deliberated over the possibility of publishing textbooks that Cambridge coaches would be prone to use. Through this correspondence, it became clear that despite their common interests in these matters, Hamilton and Tait's respective uses of quaternions were not identical. For Hamilton, mathematical truth exists prior to experimental and inductive truth. Experimental truth is merely a particular instantiation of *à priori* knowledge and *à priori* knowledge can only be garnered through rational speculation of universal relationships. Though Hamilton *had* adopted certain aspects of the symbolical network's principles, he had done so primarily to advance the legitimacy of his quaternion system at a time when symbolical algebra was increasingly popular at Cambridge.

For Tait, on the other hand, mathematics was becoming a mere hand-maiden to science. Tait had initially demonstrated his adherence to his Cambridge training in symbolical analysis, when he adopted the view that algebraic relationships are "true" in a conventional sense, as opposed to an ontological sense. That view was manifest in his jointly authored text with Steele. By the late-1850s, however, Tait began to adhere to a natural philosophical perspective that emerged prominently from his experimental experiences with Andrews, in which ontological truth could be only be determined via observational and experimental data. Tait had come to see mathematics as a tool—one that could be *used* by science, but which was not prior to it.

In his letters to Hamilton, Tait's rhetorical stance on the meaning of quaternions was coloured by references to his overlapping and shifting terrains of mathematical and natural philosophical knowledge. In his first published paper on quaternions, entitled "Quaternion investigations connected with Fresnel's wave-surface" (May 1859), Hamilton's influence in terms of subject matter, and Cambridge's influence in terms of the symbolical algebraic approach, are both patently clear. By his second paper, "Quaternion investigations connected with electro-dynamics and magnetism" (January 1860), and certainly by his third paper, "Quaternion investigation of the potential of a closed circuit" (October 1860), the intellectual terrain of Belfast's natural philosophy emerges more prominently. Let us therefore take a closer look at Tait's first publication, along with Hamilton's correspondences at the time, to get a better sense of how Cambridge's early-century symbolical philosophy contoured Tait's initial engagements with Hamilton's conceptual artifacts.

Tait's first foray into the world of non-textbook publications opened with subject matter that was dear to Hamilton's heart—the equation for Fresnel's wave surface. This was the same surface Hamilton had discussed in 1832, and which he had used to make his "prediction" of canonical refraction. In a letter to Hamilton (dated November 13th 1858), Tait noted that he had attempted to derive the equation of Fresnel's wave surface using quaternions. Yet, it was not an easy task. He wrote,

> For a week I have been hard at work trying to deduce the equation to Fresnel's wave-surface by a process purely quaternionic, starting from the data employed by Archibald Smith in the Cam. Math. Journ. As yet I have only deduced the directions of the planes of polarization for any wave-front—and the law connecting the velocities of the two rays. These come out with admirable simplicity.
>
> In attempting to find the equation to the surface, I have come upon a terrible array of *Versors*. Of the latter, I have still a sort of horror, arising principally I suppose from my having as yet avoided the use of them on every occasion in which it was possible (Wilkins 2005, 11).

In a manner reflective of his Tripos training, Tait then concluded:

> In writing this I hope you will not imagine *for a moment* that I am asking a hint from you—for *where no new principles are involved* (and this is only a case of *elimination*, as I have got easily enough over the differential part of the question) I cannot see that I should be justified in pursuing such a course—besides, I am fully alive to the advantage of a desperate struggle with unfamiliar processes, and *intend* to succeed (Wilkins 2005, 12).

Tait was treating the problem as a piece of Tripos book work; he was hoping to solve it through continued algebraic manipulation, just as he would have worked to solve difficult Tripos-style questions during his student days (for an account of "bookwork" at Cambridge, see Warwick (2003)).

Hamilton, meanwhile, claimed he had already applied quaternionic analysis to Fresnel's wave surface in prior years. He had never published those results. But in his response to Tait (dated November 19th 1858), he said he would review his materials from the 1830s to see whether he could reacquaint himself with the equation and whether his more recent attempts at a quaternionic account of the wave surface had produced any meaningful mathematical forms. From Hamilton's perspective, Tait's interest in the subject proved to be a useful opportunity to revivify his poorly-received quaternion research and to push forward with new publications. Tait's willingness to focus on "Fresnel's wave surface" flattered Hamilton's expertise.

Recall that Fresnel's wave surface refers to a particular wave front that forms as light radiates from a point within a crystal, where the speed of light varies in accordance with the direction of travel. Hamilton had initially used Cartesian coordinates to describe properties of that wave surface in the 1830s. In his correspondence with Tait, he now saw the possibility of devising a quaternionic representation that would be free of Cartesian coordinates. His stated aim was to highlight the geometrical properties of the surface, which he felt would more clearly emerge when the equation and its derivative were translated into quaternions. In a lengthy letter dated December 3rd, 1858, Hamilton explained to Tait that his understanding of the wave surface was based upon his 1833 paper, in which he had introduced the issue of conical refraction. Hamilton recalls,

> I had, *at the time*, taken care to give a correct statement of Fresnel's views, so far as I had access to the chief Memoirs in which they were contained, [thus] I suppose that the following may be an adequate *translation* of them into the *language* of quaternions (Wilkins 2005, 32).

In translating the equation for Fresnel's wave surface into quaternions, Hamilton stated he was seeking a "physical theory," where he would offer a deduction of the "EQUATION *of his* WAVE SURFACE" by assuming Fresnel's "*principles,* but [by] *my own methods*." In this account, Hamilton defined ρ as a vector of ray-velocity that is "drawn within a doubly refracting crystal from any assumed origin O," and, μ as the "corresponding vector of wave-slowness." In addition, "S" stood for a scalar entity, and "$\delta\rho$" denoted the "Fresnel Vibration," which referred to the vibration "in the crystal, or in its ether, [as] estimated on *Fresnel's hypotheses.*" Using these definitions, Hamilton presented the following quaternionic identities to Tait:

$$S\mu\rho = -1, S\mu\delta\rho = 0, S\rho\delta\mu = 0.$$

He then proceeded through a series of algebraic transformations to arrive at the claim that the equation of Fresnel's wave surface in quaternionic form would be as follows:

$$0 = \frac{(S\alpha\rho)^2}{\rho^2 - \alpha^2} + \frac{(S\beta\rho)^2}{\rho^2 - \beta^2} + \frac{(S\gamma\rho)^2}{\rho^2 - \gamma^2}.$$

Hamilton declared that the "physical significance" of this equation was (at least to himself) immediately visible in ways previously unforeseen. "If I had ever *read* [of those interpretations], or in any other manner *learned* before today," he wrote, "I must confess that I had totally *forgotten*" (Wilkins 2005, 35).

First, the format of the quaternion equation immediately suggested to Hamilton the following identity: $0 = \mathrm{S}\rho\nabla\delta\rho$. This latter equation meant that the ray-velocity multiplied by "Fresnel vibration" ($\nabla\delta\rho$), along with a scalar magnitude, would result in a perpendicular product (where, in the current context, "0" indicates perpendicular lines). In Hamilton's words, "The elastic force, $\nabla\delta\rho$, of the ether, called into play by the displacement, $\delta\rho$, is perpendicular to the direction ρ of ray-propagation in the crystal" (Wilkins 2005, 36). That physical interpretation emerged from the rules governing quaternion mathematics, Hamilton argued. It was one of the reasons he believed quaternion mathematics led to better geometric interpretations of physical phenomena. Interestingly, we might note here, Hamilton was increasingly willing to revert back to his old belief in geometric meaningfulness and physical interpretations, as he discussed the Fresnel wave surface with Tait. The focus on symbolical algebra as worthy in and of itself began to fall by the wayside in their correspondence, belying the Peacockian rhetoric found in Hamilton's 1853 preface.

That is not to deny that Hamilton's *treatment* of the problem was still symbolical in nature. Indeed, at the end of his letter to Tait, Hamilton juxtaposes Fresnel's Cartesian representation of the wave surface with his own quaternionic presentation to argue that, of both algebraic forms, the quaternionic version was superior in presentation. Hamilton writes Fresnel's *"own form of the Equation"* was:

$$(x^2 + y^2 + z^2)(a^2x^2 + b^2y^2 + c^2z^2) + a^2b^2c^2$$
$$= a^2(b^2 + c^2)x^2 + b^2(c^2 + a^2)y^2 + c^2(a^2 + b^2)z^2.$$

The same wave surface written in quaternionic format is:

$$\rho^2\{(S\alpha\rho)^2 + (S\beta\rho)^2 + (S\gamma\rho)^2\} + \alpha^2\beta^2\gamma^2$$
$$= (\beta^2 + \gamma^2)(S\alpha\rho)^2 + (\gamma^2 + \alpha^2)(S\beta\rho)^2 + (\alpha^2 + \beta^2)(S\gamma\rho)^2.$$

Though perhaps not self-evident to an external or uninitiated observer, Hamilton declares the second representation to be "simpler" and better. The Irish mathematician justified his view by arguing,

> A feature, which cannot have surprised *you*, but which might naturally strike a reader unaccustomed to quaternions, in the whole of the foregoing investigations respecting Fresnel's Wave, is the FEWNESS OF THE SYMBOLS employed, and the FULNESS OF MEANING attached to them (Wilkins 2005, 44).

Hamilton argued that if one were to ignore the steps involved in the algebraic manipulations—i.e. the *"signs of operation"*—then "we have used, I think ... scarce any but the following letters",

> ρ, to denote the *vector of ray – velocity*;
>
> u, to denote the *vector of wave – slowness*;
>
> τ, to denote the *vector of Mac Cullagh's vibration*;
>
> v to denote the *vector of Fresnel's elasticity*.

On Hamilton's view, α, β, γ, "denote certain *vector-constants of the medium*." They are not considered to be additional "letters". Hamilton further argues the symbols i, j, k were not to be counted as extra symbols either, as they can be taken for granted (given they constitute foundational *rules* for quaternions), while the variables $a, b, , x, y, z, r$ should not count as additional "letters" either, as since they are only "alluded to" in the algebraic manipulations he had performed. Lastly, the small scalar symbol δm should not be included as an add-on, as it "had entered only to be eliminated." Thus, Hamilton concludes, "I would (I think) be doing injustice to the subject, & to yourself, if you were to suspect yourself of any merely personal or friendly partiality, in regarding (as

I hope that you *do* regard) this *paucity* and *interpretability* of the *signs* as an *advantage* [to quaternions]" (Wilkins 2005, 44).

Hamilton defined simplicity as the "paucity" of symbols evident in his quaternionic manipulations, and he presented this as an objective characteristic of the system itself, after strictly selecting only a small group of symbols to characterize as new "letters" (the others being excluded by virtue of the fact that they were "foundational" or "assumed" in Hamilton's view). This selectivity allowed Hamilton to claim that the quaternionic account of Fresnel's wave surface introduced a new era in simplicity and elegance. The selection of items mentioned in the list above, and their characterization as "scarce", was a value judgment on Hamilton's part—one that was informed by his pre-existing adherence to the superiority of quaternion mathematics, and one that was motivated by his desire to legitimate the system in Tait's eyes.

Tait adopted Hamilton's language and began to laud the perceived simplicity of the system (i.e. the "paucity of symbols") in his own quaternionic work on Fresnel's wave surface (Wilkins 2005). In early 1859, he noted his amazement at the potential for simplification, which he felt quaternionic transformations brought to mathematical research. In a letter dated January 3rd, 1859, Tait wrote,

> About quaternions in general, I may remark (as indeed I very frequently feel) that the processes are sometimes *perplexingly easy*—by which I mean that one is often led in a step or two and without at once knowing it to the solution of what would be by ordinary methods a work not so much of difficulty as of labour. This however I take it must form one of its great excellencies in the hands of a person very well acquainted with it. A drawback to a beginner, but (as I am gradually being led to perceive) an immense advantage to one well-skilled in the analysis, is the enormous variety of transformations of which even the simplest formulae are susceptible; a variety fully justifying a remark of yours which not many months ago used somewhat to puzzle me. If I had gained nothing more by reading this subject than the facility of making problems and transformations for Examination papers (especially in Trigonometry) and so saving an immense amount of time and trouble, I should have considered myself amply rewarded; but I hope in time to be able to apply it to perfectly original work (if anything *can* be quite original in these days). I make these remarks because you expressed yourself willing to hear anything I had to say on the subject, and because at present they are indissolubly connected with all my ideas on quaternions (Wilkins 2005, 123-124).

Yet, despite Tait's enthusiasm, quaternions still posed various problems, some of which contradicted his own claim that the system was simpler than Cartesian-based algebra. In a letter written to Hamilton on March 2nd, 1859, Tait recounted the various difficulties he faced in dealing with certain "wave-investigations," especially as they related to "the problem of the wave front for which there is the greatest angular separation of the rays." That problem had led Tait to some "complicated and almost intractable [quaternion] equations" (Wilkins 2005, 161).[62] Despite the characterization of "simplicity", therefore, Tait soon discovered quaternions required intensive mathematical labour and inventiveness to be rendered "simple."

Revealingly, in his first published paper on Fresnel's wave-surface, Tait admits that quaternions do not actually provide any benefit over and above the classical Cartesian approaches, or any benefit in cases where *direction* is found to be unimportant. He concludes, therefore, that although "the Calculus of Quaternions in general … appears to me to possess in a marvellous degree the attributes of simplicity and suggestiveness," still "the treatment of the wave-surface is perhaps not a question in which its superiority over Cartesian methods is at once so marked, as it is in all cases where no direction in space is regarded as preeminent" (Tait 1898, 1-2). Yet, rather than relent and turn back on his overall claims, Tait continued to seek out other examples where quaternionic expressions could, in fact, be deemed "simpler" and more productive than their Cartesian equivalents. For example, Tait argued a quaternionic approach to the wave-surface would be perhaps useful in demonstrating that "the *three* directions of the axes of elasticity may be at once reduced to *two* reference lines (the wave, or the ray, axes), and a still farther reduction obtained by the introduction of a certain linear and vector operation [was possible]" (Tait 1898, 2). This claim highlights Tait's intention to move forward with his quaternion investigations, indicating his continued belief in the view that quaternions simplified mathematical problems, despite the difficult examples he faced above.

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[62] Nonetheless, he was "unwilling to try the last resource of putting the [problematical equations] in x, y, z coordinates until [he could] show the impossibility of solving it (if it *be* impossible) quaternionically" (Wilkins 2005, 161).

To bolster his own position, Tait offers a basic example of—what he takes to be—an undeniable use of quaternion mathematics. He considers the representation of ellipsoids. When put into their quaternionic form, ellipsoids gained in simplicity, he contends; furthermore, the quaternionic account of ellipsoids leads to more productive results. If the vector semi-axes of an ellipsoid are identified as ai, bj, ck (where i, j, k are the quaternion rectangular vector units), the equation of the ellipsoid can be represented in quaternionic format as follows (where S indicates a scalar value):

$$\frac{(Si\rho)^2}{a^2} + \frac{(Sj\rho)^2}{b^2} + \frac{(Sk\rho)^2}{c^2} = 1.$$

Tait claims this form is equivalent to the better known Cartesian form:

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} = 1.$$

In his demonstration of the productiveness of the quaternion approach, Tait invokes Hamilton's previous presentation of an ellipsoid, which had relied upon the quaternion variables T, κ and ι. Hamilton's representation of an ellipsoid had been [63]:

$$\mathrm{T} = (\iota\rho + \rho\kappa) = \kappa^2 - \iota^2,$$

where:

$$\iota = \frac{ac}{2}\sqrt{\frac{(a+c)}{(a-c)}}\left\{\frac{\sqrt{(a^2-b^2)}}{ab}i + \frac{\sqrt{(b^2-c^2)}}{bc}k\right\},$$

and

$$k = -\frac{ac}{2}\sqrt{\frac{a-c}{a+c}}\left\{\frac{\sqrt{(a^2-b^2)}}{ab}i - \frac{\sqrt{(b^2-c^2)}}{bc}k\right\}.$$

[63] Note that ι and κ are "tensors."

If one supposes that $a > b > c$, then the symbols ι and κ represent "vectors perpendicular to the planes of the circular sections of (α)" (Tait 1898, 2). Using a bit of algebraic manipulation, Tait demonstrates that Hamilton's account and his own are, in fact, equivalent, such that:

$$\frac{(Si\rho)^2}{a^2}+\frac{(Sj\rho)^2}{b^2}+\frac{(Sk\rho)^2}{c^2}=\frac{\mathrm{T}^2(\iota\rho+\rho\kappa)}{(\kappa^2-\iota^2)^2}.$$

Tait also shows Hamilton's description of an ellipsoid leads to a slew of other useful algebraic identities, including the following:

$$\mathrm{T}^2\iota+\mathrm{T}^2\kappa=\frac{1}{2}(a^2+c^2);$$

$$\kappa^2-\iota^2=ac;$$

$$2\mathrm{T}\iota\mathrm{T}\kappa=\frac{1}{2}(a^2-c^2);$$

$$\mathrm{T}(\iota-\kappa)=\frac{ac}{b};$$

$$\mathrm{T}\,.\,V\iota\kappa=\frac{ac}{2b^2}\sqrt{(a^2-b^2)}\sqrt{(b^2-c^2)}.$$

In using this ellipsoid example to highlight all of the algebraic identities that emerge from quaternion analyses of old problems, Tait claims that quaternion mathematics gives rise to symbolical identities not evident in inert Cartesian presentations of similar problems.

Tait's training in Cambridge's symbolical analysis is evident. Apart from the individual symbols ι and κ—which are specifically defined—*interpretations* of the algebraic identities listed above are meant to follow after the algebraic manipulation has taken place. Not only is Tait unconcerned with the necessity of predefining the various components of these mathematical manipulations, but he also writes as though the ontological reality of those symbol is a distant after-thought. Thus, despite the initial problems Tait had faced in invoking quaternions in an analysis of Fresnel's wave surface, the ellipsoid example proved that quaternions could generate new and exciting universal algebraic relationships, many of which, Tait speculated, might contain room for further physical interpretation in the future (though he nowhere provides such interpretations).

The epistemic choices Tait made throughout his first quaternion publication can be summarized as follows. He presented algebraic analysis as leading the investigation. He followed his Cambridge training in symbolical analysis by manipulating symbols and generating equivalences without feeling constrained by any the obligation to discuss the particular meanings of those manipulations. In Tait's mind, quaternionic analysis led to the construction of sleek, new algebraic equivalences, which might support geometrical or physical interpretations, but which did not necessitate such interpretations in order to be deemed valid.

By the end of 1858, Tait was a clear supporter of the quaternionic approach, or, rather, of its perceived benefits to symbolical algebraical research. He saw it as fruitful, productive and, at times, elegantly simple. Yet, he still harboured some reservations. In a paper published soon after his first account of Fresnel's wave surface, Tait reverted to discussing some of those lingering difficulties he had encountered in his earlier efforts to translate known equations into quaternionic form. Entitled "Note on the Cartesian Equation of the Wave-Surface" (1859), Tait's paper offers the following equation for the wave-surface:

$$\frac{a^2x^2}{a^2-r^2}+\&c.=0,$$

where,

$$r^2=x^2+y^2+z^2,$$

could be rewritten as,

$$\frac{x^2}{b^2c^2-{r_1}^2}+\&c.=0,$$

and where the following identity consequently holds:

$$r_1^2 = a^2x^2 + b^2y^2 + c^2z^2.$$

Although he "was led to [this transformation] by a quaternion process," Tait explains the quaternion approach itself is not "so simple as the obvious algebraic verification" (Tait 1898, 20). Tait's claim in this brief publication is historically useful. He acknowledges that quaternions do not always constitute a simpler method of mathematical analysis. His choice to continue pursuing the technique in light of these serious difficulties must, therefore, be explained.

Indeed, there were a number of reasons for why Tait's reservations would have been mitigated, leading him to continue working on quaternions despite the presence of so many obstacles. The possibility of generating original publications and the (hoped for) professional recognition that would accrue as a result, motivated Tait in important ways. In a letter to Hamilton dated November 22nd, 1858, Tait noted:

> There is one point however on which I would like your opinion. It is this—If I should ever publish anything involving the *"equation of envelopment"* XI, must I cite it as *yours*, or is the use I made of it in a particular case [in a previous letter] when I had the equation but deduced it by the consideration that a certain product of two vectors ($\alpha'\mathrm{V}\alpha\bar{\omega}$) was to be a *scalar* or at least to have its vector part always zero for an infinite number of directions of α', $\mathrm{V}\alpha\bar{\omega}$ remaining constant—which was absurd unless $\mathrm{V}\alpha\bar{\omega} = 0$—is that use, antecedent to receipt of your VII & XI [letters from Hamilton], sufficient to put it on a level with other results deduced from your book without your assistance (Wilkins 2005, 15)?

To see Tait in the light of his recent Cambridge education is to see him in the light of a professionalizing culture of mathematics—a culture in which it had become normal, and expected, for former Tripos wranglers to publish soon after graduation, thereby establishing a name for themselves. One of Tait's aims, therefore, was to gain Hamilton's support (so as to avoid claims of plagiarism or priority disputes) and to publish certain aspects of his quaternionic analysis to build his professional status.

The pressure to publish soon after graduation motivated the behavior of more than a few top wrangles in the mid-century. Consider, for instance, the case of George Sesslar (Senior Wrangler 1858). In 1859, one year after his graduation, Sesslar published an article in the *Quarterly Journal of Pure and Applied Mathematics* in which he offered a "solution of certain problems in Rigid Dynamics by a new artifice, referring the data to movable axes." As Warwick (2003) has pointed out, that "new artifice" was a time-saving tool taught to Sesslar by the Cambridge coach Edward Routh (Senior Wrangler 1854). After Sesslar published his article, Routh complained bitterly. He declared the technique had been known years earlier, openly suggesting that Sesslar had unapologetically stolen Routh's work (Warwick 2003, 160). Sesslar's choice to publish his solution to a Tripos problem, which used a technique that had been shown to him in the privacy of a coach's classroom, helps to highlight the competitive nature of mathematical professionalization at the time. The pressure to publish, and the need to establish a name as a mathematical authority, had become an overriding objective in the lives of young Cambridge graduates. In part, this was due to the growing cohort of graduating men who were competing more fiercely for scarce academic posts scattered across the country's universities and technical institutes. As soon as Tait had graduated, he entered that competition. Tait and his fellow graduates were willing to publish on topics that were still in a state of development, so as to gain the potential opportunity of having a relevant question set on a future Tripos exam and thus having the topic gain greater and a more formal recognition.

The publishing motivation underpinning Tait's engagement with quaternions throughout 1859 led him to labour on through the difficulties he had faced and to ignore his own reservations over matters of "simplicity" in quaternion mathematics. Indeed, only two months into their correspondence, Tait wrote to Hamilton saying that he would submit a short paper to the *Philosophical Magazine* or the *Quarterly Journal* on the "Advantages of Quaternions over the ordinary Cartesian Methods." He admitted it was not for the, sake of giving anything very new, but merely to record my vote in favour of the method, and if possible induce others to take it up—for I am now as fully convinced of its great *practical* value, as I was from my first slight acquaintance with it delighted with its novelty & elegance (Wilkins 2005, 17).

And in a letter to Andrews, written July 21st, 1859, Tait noted some early success as a result of his efforts:

My paper on the Wave-surface has reached me ... —and I have been asked by several men of note, to whom I have sent copies, to publish an elementary work on Quaternions. Todhunter of Cambridge, about the best authority on matters of that sort, is one of them—and I have written to Macmillan (the publisher) to enquire about terms etc...

Sir W. Hamilton has expressed his satisfaction with the project—and has only asked me to refrain from laying, or trying to lay, new metaphysical or other foundations for the Theory, wishing to reserve such for himself; and I am quite sure that I shall not feel this in any way a restraint (Knott 1911, 65).

Visions of fame

The tone of the Tait-Hamilton correspondence at this point reveals broad themes. Apart from the evident focus on developing a quaternionic approach to Fresnel's wave theory, the correspondence abounds in accounts of Hamilton's sense of excitement and trepidation at the thought of quaternions being revivified. In an exuberant letter issued early in the correspondence, Hamilton writes to Tait,

You are, by this time, *quaternionist* enough to admit, as I trust, that whatever may be the *future* success (in such hands as your own, for example) of Quaternions as an *Instrument of Investigation*, they furnish *already,* to those who have learned to *read* them ... a powerful ORGAN OF EXPRESSION, especially in *geometrical science,* & in all that widening field of *physical inquiry*, to which *relations of space* (not always easy to *express with clearness* by the Cartesian Method) are subsidiary (Wilkins 2005, 21).

In a later letter, written in 1859, Hamilton further acknowledges,

I cheerfully confess that I consider myself to have, in several respects, derived advantages, as well as pleasure, from the Correspondence. It was useful to me, for example, to have had my attention *recalled*, to the whole *subject* of the Quaternions; which I had been almost trying to *forget*; partly under the impression that nobody cared,

> or would soon care, about them. The result seems likely to be, that I shall *go on* to write some such "Manual",--not necessarily a very *short* one... (Wilkins 2005, 139).

From 1858 to the end of 1859, Hamilton envisioned the possibility of his name emerging, once again, as a prominent voice in British mathematics and science. He envisioned a new symbolical-geometrical approach to mathematics in which analytical Cartesianism would give way to analytical Hamiltonianism. And this latter approach would more profoundly and more directly indicate the *à priori* nature of space. One of the main benefits of quaternions, Hamilton contended, was that they did away with pre-existing assumptions about the geometrical nature of space. Rather than absolute measures of location based upon arbitrary axes, quaternions offered relativistic relations between positions in space. In a letter to Tait, written in 1858, Hamilton argued, "... the CALCULATION OF QUATERNIONS admits of being brought to a much greater degree of *simplicity*; & to a state which shall *assume much less,* of *previous geometrical knowledge*" (Wilkins 2005, 61). All of these claims were still speculative on Hamilton's part, but his exuberant hopes for the potential uptake of his ideas serves as a notable motif throughout his letters to Tait.

Another theme to emerge in their correspondence is Hamilton's uncertainty, even suspicion, with regard to the speed and diligence with which Tait had adopted and manipulated quaternion algebra. In a letter dated December 6th, 1858, he writes,

> I have *read,*--that is to say, in the open air, & without pen or pencil at hand,--the *first sheets* of your "Quaternion Proofs": and must say that they appear to me to be *wonderfully elegant*; and to exhibit a very remarkable degree of *mastery* (so far) over the *calculus* of quaternions, used as an *instrument*, or *expression*, and of *investigation*.—It would interest me much to know, whether (previously to our present correspondence) you had received ANY assistance from *any other students* of that Calculus. Or did you learn *all* that you had acquired, from the BOOK itself, combined (no doubt) with *your own* private exercises, of various sorts?—If the "Lectures on Quaternions" have been your ONLY Teacher, I must consider the result of such a state of things to be not merely creditable to *your* OWN talents and diligence, but also complimentary to, and evidence of, some (scarcely hoped for) *didactic capabilities* of my Volume; which ought to tend to *console* me, under my *artistic consciousness* (as an author) of the many *faults* of *execution*, that if I could afford the expense of bringing out a *New Edition,* I should be more likely to make it a NEW WORK (Wilkins 2005, 92-93).

In his ongoing efforts to legitimate quaternions, Hamilton wished to suggest that a small community of "quaternionists"—"other students," that is—did indeed exist and had existed for some time.

To solidify his role as progenitor of the quaternion system, the Irish mathematician further queried Tait so to determine whether he had been influenced by that small community, which Hamilton considered himself to be the leader of. Hamilton tells Tait,

> In Dublin, indeed, there exists a little "School" of Quaternionists, developed partly by the Lectures and Examinations, which Charles Graves and myself have given or held, and the Professor's brother, my old friend, John T. Graves, (repeatedly mentioned in my Preface,) called my attention, about a year ago, to a highly favourable, and very eloquent article, in the North American Review, for July, 1857, on the subject of the Quaternions, & of my Book. But a conscientious Author wishes rather to be *read,* than to be *praised*: and therefore I should like to be informed, *what* drew your attention to my Book, & *whether* you had any personal assistance in studying it (Wilkins 2005, 92-93)?

Tait responds the following day, acknowledging Hamilton's influence on his nascent work. Yet, he makes sure to identify those independent terrains of knowledge through which he felt himself to be navigating alone or, at least, independently of Hamilton. Tait writes,

> With regards to my study of Quaternions, I may affirm with some certainty, that when I ordered your book, on account of an advertisement in the Athenaeum, I had NO IDEA what it was about. The startling title caught my eye (in August /53) and as I was just going off to shooting-quarters (in my native district on the Tweed) I took it and some scribbling paper with me to beguile the time if a day were unusually wet or stormy. Circumstances however did not favour me, for the weather was good, and game plentiful, and I was generally too fatigued in the evenings to make much of Mathematics. However, as I told you in my first letter, I got easily enough through the first 6 lectures,--and I have still a good many notes I made at that time, from which it now seems to me that I had not then fully appreciated the simplicity of the method ... On my return to Cambridge I set to read other things, and to write my recently published Treatise on Particle Dynamics. The Theories of Heat, Electricity & Light have since occupied much of my spare time, and it was only in August last that I suddenly bethought me of certain formulae I had admired years ago at p. 610 of your Lectures—and which I thought (and still think) likely to serve my purpose exactly. [[The matter which more immediately suggested this to me was a paper of Helmholtz' in Crelle's Journal (Vol. IV) which I was reading in July last as soon as we received it, and which put the subject of Potentials before me in a very clear light. The title (in German) I forget—but an M.S. translation of my own which I have now beside me is headed *"Vortex-motion."* It refers to the integration of the general equations in Hydrodynamics when $udx + vdy + wdz$ is *not* a perfect differential.]] On this I asked Dr. Andrews with whom I was then at work if he was sufficiently acquainted with you to give me a letter of introduction.

Tait then adds that he is,

> not even acquainted with anyone who knows aught of Quaternions (except Boole of Cork—with whom however I have not exchanged a remark on the subject, and who I suspect looks on them in their *analytical* capacity *only*). So you see that if there is any credit in my progress it is entirely to your Lectures & Letters that it is due (Wilkins 2005, 97-98).

Tait was making it clear that the small group of supposed "quaternionists" that Hamilton had identified with could lay no claim to Tait's own innovative research—by lack of association, neither could Hamilton.

Not to be cut loose, however, Hamilton reminds the young mathematician, in a circumlocutory way, that the latter's recent developments of quaternion mathematics were ultimately dependent upon the former's original ideas. In a letter initiated on February 15th 1859, Hamilton recounts:

> Your N° 30 [a previous letter of Tait's] is not at my hand, & I must confess that I only *glanced* at it, being pressed about other things when it arrived, & hoping to *read* it carefully, after some time. The LOOK of the ijk convinced me, perhaps too easily, that you had not *yet* hit upon the *method* which I proposed some years ago,--though it is not so fresh, clear, & vivid in my present recollection, as to dispose me to submit, without some brushing up, to a competitive examination on the subject,--and which I *think* that I communicated in 1857 to Mr Salmon. Ungrateful brutes that the quaternions must be, they wish, whenever they can *safely* do it, to *cut* the acquaintance of their old friends and benefactors, the aforesaid i, j, k! ... You must know that I have lived (very calmly) through a perfect storm of ridicule, in my own city. Dr Wall,--the Vice Provost,--a man whom I really *love*, & venerate, & who made no objection, when it came to the point, to the allocation of some *money* from the College Chest, to assist me in bringing *out* my book,--was one of those who set the example. The product of ij not equal to ji!!! monstrous. Even Charles Graves was, for a moment, almost seduced from his allegiance—or at all events (being a very *prudent* man) he began at one time to doubt the POLICY of going *on* with quaternions. With one of his gravest faces, he said to me one day—perhaps 8 or 9 years ago,--"You cannot conceive, Hamilton, what a *prejudice* exists against the Quaternions:--and *that*, among people who have *not examined* them." Should you *prefer*, I replied, that it should exist in the minds of persons who *had* examined them? ... But without seeking farther "to unsphere The Spirit of Plato",--let me be permitted to *congratulate you*, (as well as *myself*,--most sincerely do I add this last objective case,) on your having *taken up* the quaternions. *They* will owe MUCH to *you*: but I think that you also will owe *something* to *them*. This may be only the natural vanity of an author; but I believe that an early *appreciation* of genius wins a corresponding appreciation, in its due time, from mankind, for itself: even if not accompanied, as in your case it is, & will be, by independent acts of *discovery* (Wilkins 2005, 131-132).

Despite its loquacity, Hamilton's message is clear. Tait was bound for fame based upon the likelihood of his "future" discoveries; but, as Hamilton emphasizes, none had yet been uncovered. Furthermore, Hamilton viewed Tait's developments as *extensions* of his own foundational work. Thus, he warns, it would be subject to the same level of criticism Hamilton himself had faced in previous years. Perhaps realizing Tait's independence on the matter, however, Hamilton also supported the idea that, together, the two mathematicians could override any remnants of the traditional opposition to the non-commuting algebraic system. In this partnership, however, Tait was expected to play the role of sturdy sidekick.

Indeed, Hamilton's general lack of support for Tait in both public and professional domains suggests that Hamilton's intent was to treat the young researcher as a backroom aid, rather than as an equal, professional colleague. In delivering a "communication" to the Royal Irish Academy (April 12th 1859), Hamilton noted he had specifically failed to mention Tait's name. He also wrote to Tait to explain how he had secured room for publishing a small article ("which may stretch into two or three") in the *Proceedings* of the Royal Irish Academy, by offering a short verbal communication on Fresnel's wave surface and Hamilton's own "Symbolical Forms for its Equation." At no point, however, did Hamilton mention Tait's name as second author to that proposed publication, or even as a supporting actor in the general investigation of the topic. Hamilton writes to Tait that, although "you may be surprised that I did not mention your name, I could account for it in many ways." Hamilton claims he had not mentioned Tait's name because he had been uncertain as to whether he, himself, would deliver the abstract to the society's meeting. As he ended up having the abstract delivered on his behalf by Graves, he felt that mention of Tait's name was inappropriate, as it would have caused confusion. Hamilton further claims there would be time enough in the future to mention Tait's name, adding that his own hopes for success with the renewed quaternions lay in Tait's "discretion". Hamilton expected Tait to keep their letters confidential, at least until the publication of his second quaternion textbook. The historian is therefore presented with a slew of unconvincing apologia that paints Hamilton as a skeptical and suspicious mathematician—one who hoped to harbour more than a little of the praise that would accrue from a revivified account of his algebraic system.

In order to advance these self-interested goals, Hamilton began to issue a number of stipulations, dictating the terms according to which future publications on quaternions could be issued. Thus, the third theme to emerge in the correspondence between Hamilton and Tait was Hamilton's insistence upon determining the nature and style of the publications that would inevitably emerge from Tait's research. Hamilton was keen to demarcate his professional bounds of authority. He underlined the need for Tait to refrain from engaging in metaphysical discourse regarding the "principles" of quaternions, and insisted, rather, that Tait leave those philosophically important discussions to Hamilton himself. Over the spring of 1859, Hamilton also wrote to Tait to express the fact that his hopes, fears, and ambitions were totally dependent upon Tait's willingness to play by the rules. In conciliatory tones, Hamilton wrote to Tait on July 10th, 1859, to remind him that he did not want to invoke a sense of competition or rivalry with his young follower, but,

> If I shall go on to speak of my views, wishes, or feelings, on the subject of future publication, I request you beforehand, to give any such expression of mine your most indulgent construction; & not to attribute to me any jealousy of you, or any desire to interfere, in any way, with your freedom, as author and critic.

Despite his gracious attitude, Hamilton underlined the historical status of quaternions, noting that mathematicians such as Möbius had already recognized the worth of Hamilton's system long before Tait's arrival. As a newcomer, Tait was also a latecomer. The quaternion path, Hamilton contended, was a well-trodden one, and Tait's injection of new "physical" problems in quaternionic analysis could not compare with the developments that had already taken place. Hamilton wrote:

> If we were altogether strangers, I could have no right to address you, on such a subject at all. A German Pamphlet, or Short Essay, has been recently sent to me by Möbius,--the "recognition" to which I lately alluded,--though the Author has *recognized* me before, especially in connexion with hodographs,--and of course I did not expect him to have asked *my leave*, previously, to discuss in public, my Associative Principle of Multiplication of Quaternions. The perfect independence of our researches has a charm & value of its own. But between you & me, their case is perhaps not exactly similar; as

> we have so freely corresponded, & as you are an author in the same language, & of the same country:--England, Scotland, & Ireland, being here held to have their sons compatriots.
>
> To Möbius's excellent Pamphlet, it is likely that I may return. Meanwhile I trust that it cannot be offensive to you if I confess ... that in any such future publication on the Quaternions as you do me the honour to meditate, I should *prefer* the *establishment* of "PRINCIPLES" being left, at least for some time long,--say even 2 or 3 years,--in my own hands. Open to improvement as my treatment of them confessedly is, I wish that improvement, as least to some extent, to be made & published by myself.

Although Hamilton saw worth in encouraging his acolyte to develop quaternions, he also saw potential for the diminution of his own reputation if he failed to control the specific outputs of the young mathematician.

Hamilton saw his own role as that of authorizer of all that was to be foundational to the subject; he saw Tait's role as that of provider of examples and practice problems.[64] As Hamilton put it:

> Briefly, I should like (I own it) that no book, so much more attractive to the mathematical public than any work of mine, as a book of yours is likely to be, should have the appearance of laying a "FOUNDATION": although the richer the "SUPERSTRUCTURE," on a previously laid foundation, may be, the better shall I be pleased. I think therefore that you may be content to *deduce* the Associative Law, from the rules of $i\ j\ k$; leaving it to me to consider, and to *discuss*, whether it might not have been a fatal *objection* to those *rules*, if they had been found to be *inconsistent* with that PRINCIPLE.
>
> Again, on some points which are harder to swallow, at first starting,--the associative principle seeming rather to be a truism than a paradox,--such as the assertion that the *product* of two vectors is generally a *quaternion*, & that the *square* of any one vector may always be equated to a *negative scalar*,--I should think it might be fairer to me, (you

[64] In his maneuvering, Hamilton often emphasized the use of developing "physical" applications for quaternions, while at the same time underlining the overarching importance of his own metaphysical starting principles. He wrote to Tait: "It will interest me to endeavour to understand, after a while, at least the *nature and tendency* of the researches of Dr. Andrews & yourself, on the subject of Ozone. Little as I have pursued such studies, even in books, you may judge from my Presidential Addresses, pronounced on the occasions of delivering Medals (long ago), from the Chair of the R.I.A ... that *physical* (as distinguished from mathematical) investigations have not been *wholly* alien to my somewhat wide, but doubtless very superficial, course of *reading.*—You might without offence to me, consider that I abused the license of *hope*, which may be indulged to an inventor, if I were to confess that I expect Quaternions to supply, hereafter, not merely *mathematical methods*, but also *physical suggestions*. And, in particular, you are quite welcome to smile, if I say that does not seem extravagant to *me* to suppose, that a *full* possession of those *à priori principles* of mind, about the *multiplication of vectors;*--including the Law of the Four Scales, & the Conception of the Extra-spatial Unit,--which have as yet been not much more than *hinted* to the public,--MIGHT have led, (I do not at all mean that *in my hands* they ever *would* have done so,) to an ANTICIPATION of *something like* the grand discovery of OERSTED: who, by the way, was a very à priori (and *poetical*) sort of man, himself, as I know, from having conversed with him, & received from him some printed pamphlets, several years ago. It is impossible to estimate the *chances* given, or opened up, by a new *way of looking* at things; especially when that way admits of being intimately combined, as *you* well know, with *calculation* of a most rigorous kind" (Wilkins 2005, 209).

> will pardon my using the first word that suggests itself,) & at least as likely to promote the sale of your book, if you were to leave all *à priori* & metaphysical *grounds* for *such* positions to be maintained by me, than if you should profess or propose to deduce them otherwise, than from the fundamental FORMULAE,
>
> $$i^2 = j^2 = k^2 = ijk = -1.$$

Hamilton concluded by delineating what he hoped to discuss in his own upcoming publication, indicating thereby what it was that Tait was *not* to discuss. He wrote:

> But my peculiar turn of mind makes me dissatisfied without seeking to go deeper into the *philosophy* of the whole subject, although I am conscious that it will be imprudent to attempt to gain any lengthened hearing for my reflections. In fact I hope to get much *more rapidly* on to *rules & operations*, in the MANUAL, than in the LECTURES; although I cannot consent to neglect the occasion of developing more fully my *conception* of the MULTIPLICATION OF VECTORS, and of seeking to establish such multiplication as a much *less arbitrary* process, than it may seem to most readers of my former book to be (Wilkins 2005, 228-230).

Following the end of the summer vacation of 1859, Tait responded to Hamilton's various letters on the matter of publication style and content. He tentatively sought clarification on those "rules" of publication that had caught him off-guard. In a letter dated September 7th, 1859, Tait wondered:

> I am to assume (I suppose) the laws of i, j, k—and the *distributive* property (with explanations of course)—and thence deduce the *associative*—but I suppose I am at liberty to give a condensed outline of the method employed in your Lectures (Wilkins 2005, 233).

Hamilton responded, in minute detail, outlining the boundaries that Tait ought not to trespass in their future publication endeavours, jointly or otherwise. He stated,

> You were once pleased to ask my opinion, as to whereabouts you should *begin,* in your own treatment of the subject. It would be a delicate thing for *me* to offer any advice upon, especially as it known to many of my friends that I have *long* been intending to publish a "Manual of Quaternions", myself; and in fact wrote, & showed, a part of one, as a specimen, at least 3 years ago. But, speaking generally, I might venture to *suggest*, as perhaps convenient for *all* parties, that you should, *for the present*, take for *granted* a great deal of *first principles;* for instance, the fundamental *laws* of $i\ j\ k$: and *proceed at once to examples,* or *applications*, such as you will doubtless be found *rich* in. I mean only, during such time as you can spare, just *now,* or *soon,* to the task of *composition,* as connected with the quaternions: doubtless you will find it useful, or perhaps even *necessary*, to *prefix* to your work some short account of the nature & rules of the *Calculus itself.*

> What would seem to me the BEST arrangement, *if practicable* , would be this:--that my Manual should appear *before* your Treatise, and should (prospectively) *refer to it*, as expected soon to be published, & to furnish more numerous & more interesting EXAMPLES, than my own book will contain; while *you* might (if you thought fit) refer *reciprocally*, your readers to that book of mine, (the *Manual,* rather than the *Lectures,*) as containing a fuller *Exposition* of PRINCIPLES, & on the *grounds* for their admission, that it would suit *your plan* to put forward (Wilkins 2005, 238-239).

With one foot still firmly planted in his generational need to justify the symbolical approach itself, and one foot embedded in the hopeful world of Tripos publications and dynamics (from which examples for quaternion mathematics would emerge), Hamilton saw himself as the expositor of the metaphysical and foundational aspects of the algebraic system. His hope was to clear a path for himself as the continued leader in quaternion research.

Many of Hamilton's concerns over metaphysics and mathematical foundations washed over Tait with little resistance. In part, this was due to the fact that, by the late 1850s, Tait's focus was still on developing textbooks useful for Tripos training, rather than on any philosophical exegeses. His diligence was largely motivated by his professional navigations. Recall that the training of wranglers at Cambridge had produced a veritable industry of textbook production and consumption. Tait was well aware of that industry. His first publication with Steele was testament to that fact. As Tait reiterated, he was approaching the quaternionic accounts of Fresnel's wave surface from the perspective of a student writing "book work" and preparing for the Tripos. On March 18th, 1859, Tait wrote to Hamilton to tell him he was working on translating some "illustrative problems" into quaternions—problems he had met with in Cambridge papers in past years.[65] In a short note written to Hamilton in April 1859, Tait admitted, "When I first read Art. 563 of your Lectures I thought it about as hard a piece of bookwork as I had ever met. It is less formidable now, but still not *easy*" (Wilkins 2005, 187). Thus, Tait's aim was to produce a Tripos-style textbook that would be bought

[65] One such problem was to "Find the locus of the centre of a sphere which touches two given lines in space." Tait modified the problem to read: "Find the locus of the centre of a surface of the 2nd order whose axes are given in ratio and direction, and which touches two given lines." To attack it Tait defined the vectors β and γ as parallel to the given lines and 2α as the common perpendicular. He then approached the problem using vectors (Wilkins 2005, 163-164).

and sold at Cambridge, and which would help to establish his professional status in the competitive world of examination.

By the mid-1860s, those motivations would change, however, largely due to the fact that Tait had begun to navigate through a "terrain of knowledge" characterized by the metaphors and interests associated with northern energy science (for further details related to Tait's movements away from Hamilton, see Appendix Two).

Tait's move into natural philosophy and religiosity

In the preface to his *Scientific Papers* (1898), in which Tait republished his early quaternionic publications, the elderly natural philosopher recalled that his early quaternion publications had been mostly composed prior to any significant correspondence with Hamilton. Tait tells his reader,

> Among the more detailed papers are the earlier of those in which quaternions are employed. These were written while I was endeavouring to familiarise myself with the new calculus, and were, in great part, worked out before I had any communication with Sir W. R. Hamilton except through his *Lectures* (1853); a fascinating book, which, by great good fortune, I had taken with me on a vacation tour as a companion for wet days. When I made Hamilton's acquaintance a year or two later, through Dr Andrews, I submitted to him some of the more formidable difficulties which I had met in the study of his great work, and the hints I thus obtained were of much use to me in finally preparing these papers for publication (Tait 1898, v).

Tait's historical rendering of his engagement with quaternions is, however, questionable. In his correspondence with Hamilton from 1858 to 1860, much more than just a "few hints" were passed from Hamilton to Tait. Indeed, the two mathematicians relied heavily upon one another to develop and legitimate their developing ideas, in particular with regards to the quaternion rendering of Fresnel's wave surface. Tait's 1898 claim that he had worked solo on his early quaternion publications should be read as part of his legitimization efforts—efforts that became prominent in the 1890s, by which time Tait was engaged in a distinct and independent debate with Gibbs and Heaviside over their respective developments of vector analysis. The historical account offered in the *Scientific Papers* is meant to recollect the past so as to situate Tait at the forefront of groundbreaking quaternion research vis-à-vis Gibbs and Heaviside in the mid-century.

While acknowledging that his early quaternion efforts did not present any "new" material, Tait did suggest he had offered presentations in a "new" language. Recall that Tait's quaternion publications from 1858 to 1860 had highlighted his belief that symbolical analysis is productive and worthy in its own right. In line with his late-Peacockian and De Morgan-influenced studies at Cambridge, he also held that it could produce mathematical identities that might have meaningful applications in natural philosophical contexts. Thus, although the contents of those early publications "were a translation of other men's investigations," they were also translations "into a vastly superior (though at the time they were written all-but-unknown) language" (Tait 1989, vi). In his following two papers, both of which were published after six years in Belfast, it is evident that new natural philosophical (or "physical") concerns were making themselves apparent in Tait's approach to mathematics, leading him to increasingly define mathematics as a hand-maiden to science. Let us turn then to an account of those two papers to get a sense of the shifting terrains through which Tait was actually navigating at the time of their composition.

While Tait's first paper on quaternions had dealt with Fresnel's wave surface, his second paper, "Quaternion investigations connected with electro-dynamics and magnetism," linked two emergent themes in Tait's natural philosophic life in Belfast.[66] Published in the *Quarterly Journal of Mathematics* (1860),[67] Tait used his second paper to describe "the particular cases of the mutual action of galvanic currents, and of the forces exerted by permanent magnets on each other," in which he emphasized "the superiority of the Calculus of Quaternions over the ordinary analytical

[66] Neither "electrodynamics" nor "magnetism" were subjects taught to Tait at Cambridge during his studentship there. Rather, they were topics that he had been exposed to in Belfast, while in the presence of Andrews and James Thomson (brother to William Thomson), the latter of whom had been appointed professor of engineering at Queen's University in 1857.

[67] A short continuation of this paper appears 10 months later in October 1860, also in the *Quarterly Journal of Mathematics*, and is entitled "Quaternion Investigation of the Potential of a Closed Circuit" (Tait 1898).

processes of Geometry of Three Dimensions" (Tait 1898, 22). In the introduction to that paper, Tait wrote:

> A comparison between the processes employed in this paper and those of Ampère (*Théorie des Phénomènes Électrodynamiques,* etc., many of which are well given by [Michael] Murphy in his *Electricity*) will at once show how much is gained in simplicity and directness by the use of Quaternions. The gain in simplicity will be noticed in the investigations of the mutual effects of permanent magnets, where the resultant forces and couples are at once introduced in their most natural and direct forms (Tait 1898, 22).[68]

The claim that quaternions could present natural phenomena in a more "natural" and "direct" manner than could Cartesian algebra is one that Tait would come to repeat often throughout his career.

In the present article, Tait summarizes Ampère's experimental laws as follows:

> I. Equal and opposite currents in the same conductor produce equal and opposite effects on other conductors, whence it follows that an element of one current has no effect on an element of another which lies in the plane bisecting the former at right angles. II. The effect of a conductor bent or twisted in any manner is equivalent to that of a straight one, provided that the two are traversed by equal currents, and the former *nearly* coincides with the latter. III. No closed circuit can set in motion an element of a circular conductor about an axis through the centre of the circle and perpendicular to its plane. IV. In similar systems traversed by equal currents the forces are equal.

To these laws, Tait adds "the effect of any element of a current on another is directly as the product of the quantities of the currents and of the lengths of the elements" (Tait 1898, 23). It is understandable that Tait would envision the use of quaternion mathematics in such an electro-dynamical system. The combined effects of the various forces that represent electric currents in the "laws" mentioned above could be algebraically represented as four-termed entities, composed of a length measure and three directional measures. And though the article is still, primarily, a work of symbolical (and analytical) algebra, Tait does make explicit his view that natural philosophy—that is, experimental natural philosophy—could work symbiotically with quaternions. For instance, in Section 9, Tait considers the "whole effect" of an electrical circuit when the conductor being used is

[68] Tait admits some of the "conciseness of the method is lost by the necessity of going out of the way to prove results in Quaternions, a step which would not be requisite if the Calculus were more generally known" (Tait 1898, 22).

straight and "indefinitely extended in both directions." In such an instance, Tait considers a vector perpendicular to the conductor (symbolized by $h\zeta$) found at the extreme end of the solenoid (the coiled wired through which an electric current is run to generate magnetism). In four lines of quaternion mathematics (in which Tait invokes a particular integral he had developed to represent the effect of a "closed or indefinitely extended" circuit), he produces a pithy little expression $\left(-\frac{CAaa_1}{xh}V\eta\zeta\right)$, which describes the fact that the "whole effect" of the circuit under investigation is "perpendicular to the plane passing through the conductor and the extremity of the solenoid, and varies inversely as the distance of the latter from the conductor." Tait then claims, "This is exactly the observed effect of an indefinite straight conductor on a magnetic pole, or particle of free magnetism" (Tait 1898, 26).

Nowhere does Tait cite exactly which particular experimental observations he is referring to, although it is likely he got them from Murphy's *Elementary Principles of the Theories of Electricity, Heat and Molecular Actions* (1833), a book written by the Reverend Robert Murphy at Caius College, Cambridge for "the use of students in the university" in the years when theories of electricity and heat were still on the Tripos examination. In Murphy's old Cambridge textbook, the electrical phenomena discussed are based upon the works of Siméon-Denis Poisson, from his varied memoirs on electricity, magnetism and molecular action, and M. Ampere, from his *Théorie des Phénomènes Électrodynamiques Uniquement Déduite de l'Expérience* (1827).[69] Murphy writes about mathematical considerations in combination with experimental confirmations of "some of the results deduced"

[69] With regards to the theory of heat, Murphy writes that his textbook is based on the work of Fourier in his *Theorie de la Chaleur* (1822). Ironically enough, it was Whewell's suggestion, as indicated in the preface to his *Dynamics* (2nd Edition) (1836), that someone should compose such a text. Seeking to provide a mathematical treatment of developments in natural philosophy, Murphy produced the test for use by Cambridge students. The irony emerges in the fact that it was Whewell who later deemed these subjects to lack proper foundations, and fought for their exclusion from the undergraduate curriculum.

(Murphy 1833, vi).[70] In a chapter devoted to "Electricity in Magnified Substances," he writes, of "the action of a system of magnetic particles",

> It was discovered by Oersted, that Voltaic conductors act on magnets, and conversely that magnets act on Voltaic conductors: if an electrical discharge be passed through a conductor in the form of a helix, a steel needle placed parallel to the axis of the helix and within it becomes strongly magnetized: lastly, a Voltaic conductor attracts iron filings to its surface while transmitting electricity, but when the transmission is discontinued, the filings immediately drop off (Murphy 1833, 133).

Near the end of his chapter, Murphy adds:

> The evaluation of the magnetic actions on external points, whether the system be at rest or in motion, depends on the solution of the equations [presented in the chapter]; M. Poisson to whom this theory is due, has made applications in his third memoir on Magnetism, to the case of a homogeneous sphere, hollow or solid, turning round its axis, and influenced by terrestrial magnetism, and has shewn that the effect of the rotation is very nearly equivalent to that produced by a force normal to the plane passing through the axis in the direction of the influencing action of the earth... (Murphy 1833, 145).

In Tait's discussion of the extremity of a solenoid (i.e. a helix-figured conductor), it may well be that he had Oersted or Poisson's findings in mind, as presented by Murphy. The fact the experimental works of Oersted, Poisson, Peter Barlow (1776-1862) and Francois Arago (1786-1853) emerge in Tait's quaternion publication indicates the presence of a developing terrain of natural philosophical knowledge.

Before moving to Edinburgh to serve as professor of natural philosophy, Tait issued a final paper on quaternions. That brief (two-page) account of a "Quaternion investigation of the potential of a closed circuit," published in the *Quarterly Journal of Mathematics*, built upon Tait's earlier "Quaternion investigations connected with Electrodynamics," in considering the "potential of any system upon a unit particle at the extremity of γ," where $F(\gamma)$ is a function representing the potential C. Tait argued that if $F(\gamma) = C$, the differential of this potential function, in quaternion

[70] Murphy explains the first part of his book focuses on the functions needed to mathematize electrical phenomena. The second part considers "the manner in which electricity is disposed in bodies, previous to its becoming sensible by the action of electro-motive causes"—an aspect of electrical studies Murphy thinks has "not been before made the subject of mathematical investigation" (Murphy 1833, vi).

notation, would be $S.v d\gamma = 0$, where v is a vector normal to the potential function and represents the "*direction* of the force" and S represents a scalar value (Tait 1898, 33). Tait manipulates the differential for the potential function using the algebraic rules of quaternion mathematics to argue that v also represents the force "in *magnitude*". Invoking an expression that had been developed in his previous quaternion paper, which represented the "vector force exerted by a small plane closed circuit on a particle of free magnetism," Tait presents the potential of a closed circuit as being proportional to a "solid angle subtended by [the] circuit." He concludes that his quaternion account is equivalent to the result obtained for finite circuits when one uses the older technique offered by Ampère, in which one breaks up a finite circuit into an "indefinite number of indefinitely small ones" (i.e. an infinite number of infinitesimally small circuits) (Tait 1898, 34).

In his study of the history of vector analysis, Crowe (1967) has argued that Tait served as a mere "transitional" role in the history of vector calculus, applying Hamilton's quaternions to a series of well-known problems in physics that had been previously dealt with using traditional Cartesian coordinates. Tait's account of Ampère's finite circuits, might lead one to agree with Crowe in this regard. In fact, Tait even identified himself as not presenting anything "new," so much as showing the improved style of argument, the sleekness of design, and the labour saved in employing quaternions. For instance, in his "Formulae connected with small continuous displacements of the particles of a medium," published in the *Proceedings of the Royal Society of Edinburgh* (1862), Tait writes,

> Although most of the results deduced in this Note have been long known I venture to offer it to the Society on account of the extreme simplicity of the analysis employed, and the consequent insight it affords us into the connection of various formulae (Tait 1898, 37).[71]

Yet, contrary to Crowe's account of Tait's contributions—and contrary, perhaps, to Tait's view of himself at the time—Tait's actions do demonstrate that the Scotsman was doing something situated,

[71] In that "Note" Tait invoked Hamilton's "nabla" operator to show the effect of applying the differential operator to a scalar function is to produce a vector that represents "in magnitude and direction the most rapid change in the value of the function" (Tait 1898, 38).

something idiosyncratic, and something unique. The varied terrains of knowledge that Tait had trod upon by that point led him to view quaternions not only as simplifying mathematical tools, but also as efficient engines that produced mathematical products for potential uses in natural philosophy. Quaternion mathematics wasted less energy by avoiding unnecessary mental labour, and within the context of the 1860s, they became (in Tait's view) the more moral approach to mathematics. Scottish Presbyterianism had imbued practitioners such as Thomson and Tait with a moral sense of scientific duty, mandated by the need to preserve the finite stock of God-given energy in the universe. Wasteful mathematical techniques were to be condemned as much as wasteful engine operations, wasteful labour, or wasteful moral behaviour. Tait argued the only inefficiencies to be observed in the lengthy quaternion proofs he often presented were the direct outcome of the calculus of quaternions not being "more generally known" (Tait 1898, 22). In other words, greater facility with and wider circulation of quaternion mathematics (i.e. through its inclusion on the Tripos) would greatly improve the state of mathematical knowledge in general, by improving its efficient uptake on a national scale.

Tait had also begun to emphasize the De Morgan-influenced aspects of his symbolical algebraic training—namely, that mathematical results are most laudable when interpreted in physical ways. Thus, unlike Hamilton, Tait began to focus on the physical interpretations of certain symbolical transformations. This led him to explore those natural philosophical circumstances in which quaternions could best be put to work. Forces could be represented using couples and their resultants, and this algebraic representation of directed lines in space offered direct and unmediated insight into the nature of the physical universe. As the three-dimensional analogue of couples, quaternions provided the natural philosopher and mathematician with unmediated insight into spatial ontology. It was in this manner that Tait sought to put quaternions to use, hoping they would come to serve a role in the grand experimental-philosophical project then being formulated by Tait and Thomson—namely, a unifying theory of the structure of the universe as a whole.

The University of Edinburgh, an institution in flux (1840-1870)

At the start of the 18th-century, the University of Edinburgh was still little more than an Arts College with a divinity school attached. Its role as the provider of clergy for the Church of Scotland had become institutionalized through its primary curricular focus on "Divinity, Oriental Languages, [and] Ecclesiastical History." The university expanded its scope over the course of that century. A Chair of Mathematics was established and taken over by successive generations of the Gregory clan, and courses in medicine began to attract a wider array of students. But the changes that effectively launched Edinburgh into a period of more standardized educational provision with competitive examinations, student prizes, honours degree options, and mandatory matriculation fees occurred primarily in the 19th-century (largely as the result of financial pressure linked to Britain's involvement in the Napoleonic wars).

Leading into the 19th-century, Edinburgh was already considered one of the best universities in the country. Horn (1967) argues that part of the reason for this high level of respect was the nature of the Scottish curriculum, which, contrary to Oxford and Cambridge, had continued a commitment to wide-ranging and broad education. At the dawn of the 19th-century, Oxford had chosen to focus its undergraduate curriculum on classics and logic; Cambridge, narrowed its focus on mathematical foundations. The University of Edinburgh, and its cousin institutions in Glasgow, Aberdeen and St. Andrew's, clung to the notion of a broad liberal arts curriculum—one that sought to expose students to multiple subject areas, including classical languages, biblical study, rhetoric, mathematics and "universal civil history".[72]

[72] As Thomas Jefferson would declare in 1789, Edinburgh's curriculum was "unrivalled" anywhere in the world (Horn 1967). Jefferson's claim was justified, perhaps, by the large number of Edinburgh graduates that century who were later elected as Fellows of the Royal Society (60 were from Oxford; 85 from Cambridge; 79 from Edinburgh; and 32 from other Scottish universities). No doubt, Edinburgh played a central role in the flourishing of intellectual life in 18th-century Scotland.

Yet, the first decade of the 19th-century, also brought with it severe financial constraints, as course fees dropped off and the university entered a period of sustained economic turmoil.[73] Although tenure at the Scottish universities was as much a guarantee of lifetime employment as it was for professors at English universities, the basic salaries paid to Scottish professors were puny by comparison. Most professors collected class-based fees to survive. Although the university could boast a student population numbering approximately 2,000 by 1800, most of those students avoided matriculating in order to skirt matriculation fees. And because many students also chose to take only one of two years' worth of courses, thereby avoiding graduation fees,[74] the university's coffers suffered heavily. Those two factors combined led to periodic bouts of financial tension at the administrative level, and cantankerous debates between university professors and the town council governors who managed the school's finances.[75]

Edinburgh could still, however, lay claim to an undisputed reputation in liberal arts well into the 1820s. In fact, the university served as the model for the nascent metropolitan university in London, i.e. University College, London. Yet, there was a growing sense of frustration from within. The combination of poor funding for the university, and a lack of income from student fees for

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[73] The university hit a financial rut in the 1790s when, in the midst of building repairs, expansions and renovations, private funds dried up as British citizens—including Scottish investors—were pressed upon by increased war taxation and encouraged to invest in war bonds. Some buildings, including the central part of a western block (which was later to form the Natural History museum) was left in half-finished state, its beams exposed and its upper level unroofed. Only in 1801 would parliament hand over another £5000 to help complete the unfinished, and now rotting, building section (Horn 1976, 90). Yet, while the French Revolution had soaked up financial support for the university in the form of private investment, it had conversely channeled more students—primarily English sons of aristocrats—to attend its courses. In normal circumstances, those sons would have been sent abroad for their education. Lacking the ability to attend school in France, or in other parts of Napoleonic Europe, students saw Scotland as an ideal option for foreign studies.

[74] Note that matriculation fees in Scotland were generally quite low, holding true to the longstanding Scottish tradition of not excluding any able young man from pursuing higher education. However, due to the diversion of private investments and donations from the university towards the anti-Napoleonic war effort through war-based investments, the university doubled its matriculation fee in 1806, arguing that the 18th-century fee was "too small in present times for the purchase of books to the Library." Nonetheless, given that a minority of students bothered to matriculate—choosing, rather, to simply attend courses and not graduate formally—the increase did little to improve the living standards of regular professors or their ability to expand their respective research facilities (Horn 1967, 101).

[75] Due to the continued financial stranglehold that the university and town council found themselves in, no new chairs in any department were created between 1760 and 1862. Proposed chairs in Celtic Literature and Antiquities (1807), Comparative Anatomy (1816), Intellectual Power (1823), and Political Economy (1825) were rejected. However, as Morrell (1972) has pointed out, not all of the rejections were the result of a lack of funding alone. The patronage of a new chair often threatened other professors, who relied—as has been mentioned—on student attendance to generate fees as income. The rise of new chairs sometimes threatened the sustainability and profitability of other professor's courses; thus, the chairs in Comparative Anatomy, Intellectual Power and Political Economy were all rejected by the Senatus after vociferous professorial opposition (Morrell 1972, 47).

matriculation and graduation, led to a series of highly critical and controversial accounts of Edinburgh's higher educational system.[76] One such account emerged in the Reverend Michael Russel's *View of the System of Education at Present Pursued in Schools and Universities of Scotland* (1813), in which the school master berated the teaching of varied new and modern topics (especially in natural philosophy). He lobbied for a return to the curricular focus of Euclidean geometry, Aristotelian logic and classical languages. Another criticism proffered by J.C. Lockhart, Sir Walter Scott's son-in-law, in *Peter's Letters to his Kinsfolk* (1819), lambasted the lack of organized student housing, and student codes of conduct, including the failure of the university to institute any sort of dress code for classes or graduation ceremonies. Lockhart referred to the "slovenly and dirty mass" of students who resulted in a "contaminating atmosphere" on campus. The critical author pointed out that English fathers rarely sent their sons to college with less than £300 per annum in spending money, while in Scotland, "Any young man who can afford to wear a decent coat, and live in a garret upon porridge and herrings, may, if he pleases, come to Edinburgh, and pass through his academical career, just as creditably as is expected or required" on less than £30 per annum. In 1825, *The Modern Athens* (1825) followed up arguing the "Athenian university... has sunk to rise no more" (Horn 1967, 116-117).[77]

[76] There was opportunity to be found in growing student demand for technical expertise, fuelled in part by the government's need for military expertise during the Napoleonic era. Some professors simply started offering more saleable course content, attracting thereby higher student numbers and pocketing increased income as a result. The practice of marketing one's courses continued well after Napoleon had been sent to St Helena. Professors, such as James Pillans (1778-1864), Professor of Humanity and Laws, and John Leslie (1766-1832), Professor of Mathematics, simply rearranged their courses in order to offer supplementary classes. In 1822, for instance, Pillans created a third course in humanities for his advanced students, while Leslie began to offer a course of "special physics" that he aimed at the "general physical science" student, who wanted to complete a course in physics within three years. In 1826, William Wallace (1768–1843) proposed replacing his own course in calculus with one that covered astronomy; he also proposed introducing the potential subjects of geography, navigation, gunnery and fortifications to attract more students. Lastly, Thomas Charles (1766-1844) introduced a popular laboratory course in Chemistry and Pharmacy, which was managed and taught by an assistant (Horn 1967, 95-97). Thus, despite the financial constrains put on the university itself, professors—who largely lived from their student attendance fees—began to reflect the demands of the military and civil service by extending, altering and reshaping their course offerings. In addition, extra-mural teaching became the norm for many faculties and departments, in particular in medicine, which students appealed to so as to bolster poor lecturing elsewhere, and to gain knowledge not provided within university lectures. In a student's guide to the university, printed in 1820, a catalogue of extra-mural lectures offered in Edinburgh was included; it ran to five pages, and included one or more of the courses in all medical subjects normally taught at the university (Horn 1967, 109).

[77] Despite its apparently pathetic provision of university life and education—at least according to critics such as Russel and Lockhart—student numbers actually increased throughout the first two decades of the 19th-century, resulting in a record-setting 2,400 students by 1823. The university administration also aided the blossoming of its student body by

Due to such publicized concerns over the status of the venerable Scottish institutions, the British government launched a Royal Commission exploring the state of the Scottish universities. The commission's creation was, in fact, precipitated by telling events—a public quarrel between town council and professors at Tounis College (the university's medieval name, by which it was still then referred) had erupted over authority and administrative control. The aim of the commissioners was to downplay the role of the Scottish Kirk in shaping university policy and curricular content. They also sought to insert, in the place of the church, a formal relationship between the university and the Crown.[78] That government-led initiative resulted in the 1826 Scottish Universities Commission, established by Robert Peel, then Home Secretary.[79] The commission produced a series of stipulations that altered the nature of Scottish university degrees for the next four decades. For instance, the Commissioner's final report, which emerged in 1831, stipulated that the universities of Scotland specify the objectives of their respective degree requirements and offer a reformed course load in order to better serve the prevalent social and economic needs of the country. The commissioners also stipulated that universities in the North acquire new funding structures so as to avoid the sorts of financial constraints and tugs-of-war that had hamstrung their operations in the past. One of the key outcomes of the commissioners' report was the transference of authority in decision-making

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allowing the production of various student-run magazines and publications. Many of those publications were short-lived in nature, though they nonetheless provided a unifying force for a student body that had often only the local taverns as locations for joint socializing and politicking. Student publications such as *The Cheilead or University Coterie* (1826-1827) served as a platform upon which students could advocate for housing and new courses; those publications also served as a medium through which student alumni could organize themselves. As one contributor to *The Cheilead* was keen to argue, "There is not such a disjointed body of alumni in the whole world" as that to be found among Edinburgh alumni (Horn 1967, 136). That was to change from the 1840s onwards, when it was Edinburgh's alumni that would begin to fund the university's various new scholarship programs, student prizes and even new academic chairs.

[78] The commissioners claimed the Scottish universities "possess scarcely any ecclesiastical feature, except that they have a certain number of professors for the purpose of teaching theology, in the same manner as other sciences are taught." Horn (1967) claims this was an understatement, which ignored the statutory tests that had been imposed upon all professors elected by the legislation governing the university since Queen Anne's reign. However, when the Presbytery of Edinburgh did try to influence the choice of professor of mathematics in 1805 by enforcing the religious tests, the Senatus responded by noting no professor over the past half century had been called upon to actually undertake the tests. Thus, the Commission officially recognized what had de facto became the case already in Edinburgh—though presumably an institution affiliated with and serving the Church of Scotland, most professors were not explicitly required to follow particular church doctrine. The role of the church in determining professorial choices would become contested again, however, with the Disruption in 1843.

[79] Although Davie (1964) has contended this was the beginning of the end for the independent Scottish curriculum, other historians have argued the main instigation for the commission came from a group of well-informed Scottish participants who knew all too well about the struggles existent between the town councils and the Scottish universities—struggles that had degraded and diminished the ability of the universities to produce independent research or even to offer regular courses (Morrell 1972).

away from the Town's Council to a University Court, composed of representatives from both the professional and lay populations.[80] One other important aspect of the commission was its focus on the role of alumni in university affairs. The Commissioner granted alumni the automatic right to vote for members of the new governing University Court. This latter move brought into the institutional fold those generations of Edinburgh alumni who, in previous years, had had no reason to maintain a continued interest in their *alma mater.* The increased engagement with graduates mobilized alumni into lobby groups in later years, leading some to spearhead efforts for further reform to the curriculum in the 1850s.[81] When parliament effectively passed the Universities (Scotland) Act of 1858 (based on the 1831 Commissioners' report), certain other standardized aspects of higher education, which had been missing in Scotland, and which had come to disadvantage Scottish graduates, were enshrined legislatively. Although the open lecture-style of Edinburgh had long been applauded for its liberalizing effect, the emergence of newly standardized tests for the British civil service demonstrated a gap between the skills of Edinburgh graduates and those required at the civil

[80] Indeed, it was the parliamentarian Joseph Hume, radical M.P. for Aberdeen and Lord Rector of Marischal College who first approached Peel in 1825 with regards to a setting up a Royal Commission. The protracted struggle between the Town Council and the Senate in Edinburgh was well-known; the Council was composed largely of the university's patrons, and it was responsible for the election of approximately two-thirds of the university's chairs, as well as overall supervision of university affairs. As one observer of the times suggests, the members of that council "omnipotent, corrupt, impenetrable ... Silent, powerful, submissive, mysterious, and irresponsible, they might have been sitting in Venice" (Morrell 1972, 42). Certainly Peel was aware of a slew of other concerning problems. Hume made the case for the efficient amalgamation of the two Aberdeen colleges, Marischal and King's College. Peel was aware that the problematical Apothecaries Act (1815) had raised difficulties for medical graduates of Edinburgh and Glasgow who wished to practice in England; he was aware that Edinburgh's Town Council seemed to favour, at the time, loyal Tories in its election to chairs, rather than the most qualified candidate. A plethora of concerns coloured Peel's engagement with the Scottish universities, which was a relationship he could not avoid as being Home Secretary meant he was, at the same time, patron for the Regius chairs of Scottish universities (Morrell 1972, 41). In the end, it was a dispute over which body had ultimate right to determine qualifications for graduation—the Town Council or the Senate—that led to the Senate applying directly to Peel and George the IV for the establishment of a commission. Morrell's (1972) assessment of the situation is that: "For these thirty-three men, mainly merchants and craftsmen, university patronage was merely one of many activities: they were open to corruption, particularly when they were intellectually incapable of judging the academic worth of candidates; and they enjoyed many opportunities for satisfying their personal and political interests." This is not to say the Council was always incompetent in its election of chairs. The 18th-century had witnessed the election of many individuals who would become eminent representatives of their field, including: Dugald Stewart (mathematics, 1747-1772); John Playfair (mathematics, 1785-1805); and John Leslie (natural philosophy, 1819-1832) (Morrell 1972, 43).

[81] At the time, a group of graduates formed the Association for the Improvement and Extension of the Scottish Universities. Headquartered in Edinburgh and led by James Lorimer, later professor of Public Law, the group advocated for increased parliamentary funding to provide the financial sustenance required to add new chairs of research to the universities. By the 1850s, with industrialization in full bloom and the French Revolution and its war-based requirements a distant memory, the push for expanded technical and specialist teaching fell on newly appreciative political ears. The Association sought to mimic the examination tactic used in England, especially at Cambridge—i.e. the appointment of external examiners—in order to bolster standards of examinations and ensure students were treated fairly by disinterested professors.

service level. The 1858 Act institutionalized a recommendation made by the commissioners to offer a more "regular and systematic course of study" in Arts for those students who aimed at an honours degree and especially for those students who aimed at a professional career. The Act ensured that repeat attendance in a regular course of study over "successive" years was required to qualify for a graduating degree. In addition, competition motivated by new prizes and the attainment of "academical honours" was rendered an objective for all upper-class students, especially for those intending careers in the church or in law. Regular examination was stipulated as preceding the award of a B.A. or M.A. degree, and examination for the M.A. degree could not follow sooner than one year after the B.A. had been completed. Lastly, the Act decreed that examinations would be carried out by examiners appointed by the Senatus, rather than the professors heading the subjects being examined, so as to avoid subjectivity in grading (Horn 1967).

To some degree, the Act merely institutionalized practices that had informally come into existence in Edinburgh throughout the intervening decades. For instance, in the 1830s, the Professor of Natural Philosophy, James D. Forbes, siphoned from Cambridge's Tripos the technique of using written questions as a form of examination in lieu of the oral style of examination that had long-dominated Edinburgh's courses. Forbes, Kelland and others also adopted the practice—already rendered a cultural norm in Cambridge—of using textbooks for the purposes of lecturing and examining students. Though Hamilton (in philosophy) and Kelland (in mathematics) were both concerned about an over-emphasis upon written questions (the answers to which could be memorized or deduced from the texts ahead of time), by the end of the decade both had accepted and propagated the norm of standardized testing. Students who wished to receive the degree of M.A. from the Faculty of Arts had to complete three days of examination in Classics, Mathematics and Philosophy.

The 1858 Act also changed the financial structure of the university. In addition to mandating matriculation and graduation fees, the Act's political inclusion of alumni in university governance was a boon to the school's private funding. From 1860 onward, the university opened "a new chapter in its history, stripped of nearly all its old endowments, and now dependent financially upon its fee income from various sources," including government grants, revenues from Leith harbour, student fees and private endowments.[82] One outcome of this increase in wealth was the rapid creation of a slew of new chairs meant to bolster both the research outputs and academic appeal of the university. Dr. John Muir founded the Sanskrit Chair in 1862; another private benefactor founded the Chair of Engineering in 1868; Sir Roderick Muchison founded the Chair of Geology from his private funds in 1871, and, in the same year, the Merchant Company founded the Chair in Political Economy and Mercantile Law. The Trustees of the Reverend Dr. Andrew Bell later founded, both in Edinburgh and St. Andrew's, Britain's first chairs in education in 1876. As an indication of how much of a novelty all of this administrative and academic activity was, note that from 1582 to 1862 (a 280-year period) only two new chairs had been created from private funds. By comparison, from 1862 to 1882 (a 20-year period), seven chairs had been funded with a total endowment figure of £58,000 in private funds (Horn 1967, 186-188).

One issue not addressed by the commissioners was the establishment of a specific "science" degree. The commissioners had believed there would be little long-term interest for such a degree. Yet, the Great Exhibition of 1851 heightened fears across Britain that the country was again falling behind in its technical and engineering superiority. In his 1859 speech as President of the BAAS, Prince Albert stated,

> We may be justified in hoping, however, that by the gradual diffusion of Science, and its increasing recognition as a principal part of our national education, the public in general, no less than the Legislature and the State, will more and more recognize the claims of Science to their attention; so that it may no longer require the begging-box, but speak to the State, like a favoured child to its parent, sure of his parental solicitude for its welfare; that the State will recognize in Science one of its elements of strength and

[82] The Representation of the People (Scotland) Act of 1868 granted graduates of Scottish universities a vote in determining who would be the parliamentary representative of their respective universities. This newly granted parliamentary vote added to the inducements to graduate, thus increasing revenue from graduation fees. Meanwhile, in 1864, Dr John Muir, who founded the Chair of Sanskrit, also formed the Association for the Better Endowment of the University of Edinburgh. The combination of increased privileges—both professional and political—associated with graduation and the marketing efforts of Muir's Association led to a windfall in private endowments, such as the 1879 endowment donated by Dr. Vans Dunlop, which was sufficient for 18 £100 scholarship for three years running (nine in Arts, eight in Medicine, and one in Law) (Horn 1967, 186).

> prosperity, to foster which the clearest dictates of self-interest demand (Morrell 1973, 356).

After being appointed to the chair of natural philosophy in Edinburgh in 1860, Tait soon rectified the omission of a science degree. Four years after his arrival, the university bowed to Tait's consistent lobbying for the establishment of an experimental laboratory (similar to the one he had worked in at Belfast). The Senatus then introduced the degrees of Bachelor and Doctor of Science, both of which could now be taken in mathematics and science (Horn 1967, 179).

Tait in Edinburgh (1860-1880)

The chair of natural philosophy at the University of Edinburgh had become vacant in 1860, when James Forbes retired from the position. Competing against a list of candidates that included Maxwell (then at Marischal College) and Routh (a rising "coach" from Peterhouse College), Tait succeeded in part due to his apparent lecturing abilities. As the *Courant* of Edinburgh reported at the time,

> It will be no disrespect to the warmest friends of the successful candidate, and we do not mean to dispute the decision of the curators, by saying, that in Professor Maxwell the curators would have had the opportunity of associating with the University one who is already acknowledged to be one of the remarkable men known to the scientific world. His original investigations on the nature of colours, on the mechanical condition of stability of Saturn's Rings, and many similar subjects, have well established his name among scientific men; while the almost intuitive accuracy of his ideas would give his connection with a chair of natural philosophy one advantage, namely, that of a sure and valuable guide to those who came with partial knowledge requiring direction and precision. But there is another power which is desirable in a professor of a University with a system like ours, and that is, the power of oral exposition proceeding upon the supposition of a previous imperfect knowledge, or even total ignorance, of the study on the part of pupils. We little doubt that it was the deficiency of this power in Professor Maxwell principally that made the curators prefer Mr. Tait...We have never heard Mr Tait lecture, but we should augur from all we can learn that he will have great powers of impressing and instructing an audience such as his class will consist of, combined with that conscientious industry which is so necessary in a successful professor (Knott 1911, 16-17).

What is revealing about the *Courant* account is that the competition for the post was one fought, primarily, between two Scots: Maxwell and Tait. The *Courant* ignores entirely the other candidate, and Englishman, Edward Routh. One can, therefore, imagine that a factor much more important than Tait's lecturing ability helped him to succeed in his effort to gain the chair—namely, his Scots ethnicity.[83]

That is not to say that Tait lacked in particularly useful lecturing qualities. On the contrary, as one of his students, J.M. Barrie, recounted in vivid and romantic overtones in *An Edinburgh Eleven: Pencil Portraits from College Life* (1894):

> Never, I think, can there have been a more superb demonstrator. I have his burly figure before me. The small twinkling eyes had a fascinating gleam in them; he could concentrate them until they held the object looked at; when they flashed round the room he seemed to have drawn a rapier. I have seen a man fall back in alarm under Tait's eyes, though there were a dozen Benches between them. These eyes could be merry as a boy's, though, as when he turned a tube of water on students who would insist on crowding too near an experiment, for Tait's was the humour of high spirits (Barrie 1894, 46-47).

Tait was apt to calling himself a "lecturing machine" whose aim it was to instruct the youth of the country in "the common sense view of the universe we live in" (Knott 1911, 18).[84]

In terms of content, his lectures remained largely untouched from 1860 to 1881, except for the addition of supplemental material from his own publications throughout that period.[85] Typically, Tait dwelt on the "properties of matter" as an introduction to his natural philosophy courses—properties, he argued, that constituted the foundations upon which a more detailed study of experimental natural philosophy could take place. Tait also spent the first few days of his course talking about "means by which we gain knowledge of the physical universe." As his biographer later recounted:

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[83] Kelland had, in fact, been the only non-Scot, English-trained academic to be appointed to a post in the University of Edinburgh by the time Tait joined the institution's faculty.

[84] Among his students, Knott (1911) identifies three groups: the first group included those students who simply pursued an ordinary degree, and for whom Tait served as little more than a natural philosophy lecturer; the second group included those students who gained intimate knowledge of Tait through the Advanced Classes and optional laboratory course; the third group included those students who served as Tait's personal laboratory assistants.

[85] Tait's lectures notes were mostly just "jottings of headings with the experiments indicated and important numerical values interspersed." In his original notebook, "which was still in use in 1881, these headings were entered with intervening spaces so as to allow for additions as time went on." In 1881, Tait rewrote his notes into a "smaller octavo book" which he used until he stopped lecturing (Knott 1911, 19).

> The conceptions of time and space, and the realities known as matter and energy, were introduced and placed in their right setting from the physical standpoint. These preliminaries disposed of, Tait began his systematic lectures on the properties of matter. His aim was to build a truly philosophical body of connected truths upon the familiar experiences of the race. In ordered sequence the various obvious properties of matter were considered, first, in themselves, then in their theoretical setting and their practical applications. Thus, to take but one example, the discussion of the divisibility of matter led to the consideration of mechanical sub-division and of the elementary principles of the diffraction and interference of light, illustrated by colours of soap films, halos and supernumerary rainbows. The fuller explanation of these was, however, reserved for a later date when the laws of physical optics were taken up in more detail. In this way the intelligent student was able during the first two months to gain a general outlook upon physical science. The nature of the course may be inferred from the contents of his book *The Properties of Matter*; but no written page could teach like the living voice of the master (Knott 1911, 20).

Tait lectured on the "properties of matter" for three hours a week, and he devoted two days a week to dynamics. Having "disposed" of the "properties of matter," Tait then turned to the subjects of "heat, sound, light and electricity." With regards to sound, light and electricity, however, Tait had only a few "systematic notes of geometrical optics but none on physical optics or electricity," while the "properties of matter" occupied more and more of the finite number of weeks in each session, as Tait included more elaborate experiments on the matter of atoms, the ether and vortex tubes (Knott 1911, 20).

Despite Tait's status as a natural philosopher and an experimentalist, his philosophical outlook was not that of the Baconian sort. On the contrary, he emphasized the lack of certainty and the "untrustworthiness" of the senses, urging students to "illuminate the dark places with the light of reason, with the search light of scientific imagination" (Knott 1911, 20). Tait believed in a rationalist approach to knowledge, where natural law emerges and manifests itself in partially perceived physical phenomena. Tait's epistemological stance reflects his inculcation into that hybrid Lockean school of empiricism/rationalism that had provided the symbolical algebraists of the early century with justifications for their methods. For Tait, as for his predecessing colleagues, knowledge is gained through a process of abstraction. However, set within the natural philosophical and religious terrains of Scotland, that meant that experimental knowledge provided particular and temporal case studies from which the natural philosopher could abstract universal relations—relations indicative of God's ordered universe. Tait held that universal equivalences in symbolical analysis were not enough; ultimate truth can only be accessed by applying mathematical equivalences to experimental knowledge in order to abstract physical relations and natural laws.

The degree to which experimental manifestations of particular natural philosophical concepts constituted crucial aspects of Tait's daily life as a lecturer in Edinburgh is demonstrated in Tait's recollection of the first lecture he delivered to students on November 5th, 1860. Tait recalls,

> fancying that a dry technical lecture to commence with might perhaps keep off rather than attract amateur students, I gave a set of experiments—the most striking I could muster—professedly without any explanation—in fact gave them as examples of the objects of Nat. Phil...I gave a 20 m[inute] lecture on the study, and the arrangements for the present session, and then plunged into the paradoxes. I reserved as the last the beautiful one of balls and egg shells suspended on a vertical jet of water, as they cannot be shown without some risk of a wetting to the performer and the nearest of the audience. Tomorrow I bring into play the large American induction coil, and show the rotation of a stream of violet light in vacuo round a straight electromagnet. I shall also show an inch spark in air ... and the discharge by it about 10 times per second of a jar with about 2 square feet of tin foil. There is no self acting break—for safety the interruption is made by a toothed wheel worked by hand—which for short experiments is much preferable. I shall also show the huge Cöln magnet (made under Plücker's direction) which took six of us to heave it up a gently inclined plane into the class room this afternoon (Knott 1911, 23).

Thus, Tait's lectures included mathematical applications that reflected both his years of training in analysis and his newly developing natural philosophical interests. Fourier analysis, Green's Theorem, and the "theory of strains" which, as Knott recounts, "was, indeed, a subject peculiarly his own, [where] many of his demonstrations, although given in ordinary Cartesian coordinates, were suggested by the quaternion mode of attack," played prominent roles in Tait's courses (Knott 1911, 22). From 1868 onwards, Tait also had access to the Physical Laboratory.[86] The young professor thus

[86] During his initial years at Edinburgh, Tait often wrote to his friend and former colleague, Thomas Andrews in Belfast, recounting his experimental approach to natural philosophy at his new university. In many of those letters, Tait expressed his appreciation for Andrews' experimental methods and training. He also wrote to tell Andrews about his ongoing experiments. On January 29, 1861, Tait wrote: "My dear Andrews, I would have written to you sooner, had not

offered voluntary courses in experimental natural philosophy. Although the material covered therein was not examined for the ordinary degree at Edinburgh, Tait trained students in the methods of experimentalism that he had absorbed during his years of work in Andrews's laboratory.[87]

Tait's quaternions, 1860-1870

Working within this environment of Edinburgh's standardized curricular demands, Tait produced four more journal articles on quaternions in 1862, 1863, 1868, and 1870, respectively, as well as an "introductory" textbook entitled *Elementary Treatise on Quaternions* (1867). All of those publications, except for one, were written before Tait opened his experimental laboratory in 1868. Tait's focus on quaternions in the first half of the 1860s was, in part, motivated by a continuing lack of experimental space within which he could carry out his own thermodynamical research, even though he had become a part of the northern group of natural philosophers then working on the "science of energy" (a group that included Thomson, Joule and Rankin among others). And although Edinburgh's financial fortunes were set to improve, given the imposition of the 1858 Act and its revenue-generating recommendations, the first large private endowments did not start to flow into university coffers until the late-1860s. Thus, given Tait's lowly paycheque and the substandard salary he and other professors at the university still received, his 1867 quaternion textbook constituted an important income-generating opportunity. There was a lucrative aspect to quaternion maths in

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my hands been full of the January Examinations, and some experiments which Principal Forbes asked me to make ... In a paper which is I believe to appear in the Phil. Mag. for February, and which was read some weeks ago at the R.S.E., he states that few people living have ever seen Ampère's experiments for the repulsion of a current on itself—and that he had never succeeded in getting it. At his request I tried it, and succeeded with a single cell of Grove's battery. With twelve cells the floating wire almost jumped out of the trough! As there is some slight objection to this form of the experiment on account of the thermoelectric effects which occur at every change of metal in the circuit, I devised a floating conductor of *glass tube* full of mercury to replace the copper wire. The mercury is so much worse a conductor than copper that it required four cells to give a good effect" (Knott 1911, 67). And at the end of that year, he wrote to Andrews to say: "I find that I cannot manage to visit Belfast at present—my simple reason is that I am to bring home from Glasgow (where I am going to stay a day with Thomson) two galvanometers and an electrometer on Saturday next—and I must have one galvanometer and electrometer fitted up during the holidays, as I shall just have reached the critical point of *Radiant* heat when we stop. The new galvanometer works by reflexion, and can therefore be easily shown to a large class, which was impossible with needle ones—besides it is delicate enough to show an effect even by frog-currents..." (Knott 1911, 67).

[87] Tait charged an initial fee of two guineas for students wishing to use the laboratory in the first winter session, after which time no further fees were issued even if the student continued to work in the laboratory throughout the year. Those students who returned to the laboratory courses after the first session were nicknamed "veterans"; Tait used the "veterans" as experimental assistants in the same way he had served Andrews in Belfast. Knott himself was one of those "veterans", having served in the laboratory from the late-1870s to early 1880s.

Scotland. Tait aimed to lecture on the matter using his own textbooks in his advanced courses, while his sympathetic colleague, and later co-author, the Cambridge-trained Philip Kelland, taught the topic as part of his mathematical courses in analysis. Driven thus by the combined needs of publishing within professional journals and generating supplemental income, Tait continued his research into quaternions by more explicitly morphing them into tools for use in natural philosophy.

Indeed, Tait's papers of 1862 and 1863 diverged significantly in tone from those he had produced in the late-1850s. In his later papers, Tait specifically identified quaternions as natural philosophical tools, rather than as symbolical analytical tools. For example, in his "Formulae Connected with Small Continuous Displacements of the Particles of a Medium," published in the *Proceedings of the Royal Society of Edinburgh*[88] on April 4th, 1862, Tait first promised "on a future occasion to give large further developments especially bearing on physics" (Tait 1898, 37). The natural philosopher went on to use quaternion calculus to discuss the equation of "one of a system of surfaces," symbolized by $F\rho = C$, where the differential of the equation is $S.\nu d\rho = 0$, and where "ν is a vector perpendicular to the surface, and its length is inversely proportional to the normal distance between two consecutive surfaces," and $d\rho$ is a tangent vector to the surface (which is perpendicular to ν), and S is a scalar value. If the surface is equipotential or isothermal, then - ν represents the direction and magnitude of the force or vector-gradient of temperature at any point on that surface. Tait invokes Hamilton's vector-operator (later termed "nabla" by Maxwell), represented by the inverted triangle:

$$\nabla = i\frac{d}{dx} + j\frac{d}{dy} + k\frac{d}{dz},$$

which, according to the rules of quaternion algebra, gives

[88] Given Tait's lack of status as a fellow of the Royal Society of London, he was not likely to publish in that venerable institution's journal. His association with the Royal Society of Edinburgh was useful in that it provided a vehicle for his quaternion publications. Yet, given the niche nature of quaternion mathematics, it is also likely the *Proceedings of the Royal Society of Edinburgh* constituted the *only* journal that would have published Tait's quaternion researches.

$$\nabla^2 = -\frac{d^2}{dx^2} - \frac{d^2}{dy^2} - \frac{d^2}{dz^2}.$$

The result of this operator is to transform the equation for the system of surfaces mentioned above into v, a vector perpendicular to the surface. As Tait put it,

> It follows that the *effect of the vector operator* ∇, *upon any scalar function of the vector of a point, is to produce the vector which represents in magnitude and direction the most rapid change in the value of the function* (Tait 1898, 38).

In his paper, Tait used the "nabla" operator to consider the quaternion expression for a variety of small (i.e. infinitesimal) particle displacements on various surfaces, including the surfaces of spheres and spheroids.

In his 1863 paper, Tait pushed even further in his natural philosophical musings, saying that his aim was to give his reader an "idea of the nature of the physical applications of quaternions." That paper demonstrates that the "quaternion operator" (referred to earlier using Hamilton's terminology of "vector operator") can cause "many interesting and important transformations" on any vector, where the vector of any point can be denoted by $\rho = ix + jy + kz$. Tait demonstrates the symbolical transformation of a "simple example," as well as "more complex functions," to argue the application of the quaternion operator to both vectors and to quaternions results in rotations and distortions of the original vectors and quaternions. One solution to $\nabla q = a$, where q and a are quaternions, is $q = S\zeta\rho + V\epsilon\rho + \phi\rho$, meaning that a quaternion that is operated upon by the "quaternion operator" produces yet another quaternion. Such transformations "present no difficulty" in the symbolical sense—they are due to the algebraic rules governing quaternion mathematics. Tait's focus is, therefore, on "the ready applicability to physical questions of one or two of those [quaternion transformations]," which he says is "a property of great importance, as it may now be asserted that the next grand extensions of mathematical physics will, in all likelihood, be furnished by quaternions" (Tait 1898, 45).

As a first example, Tait looks at σ as the vector-displacement of a point in a homogeneous elastic solid. If we use p to represent the pressure produced, then we find that the pressure "p" can be represented quaternionically as $p = kS\nabla\sigma$. This means the pressure produced in a homogeneous elastic solid is equivalent to some constant "k", which depends upon the compressibility of the solid. When $k = 0$, then pressure is also zero. Otherwise put, $S\nabla\sigma = 0$, where the scalar value "S" of the quaternion and the transformed quaternion, σ, are operated upon by "nabla" (Tait 1898, 45). Tait interprets this finding "physically" by citing Thomson's previous claim in the *Cambridge and Dublin Mathematical Journal*, that "The forces produced by given distributions of matter, electricity, magnetism, or galvanic currents, can be represented at every point by displacements of such a solid producible by external forces" (Tait 1898, 45). Tait presents this finding of Thomson's in "quaternion form" so as to "show the insight gained by the simplicity of the [quaternion] method." He concludes, we can represent the "vector-force exerted by one particle of matter or free electricity on another" as being $\sigma = -\nabla\frac{1}{T\rho}$ (where "T" refers to the tensor part of the quaternion being deploy.

Tait also considers the case of a small closed current to determine the comparative effect of a free particle or element of current versus a closed current or magnet. The quaternion presentation of these problems indicates the "effect of an element of a current on a magnetic particle is expressed directly by the displacement, while that of a small closed current or magnet is represented by the vector-axis of the rotation caused by the displacement" (Tait 1898, 46). In Tait's mind, quaternion mathematics allows for the better visualization of what is going on at the microscopic level in nature. Because a quaternion can represent both magnitude and rotational displacement, its use allows for easier comparisons between phenomena—some of which may involve magnitude displacements, others of which many involve rotational displacements, and others of which may involve both magnitude changes and rotational displacements at the same time. All of this understanding is compacted into fewer symbols in quaternion accounts (as compared to Cartesian accounts), Tait claims, thereby allowing for greater efficiency in mathematical thought and improved physical understanding.

The "science of energy," 1850-1870

In 1845, William Thomson struggled to determine the total force of attraction between two oppositely charged spherical conductors. This complicated conundrum had stumped various practitioners before him. Thomson's solution occupied only three lines of analysis (Wise 1989). His solution was the combined product of his training in Cambridge-style analysis, which had familiarized him with the notion of converging series, and his belief in the normative metaphors of "work" and "efficiency". Those latter terms served as organizing metaphors in political economy, social affairs, and scientific theories in Scotland. For example, Thomson's solution to the spherical conductors problem ultimately showed that the "ponderomotive force"—or the total force of attraction—between two spheres charged by a battery that maintains a potential difference (V) between them can be represented as the "total tendency of the system to minimize its work content" (Wise 1989, 264).

The belief in the virtues of minimizing work in physical systems was a notion already existent in diverse forms within the northern world of steam engine manufacturing. For Thomson,

> "Work" was not an abstract concept, nor merely the capacity of an engine; it was a motivation, a goal of action, the source of value and progress in the modern world, both material and moral. 'Waste' was its opposite, the source of decadence (Wise 1989, 265).

This opposition of "work" and "waste" was not peculiar to industrializing Scotland, either, although it found its greatest expression there. "Work" and "waste" were products of previous Enlightenment efforts in revolutionary France, where the working poor had opposed the wasteful actions of the ruling aristocratic classes. In mathematical circles, the efficient work produced by alegebraic techniques, as espoused by Étienne Bonnot de Condillac, served as a means of eliminating useless and wasteful mathematical procedures. Intellectual "work" was opposed to intellectual lethargy. The principles of "balance" and "equilibrium" entered in the metaphor of a "lever". For Condillac, analysis was like a "lever of the mind." To think is to "*weigh, balance, compare*" (Wise 1989, 271). Thus, balanced equations, such as $x - 1 = y + 1$, and $x + 1 = 2y - 2$, allowed for the determination of solutions of unknowns, by balancing known constants with unknown variables in a system of equations. The early British algebraists adopted these Enlightenment metaphors, advocating for the efficiency of the symbolical approach. Universal relationships—or balanced identities—could be determined using succinct and productive methods, as opposed to relying upon the wasteful and inefficient methods of geometry.

Through his Cambridge training, as well his upbringing within the Scottish industrializing north where his father had advocated for the development of industrially-applied mechanisms in the engineering sciences, Thomson had become steeped in the metaphoric language of "work," "equilibrium" (or balance) and "efficiency". The fact his scientific outputs and mathematical analyses reflected this language indicates it was more than mere imagery for the scientist. "Work," "efficiency" and "balance" were ontological characteristics of the world. From the early 1850s, Thomson, along with his colleague in engineering, Macquorn Rankine (1820-1872), introduced the terms "actual energy" and "potential energy"—offering, thereby, an equation for balanced energy distribution in the universe. Along with the Mancunian experimentalist, James Prescott Joules (1818-1889), Maxwell and Fleeming Jenkin (1833-1885), Thomson constructed a "science of energy" through the re-interpretation of works already written on heat and heat transfer by the French military engineer, Sadi Carnot (1796-1832), and his publicist, Émile Clapeyron (1799-1864), as well as Rudolph Clausius (1822-1888). He also used his own and Joule's experimental findings. Thomson (later with Tait) became enmeshed in the construction of a science of "thermodynamics" based upon evaluations of "heat loss," "work done" and "efficiency" gained in material engines.

The phrase "science of energy" first found the light of day in Rankine's 1855 paper, entitled "Outlines of the Science of Energetics," delivered to the Glasgow Philosophical Society. Rankine had called for the fuller establishment of a science "whose subjects are material bodies and physical phenomena in general." In 1857, Maxwell then drafted a paper, revealingly entitled "Sketch of an Introduction to the Higher parts of Mechanics, being the Science of Energy as it relates to the Motion and Rest of a Single Particle, of Two Particles or a system of Particles, this system being either of invariable form, elastic or fluid." The phrase "science of energy" only really gained widespread public recognition, however, in the 1860s, when Tait and Thomson jointly published their *Treatise on Natural Philosophy* (1867), and when Tait followed up with his *Sketch of Thermodynamics* (1868).

The "science of energy," as expressed by Rankine, Maxwell, Thomson and Tait presented a world that was no longer governed by action-at-a-distance "forces". Nor was it governed by discrete particles moving through massive voids. Rather, the physical phenomena of the universe were to be explained in terms of a continuous matter—an ether—that possessed "potential energy", the transference of which into "kinetic energy" results in a unidirectional dissipation in the total stock of the universe's usable energy over time. In addition to offering a "balanced" account of energy distribution, this northern account was also situated within the Presbyterian terrain of universal degradation and moral obligation. The Presbyterian universe had been set into motion by God. It was governed by basic laws (in so far as God followed his own rules) and the "energy" inherent in it existed by the grace of divine power. The Day of Judgment is embodied in the form of the dissipation of useful or "potential" energy. In other words, the total "energy" of the universe remains constant and the amount of "potential" energy is in steady decline. The universe is irrevocably losing "potential" energy, and thus becoming less useful and less inhabitable for humans as time progresses.

The upshot of this physical theory was moral in nature. The northern scientists viewed it as a moral duty to conserve energy, as "energy" it is only temporally available. Wasteful work was deemed immoral, and wasteful mathematics was an insult to the divine order that God put into place. It became the normative duty of natural philosophers to determine the best means of conserving energy and preventing the inevitable dissipation. Tait's engagement with this unidirectional "science of energy", and the network of metaphorical artifacts it possessed, helps to explain, in part, why he continued to engage with quaternions following Hamilton's death in 1865, despite the fact Hamilton's *Elements of Quaternions* (1866) had been poorly received, and despite the fact Thomson famously refused to allow Tait to use quaternions in their joint *Treatise* (as Thomson claimed there was no demonstrable benefit to the technique). From Tait's perspective, quaternions were less wasteful, more efficient, and, thus, more moral in nature (for more details related to the "science of energy," see Appendix Three; for an account of Tait's revised *Treatise on the Dynamics of a Particle* (1865), see Appendix Four).

An Elementary Treatise on Quaternions (1867)

Recall that Hamilton had insisted upon Tait delaying his *Elementary Treatise on Quaternions* until after the former had published his own revised account of quaternion mathematics. Hamilton's text did eventually emerge in 1866, a year after his death. As far as can be gleaned from Tait's obituary account of the Irish mathematician, Tait showed no sign of regret or annoyance at the fact he had had to delay his own quaternion textbook due to Hamilton's stringent stipulations. Rather, he lauded Hamilton as a "genius" whose name will "undoubtedly be classed with those of the grandest of all ages and countries, such as Lagrange and Newton" (Tait 1866, 37). Perhaps such praise is not surprising given that an obituary is not the place to lambast one's former (and now deceased) colleague. Nonetheless, by the mid-1860s, Tait had diverged from his Irish colleague and correspondent in important ways. Rather than viewing new symbolical systems as justified in and of themselves, he had begun to relegate symbolical mathematics to second-class status vis-à-vis natural philosophical knowledge. Tait had become a "natural philosopher, using mathematics for the elucidation of what might be called the metaphysics of molecular actions." In addition, Tait's "abhorrence of long and intricate mathematical operations is strongly expressed more than once"

(Knott 1911, 113). Thus, one of the motivations powering Tait's continued pursuance of quaternion mathematics was its perceived ability to short-cut mathematical computations, thereby allowing for greater efficiency in the pursuit of, what had become, the overarching science of Tait's career—namely, thermodynamics.

The first edition of the *Elementary Treatise* was published in 1867; the second edition in 1873; the third, with significant changes due to Tait's interaction with Cayley, was published in 1890. It is the first two of these editions that concerns us here. Tait uses the first edition of the *Treatise* to highlight the simplicity and efficacy of quaternions, and also their applicability to experimental findings. This latter emphasis is clear in the examples he uses as "problems" in the text. Note the *Elementary Treatise* still embodies the Cambridge-inspired symbolical algebraic philosophy in its algebraic manipulations; however, the approach is now delivered upon a terrain deeply contoured by the northern science of energy. Tait, for instance, demonstrates in Chapter V an idiosyncratic approach to quaternions in his discussion of the solution of equations (an approach not adopted previously by Hamilton). Tait expounds upon his view that the linear vector function can be used in accounts of "strain". Tait had considered "strains" in fine detail the *Treatise on Natural Philosophy*, also published in 1867. But whereas the consideration of the malleability and flexibility of a surface occupies a series of pages in Thomson and Tait's textbook, Tait deals with it in the matter of a few lines of quaternion analysis in his *Elementary Treatise on Quaternions.* Tait does defer to Hamilton, but he adds "I have not servilely followed even so great a master, although dealing with a subject which is entirely his own" (Tait 1867, vi).

In the Preface, Tait warns his reader that the text is being published years after his "best" quaternion work had been carried out, by which he was presumably referring to his years of correspondence with Hamilton. Due to Hamilton's request to delay his publication, as well as the new duties Tait had encountered within the domain of responsibilities bearing upon him as natural philosophy professor in Edinburgh, it had been years since Tait had engaged with quaternion analysis in as flurried a manner as he did in the late-1850s. The natural philosophy professor emphasizes the current text was also finalized and polished off at the same time that he and Thomson were heavily engaged in writing their *Treatise*—a text in which the economy and efficiency of nature, as manifest in the conservation and dissipation of energy, are central themes. It is not surprising to find, therefore, an emphasis on the economy of quaternion techniques in the present text. The notion of "efficient", non-wasteful work had already been present in Tait's earlier publications. He, as with many young Presbyterians and other northern religious actors, had been influenced by the theological notion of the universe's demise and the human duty to effectively and efficiently steward God's resources. In 1867, those claims emerged distinctly. Tait writes the examples chosen for the present text, "though not specially chosen so as to display the full merits of Quaternions, will yet sufficiently show their admirable simplicity and naturalness."

In an important divergence from Hamilton, Tait says he has also kept in view "as the great end of every mathematical method," the physical applications of quaternions. In addition, he treats the subject of quaternions "as much as possible" from a geometrical, instead of an analytical, point of view. It is possible to premise the properties of i, j, k, and then to construct from them the whole system "just as Hamilton himself dealt with Couples, Triads, and Sets." Although this method "may be interesting to the pure analyst ... it is repulsive to the physical students, who should be led to look upon i, j, k from the very first as geometric realities, not as algebraic imaginaries" (Tait 1867, vii-viii). Thus, Tait sets out his textbook as an exegesis on the geometrical nature of quaternion mathematics. Notably, this later claim is mostly a pedagogical one. As we will see, Tait's presentation of quaternions is still driven by symbolical algebra, rather than geometrical induction. He presents the claim—in line with the late-Peacockian and De Morgian doctrine—that geometrical and physical interpretation is possible and desirable within symbolical analysis, but the geometrical "proofs" he offers remain heuristic, rather than foundational.

Indeed, despite Tait's stated intentions to move away from pure analysis, he notes the most important aspect to grasp with regards to quaternions is their non-commutative nature in multiplication. Tait writes,

> The most striking peculiarity of the Calculus is that *multiplication is not generally commutative*, i.e. that *qr* is in general different from *rq, r* and *q* being quaternions. Still it is to be remarked that something similar is true, in the ordinary coordinate methods, of operator and functions: and therefore the students is not wholly unprepared to meet it. No one is puzzled by the fact that $log.cos.x$ is not equal to $cos.log.x$, or that $\sqrt{\frac{dy}{dx}}$ is not equal to $\frac{d}{dx}\sqrt{y}$. Sometimes, indeed, this rule is most absurdly violated, for it is usual to take cos^2x as equal to $(\cos x)^2$, while $cos^{-1}x$ is not equal to $(\cos x)^{-1}$. No such incongruities appear in Quaternions; but what is true of operators and functions in other methods, that they are not generally commutative, is in Quaternions true in the multiplication of (vector) coordinates (Tait 1867, viii).

Tait's tricky language deploys the claim that the commutative principle is akin to a problem in the order of operations, such that abandoning the commutative principle is the equivalent of simply assuming a new order of operations. Tait uses this to rhetorical effect. His underlying suggestion is that symbols in themselves hold no weight. The meaning of the operation is not to be located in the constituent symbols or in their ordered presentation; rather, it is to be found in the physical interpretation wrung out of the operation. More to the point, in Tait's mind, certain physical phenomena can be represented by non-commutative operations. This justifies the existence of a symbolical operation that might, otherwise, seem out of place in the more traditional cannon of mathematical rules.

As for the place of quaternions in the grander history of Cartesian geometry, Tait makes clear the former system is superior. He writes,

> It must always be remembered that Cartesian methods are mere particular cases of Quaternions, where most of the distinctive features have disappeared; and that when, in the treatment of any particular question, scalars have to be adopted, the Quaternion solution becomes identical with the Cartesian one. Nothing therefore is ever lost, though much is generally gained, by employing Quaternions in preference to ordinary methods. In fact, even when Quaternions degrade to scalars, they give the solution of the most general statement of the problem they are applied to, quite independent of any limitations as to choice of particular coordinate axes (Tait 1867, viii-ix).

Tait adds that in

> seeking to supply a real want (the deficiency of subjects of examination for mathematical honours, and the consequent frequent introduction of the wildest extravagance in the shape of data for [Tripos] "Problems"), [there] is [a] danger of making too much of such elegant trifles as Trilinear Cöordinates, while gigantic systems like Invariants (which, by the way, are as easily introduced into Quaternions as into Cartesian methods) are quite beyond the amount of mathematics which even the best students can master in three years' reading. One grand step to the supply of this want is, of course, the introduction into the scheme of examination of such branches of mathematical physics as the Theories of Heat and Electricity. But it appears to me that the study of a mathematical method like Quaternions, which, while of immense power and comprehensiveness, is of extraordinary simplicity, and yet requires constant thought in its applications, would also be of great benefit. With it there can be no "shut your eyes, and write down your equations," for mere mechanical dexterity of analysis is certain to lead at once to error on account of the novelty of the processes employed (Tait 1867, ix).

And although quaternions are "novel," they are rooted in a long-running and venerable tradition related to $\sqrt{-1}$.

In Chapter I of the *Elementary Treatise*, Tait introduces the issue of the meaning of $\sqrt{-1}$. Still ambiguous and still considered by some mathematicians to be unjustified, Tait argues it is important to provide a history of that interesting symbol, and its related functions, so as to demonstrate the undeniable use of extending the imaginary system. Tait writes,

> For more than a century and a half the geometrical representation of the negative and imaginary algebraic quantities, -1 and $\sqrt{-1}$, or, as some prefer to write them, $-$ and $-^{1/2}$, has been a favourite subject of speculation with mathematicians. The essence of almost all of the proposed processes consists in employing such quantities to indicate the *direction*, not the *length,* of lines (Tait 1867, 1).

It had become routine, by the 17th-century, to support the notion that if "positive quantities were measured off in one direction along a fixed line, a useful and lawful convention enabled us to express negative quantities by simply laying them off on the same line in the opposite direction." This is the essence of the "Cartesian method" as it is employed in "Analytical Geometry and Applied Mathematics." By the end of the 17th-century, Wallis had developed the Cartesian argument further, adding that one could "represent the impossible roots of a quadratic equation by going *out of* the line on which, if real, they would have laid off." This is equivalent to considering $\sqrt{-1}$ to be a "directed unit-line perpendicular to that on which real quantities are measured" (Tait 1867, 1-2).

Over the course of the following 24 pages, Tait narrates an uncontested history in which quaternions emerge out of two centuries of devoted mathematical work aimed at developing Wallis's claim. In many ways, Tait builds upon Hamilton's similar claims, as issued in the latter's preface to the *Lectures* (1853). Firstly, he accounts for how it is that in a plane of two dimensions (where rectangular axes are used), each unit of length along Oy, can be considered to be $+\sqrt{-1}$. Conversely, on Oy', each unit length can be $-\sqrt{-1}$. Meanwhile, Ox becomes a unit of $+1$ and Ox' becomes -1. In circular order, the four lines of unit length stated in "positive rotation" (i.e. counter-clockwise) are $1,\ \sqrt{-1},\ -1,\ -\sqrt{-1}$. Secondly, Tait notes that in this series, each term is determined by the multiplication the preceding term and $\sqrt{-1}$. Therefore, $\sqrt{-1}$ can also be viewed as an "operator"—something that acts upon a quantity or position to produce another quantity or position. This operator is "analogous to a handle perpendicular to the plane of xy, whose effect on any line is to make it rotate (positively) about the origin through an angle of 90°" (Tait 1867, 2). In this system, a point is defined by a single expression—an "imaginary expression" such as $a + b\sqrt{-1}$. This expression can be considered a single quantity, which denotes the point whose coordinates are a and b. Alternatively, it can be an expression for the line that joins the point with the origin. In the latter sense, the expression $a + b\sqrt{-1}$ contains a measure of "direction" and "length" for the line in question, where the line is inclined at an angle $\tan^{-1}\frac{b}{a}$ to the axis x (a runs along the x-axis, while the real quantity b runs along the y-axis).[89]

In his historical narration, Tait contends De Moivre (whose name is now linked now to "De Moivre's Theorem") led "us still farther in the same direction." Rather than using $\sqrt{-1}$, De Moivre employed

[89] If one were to use $\sqrt{-1}$ to operate on this symbol, the resulting outcome would be $-b + a\sqrt{-1}$, which denotes the point whose x and y coordinates are $-b$ and a; it also denotes the line that joins that point with the origin. The length of such a line would still be $\sqrt{a^2 + b^2}$, but the angle between the line and the x –axis would now be $\tan^{-1} -\frac{a}{b}$, which is 90° greater than before (Tait 1867, 2).

the more general $\cos a + \sqrt{-1}\sin a$, which has the effect of turning any line through a positive angle a in the plane of x, y. Using this approach, the operator $\sqrt{-1}$ is considered to be a special case of the more general operator above (when $a = \frac{\pi}{2}$). Algebraic multiplication can demonstrate:

$$(\cos a + \sqrt{-1\sin a})\left(a + b\sqrt{-1}\right) = a\cos a - b\sin a + \sqrt{-1}(a\sin a + b\cos a).$$

This identity contains within it a number of useful interpretations, Tait explains. "The reader will at once see that the new form indicates that a rotation through an angle a has taken place, if he compares it with the common formulae for turning the coordinate axes through a given angle," he writes. In symbolical terms:

$$\text{Length} = \sqrt{(a\,cos\,a - b\,sin\,a)^2 + (a\,sin\,a + b\,cos\,a)^2}$$

$$= \sqrt{a^2 + b^2}\ \text{as before.}$$

$$\text{Inclination to axis of x} = \tan^{-1}\frac{a\sin a + b\cos a}{a\cos a - b\sin a} = \tan^{-1}\frac{\tan a + \dfrac{b}{a}}{1 - \dfrac{b}{a}\tan a}$$

$$= a + \tan^{-1}\frac{b}{a}.$$

The student can thus anticipate the structure of quaternions by recognizing a quaternion can be represented as $N(\cos\theta + \varpi\sin\theta)$, where N is a numerical quantity, θ a real angle, and $\varpi = -1$. The difference between this expression for a quaternion and De Moivre's theorem is that ϖ is not the equivalent of the "algebraic $\sqrt{-1}$, but may be *any directed unit-line* whatever in space"—this being the "chief invention" Hamilton had introduced to mathematics in the previous decade.

In this narrative extending from Wallis to De Moivre to Hamilton, other intervening figures appear. In the 19th-century alone,

> Argand, Warren, and others, extended the results of Wallis and De Moivre. They attempted to express as a line the product of two lines each represented by a symbol such as $a + b\sqrt{-1}$. To a certain extent they succeeded, but simplicity was not gained by

> their methods, as the terrible array of radicals in Warren's Treatise sufficiently proves (Tait 1867, 3).[90]

Tait maintains,

> Beyond this, few attempts were made, or at least recorded, in earlier times, to extend the principle to space of three dimensions; and, though many such have been made within the last forty years, none, with the single exception of Hamilton's, have resulted in simple, practical methods; all, however ingenious, seeming to lead at once to processes and results of fearful complexity ... It was reserved for Hamilton to discover the use of $\sqrt{-1}$ as a *geometric reality*, tied down to no particular direction in space, and this use was the foundation of the singularly elegant, yet enormously powerful, Calculus of quaternions (Tait 1867, 4-5).

Quaternions were no longer mere symbols; they were no longer preferable simply for their seeming elegance and simplicity. They were to be admired, also, for their existence in "geometrical reality". Tait explains,

> While all other schemes for using $\sqrt{-1}$ to indicate direction make one direction in space expressible by real numbers, the remainder being imaginaries of some kind, leading in general to equations which are heterogeneous; Hamilton makes all directions in space equally imaginary, or rather equally real, thereby ensuring to his Calculus the power of dealing with space indifferently in all directions (Tait 1867, 5).

Quaternions are entirely independent of any particular axes or "any supposed directions in space"; the calculus of quaternions takes its reference lines "solely from the problem it is applied to" (Tait 1867, 5). Tait has combined the "reality" of geometry with the "reality" of imaginary numbers to argue quaternions are exceptionally powerful as operators in a new domain of symbolical algebra that also happens, at times, to have geometrical analogues.

Tait elaborates upon this relativism by drawing a comparison to Cartesian geometry in three dimensions, which he says is simply a "mere particular case of Quaternions in which most of the distinctive features are lost." Though quaternions can be generated *ab initio*, with no reference at all

[90] Tait notes a "curious speculation" accredited to Servois, published in 1813 in Gergonne's *Annales*, is the "only one [of the intervening mathematical attempts], so far as has been discovered, in which the slightest trace of an anticipation of Quaternions is contained." In that attempt, Servois "endeavoured to extend to *space* the form $a + b\sqrt{-1}$ for the plane" and he is "guided by analogy to write for a directed unit-line in space the form $p\cos\alpha + q\cos\beta + r\cos\gamma$, where α, β, γ are its inclinations to the three axes." Servois, however, rejected the idea that p, q, r could be reduced to the form $a + b\sqrt{-1}$; Tait argued Servois was wrong in so thinking. Hamilton did reduce them by invoking i, j, k in quaternion calculus (Tait 1867, 4).

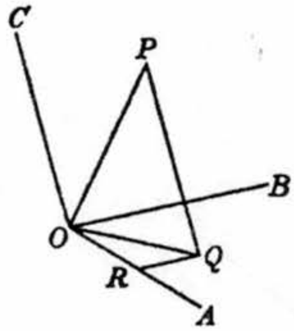
to Cartesian techniques, Tait draws on the insights inherent to Cartesian geometry so as to avoid confusion for those students reading the book and for whom the text is primarily directed (Tait 1867, 5). The first lesson is that, if we have two points, A and B, to determine the location of B to A (whether we use Cartesian or polar coordinates) we must determine both the distance between the points and the direction in which that distance is to be travelled. A "vector" is that particular line that takes us from point A to point B (it travels from one point to the other in a definite direction). "It may thus be considered as an instrument which *carries* A to B; so that a vector may be employed to indicate a definite *translation* in space," he explains (Tait 1867, 6).

Despite Tait's earlier insistence upon "geometrical reality," his pre-established views on the malleability and conventionality of symbolical thinking prevail and "geometrical reality" is interpreted upon that terrain. Tait reminds his students,

> We may here remark, once for all, that in establishing a new Calculus, we are at liberty to give any definitions whatever of our symbols, provided that no two of these interfere with, or contradict, each other, and in doing so in Quaternions *simplicity* and (so to speak) *naturalness* were the inventor's aim (Tait 1867, 6).

For Tait, quaternions can constitute a geometric reality, although that geometry is entirely constructed and symbolically generated. Thus, Tait proceeds to define a vector as having a specific length and direction. If $\overline{AB}$ is represented by a, then we require three numbers to specify the location of a in space—either three coordinate numbers relative to a specific set of axes, or two coordinates relative to axes and a directional unit. When two vectors, a and b, are the same length and parallel to one another, they are considered to be equivalent vectors in space. If the second vector, $\overline{CD}$, is parallel and of equal magnitude to $\overline{AB}$, then we can state $\overline{CD} = \overline{AB} = a$. This is an extension of the "meaning of an algebraic symbol," Tait writes, given that we have now determined that an equation between two vectors, namely $\alpha = \beta$, where β refers to the second vector, always means the two vectors are the same in length and in direction (though not necessarily located at the same place).

Tait then defines + and - symbols in the new "Calculus" by noting that both of these symbols indicate a translation. If $A, B,$ and C are any three points, such that $\overline{AB} = \alpha$, $\overline{BC} = \beta$, $\overline{AC} = \gamma$, then we can state $\alpha + \beta = \gamma$, or $\overline{AB} + \overline{BC} = \overline{AC}$. This is simply to say that vectors are to be "compounded" in both magnitude and direction "like simultaneous velocities." This process is similar to the addition of coordinates in Cartesian algebra, where the resultant set of coordinates is the outcome of combining two constituent coordinate pairs, where the symbol $(-)$ simply means that the direction of the vector has been reversed. In any vector triangle, we find that $\overline{AB} + \overline{BC} + \overline{CA} = 0$. This result is nothing other than the "well-known propositions regarding composition of velocities, which, by [Newton's] second law of motion, gives us the geometrical laws of composition of forces" (Tait 1867, 8). Any vector can thus be resolved into three components that are parallel, respectively, to any three given vectors.



(Tait 1867, 9)

In Tait's text, the multiplication and division of vectors is dealt with in purely symbolical terms, with images added only for the edification of students. He states that if we "compound any number of *parallel* vectors, the result is a numerical multiple of any one of them." In other words, if A, B, C are in a straight line, then $\overline{BC} = x\overline{AB}$. In this equation, x is a ratio by which we can compare the length of $\overline{BC}$ to that of $\overline{AB}$. It is positive when B lies between A and C; it is negative otherwise. The ratio between the two vectors is not only one of magnitude, but also one of direction—in other words, it is a quaternion.

Tait then introduces a claim of particular advantage in quaternion calculus—namely, the notion of three "mutually perpendicular unit-vectors." Those unit-vectors are what Hamilton defined as i, j, k. It is possible, Tait explains, to express any vector as a composition of multiples of these three unit vectors. Any vector in (three-dimensional) space can be expressed as $\rho = xi + yj + zk$. In this set up, x, y, z represent lengths (magnitudes) of coterminous edges in a rectangular parallelepiped, where ρ is the vector-diagonal, such that the length of that vector is $\sqrt{x^2 + y^2 + z^2}$. Tait demonstrates what it would mean for two such vectors to be equivalent. If ϖ is defined to be any vector, then $\varpi = \xi i + \eta j + \zeta k$. If we then state that $\rho = \varpi$, it would mean that the following three equivalences must hold among the constituent parts of the respectively equivalent vectors:

$$x = \xi, y = \eta, z = \zeta.$$

In words, this means that if,

> [We] suppose i to be drawn eastwards, j northwards, and k upwards, this is equivalent merely to saying that if two points coincide, they are equally to the east (or west) or any third point, equally to the north (or south) of it, and equally elevated above (or depressed below) its level (Tait 1867, 10).

As for the algebraic rules governing vector combinations, Tait explains that the commutative and associative laws hold when we combine vectors using both the $+$ and $-$ symbols (or operators). It does not matter in what order vectors are added or subtracted, the result will always be the same. If A, B, C, D are the corners of a parallelogram, then,

$$\overline{AB} = \overline{DC},$$

$$\overline{AD} = \overline{BC},$$

and,

$$\overline{AB} + \overline{BC} = \overline{AC} = \overline{AD} + \overline{DC} = \overline{BC} + \overline{AB}.$$

In the same parallelogram defined above, we find that:

$$\overline{AD} = \overline{AB} + \overline{BD} = \overline{AC} + \overline{CD},$$

or,

$$\overline{AB} + \overline{BC} + \overline{CD} = \overline{AB} + \left(\overline{BC} + \overline{CD}\right) = \left(\overline{AB} + \overline{BC}\right) + \overline{CD}.$$

In other words, both the associative and the commutative rules of regular algebra hold in vector algebra when acts of addition or subtraction are performed.

Such operations can, however, be symbolized more concisely, using quaternion notation. Tait explains that given the equation, $\rho = x\beta$, where ρ is the vector that connects a variable point with a given origin, β is a definite vector, and x is an indefinite number that describes a "straight line drawn from the original parallel to β," the straight line from A, where $\overline{OA} = a$, parallel to β, has the equation $\rho = a + x\beta$. This means "we may pass from O to P directly, by the vector $\overline{OP}$ or ρ; or we may pass first to A, by means of $\overline{OA}$ or a, and then to P along a vector parallel to β" (Tait 1867, 11). The equation $\rho = a + x\beta$, is of utmost importance in quaternion mathematics, as it "enable(s) us to throw the general equation of a straight line in space." It describes a directed line in space, and if more indefinite quantities are added, such that $\rho = ya + x\beta$, we get an expression for a plane surface, by virtue of the fact the equation describes the plane in which the lines a and β both lie. To extend the matter further, we can stipulate that $\rho = \gamma + ya + x\beta$, which represents a three-dimensional space, as it describes a plane that passes through the "extremity of γ, and [is] parallel to a and β" (Tait 1867, 12).

Using the quaternion tools presented so far, Tait proceeds in Section 31 to "prove" a series of well known Cartesian theorems in geometry using quaternion notation (e.g. he demonstrates, in vector form, "The bisectors of the sides of a triangle meet in a point, which trisects each of them") (Tait

1867, 14). In Section 32, he explains how to differentiate a vector with reference to a single numerical variable, of which the vector is given as an explicit function. He writes,

> This process is of very great use, especially in quaternion investigations connected with the motion of a particle or point [as] it will afford us an opportunity of making a preliminary step towards overcoming the novel difficulties which arise in quaternion differentiation (Tait 1867, 22).

And in one of his characteristic revivifications of Newtonian imagery (i.e. characteristic of his 1860s writing in thermodynamics), Tait makes the claim that, in inventing these innovative quaternion tools, Hamilton had revivified Newton's "original methods" in differential calculus. Tait writes,

> It is a striking circumstance, when we consider the way in which Newton's original methods in the Differential Calculus have been decried, to find that Hamilton was *obliged* to employ them, and not the more modern forms, in order to overcome the characteristic difficulties of quaternion differentiation. Such a thing as a *differential coefficient has absolutely no meaning in quaternions,* except in those special cases in which we are dealing with degraded quaternions, such as numbers, Cartesian coordinates, etc. But a quaternion expression has always a *differential*, which is, simply, what Newton called a *fluxion*.

Revealing his continued efforts to idolize Newton, Tait further claims,

> As with the Laws of Motion, the basis of Dynamics, so with the foundations of the Differential Calculus; we are gradually coming to the conclusion that Newton's system is the best of all (Tait 1867, 23).[91]

These claims are especially interesting given that Tait's mathematical work is not "Newtonian" in so far as Newton relied upon induction from geometrical understanding.

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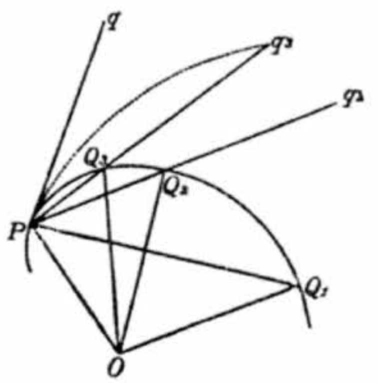
[91] In his obituary of Hamilton, published a year earlier, Tait discusses the revivification of Newton due to quaternion differentiation. He writes of Hamilton's work that "among the many curious results of the invention of quaternions, must be noticed the revival of *fluxions*, or, at all events, a mode of treating differentials closely allied to that originally introduced by Newton. The really useful, but over-praised differential coefficients, have, as a rule, no meaning in quaternions; so that, except when dealing with scalar variables (which are simply degraded quaternions), we *must* employ in differentiation fluxions or differentials. And the reader may easily understand the cause of this. It lies in the fact that quaternion multiplication is *not commutative*; so that, in differentiating a product, for instance, each factor must be differentiated where it stands; and thus the differential of such a product is not generally a mere algebraic multiple of the differential of the independent quaternion-variable. It is thus the whirligig of time brings its revenges. The shameless theft which Leibniz committed, and which he sought to disguise by altering the appearance of the stolen goods, must soon be obvious, even to his warmest partisans. They can no longer pretend to regard Leibniz as even a second inventor when they find that his only possible claim, that of devising an improvement in notation, merely unfits Newton's method of fluxions for application to the simple and symmetrical, yet massive, space-geometry of Hamilton" (Tait 1866).

In lauding the "Newtonian method" and Newton's "fluxions," Tait was not directly revivifying Newton's 17^{th}-century concepts of ultimate ratios and geometrical limits. Rather, what Tait had in mind was Cauchy's introduction of the 19^{th}-century notion of a "limit". His revivification of "Newton" was a socio-political move meant to represent quaternions as venerable British productions, just as Hamilton had sought to present them as venerable Irish productions in earlier years. Though European mathematicians were mentioned in Tait's historical rendering as discussed above, nowhere are the highly symbolical concepts of quaternions associated with European mathematical traditions in analysis. Consider, for example, Tait's account of the following problem. If we suppose ρ is the vector of a curve in space, then ρ can be expressed as "the sum of a number of terms, each of which is a multiple of a given vector by a function of some *one* indeterminate." In symbolical terms, if P is a point on the curve, then $\overline{OP} = \rho = \phi(t)$. This means that the vector ρ can be expressed as a function of some variable t. Furthermore, if Q is any other point on the curve, then, $\overline{OQ} = \rho = \phi(t_1) = \phi(t + \delta t)$, where δt is "any number whatever" (Tait 1867, 23). This symbolism and vocabulary clearly indicates Tait's inculcation into the post-Cauchian tradition of treating limits symbolically. Consider also the fact that Tait employs a curve as a function of a variable t (here "time"), and he describes the determination of the velocity of the point at P as it moves incrementally along the curve over time. This approach relies upon the notion of an infinitesimal time progression inputted into a function. The invocation of infinitesimal increments symbolized by δ is undeniable. Yet, in Section 35, Tait explicitly labels this approach "Newton's Method".



(Tait 1867, 24)

Tait's revivification of the Newtonian method should be read as a legitimization effort—a valiant attempt to render wholly "British" the quaternion-symbolical technique, and to link to the venerable institution of Newtonianism to his fledgling project.

Tait recognizes also that there is unforeseen novelty in the method. Herein lays his attempt to generate respect for his own, and Hamilton's, genius at having discovered and developed the technique. In Chapter II, Tait explains the operations of multiplication and division of vectors. It is in understanding these operations that we

> Come to the consideration of points in which the Calculus of Quaternions differs entirely from any previous mathematical method; and here we shall get an idea of what a Quaternion is, and whence it derives its name (Tait 1867, 32).

If the given vectors are parallel to each other, Tait writes, then it is not difficult to determine the ratio between them, which will be a purely numerical ratio that refers to the difference in their lengths. The ratio will be positive if the vectors point in the same direction and negative if they point in the opposite direction.

The issue becomes slightly more complex if the vectors are not parallel. When that occurs, then the ratio between the two directed lines involves both magnitude (a length ratio) and direction (a rotation ratio). If we let $\overline{OA}$ and $\overline{OB}$ represent the two vectors in question, and if we draw them from the same common origin (O), we can reduce the question to finding the ratio between two vectors that share a common starting point. To determine the ratio between them, we want to know what must be done to $\overline{OA}$ to transform it into $\overline{OB}$. The following processes exist for doing so:

> 1st. Increase or diminish the length of $\overline{OA}$ till it become equal to that of $\overline{OB}$. For this only *one* number is required, viz. the ratio of the lengths of the two vectors. As Hamilton remarks, this is a positive, or rather a *signless*, number.
>
> 2nd. Turn $\overline{OA}$ about O until its direction coincides with that of $\overline{OB}$, and (remembering the effect of the first operation) we see that the two vectors now coincide or become identical. To specify this operation *three* more numbers are required, viz. *two* angles (such as node and inclination in the case of a planet's orbit) to fix the plane in which the rotation takes place, and *one* angle for the amount of this rotation (Tait 1867, 32-33).

The "ratio of two vectors, or the multiplier required to change one vector into another," depends upon *four* distinct numbers—hence the nomenclature of "Quaternion" (Tait 1867, 33). The quaternion operator instigates both a change in magnitude and a change in direction in space. The quaternion can, therefore, be decomposed into a "*stretching* factor," which performs a change in magnitude, and which is called a "Tensor" (denoted by T), and a "*turning* factor," which performs a change in direction and which is called a "Versor" (denoted by U).

As an example, consider the vectors $\overline{OA} = \alpha, \overline{OB} = \beta$. If q is the quaternion that changes α into β we find that $\beta = q\alpha$, which can also be written in the forms $\frac{\beta}{\alpha} = q, \text{or}\, \beta\alpha^{-1} = q$. Those algebraic identities only hold, Tait says, "if we agree" upon the following equivalences: $\frac{\beta}{\alpha}.\alpha = \beta\alpha^{-1}.\alpha = \beta$. In so far as those equivalences are agreed upon, the system works to transform one vector into any other, so long as both vectors are placed at the same starting point. In Hamilton and Tait's notation, the transforming operator, or quaternion, can be represented as $q = TqUq = UqTq$, which indicates that every quaternion is composed of a "tensor," a magnitude ratio between two vectors, and a "versor," a directional ratio between two vectors.[92]

Tait's account of why quaternions are not commutative in multiplication (or division for that matter) is worth elaborating upon as it is this characteristic that eventually renders quaternions unpalatable to critics such as Thomson. To explain the commutative principle in quaternion mathematics, Tait considers first a simple quaternion—one in which the tensor has a unit value of 1, and which can therefore be represented by its versor part alone. When this is the case, a versor can be represented as the arc of a great circle on a unit sphere. In such circumstances, it is easy to prove that "quaternion multiplication is not generally commutative." Note that Tait is not studying the behaviour of directed lines of a unit-circle in order to "prove" the truth of a mathematical principle.

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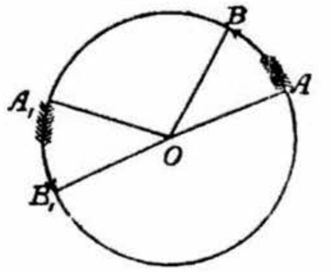
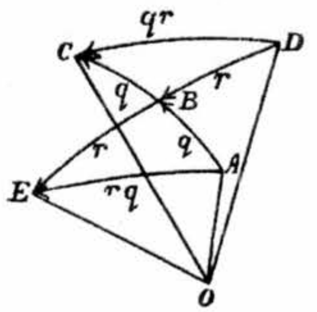
[92] If a_1 and B_1 are vectors of unit length parallel to α and β, respectively, then $T\frac{\beta_1}{a_1} = 1, U\frac{B_1}{a_1} = U\frac{\beta}{\alpha}$.

Rather, Tait's "proof", i.e. the "great circle", is motivated by pedagogical concerns. The ultimate proof of non-commutativity in quaternion mathematics is not located in the behaviour of its geometric analogue; it is located in the definition of equivalences set within the rules of quaternion mathematics. The non-commutative aspect of quaternions can, however, be geometrically demonstrated by using great circles on a sphere as a means of enlightening debutant quaternionists. Tait explains the matter as follows:

> Let q be the versor $\widehat{AB}$ or $\frac{\overline{OB}}{\overline{OA}}$. Make $\widehat{BC} = \widehat{AB}$, then q may also be represented by $\frac{\overline{OC}}{\overline{OB}}$. In the same way any other versor r may be represented by $\widehat{DB}$ or $\widehat{BE}$ and by $\frac{\overline{OB}}{\overline{OD}}$ or $\frac{\overline{OE}}{\overline{OB}}$. The line OB in the figure is definite, and is given by the intersection of the planes of the two versors; O being the centre of the unit-sphere (Tait 1867, 36).





(Tait 1867, 36)

Tait concludes, $r\overline{OD} = \overline{OB}$, and $q\overline{OB} = \overline{OC}$. In sum, $qr\overline{OD} = \overline{OC}$, or alternatively $qr = \frac{\overline{OC}}{\overline{OD}}$. This latter identity can be represented by the arc $\widehat{DC}$ of a great circle. Meanwhile, rq is "easily seen to be represented by the arc $\widehat{AE}$. In sum, Tait demonstrates $q\overline{OA} = \overline{OB}$, and $r\overline{OB} = \overline{OE}$, whence $rq\overline{OA} = \overline{OE}$, and $rq = \frac{\overline{OE}}{\overline{OA}}$. The versors rq and qr can be represented by arcs of equal length, and they are, therefore, "unequal."[93]

Based on this geometrical demonstration, it is clear that versor multiplication does not commute. The propositions developed for simple versors hold also for quaternions, given that the former are a basic form of the latter (Tait 1867, 37). Using a similar geometrical technique for heuristic purposes, Tait demonstrates the Associative Law of algebra *does* hold for quaternions. In other words, $p.qr = pq.r$. He also uses geometrical devices to demonstrate that the Distributive Law holds in quaternion multiplication and division; so too does the Index Law, such that $q^m \cdot q^n = q^{m+n}$, so long as m and n are positive. Having established these various rules of the game, Tait argues the student is now prepared to consider the special rules of quadrantal versors, which give rise to all of the properties of quaternions.

The properties of the quadrantal versors are those that Hamilton "discovered" in 1843, Tait recalls, adding they "led almost intuitively to the establishment of the Quaternion Calculus." Tait prefaces his account by stating,

> We shall content ourselves at present with an assumption, which will be shewn to lead to consistent results; but at the end of the chapter we shall shew that no other assumption is possible, following for this purpose a very curious quasi-metaphysical speculation of Hamilton's (Tait 1867, 43).

That assumption goes as follows: suppose we have a system, of three mutually perpendicular unit-vectors, all drawn from one point, which we will call I, J, K; suppose also that these are constructed so that a positive (or left-handed) rotation through a right angle about I as an axis brings J to

[93] They are generally unequal unless the planes of q and r coincide.

coincide with K. A positive quadrantal rotation about J will make K coincide with I; when that rotation is about K, it will make I coincide with J. If we then assume that I is drawn eastwards, J northwards, and K upwards, then a positive (left-handed) rotation about the eastward line of I brings the northward line J into a vertically upward position of K, and so on. The operator that turns J into K is a "quadrantal versor."

The axis of that versor is I, or i, as Hamilton termed it. Thus, we can establish the following algebraical rules of operation which govern the behaviour of these operators:

$$\frac{K}{J} = i, \text{or } K = iJ,$$

$$\frac{I}{K} = j, \text{or } I = jK,$$

$$\frac{J}{I} = k, \text{or } J = kI.$$

With this understanding in hand, the following algebraic equivalence can be identified:

$$\frac{-J}{K} = \frac{K}{J},$$

which means that "a southward unit-vector bears the same ratio to an upward unit-vector that the latter does to a northward one." Therefore:

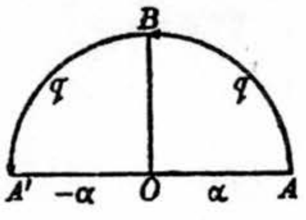
$$\frac{-J}{K} = i, \text{or } -J = ik,$$

$$\frac{-K}{I} = j, \text{or } -K = jI,$$

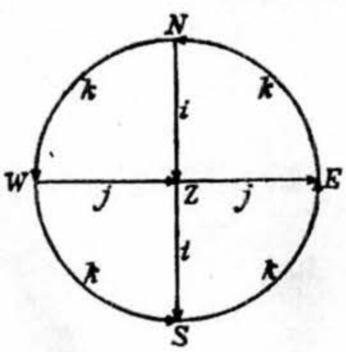
$$\frac{-I}{J} = k, \text{or } -I = kJ.$$

By combining the identities $K = ij$ and $-J = ik$, we then find that $-J = ik = i(iJ) = i^2 J$; therefore, $i^2 = -1, j^2 = -1,$ and $k^2 = -1$. In sum, Tait explains, the square of every quadrantal versor is negative unity.

Tait also provides a heuristic "proof" for this claim in geometrical form per the image below:



(Tait 1867, 45)



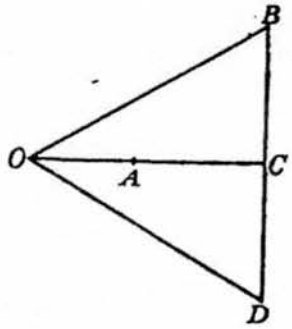
(Tait 1867, 46)

For Tait, this geometrical understanding comes *after* the establishment of, and agreement upon, the symbolical equivalences stated earlier. Although Tait provides an image to exemplify what is meant by the related identities, $ji = -k,\ kj = -i,\ ik = -j$, he maintains that quaternions are an algebraical system governed by algebraical rules. As Tait concludes,

> These equations [the three mentioned earlier], along with $i^2 = j^2 = k^2 = -1$, contain essentially the whole of Quaternions. But it is easy to see that, for the first group, we may substitute the single equation $ijk = -1$, since from it, by the help of the values of the squares of i, j, k, all the other expressions may be deduced. We may consider it proved in this way (Tait 1867, 47).

In so far as the algebraic rules governing quaternions are accepted as legitimate symbolical equivalences, quaternion operators can be used as devices in physical and dynamical research.

The last consideration Tait makes in his account of quaternions is the alternative manner of representing quaternions themselves. So far, Tait says, we have considered the quaternion to be a product of a tensor (a magnitude ratio between two vectors) and a versor (a direction ratio between two vectors). There is, however, a second way to construct quaternions—namely, as a *sum* of parts. To represent a quaternion as a sum of parts, the mathematician can let $\frac{\overline{OB}}{\overline{OA}}$ represent any quaternion (a ratio between two vectors). If one draws BC perpendicular to OA, then $\overline{OB} = \overline{OC} + \overline{CB}$.



(Tait 1867, 49)

We see, therefore, that $\overline{OC} = x\overline{OA}$, where x is a number, whose sign is the same as that of the cosine of $\angle AOB$. Since CB is perpendicular to $OA, \overline{CB} = \gamma\overline{OA}$, where γ is a vector perpendicular to OA and CB (i.e. it is perpendicular to the plane of the quaternion). Hence, we find that:

$$\frac{\overline{OB}}{\overline{OA}} = \frac{x\overline{OA} + \gamma\overline{OA}}{\overline{OA}} = x + \gamma.$$

In other words, a quaternion can be decomposed into two parts, where one part is numerical (x), and the other part is vectorial (γ). Hamilton termed these parts a "scalar" and a "vector," respectively, and he denoted them by the symbols S and V, which were then prefixed to the quaternion expression. Using this specific notation, one can express a quaternion as $q = Sq + Vq$ (Tait 1867, 49).

Using Cartesian coordinates, however, one can also demonstrate that any vector can be represented also as $xi + yj + zk$, where x, y, z are numbers (i.e. "scalars," in Hamilton's terminology), and i, j, k are three non-coplanar vectors, represented in a rectangular system of unit-vectors. If any scalar is denoted by the symbol w, then the quaternion q can be expressed as $q = w + xi + yj + zk$. This version of the quaternion most clearly expresses the "essential dependence on four distinct numbers, from which the quaternion derives its name," Tait writes (Tait 1867, 51). This latter Cartesian form most clearly indicates why the "quaternion" is, in fact, called a *qua*ternion. But, Tait notes, it is a cumbersome means of representing the quaternion. Hamilton's use of S and V summarizes quaternion properties more briefly, and therefore it is Tait's preferred presentation.

Consider, for example, the multiplication of two quaternions, $\alpha\beta$. One finds that in Cartesian coordinate form, the mathematician must write the following to express the multiplication:

$$\alpha\beta = (xi + yj + zk)(x^{'}i + y^{'}j + z^{'}k)$$

$$= -(xx^{'} + yy^{'} + zz^{'}) + (yz^{'} - zy^{'})i + (zx^{'} - xz^{'})j + 9xy^{'} - yx')k.$$

The mathematician can then determine:

$$\beta\alpha = -(xx^{'} + yy^{'} + zz^{'}) - (yz^{'} - zy^{'})i - (zx^{'}xz^{'})j - (xy^{'} - yz^{'})k.$$

Therefore, $\alpha\beta \neq \beta\alpha$. Although the scalar part of this quaternion multiplication is identical, the signs of the respective vectors are not. Rather than relying on reverting to such cumbersome expansions, however, Tait invokes Hamilton's S and V notation to offer the same account, but more succinctly. In the multiplication of two quaternions, the mathematician can simply state (Tait 1867, 55),

$S\alpha\beta = S\beta\alpha$, meaning the scalar parts remain equal;

$$V\alpha\beta = -V\beta\alpha, \text{meaning the vector part changes sign.}$$

Throughout the remainder of the chapter, Tait offers various quaternion accounts of problems in plane geometry, trigonometry, and spherical geometry. He reiterates his belief that the students will not fail to notice in such examples the simplicity and succinctness of the technique over its Cartesian equivalent.[94] In Chapter IV, for instance, Tait delves into the differentiation of quaternions, which he says is akin to ordinary differentiation, such that, if $r = F(q)$ is a function of a quaternion, q, then:

$$dr = dFq = \int_{\infty} n \left\{F\left(q + \frac{dq}{n}\right) - F(q)\right\}.$$

Tait demonstrates the differentiation of any function of a quaternion, q, leads to an equation of the form:

$$dr = f(q, dq),$$

where f is linear and homogeneous in dq. Tait also describes the technique for determining an unknown quaternion in a linear function, although he acknowledges an obstacle here. He writes,

> No general method of solving quaternion equations of the second or higher degrees has yet been found; in fact, as will be shown immediately, even those of the second degree involve (in their most general form) algebraic equations of the *sixteenth* degree (Tait 1867, 125-126).

Over the course of the remainder of the book, Tait provides various examples in "geometry of the straight line and plane," "the sphere and cyclic cone," "surfaces of the second order," "geometry of curves and surfaces" (in which he introduces Hamilton's vector function), and finally "kinematics" (in which he demonstrates solutions in vector-velocity, acceleration and strain). It is in this latter

[94] Tait also defines Hamilton's "biquaternion", which is a quaternion that includes, as its scalar value, the "normal" algebraic imaginary number $\sqrt{-1}$. In this instance, the imaginary number is not a versor but a "number" in the same sense that a real number is a magnitude in ordinary quaternions (Tait 1867, 81-82).

chapter on kinematics that Tait reissues some of his earlier published work, as well as some problems discussed by Thomson and himself in dynamics. Tait demonstrates, for instance, that if a surface is equipotential or isothermal, then "$-\nu$ represents in direction and magnitude the force at any point or the flux in heat," where $\nu = \nabla F\rho$ (and where $F\rho = C$ is the equation that represents one surface in a system of surfaces). This problem demonstrates the effect of the vector operator, ∇, on any "scalar function of the vector of a point," which is to produce "*the vector which represents in magnitude and direction the most rapid change in the value of the function.*" The effect of ∇ on a "*vector function,*" such as $\sigma = i\xi + j\eta + k\zeta$, is as follows:

$$\nabla\sigma = -\left(\frac{d\xi}{dx} + \frac{d\eta}{dy} + \frac{d\zeta}{dz}\right) - i\left(\frac{d\eta}{dz} - \frac{d\zeta}{dy}\right) - \&c.$$

This "semi-Cartesian" form allows the mathematician to conclude,

> if σ represents a small vector displacement of a point situated at the extremity of the vector ρ (drawn from the origin), $S\nabla\sigma$ represents the consequent cubical compression of the group of points in the vicinity of that considered, and $V\nabla\sigma$ represents twice the vector axis of rotation of the same group of points. Similarly,
>
> $$S\sigma\nabla = -\left(\xi\frac{d}{dx} + \eta\frac{d}{dy} + \zeta\frac{d}{dz}\right) = -D_{\sigma},$$
>
> or is equivalent to total differentiation in virtue of our having passed from one end to the other of the vector σ (Tait 1867, 268).

This finding closely reflects the work Tait had produced in thermodynamics in the mid-1860s.

In fact, Tait's "applications" deal mostly with problems in vector dynamics, which Tait would have considered to be most applicable to the developing domain of thermodynamics. And, in the final chapter on "Physical Applications", the natural philosopher concludes,

> by giving a few instances of the ready applicability of quaternions to questions of mathematical physics, upon which, even more than on the Geometrical and Kinematical applications, the real usefulness of the Calculus must mainly depend—except, of course, in the eyes of that section of mathematicians for whom Transversals and Anharmonic Pencils, &c. have a [sic] to us incomprehensible charm. Of course we cannot attempt to give examples in all branches of physics, nor even to carry very far our investigations in

> any one branch: this Chapter is not intended to teach Physics, but merely to show by a few examples how expressly and naturally quaternions seem to be fitted for attacking the problems it presents (Tait 1867, 277).

The first problem Tait discusses in the chapter is the question of how to account for the forces that act on a rigid body, in particular when a force, β, at the point whose vector is a, causes a slight displacement of the body in question. In such instances, the point a becomes $a + \delta a$, and the whole "work done" is $\sum S\beta\delta\alpha$ (which vanishes as the "forces are such as to maintain equilibrium") (Tait 1867, 276). Other problems that appear in the chapter include, not surprisingly, Fresnel's wave surface and Ampère's theory of electrodynamics. But, Tait concludes with a nudge for his colleague and co-author, William Thomson, by arguing,

> It would be easy to give many more of these transformations, which really present no difficulty; but it is sufficient to show the ready applicability to physical questions of one or two of those already obtained; a property of great importance, as extensions of mathematical physics are far more valuable than mere analytical or geometrical theorems ... Thomson has shown that the forces produced by given distributions of matter, electricity, magnetism, or galvanic currents, can be represented at every point by displacements of [an elastic] solid producible by external forces. It may be useful to give his analysis, with some additions, in a quaternion form, to show the insight gained by the simplicity of the present method (Tait 1867, 309).

The general problem of rigid body dynamics that would come to serve as the final theme upon which Tait would conclude his 1860s discussions of quaternion mathematics, served as the thesis in his detailed paper, "On the Rotation of a Rigid Body about a Fixed Point," published in the *Transactions of the Royal Society of Edinburgh*" (1868). It is to this account we turn now, as the final publication in a decade through which Tait had developed a personal vision of quaternion mathematics as contoured by his developing concerns in thermodynamics.

Tait's quaternions, 1868-1870

Tait maintains an adherence to the conventional nature of symbolical algebra throughout the 1860s; the message concluding his 1868 text, however, is one that resonates more profoundly with his developing thermodynamic commitments. Tait writes quaternions are useful in mathematical physics, because they do *"work"* in an efficient manner. In his paper on rigid body rotation, Tait reiterates the notion that quaternions are simple in their structure and thus help to advance scientific inquiry by offering useful tools for the efficiently-minded mathematical experimentalist. Tait contends further that,

> Although it is very improbable that there remains to be discovered any new, and at the same time simple, fact connected with a question which has been elaborately treated by many of the greatest mathematicians of this and the preceding century, the employment of a new mathematical method may enable us to present some their results in a more intelligible form, and with far less expenditure of analytical power than has hitherto been deemed necessary; and it may give us such an insight into the question, that we shall be able to easily discover the mutual relations among the various processes which have been already employed; so far, at least, as these differ in principle, and not merely in the peculiar co-ordinates assumed for the purpose of simplifying the equations. Such a method is that of *Quaternions*, which seems to be expressly fitted for the symmetrical evolution of truths which are usually obtained by the ordinary Cartesian methods only after great labour of calculation, and by modes of attack so indirect, and at first sight so purposeless, as to bewilder all but a very small class of readers. Quaternions afford so clear a view of the nature of the question they are applied to, that even the student, if he have some little knowledge of them, can often see *why* a transformation is made, whose object he would have been unable to discover had the problem been masked in the unnecessarily artificial difficulties of Cartesian geometry, or the outrageously repulsive formulae of spherical trigonometry (Tait 1898, 86).

Tait's hopes were running high. There was the potential profitability of his 1867 textbook to keep in mind should quaternions come to be more widely adopted as examinable mathematical techniques in Edinburgh and, especially, at Cambridge. There was Tait's continuing need to sell textbooks to supplement his student fees and to generate income from Kelland's class (in which students were already learning about quaternions through Kelland's tutelage). Most importantly, Tait still harboured the vision of a future in which his quaternion mathematics would complement his and Thomson's publications in the emergent field of thermodynamics, i.e. the Presbyterian-inspired "science of energy".

With a nod in this direction, Tait's paper on rigid body motion that argues the best existing account of such motion is to be found in Poinsot's work (1851), "Théorie Nouvelle de la Rotation des Corps," published in *Liouville's Journal*. Though Poinsot had used analytical techniques in his work, his research was predominantly geometrical in nature—a fact that nonetheless placed it above the works of others before him, who had relied solely on "analytical methods" to account for motion in space. Tait proposed to go beyond Poinsot's account by having "the reader bear in mind that a *quaternion* equation is quite as suggestively intelligible, to those who understand it, as any geometrical diagram can possibly be" (Tait 1898, 87). Tait's emphasis is on quaternions as geometrical truths. Yet, this is not to say he has abandoned his analytical training; for what Tait means is that quaternions demonstrate geometrical truths better than even classical geometry does. He writes,

> It is *more* readily intelligible than diagrams usually are; for, in reading a work illustrated by figures, we have generally to go through a laborious explanation of what the figure is intended to represent before we can make use of it for further developments. On the other hand, a purely quaternion[ic] formula draws, as it were, its own figure in the reader's mind, and saves him at least the trouble just mentioned. In this way every one has his figures drawn so as best to suit himself, and is not perplexed by having to pick up the principles on which they have been drawn for him by another, very probably of a different mode of thought (Tait 1898, 87).

Tait insists quaternions are not like "ordinary so-called analysis," because they can have geometrical analogues. Yet, they are more powerful than classical geometry, because they make fewer assumptions and are universalized in symbolical form.

Tait's motivation for the present article is to simplify "by a symmetrical process, the usual modes of treating the rotation of a rigid body" (Tait 1898, 87). Tait wants to make old processes more efficient by applying the engine of quaternions to rigid body problems. He writes that, in 1862, he had become aware of Cayley's paper, "Report on the Progress of the Solution of certain Special Problems of Dynamics," published in the *British Association Report*. In that paper, Cayley had offered his readers an account of rigid body rotation, which sparked Tait's interest in offering a simpler and more "symmetrical" account of the motion in question. Indeed, it was Cayley's paper that led Tait to grasp the "notion of attaining symmetry, by seeking the single rotation which would bring the body from some initial position to its actual position at a given time, which had been suggested to me by Hamilton's beautiful results" (Tait 1898, 88).

The "symmetry" obtained by Cayley required, however, "A brilliant display of analytical power at great expense of time and bewilderment to the ordinary reader." Thus, Tait's aim was to offer a "neater" account of such symmetry, by offering an account of a "transition [that] can at one step be effected from any initial position to the actual position of the body at a given time" (Tait 1898, 88).

In his account, every "infinitely small displacement of a Rigid System, one point of which is fixed, takes place about an instantaneous axis." Tait offers a quaternionic account of the motion of such infinitesimal displacements, claiming,

> The essential difference between this [quaternion] process and the ordinary one, consists in using rotations about *each* of the three axes fixed in the body, instead of one about one axis, followed by another about a second, and then a final rotation about the *first* axis instead of the third (Tait 1898, 97).

The quaternion method is faster, more succinct and does the same amount of work but with less energy wasted on the part of the mathematician. Tait admits the kinematic problem—the one in which the mathematician must determine the quaternion which "gives the position of the body at any time"—is more difficult to carry out and "does not appear, so far as I have yet examined the question, to lead to any very simple expressions." Yet, he states his hope that on a "future occasion" the problem would be reduced to powerful and efficient results (Tait 1898, 125).

By the 1870s, Tait's attention had turned toward the construction of "definite integration, of the kinds required in physics, applicable to quaternion symbols and not merely scalar variables." In his paper "On Green's and other Allied Theorems," published in the *Transactions of the Royal Society of Edinburgh* (1870), for instance, Tait explained he had long consulted Hamilton about the lack of integral techniques for quaternions—a gap that had hindered the application of Hamilton's vector (or "quaternion") operator, ∇, from fulfilling "its promise of usefulness in physical applications."

Though Hamilton had apparently asserted he was in the process of developing such a technique, it was never published; upon Hamilton's death in 1866, an unfinished final chapter of his *Elements* (1866) suggested he was headed toward such an elaboration, but his efforts were never stated clearly or in any detail in the unfinished manuscript. Nowhere, Tait notes, did Hamilton leave scraps of his thoughts on the matter (Tait 1898, 136). Tait, for his part, wants to "supply the want" that Hamilton had left unfulfilled. In his 1870 publication, Tait indicates he has successfully achieved the mission in a "simple, though not very direct process." Tait states his approach is not perfect, but it works "so far at least as to enable me to use quaternions in inquiries connected with potentials." The work explored Green's and allied theorems in the field of dynamics, making the claim that, through quaternion symbolism, new and important physical relations were manifest. As Tait argued in that paper,

> Even the little advance that I have made in the present paper has enabled me to see, with a thoroughness of comprehension which I had despaired of attaining (at least by Cartesian processes), the mutual relationship of the many singular properties of the great class of analytical and physical magnitudes which satisfy what is usually known as Laplace's equation. This is, of course, due solely to the simplicity and expressiveness of quaternions in general (Tait 1898, 137).[95]

Throughout the 1870s, Tait proceeded to devote the bulk of his research efforts to publishing on matters of thermodynamics. The only other quaternion text to appear in that decade was his textbook *Introduction to Quaternions* (1873), co-authored by Philip Kelland (1808-1879). Kelland had been the first English-trained (i.e. Cambridge-trained), English-born person to be elected into a Scottish chair. As professor of mathematics, Kelland had brought with him the Cambridge tradition in symbolical analysis as it had been taught in the late-1820s and early-1830s. he had been tutored

[95] His findings in that paper are repeated in a paper entitled "On Some Quaternion Integrals," published in the *Proceedings of the Royal Society of Edinburgh* in 1870. He presents the integral,

$$\int P d\rho = dsV.Uv\nabla P,$$

where "P is any scalar function of ρ, and the single integral is extended round any closed curve, while the double integral extends over any surface bounded by the curve, v being its normal vector." If $\sigma = iP + jQ + kR$ then, $\int \sigma d\rho = \iint ds(S.Uv\nabla\sigma - V.(VUv\nabla)\sigma)$, for which the vector and scalar parts are equal (Tait 1898, 159).

by William Hopkins (the famed coach) before graduating Senior Wrangler and first Smith's Prizeman in 1834. Kelland was therefore trained at Cambridge before the major government-led reforms to Cambridge's curriculum took effect—the same reforms that effectively excised natural philosophical topics such as magnetism and electricity from the curriculum. Therefore, Kelland—though a symbolicist—was not unaware of developments in other fields of natural philosophy. His position at Edinburgh, in particular during the rise of thermodynamics in the late-1850s and throughout the 1860s, would have put him in a prime position to appreciate the lucrative nature of "applied" mathematics. Thus, it was Kelland that increasingly sought to incorporate quaternion mathematics into Edinburgh's mathematical courses, despite their lack of uptake at Cambridge, perhaps hoping thereby to gain income himself from the rising star of Tait, whose natural philosophy and experimental laboratory courses were becoming increasingly popular. Kelland also required textbook sales to supplement his student fees, and textbooks, he knew, were most successful when they were written to align themselves with specific course content, drawing in potential students to the lectures being delivered and profiting from those students who required Kelland's math courses to graduate. Indeed, Kelland had become so accustomed to the textbook culture that it heavily supplemented his income as a professor in a Scottish university. This is not surprising given that the 1858 Scottish Universities Act reforms had institutionalized standardized examinations and course requirements at Edinburgh. Kelland's mathematical texts began to replicate the style and tone of popular coaching texts then in use at Cambridge.

The focus on Peacockian symbolical analysis is evident throughout Kelland's works. Indeed, Kelland's continued membership within the amorphous community of symbolical algebraists in England who were trained in the early half of the 19th-century is evident in his *Elements of Algebra* (1860), written for Edinburgh mathematics students, in which Kelland made specific claims with regards to the nature of arithmetical and algebraic knowledge. Kelland wrote,

> In a great number of arithmetical operations, it is impossible to carry on a continuous train of reasoning by means of which the various data are successively introduced in their proper places, and the conclusions to which they respectively lead, combined and worked into the final result. It may be that the data themselves depend on the result, or on some property which can only be expressed at a period subsequent to their introduction into the calculation; it may be that the required conclusion is only a particular case of more general considerations, contemplated in, and necessarily introduced by, the hypothesis. From whatever case it may arise, it will and does frequently happen that we must of necessity have in view the conclusion itself, or something involving it, even at the very outset of the solution.

He continued,

> Should we attempt to proceed by arithmetic simply, it would be requisite to employ some specific artifice suggested by the nature of the particular problem, whereby the introduction of the thing sought might be rendered obvious in the process, and thus the mind might be relieved from the complex considerations incident to calculations applied to the thing yet to be found.
>
> The science of Algebra has for its primary object the exhibition to the eye, of all the operations which in this case would have to be represented only to the mind. Whereas, in arithmetic, nothing can be represented but that which is known either by the conditions of the problem, or by the calculations which have resulted from them; in algebra no operations are suffered to remain without representation. In the former case, then, the mind has, without assistance, to pursue the track, and to keep in view both the previous operations and the results which are to be obtained from them to the full extent in both directions; in the latter, the mind is relieved from all retrospection, and almost all prospective action, and is concentrated on the point immediately before it; the eye being made the guide to what is to follow, as well as the depositary of that which has been effected (Kelland 1860, 1-2).

Algebra is an artifice, Kelland claimed, but one that is useful in that it relieves the mind of cumbersome duties, by representing individual numbers and objects by symbols, so that "*memory* is almost, if not altogether, dispensed with, through the register that is kept of the state of the operations as they proceed." Rather than recalling specific and temporal instances of real objects, algebra allows the user to deal with an infinite number of particular instances by relying upon a few universal algebraical rules. "That, therefore, which constitutes the transition from Arithmetic to Algebra is nothing more than this," Kelland argued. "That the *number required is represented in a visible form to the eye.*"

By the 1870s, Kelland had become a veteran textbook writer. He had produced the *Theory of Heat* (1837) a few years after his arrival to Edinburgh, followed up two decades later by *Transatlantic Sketches* (1858) and the *Elements of Algebra For the Use of Schools and Junior Classes in Colleges* (1860). His final textbook contribution was the *Introduction to Quaternions* (1873), a second edition of which was issued in 1882, and a third edition of which emerged in 1904, all jointly authored with Tait. The book was written for a class of students to whom "quaternions" would have been introduced as an established part of the symbolical algebraic mathematical canon. Written mostly by Kelland, the book presents Hamilton's directed lines (i.e. vectors) and quaternions as entirely justified and not in need of further legitimization. Kelland's text also provides a simply-structured set of explanations and practice problems meant to train students in quaternion methods as an established symbolical algebraic field of study. In the preface to the *Introduction to Quaternions*, Kelland makes clear that he had "for many years past ... been accustomed, no doubt very imperfectly, to introduce to my class the subject of Quaternions as part of elementary Algebra, more with the view of establishing principles than of applying processes."[96] In presenting his students with the underlying principles of mathematics, he has had recourse to the history of mathematics, mentioning the "names of great men" who helped mathematics become what it was by the 1870s. One of those great men, Kelland writes, is Hamilton, whose name he thought would eventually be stamped "with the seal of immortality" (Kelland and Tait 1882, viii).

Kelland justifies introducing quaternions at an elementary level by arguing that quaternions speak to the founding principles of mathematical knowledge itself. Quaternions "bring those principles face to face with operations, and thus not only satisfies the student of the mutual dependence of the two, but tends to carry him back to a clear apprehension of what he had probably failed to appreciate in the subordinate science" (Kelland and Tait 1882, viii). Kelland's personal appreciation for quaternions comes from his belief that, "by one uniform process," they offer a wide variety of

[96] As Tait indicates in the preface to the second edition (1882), Kelland was more concerned with the metaphysics of mathematical principles than he himself had been.

results. Hearkening back to the image of symbolical algebra as a theory of efficient mind processes, as advanced by the early algebraists in the 1820s and 1830s, Kelland writes, "What is of the utmost importance in an educational point of view" is the fact that "the reader of this subject does not require to encumber his memory with a host of conclusions already arrived at in order to advance." Rather, "every problem is more or less self-contained," and this "is my apology for the present treatise" (Kelland 1882, viii).

The first nine chapters of the *Introduction* cover the basic rules of quaternions, including a series of test-style problems meant to familiarise students with addition, subtraction, multiplication, division and calculus of quaternions. Tait wrote the last chapter, on the application of quaternions to kinematics, which presents a quaternion accounts of "strain" in fluids and solid bodies. The joint 1873 production did not include any new research; it constituted, rather, an elementary clarification of quaternion symbolism as issued in an exam-minded workbook for students who were keen to adopt the new technique then being lauded by two Edinburgh professors, and which Kelland, at least, had institutionalized in his mathematics lectures and exams throughout the 1860s and 1870s.

Thermodynamics and quaternions in the 1870s

Recall that throughout the 1860s and 1870s, Tait and Thomson had collaborated on a series of joint projects, the best known of which became their co-authored *Treatise*. Thermodynamics embodied the principles of work, efficiency, and productivity. Thus, as Tait collaborated with Kelland to produce an elementary text on quaternion mathematics, thermodynamics had come to define the primary terrain of knowledge upon which the natural philosopher trod. To get a sense, therefore, of Tait's views in thermodynamics as they became more profound in the 1870s, it is useful to appeal to his lectures on the "physical sciences," as delivered to his class in 1874 and published in 1876.

One of the most important themes to emerge in Tait's *Lectures on Some Recent Advances in Physical Science* (1876) is the idea that "energy" must serve as the overarching and all-encompassing conceptual category for all science. Tait tells his students,

> Just as Gold, Lead, Oxygen etc. are different kinds of Matter, so Sound, Light , Heat, etc. are now ranked as different forms of Energy, which, as we shall presently see, has been shown to have as much claim to objective reality as matter has.

For Tait, "energy" exists. It is not mere metaphor. It is not a positivistic image that helps to describe perceived phenomena. It exists at the ontological level within the structure of the universe. It causes natural events to occur. "Energy" can be mathematically described—or, rather, the effects of energy can be mathematized. But, "energy" is more than a mere symbolical equivalence. "Energy" bears direct relation to ontological reality. As Tait insists, energy "enables us to coordinate all the parts, however apparently diverse, of the enormous subject of Natural Philosophy."

Furthermore, the notion of "energy" replaces problematical accounts of "force" which, Tait argues, had long hindered natural philosophical discourse in Britain and abroad. "Force," Tait explains, is often treated as that which causes some sensation. "But," he warns,

> We must be particularly cautious as to the way in which we treat the evidence of our senses in such matters. Think of Sound and Light, for instance—which, till they affect a special organ of sense, are mere wave-motions. The sensation is as different from the cause in such cases as are the bruise and the pain produced by a cudgel or a cricket ball from the mere motion of those portions of matter before impact on a part of the human body. In all likelihood a similar (probably a more sweeping statement) is true of force (Tait 1876, 15-16).

The classical conception of "force" comes from Newton's first Law of Motion, which can be stated as, "Force is any cause which alters or tends to alter a body's state of rest or of uniform motion in a straight line." The difficulty with such a law is its claim to identify a "cause," "for this, amongst material things, usually implies objective existence." By contrast, Tait argues,

> in every case in which force is said to act, what is really observed, independent of the muscular sense (whose indications, like those of the sense of touch in matters concerning the temperatures of bodies, are apt to be excessively misleading), is either a

> transference, or a tendency to transference, of what is called energy from one portion of matter to another (Tait 1876, 16).

When such transferences take place, there is a "relative motion of the portions of matter concerned" and the "so-called force" to be accounted for is the "rate of transference of energy per unit of length for displacement in that direction." Ultimately, "force ... has no necessarily objective reality any more than has Velocity or Position" (Tait 1876, 16). In light of these problems, Tait writes,

> it has been generally recognized that there is something else in the physical universe which possesses to the full as high a claim to objective reality as matter possesses, though it is by no means so tangible, and therefore the conception of it was much longer in forcing itself upon the human mind. The so-called "imponderables"—things of old supposed to be matter—such as heat and light, *et cetera*, are now known by the purely experimental, and therefore the only safe, method [is to see these as] varieties of what we call Energy,--something which, though not matter, has as much claim to recognition on account of its objective existence as any portion of matter.

Tait continues,

> The grand principle of Conservation of Energy, which asserts that no portion of matter can be brought into existence by any process at our command, is simply a statement of the invariability of the quantity of energy in the universe,--a companion statement to that of the invariability of the quantity of matter (Tait 1876, 17).

In brief, "energy" can be defined as the "ability to do work." And the energy to do work is associated with the position of an object, i.e. its "potential energy". The energy already present in a body in motion is termed "kinetic energy" (Tait 1874, 18-19).

Linked to these forms of energy is the concomitant notion of "dissipation." Tait explains that

> the Dissipation of Energy is by no means well understood, [and] any many of the results of its legitimate application have been received with doubt, sometimes even with attempted ridicule [but] it appears to be at the present moment by far the most promising and fertile portion of Natural Philosophy (Tait 1874, 21).

Though the principle of the "conservation of energy" stipulates energy can neither be destroyed nor created in any ultimate sense (apart from the involvement of God in acts of creation), there is nonetheless a "dissipation" that occurs such that potentially useful storehouses of energy are progressively transformed into unusable and dissipated forms of "kinetic" energy. The universe is headed unidirectionally towards unusable energy status.

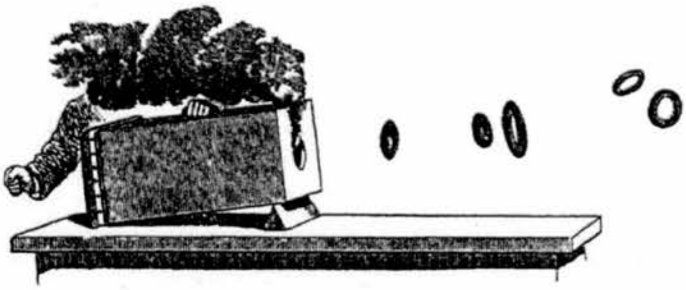
In an elongated historical narrative, Tait then tells his students major advancements in mathematical knowledge had been effected over the previous 20 years. The concepts of space and time had largely shaped human experience and scientific inquiry. Yet, what those concepts actually define is still poorly understood. Hamilton had metaphysically defined algebra to be the "science of pure time," and geometry to be the "science of pure space." Yet, a definite determination of a notion of "pure time" remained elusive. For instance, even the rotation of the Earth as an absolute measure of time was "by no means a uniform quantity," Tait explains. Thus, humanity is still awaiting the arrival of some objective standard that would come in the form of a measurement of the period of vibration of a molecule of heated gas, such as hydrogen, to determine an absolute standard of time. Meanwhile, the notion of "pure space" had similarly been torn asunder by "careful scrutiny by mathematicians of the highest order, such as Riemann and Helmholtz." Indeed, "The result of their inquiries leaves it as yet undecided whether space may or may not have precisely the same properties throughout the universe" (Tait 1876, 5). These studies suggest the Earth may be moving through segments of the universe that are not always composed of the same fundamental properties—thus leading to varying physical phenomena for which no explanation yet exists. To study such matters requires "mathematics of a transcendental character" (Tait 1876, 6)—best developed, Tait explains, through symbolical analytical methods, such as quaternions.

From a Whig historical perspective, the historian might wonder why Tait nowhere links his account of "space" and Riemannian geometry to his quaternion mathematics, as Clifford would later do, given that he was clearly aware of Riemann's ideas. Recall the initial "spark" that led Tait to reread Hamilton's *Lectures* (1853). In so far as experiments had revealed anything about spatial ontology, Tait considered "vortex-atoms" to be the most credible model to adopt. Informed by Helmholtz and Thomson's early conceptions of vortex-atoms, Tait's view of the structure of the universe favoured a vortex account, in which physical phenomena are the result of molecular interactions, as opposed to spatial curvature—a point that Clifford had, by this point, already speculated upon. Tait writes in his *Lectures* (1876), Helmholtz's mathematics and Thomson's physical speculations on the fundamental structures of the universe were "by far the most fruitful in consequences of all the suggestions that have hitherto been made as to the ultimate nature of matter." Helmholtz had hypothesized that a vortex-ring could take on any "internal filament" shape. Rather than a simple circle, the ring could be composed of any number of internal knots. Convinced by Helmholtz's account, Tait felt that complex rotating structures likely constituted the foundation of the entire universe's microscopic structure. "Matter may really be only the rotating portions of something which fills the whole of space; that is to say, vortex-motion of any everywhere present fluid," Tait writes.

Helmholtz had initially mathematized the properties of such "vortex-motion" "in a most beautiful investigation, in which he broke ground in a department of hydro-kinetics, the difficulties of which, up to his time, had kept mathematicians almost completely aloof" (Tait 1876, 290). And what impresses Tait so deeply about that account of atomic structure was its potential to serve as an absolute model for spatial structure. He claims,

> Especially does it give us a glimpse, at least, of an explanation of the extraordinary fact, that every atom of any one substance, wheresoever we find it, whether on earth or in the sun, or in meteorites coming to us from cosmical spaces, or in the farthest distant stars of nebulae, possesses precisely the same physical properties. So convinced are we, by experiment and observation, that hydrogen, in the farthest nebulae, in the farthest stellar system, vibrates (when heated) in precisely the same fundamental modes, and in precisely the same periods, as it does in a Geissler's vacuum-tube in our laboratories; that, as we have already seen, any *apparent* exception to this is hailed as a certain source of information about the relative motion of such bodies with regard to the earth, and in some cases may give an invaluable method of obtaining their actual distances from us (Tait 1876, 291).

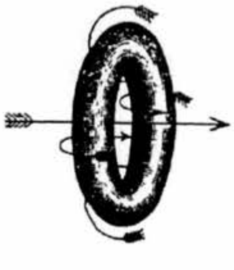
When vortex rings are forced to collide, Tait explains, they behave by producing shapes that are not in equilibrium. Because a "circle" is the vortex-ring's most stable state, an elliptical or square vortex-ring is seen to "vibrate about that circular form as about a position of equilibrium" (Tait 1876, 292). Tait demonstrated this fact to his students using the particular apparatus pictured below.



(Tait 1876, 292)

Tait states then that quaternions are an exceptionally useful means of describing vortex-motion in the universe.

It is not, therefore, that Riemannian geometry was unknown to Tait, or that he deemed it unimportant. It is, rather, that the notion of non-Euclidean geometrical structures came into Tait's worldview long after Helmholtz and Thomson had already established vortex-atoms a concept by which Tait could comfortably define ontological matter, and mathematize its structure. Hence, we find that in Tait's *Lectures*, in the two chapters on the "structure of matter", non-Euclidean geometry is not mentioned once. Rather, the various accounts of the "atom"—as either soft and malleable, or hard and individualized—offer a detailed account of what Tait takes to be the most fruitful and promising version of microscopic ontology, i.e. "vortex-atoms". Quaternion symbolism fit well within this "physical" view, as it succinctly describes rotations of rigid body motion at microscopic levels. Because the vortex-ring is constantly rotating about its core (the outer portion is moving "backwards" while the inner portion is moving "forwards"), it is easy to see why Tait felt quaternion mathematics were particularly useful in describing the motion of such rings in nature.



(Tait 1876, 296)

As Tait argues, the subject of such "rotary fluid motion is such a forbidding one, from a purely mathematical point of view," and

> to investigate what takes place when one circular vortex-atom impinges upon another, and the whole motion is not symmetrical about an axis, is a task which may employ perhaps the lifetimes, for the next two or three generations, of the best mathematicians in Europe (Tait 1876, 298).

And despite the difficulties he himself had encountered in pursuing quaternion descriptions of vortex motion, Tait alludes to quaternions in the pursuit of those difficult mathematical speculations, by insisting,

> Some mathematical method, enormously more powerful than anything we at present have [must] be devised for the purpose of solving this special problem. This is no doubt a very formidable difficulty, but it is the only one which seems for the moment to attach to the development of this extremely beautiful speculation; and it is the business of mathematicians to get over difficulties of that kind (Tait 1876, 298).

At the time of writing, Tait had already authored his final chapter in the *Introduction to Quaternions* (1873), in which he had emphasized the use of quaternions in describing problems of "strain"—a phenomenon that represents the sort of distortions that vortex-structures experience in their non-uniform and asymmetrical motions about the vortex filament. Tait thus used his 1876 lectures to enjoin his students to pursue such new and "powerful" mathematical techniques in their scientific enquiries.

Presbyterian politics and Tait's attack on agnosticism

Tait's particular religious views fed into these mathematical and thermo-dynamical views. As already mentioned, Tait, along with Thomson and other northern scientists of "energy", viewed the "conservation" and "dissipation" of "energy" as both scientific fact and moral revelation. One significant outcome of this belief system, at least on Tait's part, was his public and vitriolic defense of the all-mighty creator—a theme that became most prominent in his published works in the 1870s (for a more detailed account of the Scottish Kirk politics that fed into this terrain, see Appendix Five). An early indication of Tait's vocal religious identity appears in his public debate with John Tyndall, then professor of natural philosophy at the Royal Institution. Tyndall had spent years as a surveyor and teacher of general science before obtaining his doctoral degree at the University of Marburg in Germany. A critic of the dual concepts of "conservation" and "dissipation", defined by the northerners, Tyndall argued the notions were religious stand-ins for scientific theory. They engendered an all-mighty god who was able to randomly intervene to alter any state of affairs. To Tyndall this was metaphysics—not science. Tyndall chose to delegitimize the northerners' claim to being the progenitors of "energy" science by locating the origins of the concept of "conservation of energy" in the works of a German scientist, instead. In a speech "On Force," delivered to the Royal Institution on June 6, 1862, Tyndall argued the notions related to the transference and conservation of "energy" ought to be accredited to the German, J.R. Mayer, as opposed to Joule or his colleagues in Scotland. Joule had initially posited experimental relationship between work and heat in the 1840s. Indeed, it was Joule's work on heat relations that attracted Thomson to the subject, when the former published his 1843 article, "On the Mechanical Value of Heat." Tyndall recognized Joule's claims, but he qualified them by saying Mayer had, in fact, already calculated the mechanical equivalent of heat "from the velocity of sound in air" in 1842 (Lloyd 1970, 212).

What ensued was a battle for legitimacy fought out on the pages of various publications. The effort to legitimate the emergent field of "thermodynamics" as a Scottish affair was no small business (Smith 1998). It not only engendered a claim to fame on part of the actors involved, but it also implied a right to generate wealth on the basis of corollary and marketable patents or textbooks. Tait, therefore, urged Thomson to respond to Tyndall's unprecedented attribution of primacy and originality to Mayer. The result was an article entitled "Energy," co-authored by Thomson and Tait and published in the October issue of *Good Words*, a religiously-minded family publication. That article reconstructed the history of "energy" by relegating to Mayer a minor role. Tait and Thomson wrote,

> It especially startles us that the recent attempts to place Mayer in a position which he never claimed [read creator of the heat-work relationship], and which had long before been taken by another [read Joule], should have support within the very walls wherein Davy propounded his transcendental theories (Lloyd 1970, 216).

In Tyndall's responded to the *Good Words* article, the Irishman pointed out the religious and non-scientific nature of the publication in which Tait and Thomson had published their article. Tyndall, an open agnostic, claimed he had been tardy in responding to the article, because the authors had made such a poor choice in choosing *Good Words* as their publishing vehicle. Tyndall wrote,

> When ... it is known that the other articles in the number to which I refer, bear such titles as "The Childhood of Jesus," "The Trial Sermon," "The Bands of Love," "At Home with the Scriptures," & etc. I think I may be excused if the article on Energy, in the scientific sense of the word, imbedded in such matter as those titles indicate, escaped my attention (Lloyd 1970, 216).

Tait and Thomson's claims were not proper science, Tyndall claimed, given that they had chosen to publish in a non-scientific and non-credible journal. A public debate between Tait and Tyndall ensued for months.

This public controversy was just one of many instances that indicate the manner in which Tait's Scottish Presbyterianism overlapped with his other terrains of mathematical and natural philosophical knowledge throughout the 1860s and 1870s. Presbyterian politics had come to inform Tait's approach to science and mathematics in profound ways. One means of describing the manner in which it did so is to appeal to Tait's jointly authored *The Unseen Universe* (1875).[97] That text was co-authored by Balfour Stewart (1828-1887), a former assistant to Forbes in Edinburgh and director of Kew Observatory. Following a train accident that left Stewart severely disabled, he was appointed

[97] In 1874, Tyndall delivered his presidential address before the BAAS at Belfast, at which point he expounded upon the links between evolutionary theories of the universe and thermodynamical principles of energy. Following Tyndall's speech, a rumour began to spread around the University of Edinburgh that Tait was engaged in writing a book that would "overthrow materialism by a purely scientific argument" (Knott 1911, 236). Although initially published anonymously, "Tait's scientific style" and his "views of the historic development of the modern theory of energy" made it evident to many of his first readers that he was, indeed, the author, along with Balfour Stewart. The work became so popular that by 1876 Tait and Stewart had released a *fourth* edition with their names on the title page. The fourth edition included an introduction not present in the first, which explained in explicit language the motivation for writing the book and the objectives of the book. Tait and Stewart introduced that section in response to critics who had, in their view, failed to understand the proper intention of the authors. A sixth edition was released by the end of the same year (Knott 1911, 236-237).

to a professorship of physics at Owens College, Manchester. His publications centred on radiation and magnetism; following the accident, however, he also began to write profusely on metaphysical matters in seeking theological justifications for science. Like Tait, Stewart was also imbued with an evangelical pride in post-Disruption Scottish Presbyterianism. The politics of the Free and Established kirks in Scotland manifested themselves in Stewart's life choices. A deeply religious man who believed in an ultimate creator and who disdained nascent "secular" theories of human evolution, which had begun to dominate English social discourse in the mid-1860s, Stewart was all too willing to present a theological account of "energy" science. Tait and Stewart produced the anonymously authored *Unseen Universe* as a means of describing scientific discovery as the slow revelation of Divine power. By 1876, the book had become wildly popular. Just one year after its initial publication, the authors issued a revised 6th edition, though this time their names appeared on the byline.

In the preface to their 6th edition, Tait and Stewart clarify that, although their scientific claims rely heavily upon the unquestioned truth of the "Principle of Continuity", which they hold to mean that nothing is created or destroyed instantaneously, the authors do not,

> assert the eternity of stuff or matter, for that would denote an unauthorized application to the invisible universe of the experimental law of the conservation of matter which belongs entirely to the present system of things (Stewart and Tait 1876, vii).[98]

In other words, empirical knowledge is sound and trustworthy, though it is inherently temporal. It is transient and subject to the whim of the creator. Assertions of the absolute existence of any scientific law or object conceptually usurp the central figure of God. The premise of the book is the

[98] Tait and Stewart note their surprise and annoyance at the misuse of their words a year after first publishing the *Unseen Universe*. "Pages of so-called 'extracts' from our book have been strung together, now by some writers of the High Church school, anon by writers of the very lowest Evangelical type, in each case with absolute disregard of their original collocation and surroundings, and the result is of course as utterly unfair a representation of our meaning as could possibly be given," they write. "These 'extracts,' which are always scrupulously enclosed in inverted commas, are not merely altered in meaning by being arbitrarily detached from the context—they are often altered by the insertion of terms (e.g. *luminiferous force!)* which we, as scientific men, could not possibly have employed" (Tait and Stewart 1876, ix). A study of the uses of the *Unseen Universe*, and the varied responses it evoked both popularly and within the closed circles of scientific practice, would indicate the degree to which the scientific community of Britain in the 1870s was composed of a heterogeneity of religious opinions, many of which shaped scientific outputs in fundamental ways.

simple claim that "Science" and "Religion" are not incompatible foes. This "ought to be self-evident to all who believe that the Creator of the Universe is Himself the Author of Revelation," the authors write.

Yet, agnostic and atheist accounts of scientific phenomena had unduly "alarm[ed] even the firmest of human faith" (Stewart and Tait 1876, ix). To counter the agnostic views then becoming popular, Stewart and Tait contend they are not propounding a simplistic argument from design. It is not the case that an appreciation of nature—even a detailed, experimental appreciation—necessarily tells one anything about the nature of God's mind. Rather, "The statements in the New Testament scriptures regarding God are necessarily mysterious," but the "mystery can be no test of their truth or falsehood, inasmuch as it must in such regions be the almost inevitable accompaniment of truth." Rather,

> The question is not whether they are mysterious, but whether they are consistent with themselves, and with the knowledge we derive from other sources. We therefore devote considerable portions of this volume to a proof that the conception of god which the majority of Christians derive from the New Testament is in no way inconsistent with that deduced from scientific principles (Stewart and Tait 1876, 19).

The conclusion they came to is that, insofar as "continuity" and the "conservation of energy" hold true, there must be life after death.

The "unseen universe" contains that "energy" that is temporally visible in the "seen universe", but which is invisible once it dissipates from "potential" to final "kinetic" forms. What ultimately forms the "kinetic" universe beyond the observed cannot be determined by any experimental findings or any natural philosophical musing. The reconciliation between science and religion indicates that revelation compliments scientific data rather than opposing it. The "disbelievers in such a doctrine" (specifically, in the doctrine of life after death) "form a minority," the authors recognize, and,

> at the same time it must be acknowledged that the strength of this minority has of late years greatly increased, so much so that at the present moment it numbers in its ranks

> not a few of the most intelligent, the most earnest, and the most virtuous of men (Stewart and Tait 1876, 23-24).

Interesting for the present study is the fact that the words "mathematics" and "algebra" appear rarely in *The Unseen Universe.* In fact, the only mention of "mathematics" that appears in the text is at the end of the text, where the authors quote William Stanley Jevons (1835-1882), who, in his *Principles of Science: a Treatise on Logic and Scientific Method* (1874), had stated the benevolence of an almighty power is constantly in question given that even the slightest experience of pain or sorrow suggests God is not so benevolent after all. Jevons had also noted the philosophically problematical nature of non-Euclidean geometries, arguing "If we cannot succeed in avoiding contradiction in our notions of elementary geometry, can we expect that the ultimate purposes of existence shall present themselves to us with perfect clearness?" (Stewart and Tait 1876, 254). Jevons concluded, therefore, that we are "finite minds" attempting to understand "infinite problems", and we are constantly faced with illogical outcomes in the form of disharmonies. For Stewart and Tait, however, these "disharmonies" are not proof of linguistic, experimental or mathematical "illogic". They are mere proof of the limitations of human knowledge, and they necessitate a belief in the "unseen universe", where the contradictions of illogic are unraveled.

The lack of much mathematical discussion in *The Unseen Universe* might strike the reader as odd, given that both Stewart and Tait were trained mathematicians and given that both had devoted significant portions of careers to the elaboration of mathematical techniques and mathematized physical phenomena. The omission is not so surprising, though, when considered in light of Stewart and Tait's aim in writing the book. Their primary aim was to publish a popular book for wide-spread consumption, not a textbook or treatise. We find, therefore, no detailed mathematical formulae or even mathematical representations of thermo-dynamical processes. We do find a segment on the historical rise of "energy" science, where mathematical objects are discussed in colloquial terms. For example, Stewart and Tait begin by making the experimental claim that "Experience of the most varied kind consistently shows us that we cannot produce or destroy even the smallest quantity of matter" (Stewart and Tait 1876, 101). The authors discuss other mathematics in general terms by referring to the heroic role of scientists, such as Hamilton, whose notion of directed magnitudes had come to play an important part in describing the physical phenomena that experimentalists had observed over the past "thirty or forty years." In the story of historical progress from *vis viva* to "energy of motion," or "kinetic energy," scientists had been given a helping hand by the mathematical tool of directed quantities, which Hamilton had discovered.

More importantly, however, the *Unseen Universe* and its religious underpinnings explain why Tait attacked Clifford's *Elements of Dynamics* and his *Common Sense of the Exact Sciences* in the 1880s—two books that propounded Clifford's views on quaternion and bi-quaternion mathematics. Tait was not opposed to Clifford's mathematical claims, although they differed in many ways from Tait's own claims. He was opposed, rather, to Clifford's religious views—or, rather, the lack thereof. When Clifford read the first anonymous edition of the *Unseen Universe,* he responded to it in no uncertain terms (see Chapter Four). Indeed, Clifford led many agnostic attacks on metaphysics and any attempt to relate divinity to scientific knowledge. Tait reflected upon Clifford's harsh reviews in a letter written to Roberston Smith, a theologian and scholar of Semitism, on June 5th, 1875. Tait wrote,

> You have of course seen Clifford's painful essay in the *Fortnightly Review* ... [and] an advanced ritualist, MacColl, has cracked us up in a letter to the *Guardian* last week. This week the *Spiritualist* said that with a few slight changes the book would be an excellent text-book for *its* clients. The *Edinburgh Daily Review* says we are subtle and dangerous materialists. Hanna (late of Free St John's here) says "the work is the most important defense of religion that has appeared for a long time." Which of these is nearest the truth (Knott 1911, 239)?

Tait's Presbyterian brand of Christianity had, by the mid-1870s, formed an integral part of his identity as a natural philosopher and mathematician. In 1878, for instance, Tait chose to further defend this Presbyterian progression in an article published in the *International Review* (November 1878), entitled "Does Humanity demand a New Revelation?" He contended,

> It would therefore appear, from the most absolutely common-sense view—independent of all philosophy and speculation—that the only religion which *can* have a rational claim on our belief *must* be one suited equally to the admitted necessities of the peasant and of the philosopher. And this is one specially distinguishing feature of Christianity. While almost all other religious creeds involve an outer sense for the uneducated masses and an inner sense for the more learned and therefore dominant priesthood, the system of Christianity appeals alike to the belief of all; requiring of all that, in presence of their common Father, they should sink their fancied superiority one over another, and frankly confessing the absolute unworthiness *which they can not but feel*, approach their Redeemer with the simplicity and confidence of little children.

To question the underpinnings of God in science, or scientific development, is to futilely act against the fundamental ignorance of humanity. As we will see in the next chapter, Clifford attempts to do just that. And so, insofar as Tait opposed Clifford's mathematical works, it was an opposition that emerged from Tait's navigations through a deeply religious terrain of Scottish Presbyterian belief—a terrain that equipped him with a particular sense of what constituted moral thinking, in mathematics and otherwise.

Conclusion

From studying symbolical algebra at Cambridge to working as an experimental natural philosopher in Scotland, Tait trod upon multiple overlapping "terrains of knowledge" in his intellectual pursuits. Despite Tait's detailed correspondences with Hamilton, he did not engage with quaternions exactly as Hamilton had. Whereas Hamilton had emphasized the symbolical algebraic aspects of quaternions in order legitimate his work in the eyes of his British colleagues at Cambridge, Tait did not question the symbolical underpinnings to quaternion analysis. The symbolical algebraic nature of quaternions was, to him at least, an unquestioned given. As a budding young mathematician, Tait's immediate focus was, rather, on writing textbooks for the Tripos-oriented culture at Cambridge. Quaternions started off as a potentially lucrative research topic for Tait; they satisfied a need to professionalize through publications and to produce money-making textbooks so as to draw in students to Tait and Kelland's lectures at the University of Edinburgh. By the mid-1860s, Tait viewed quaternions as a potential tool in thermodynamics; their efficiency and simplicity were representative of a more moral form of waste-avoiding mathematics.

Tait's engagements with quaternions varied according to the prominence of the particular terrains of knowledge that he was navigating through at certain points in his career. Those terrains equipped him with the symbolic algebraic tools he needed to engage with quaternions meaningful and also the conceptual resources required to reinterpret quaternions, as he did later in his career. In explaining why Tait engaged with quaternions from the 1850s to his 1880s criticism of Clifford, the historian must appeal to those terrains (i.e. the symbolical algebraic, professional, thermodynamic, and moral terrains) that provided him with a realm of possible and legitimate actions, and which also motivated him to use quaternions in ways that he felt were reasonable and meaningful.

Clifford used many of Tait's claims with regards to quaternions in specific ways. He also rejected many of Tait's other, mostly religious, claims. It is to Clifford that we turn now to understand how he engaged with quaternions while navigating through entirely different "terrains of knowledge" and how he therefore defined, analyzed and utilized quaternions in unique ways.

Chapter Four: William Kingdon Clifford and quaternion engagements, 1863 to 1879

Introduction

From the rise of Darwinist discourse to the laying of the Transatlantic cable, mid-Victorian British society was caught up in a storm of conceptual reorganizations and neo-categorizations. Biologists reclassified the organic history of humans; medical doctors responded to popular imagery that reordered the contemporary body;[99] and the national government (of William Gladstone) sought to recast poverty, penury and social decay as communal problems in need of government intervention. The reorganization of gender roles meant that women—their bodies, their intellects and their professional aptitudes—became a matter of debate in unprecedented ways. John Stuart Mill (1806-1873) advocated for the rights of women in theoretical form, and Florence Nightingale (1820-1910) put words into action by successfully raising over £50,000 to open a professional school of nursing for female students at St. Thomas' Hospital. Meanwhile, the secular environment of UCL constituted the first non-ecclesiastical environment to test Mill and Mary Wollstonecraft's (1759-1797) theories about the admittance of female students on equal academic terms.[100] Throughout Britain, class- and gender-based hierarchies were facing pressure to reform.

The rise in popular science literature also opened up new domains in citizen engagement, forcing scientists, mathematicians and educationalists to take up disputes within the cultural settings of community lecture halls and quotidian publications. Thomas Henry Huxley (1825-1895), Alfred Russel Wallace, (1823 –1913), Tyndall, Tait and Clifford all took up the challenge. Others, including notable members of the Royal Society and the BAAS, reacted in reactionary ways, seeking to define science by its aloofness and independence from popular modes of expression (Lightman 1997).

[99] The publication in 1866 of a picture depicting Jean Battista dos Santos with an extra leg and two penises, for instance, became a site of cultural dispute between established medical practitioners who sought to safeguard traditional categories of the "body" and "medicine", and populist critics who sought to establish anomalies as standards for new norms (Gates 1997).

[100] UCL first opened its degree-granting doors to women in 1878; King's College came under pressure to do the same, and finally opened its Ladies' Department of King's in Kensington Square in 1885.

Throughout this period, Britain also experienced deep-rooted political and economic transformations initiated by Gladstone (1809-1898). As leader of the Liberal Party, and Prime Minister from 1868, Gladstone defined a socio-political milieu which formed consensus around issues of national elementary schooling, justice reforms, civil service reforms, and home-rule for Ireland. Though defeated in 1874 by the Conservative Benjamin Disraeli, Gladstone continued to act as a vocal and passionate critic of government policy,[101] lambasting Disraeli's disastrous approach in Turkey's Eastern European escapade (then known as the "Eastern Crisis"), and criticizing British support for Turkish military repression in the Balkan states. Gladstone's efforts to uphold the rights of minorities served as a defining theme throughout his years in power. It was during Gladstone's initial years as Prime Minister, for example, that that the country sought to redress the rampant poverty and religious segregation that individualistic Victorian ideals had entrenched in the collective psyche as personal, rather than social or national, problems.

It was during the Gladstonian administration that humanitarian-esque organizations, such as the Century Club (1865-1881), also saw their most active years. The Century Club involved an élite group of politically active men, many of whom had graduated from Balliol College (Oxford) or Trinity College (Cambridge), and many of whom were active lawyers, journalists or social theorists throughout the 1870s. One of the club's mandates was to serve as a catalyst for university reform—its primary objective being to transform Oxford and Cambridge as "bastions of Anglican privilege into non-sectarian centres of scholarly excellence" (Kent 2008a). Among the club's members were Walter Bagehot (1826-1877) and Thomas Henry Huxley (1825-1895), both of whom were writers for the *Saturday Review*. Writers for other publications, such as the *Fortnightly Review,* the *Cornhill Magazine, Macmillan's Magazine* and *The Reader* (edited by Herbert Spencer) also populated club ranks. The metaphysical views of members varied, but they were predominantly liberal and agnostic. Vocal members, such as the parliamentarian Lord Amberley (formerly John Russell), used the club as a soapbox upon which to lobby for the provision of contraception, while others, including the English

[101] Gladstone was elected back into power in 1880.

critic and essayist, Walter Pater (1839–1894), the writer, Albert Osliff Rutson (1838–1890), and the reporter for the *Pall Mall Gazette*, John Addington Symonds, used it to preach the virtues of political homophilia. Other members used the club to advocate for the northern political cause in the American civil war, to demand reform to the British monarchy, and to push for the protection of trade unions across the country.

Another Gladstonian organization—and one in which Clifford was an active member—was the Metaphysical Society (1869-1880). Boasting a membership of 62 people who were public figures across Britain's philosophical, moral, scientific and theological spectrum, the Metaphysical Society was founded by the architect and impresario, James Knowles (1831-1908), friend of Alfred Tennyson. The apparent inspiration for the group came from Tennyson's experience with Cambridge's Apostles Club (Kent 2008b). Knowles devised the idea of creating a London-based organization similar to the Apostles in which emergent debates over the theological and moral implications of Darwinian, Huxleyan, Spencerian and Millian views could be hashed out. Some well-known potential participants declined membership, including Mill, Spencer, and the financier and creator of the journal *Mind,* Alexander Bain. Others joined eagerly. They included Huxley, Bagehot, Tennyson, the utilitarian philosopher, Henry Sidgwick (1838-1900), the jurist and legal affairs author, Frederick Pollock (1845-1937), and Gladstone himself.[102] Over the course of the decade, Clifford delivered three papers to the Metaphysical Society, all of which advanced a materialist conception of "mind," in which "mind" was presented as nothing other than a conglomeration of contiguous particles and energy transference.

Prominent discourses in Britain reflected the re-ordering, restructuring and recasting of "nature" and its concomitant social categories. Not surprisingly, Clifford's religious and metaphysical outlooks underwent serious and irrevocable change from his inculcation into Cambridge in 1863 to his early death as a UCL professor of mathematics in 1879. Clifford's involvement in societies that lobbied for

[102] Gladstone chaired the society from 1874 to 1875.

more democratic educational provisions—including the improved provision of university education for working-class men and women—put him in opposition to cohorts of more established members within the country's burgeoning scientific community. But it also opened doors to communication with members of those groups that sought more profound alteration to scientific practice, in particular those influential agnostics and supporters of the theory of natural selection who had formed the exclusive dinner club known as the "X-Club". Clearly, Clifford navigated through various "terrains of knowledge" throughout his short career as a student at Cambridge, and later as a professional mathematician in London. While a student at Cambridge in the mid-1860s, Clifford had been exposed to a symbolical algebraic curriculum heavily defined by the second generation of algebraists, such as Cayley and Sylvester. By the 1870s, however, Clifford's mathematical training at Cambridge had become contoured by his wider socio-political interests. After receiving his M.A., for instance, Clifford sailed with an English expedition to observe the solar eclipse in December. His ship hit poor weather and was ship-wrecked near Catania, Italy, but despite the life-threatening nature of the voyage, Clifford never expressed regret for embarking on the trip. If anything, it motivated his interest in developing theories in physics and astronomy, as well as geology, evolution and experimental science more generally.

As Clifford first encountered quaternions in the late-1860s, he did so while navigating through the "terrains" of Cambridge symbolical algebra and Darwinism—both of which he was exposed to as a young student in the middle of the decade. Not surprisingly, Clifford adopted a sense of conventionalism in mathematics, which was based on the idea that, because humans are constantly evolving, our symbolical equivalences cannot be deemed *à priori*, universally true or "permanent," as Peacock would have had it. By the end of the decade, that belief developed into to a sense of Riemannian-inspired "empiricism" in symbolical analysis. For Clifford, it was not enough to just interpret symbolical analysis *after the fact*, as De Morgan would have it. Rather, the mathematician ought to try to understand spatial ontology in his mathematical manipulations. Hence, Clifford developed his understanding of symbolical geometry as a series of "steps in space"—a concept that came to define his pedagogically-minded accounts of quaternion mathematics in the 1870s. Thus, the "terrain of knowledge" identified here as "non-Euclidean geometries in Britain" also played a significant role in contouring his engagements with quaternions. By the 1870s, Clifford entered into a final "terrain of knowledge", identified here as the educational structure of University College, which led him to publish textbooks on his "dynamics," i.e. his quaternion mathematics. In order to understand why Clifford engaged with quaternions in the symbolical algebraic, conventionalist and, even, empiricist ways that he did over the course of those two decades, the historian must appeal to these overlapping "terrains" in Clifford's life—"terrains" that constituted an intricate and complex conceptual topology through which Clifford navigated over the course of his brief career.

Clifford's early education, a brief overview (1860-1868)

Born in Exeter, William Kingdon Clifford (1845-1879) grew up in a single-parent household after his mother died from childbirth. His father, a justice of the peace named William Clifford, sent Clifford to Templeton's school in Exeter for his initial studies. Clifford then moved to King's College, London in 1860 at the age of 15. He spent the next three years of his life studying a curriculum based on classics, mathematics, and natural philosophy (including some experimental courses). This part of Clifford's early education is interesting as it provided him with fertile preparation for his later move to Cambridge. By the 1860s, King's College had become a historically interesting institution in its own right. It was initially established in 1829, by the Duke of Wellington, then Prime Minister, with the support of King George IV. The primary motivation behind its foundation was the establishment of an alternative, urban, London-based university that could rival the secular "Godless college in Gower Street"—namely University College, London, which had been founded a few years earlier in 1826. King's College was established with the specific aim of advancing and entrenching the Church of England's doctrine. It underwent a significant realignment in its curricular duties in 1836 when, together with UCL, it formed the University of London—an institution with the explicit aim of educating working- and middle-class men. King's nonetheless retained a specifically religious mandate.

By the time Clifford entered its halls in 1860, King's had become one of the pre-eminent urban colleges in the country, where men from working- and middle-class backgrounds could gain an education in a variety of subjects. Just as University College, London had been influenced by the broad-based and liberally-minded Scottish curriculum (many Scots, including Brewster, had aided in the formation of UCL's original liberal arts curriculum), King's had been established with a Scots curriculum in mind. Though many of its students aimed for a career in the Anglican Church, the university also became an urban conduit through which young men, such as Clifford, could train in their teenage years in preparation for entry to Cambridge's undergraduate program.

By 1860, King's could claim the special ability to aid students in this regard. The university had appointed Maxwell to the post of Professor of Natural Philosophy, which he held from 1860 to 1865. Maxwell was able to pass on his skills as Senior Wrangler to help prepare students for the Tripos-oriented curriculum at Cambridge. According to Maxwell's correspondences, although the syllabus for his lectures at King's in the first session of 1860 followed the general syllabus used by his predecessor, Thomas M. Goodeve (1821-1902), who had authored *Elements of Mechanism* (1860), Maxwell made sure to add an emphasis on "fundamental principles," including lectures on mechanics, "properties of matter," and characteristics of heat. He also supplemented his lectures with mathematical analyses of the same topics. Whereas Goodeve had devoted much time in his second and third-year classes to astronomy and mechanics, Maxwell offered advanced courses in mathematics that included rigid body dynamics, the motion of an incompressible fluid and its applications to electricity and magnetism, as well as accounts of waves and their applications to sound and light.[103] King's, therefore, provided Clifford with a strong foundation in analytical

[103] In the 1861-62 session, Maxwell deleted his mathematical lectures from his elementary classes, citing the toll that teaching duties were taking on him. Over the course of the five years that he was at King's, his classes began to

mathematics prior to his entry to Cambridge. Given its Anglican orientation, and Maxwell's conservative religious views, King's did not challenge any of Clifford's religious beliefs.

With a King's College education in hand, and High Churchman beliefs firmly in mind, Clifford entered the halls of Cambridge University as a student at Trinity College. His aim was to complete a mathematical degree in the hopes of securing social and financial security. But as Pollock recounts in his later biography of Clifford, the young mathematician-in-the-making was also "reputed with truth [to be] an ardent High Churchman" (Clifford 1886, 2). Within the evolving and socially active atmosphere of Cambridge's undergraduate life, Clifford soon began to diversify his extra-curricular activities and his metaphysical belief structures began to crumble. He became an active athlete at the university and a vocal member in various student organizations.[104] He was exposed to varying anti-Church reformist attitudes and a plethora of critical literature in socio-political and educational matters. His High Churchman attitudes faded away and were replaced by distinct anti-Church sentiments. Pro-Darwinist discourse entered his vocabulary and, by 1866, Clifford was awarded the college declamation prize for an oration on Sir Walter Raleigh. He was elected to the university debating club and the Cambridge Conversazione Society (another name for the Apostles Club), where he was exposed to vigorous and lively debate related to Darwin's *Origin of Species* (1859), as well as the works of Spencer, Mill, Benedict de Spinoza and Guiseppe Mazzini. In his biographical sketch, Pollock notes that the literary exposure Clifford experienced at Cambridge altered his opinions on most religious matters. They shattered his High Church Anglicanism and instilled the

increasingly focus on matters of sound, light and radiant heat, the properties of bodies "as affected by pressure and heat" and "magnetism and electricity" (Maxwell 1990, 3).

[104] Pollock recalls Clifford taking more pleasure from an account of his athletic ability in *Bell's Life* than from accounts of his academic prizes. Pollock also recalls Clifford writing to him in 1869, as a fellow at Trinity, saying "I [Clifford] am at present in a very heaven of joy because my corkscrew was encored last night at the assault of arms: it consists in running a fixed upright pole which you seize with both hands and spin round and round descending in a corkscrew fashion." Perhaps not unlike other Cambridge men at the time, for whom undergraduate education was also a time of generating long-lasting friendships, Clifford and Pollock engaged in the stunts normal to young people engaged in the process of maturation. As Pollock recalls, "[Clifford's] nerve at dangerous heights was extraordinary. I am appalled now to think that he climbed up and sat on the cross bars of the weathercock on a church tower, and when by way of doing something worse I went up and hung by my toes to the bars he did the same" (Clifford 1886, 5).

young man with a deep-rooted belief in agnosticism—a belief that would last throughout the remainder of his adult life.

By 1867, Clifford emerged from his Cambridge training ranked Second Wrangler on the Tripos exam (and second Smith's Prizeman). Opinions on his standing were mixed. While many of Clifford's athletics friends were amazed that he had achieved as high a rank as Second Wrangler (given his apparent lack of study time and his explicit devotion to sporting activities) (Lewis 2006), Clifford's coach, Percival Frost (1817-1898) (Senior Wrangler, 1839), had actually expected Clifford to rank first. Frost accredited Clifford's second-place finish not only to his abundant engagement in non-curricular activities, but also to his lack of adherence to the standard mathematical curriculum. Indeed, as Pollock recalls of Clifford's undergraduate days,

> Clifford's mathematical course at Cambridge was a struggle between the exigencies of the Tripos and his native bent for independent reading and research going far beyond the subjects of the examination; and the Tripos had very much the worse of it. If there was any faculty in which he was entirely wanting, it was the examination-faculty ... it is my belief that, from the point of view to which the class-list is an end in itself, Clifford omitted most of the things he ought to have read, and read everything he ought not to have read (Clifford 1886, 9).

Another one of Clifford's teachers, James Joseph Sylvester (1814-1897), concurred in the matter. In a letter to Pollock, Sylvester writes,

> I believe there is little doubt that he [Clifford] might easily have been first of his year had he chosen to devote himself exclusively to the University curriculum instead of pursuing his studies, while still an undergraduate, in a more extended field, and with a view rather to self-culture than to the acquisition of immediate honour or emolument (Clifford 1886, 9-10).

That is not to say Clifford failed to engage with his Tripos training. On the contrary, Clifford's early mathematical publications from 1863 to 1867 (all produced while he was still an undergraduate) indicate a deep engagement with the mathematical topics he was being taught as part of his Tripos preparations. However, Clifford's self-guided studies, including his private reading of Herbert

Spencer, Charles Darwin and other social and political theorists of the mid-century, provided him with alternative conceptual resources—resources that he eventually used to recast non-Euclidean geometries in the light of "differentiation" and "integration" in evolution. And although Clifford did not graduate Senior Wrangler, a second-place finish was enough to garner him a fellowship at Trinity College, which he held from 1868 to 1870. During his first public lecture as Trinity fellow, Clifford discussed a topic entitled "On some of the conditions of mental development." In that lecture, he elaborated upon Spencer's account of the biological evolution of the brain, demonstrating himself to have become a Spencerian-Darwinist on most evolutionary matters, as well as a radical secularist and a gifted orator. Socially-charged and politically abrasive speeches came to colour Clifford's career over the course of the following decade. The issue of class-based education, as well as the improvement of educational standards among the poor and women, increasingly entered into Clifford's discourses, especially after he was appointed Professor of Applied Mathematics at UCL in 1871.

The University of Cambridge and the morphing of symbolical algebra, 1860-1870

As in Scotland, with the Scottish universities commission, England experienced a round of university reform throughout the 1850s, embodied in the form of a Royal Commission. The commission's report was presented to parliament in 1852, and the corollary bill was introduced to the house in 1856. That legislation sought to change the university's governing structures, by modifying Cambridge's constitution in order to allow for the inclusion of young academics in its governing structures. A new university council (similar to Edinburgh's new university court), was established, and it was composed of resident masters of arts, university professors, heads of houses and higher graduates (Heywood 1868). This restructuring maintained Cambridge's link to the Church of England, given that the majority of the council members were still members of the clerical profession; however, it allowed for the emergence of a prominent lay element within the professoriate, which was now granted the ability to choose council members. The majority of new professors appointed to the university at the time were laypeople. Many of Cambridge's colleges had, in fact, already moved toward eliminating religious testing for their fellowships, but the trend was officially institutionalized in a recommendation to do so by the 1856 commission.[105] Smaller colleges, meanwhile, continued to struggle with the newly secular norms. As Heywood (1868) has concluded:

> Reform...seem[ed] to be most promoted in the largest colleges, and even there the assistance of the legislature [was] needed to enable them to open their fellowships without any subscription to the test of "conformity with the liturgy" (Heywood 1868, 10).

Despite the resistance of the smaller colleges, and despite the fact official affiliation with the Church of England was left untouched by the Commission, the increasingly lay nature of the professoriate meant that students were able to engage with a diversity of literature with greater ease than ever before from the late-1850s onwards.

College fellowships were, however, still academically elitist in nature. Fellowships to some colleges, including Trinity, were particularly exclusive. They were generally offered only to those students who ranked high on one of the "principal" Triposes. Potential fellows had to rank high on college-specific exams, and only Trinity students could qualify to write the Trinity test. Criticizing this continuing practice of exclusion and elitism, John Seeley, M.A. Fellow of Christ's College and professor of Latin, argued in *Essays on a Liberal Education* (1867) that fellowships should be made available to all university students (not just high-ranking students from particular colleges). Seeley contended a high rank on any particular Tripos merely indicated intellectual ability in one domain, whereas "The fellowship should be a reward of general intellectual merit." This "difficulty" could be resolved by,

> requiring all the candidates assumed to be first class men, to write an English essay upon one of several subjects put before them. In this way you might discover

[105] One of the key recommendations made in 1868 by curriculum reformers was that students who had graduated from the University of London should be offered a scholarship for entry to Trinity, given that those students (which included attendees at both UCL and King's College) had successfully completed the university's matriculation examination. The matriculation examination of the University of London corresponded to the former "Little-go" of Cambridge and Oxford, respectively, and it covered the subjects of Greek and Latin, English language, English history, modern geography, mathematics and natural philosophy, chemistry, and either French or German (Heywood 1868).

whether the classical man had any power of thought, and the mathematician any power of language (Heywood 1868, 6).

Seeley was not alone in feeling the Tripos culture at Cambridge had become parochial and that the long-running college tradition of granting fellowships based on insular college examinations was feeding an institution-wide narrow-mindedness. Cambridge was still dominated by private coaches and private lessons that were heavily exam-oriented. The 1860s brought with it debates over whether the university should introduce a course-fee system similar to that seen in Scottish universities, where professors relied upon the attractiveness of their courses to draw in students. To encourage students to demonstrate broad academic ability, Seeley lobbied for Scottish-style lecturing, such that every college would be open to the entire body of university students, and "so that each lecturer might be directly interested in increasing the numbers of his class, and might receive a capitation fee for each attendant in his lecture room" (Heywood 1868, 6). An expansion in the number and types of degrees available also occurred, as the Commission mandated that honours degrees should be offered in law and the natural and moral sciences in addition to traditional degrees in mathematics and the classics.[106] Still, the honours degree in mathematics remained the most lauded and most widely-recognized exam, and it was awarded only after the completion of eight days of rigorous examination. In addition, it was the mathematics Tripos that continued to determine the dissemination of most college fellowships.[107]

In terms of content, the Tripos differed little in the 1860s from its 1850s version. Clifford's exam tested students on Euclid and conic sections; arithmetic; algebra; plane trigonometry; statics and dynamics; hydrostatics and optics; Newton's *Principia*; and associated problems in astronomy and natural philosophy (Heywood 1868, 4). The content had changed little from when Tait was a student

[106] The Natural Sciences Tripos had come to include the following four branches of science: chemistry; geology; botany; and zoology. The student could pick one of these four branches to be examined on. The university held its first examinations in the Moral Sciences Tripos and the Natural Sciences Tripos in 1851.

[107] The timing of the examination had not changed much; it was still held at the beginning of January, with the classical Tripos following at the end of that month.

a decade earlier. This continuity in exam content was largely due to those stringent reforms (see Chapter Three) set in place in the 1849-1850 reform period, when the Board of Mathematical Studies stipulated certain subjects were too "experimental" or too "poorly-founded" to be included on the exam. In was only in 1865 that a slight alteration to the mathematical Tripos occurred, in that the Board recommended Laplace's coefficients and the figure of the earth (considered as heterogeneous) be added. Then, in a report dated May 8th, 1867, the Board further recommended 35 new subjects be added to the Tripos, including elliptic integrals, elastic solids, heat, electricity, and magnetism. However, those topics would not appear on the exam until 1873—years after Clifford had already left the elite hallways of Trinity for the urban passage ways of UCL.

If there was a "terrain of knowledge" that the historian might be led to believe was similar for both Tait and Clifford in their respective navigations, it would have been the formal content of the Tripos exam. However, the Tripos did not exist in a bubble. The social surroundings of Cambridge, including the student organizations each actor participated in and the differing styles of coaching that each actor was privy to, not to mention the disparate pre-training that Tait at Edinburgh and Clifford at King's College would have received prior to moving to Cambridge, rendered their Tripos experiences divergent. Despite the seemingly identical nature of their respective curricula, there were, in fact, a number of important differences in each actor's respective Cambridge experience. One key difference includes the individuals who served as coaches to Tait and Clifford. By the 1850s, the renowned William Hopkins, "senior-wrangler maker," was retiring. In his place a slew of younger coaches had emerged, many of them having been coached by Hopkins himself (or by John Hymers, Hopkins's rival). That new generation of coach included the rising stars of mathematics at the time: Percival Frost (Senior Wrangler, 1839); Isaac Todhunter (Senior Wrangler, 1848); William Besant (Senior Wrangler, 1850); and Edward Routh (Senior Wrangler, 1854) (Warwick 2003, 231). Clifford studied with the first and eldest of that new generation—namely, Percival Frost.

Although little is known about Frost, Karl Pearson (1857-1936) was able to later compare Frost to Routh, as Pearson had studied under both. Pearson recounted that Frost was a "dear old boy," an engaging conversationalist who would often get sidetracked into discussions on theology, mathematical paradoxes, or illustrations of dynamical principles. His tutoring regime was nothing like the strict and regimented training ground established in Routh's classrooms (Warwick 2003, 237). Frost was an unusual choice as coach for someone who aimed to be top wrangler. From 1865 to 1880, Clifford was Frost's only student to achieve a rank in the top three wrangler positions. It is possible that Clifford was attracted to Frost due to the latter's wide-ranging interests. It is also likely that, as one of the less sought-after coaches, Frost was less expensive than Routh, and more fitting for a modest budget. No record of Clifford's reasons for choosing Frost exists, although the fact Clifford was awarded a "small" scholarship to go to Cambridge indicates he likely had few funds to throw around.

The choice nonetheless ended up being a fruitful one, in both mathematical and social terms. It led, for instance, to a long-term friendship between the two men. Frost recounts in a letter to Clifford's posthumous biographers that,

> We were capital friends, yet I was so much engaged with a large number of pupils that I did not see very much of him except in a professional way [during Clifford's fellowship years]. Even when he came to see me out of his working hours we used to get upon some mathematical curiosity, and both being fond of mathematics for their own sakes, we have often pursued our amusement into the small hours—once between 2 and 3—for which his tutor called him to account, good-naturedly excusing him when he heard of how he had been occupied. He often used to amuse me by solving in his head difficult problems, when some conversation like the following would take place. *Fr.* The men in the next room tell me this problem won't come out: there must be a mistake: just read it over and tell me where the setter has blundered. *Cl.* (reads it over and thinks a few minutes) I see how it is, he has, &c., &c (Clifford 1882, xvii).

Given Frost's wide-ranging metaphysical interests, it is likely he played a key role in exposing Clifford to literature that diverged significantly from the typical stuff of Tripos studies.

Important, also, for Clifford's mathematical upbringing was the fact that in 1863 Arthur Cayley (1821-1895) became Sadlerian Professor (a post he held until his death in 1895). A student during the rise of the second generation of symbolical algebraists, Cayley was at the forefront of a distinct geometrical school in mathematics that emerged at Cambridge from 1863 to 1940 (Barrow-Green and Gray 2006). Though Cayley had few pupils, his influence was widely felt in his rehabilitation of developing conceptions in geometry. Cayley was not simply re-introducing "geometry" as a counter to the rise of symbolical analysis, or re-positing it in opposition to the reigning dominance of algebra over the previous 40 years. On the contrary, Cayley's geometry was an output of the symbolical tradition. According to Barrow-Green and Gray (2006), Cayley ensured "a close correspondence between the algebra and the geometry." For instance, he viewed "geometry" as including "complex projective geometry" (Barrow-Green and Gray 2006, 317). In the 1860s, Cayley engaged in a variety of geometrical developments, including the extension of Riemann's classification of curves. Though Cayley was "baffled" by some of Riemann's claims, he also served as a conduit through which students, such as Clifford, were able to access accounts of those works. Thus, Cayley's geometrical studies formed a distinct kind of "geometrical" knowledge—something altogether different in nature and approach from that of "geometry", as it would have been understood and taught at more elementary levels, or even at advanced levels in previous decades.

In addition to Cayley's labours, professional publications in wide circulation at Cambridge, including the *CMJ* (also known as the *Cambridge and Dublin Mathematical Journal* for some years), as well as the *Quarterly Journal of Applied and Pure Mathematics*, hosted bouts of debate over the issue of how to define and delineate between the new "geometry" and "symbolical algebra". The Irishman and mathematician, George Salmon (1819-1904), as well as the Trinity mathematician, William Walton (1813-1901), engaged in direct debate over the matter in the pages of the *CMJ*. While Duncan Gregory (former editor of the *CMJ*) and Walton jointly argued that analytical representations of curves could be deemed valid even when certain aspects of those symbolical representations had no imaginable geometrical analogue (i.e. solutions to the analytic form of a circle could have imaginary as well as real values), Salmon contended that any analytical representation of a curve that diverged from meaningful geometrical representation should be deemed irrelevant and even absurd (Richards 1988, 52). The authors agreed to disagree, but their strained consensus come down to accepting a linguistic distinction between "algebraic geometry," in which spatial structure determines the realm of legitimate solutions, and "geometric algebra," in which symbolical equivalences determine the realm of possible solutions. The *CMJ* continued to host similar debates over the course of the 1860s and 1870s, and it was within this milieu of symbolical algebraic and geometrical debate that Clifford was trained to identity as a "geometer"—read, a "symbolical analytic" geometer.

As with Tait, Clifford's educational "terrain of knowledge" was the product of a profound transformation in curriculum that had been effected at Cambridge over the first half of the century. In an account of his Clifford's "geometry," Stephen Smith reveals the situated nature of Clifford's mathematical practices. Reconciling Clifford's "geometrical" understanding with his "symbolical algebraic" approach, Smith writes,

> Clifford was above all and before all a geometer. Nine-tenths, and more, of the contents of this volume [*Mathematical Papers*], including nearly all the systematic researches recorded in it, are geometrical. It is true that in the treatment of geometrical questions he shows a marked preference for symbolical methods; and, as might be expected, a marvelous command over analytical expression. It may even be true that the limitations involved in a scrupulous adherence to the methods of pure geometry would have been distasteful to him. Of his skills in the use of these special methods to the "Problems and Solutions" so liberally contributed by him to the *Educational Times* afford abundant proof. But among his more elaborate papers there is perhaps but one, the "Geometry of an Ellipsoid", which would satisfy purists of the school of Poncelet and Chasles, as being wholly free form the contamination of analytical methods; and even in this beautiful application of the method of stereographic projection—in the generalized sense in which that method is used in modern pure geometry—the "imaginary circle at infinity" occurs in the first sentence. But, whatever the method employed—and in variety of method Clifford takes an evident pleasure—the properties of space remain the perpetual subject of his contemplation. Purely analytical researches undertaken, without any impulse from or reference to geometry, are few and far between (Clifford 1882, xxxvii-xxxviii).

Over the course of his Cambridge career, Clifford approached elliptic and Abelian functions from an analytical-geometrical perspective, and he investigated a particular proposition of Poncelet's regarding conics, and a theorem of Cayley's regarding elliptic functions, to produce a paper entitled "On the Transformation of Elliptic Functions," which was both geometrical and analytical in nature. For Clifford, symbolical analysis drove geometrical investigation.

Clifford's early engagement with Poncelet and Cayley's descriptive (or "projective") approaches to geometry indicates the consequence of taking this approach. While staunch opponents of non-Euclidean geometry (including Cayley), saw a dichotomy emerge—one in which the mathematician had to choose between non-Euclidean geometry or projective geometry—Clifford acted as though no such dichotomy existed. His early musings on projective geometry were not tossed aside in favour of his non-Euclidean interests in later years; they were, rather, employed as complementary forms of investigation. As Smith concludes,

> [Clifford] was a geometer of a type peculiarly his own; and his dealings with the science were characterized by an amount of skepticism and an amount of faith which one would hardly expect to find combined in a mathematician (Clifford 1882, xxxix).

Clifford's ability to navigate through these varied conceptual environments without feeling the need to associate irrevocably with one branch of mathematics over another indicates that the Tripos-oriented, Cambridge mathematical "terrain of knowledge" upon which he trod differed significantly from Tait's.

Clifford's Cambridge publications, 1863-1871

When set against the background of Cambridge's tradition in symbolical analysis and its Tripos regimentation, as well as the emerging socio-political and biological discourses popular among Cambridge's students at the time, Clifford's contributions to professional mathematical journals in the years spanning 1863 (his entry to Cambridge) to 1870 (his departure, following his two-year fellowship) constitute artifacts demonstrative of the colourful education that Cambridge offered to its students from the mid-1860s onwards. Between 1863 and 1867, Clifford published 10 papers, most of which broadly mapped onto the subjects he was then learning. His earliest paper, "On Jacobians and Polar Opposites" (1863), published in *The Oxford, Cambridge and Dublin Messenger of Mathematics*, explores determinants. His second paper, "Analogue's of Pascal's Theorem," published in the same year in *The Quarterly Journal of Pure and Applied Mathematics* (1864), indicates Clifford's familiarity with Cayley's work in analytical geometry. It explores, Cayley's theorem that "*Every curve of the m^{th} order, (m not being less than n or p, nor greater than $n+p-3$), which passes through all but $\frac{1}{2}(n+p-m-1)(n+p-m-2)$ of the intersections of two curves of the n^{th} and p^{th} orders, passes also through the remaining intersections*" (Clifford 1882, 73).

By the time Clifford completed a year and a half of his undergraduate career, he had also produced a paper (published in two parts, in 1865 and 1866 respectively) under the general heading "Analytical Metrics." That article indicates Clifford's blossoming interest in the demarcation between different "types" of geometrical knowledge. It also indicates the sort of vocabulary Clifford had, by then, adopted and the mathematical culture into which he was being inculcated during that fluctuating curricular period in mid-Victorian mathematics. In the tradition of Cayley, Clifford introduces his "Analytical Metrics" paper by demarcating between two types of geometry. He writes,

> Any one must have observed that there are two kinds of theorems in Geometry; one kind having reference to *position* only, the other kind having reference to *magnitude*. Pascal's theorem is an example of the first, or *graphic* geometry; Euclid is an example of the second, or *metric* geometry. It may be possible to state the same theorem in two ways, so as to make it either metric or graphic. In such a case the graphic statement may be distinguished by the fact that it is *unaltered by projection.* In fact, the word *graphic* is co-extensive with *projective.* And so, bearing in mind the properties of projection, we may define Metric Geometry as that science which has to do with the magnitudes of angles, distances, areas, and volumes (Clifford 1882, 81).

Clifford goes on to highlight some of the misconceptions regarding geometrical developments over the previous few decades. He writes,

> It seems, at first sight, as if the method of coordinates, especially in its more complete homogeneous form, were almost wholly applicable to Graphic Geometry, and altogether unfit for the study of Metrics. And this idea is strengthened by the fact that nearly the whole of Graphic Geometry is due to the method of coordinates, while the science of Metrics has hitherto benefitted by it very little indeed (Clifford 1882, 81).

Clifford invokes the works of Jean-Victor Poncelet (1788-1867) to argue that all circles pass through the same two points at infinity, and that all angles and lengths can be expressed as "graphic functions" of those two points. He summarizes Ludwig Immanuel Magnus (1790-1861) in noting that a "linear transformation is virtually equivalent to projection, and that graphic properties are in fact those which are unaltered by linear transformation." All "graphic properties, therefore, may be stated in terms of a *finite number* of expressions," which include "any set of loci, the invariants, or functions of the coefficients, and covariants, or connected loci, which are unaltered by linear transformation." In sum, metric geometry can be analytically reduced to graphic geometry and graphic geometry is necessarily "of finite extent and exhaustible." This, Clifford contends, is the value of "Analytical Metrics" (Clifford 1882, 81). It is an approach that can be gleaned from Cayley's "Sixth Memoir Upon Quantics" (1859), he adds, noting that Cayley had argued metric properties were not inherent to any given figure. Rather, metrical relations (or properties) exist only in relation to an "absolute"—a curve that exists in a plane beyond the plane of the figure itself.

Clearly, Clifford's interest in projective geometries stems from his exposure to the works of Poncelet and Cayley, and Cayley's "solutions" to various problems in projective geometry formed a prominent theme in Clifford's early publications. For example, in his paper, "On Triangular Symmetry," published in *Mathematics from the Educational Times* (1865), Clifford expounds upon the projective properties of an equilateral triangle.[108] Although none of Clifford's papers up until 1867 constitute a major production, they did collectively demonstrate his engagement with the geometrical works of

[108] Clifford also demonstrated an early familiarity with Salmon's geometrical work in a paper published the following year in the same journal, entitled "On Some Extensions of the Fundamental Proposition in M. Chasles's Theory of Characteristics." In that paper, Clifford considered a variable system of two points on a right line that are related such that "when the second point is taken arbitrarily the first has a positions, and when the first point is taken arbitrarily, the second has b positions; then there are $a + b$ points on the right line at which the system of two points coalesces into one point" (Clifford 1882, 415).This principle was extended by Salmon in the case of two dimensions, Clifford notes. In his paper, Clifford attempts to further extend the principle to two directions and to systems of more than two points.

Poncelet, Cayley and Salmon, as well as his familiarity with those theories of dynamics taught to him through Routh's *Elementary Treatise on the Dynamics of a System of Rigid Bodies* (1860).

Meanwhile, Clifford's more popular discourses at the time indicate the degree to which Cambridge was altering his overall philosophical outlook on life. Clifford delivered an oration in 1866, upon his acceptance of the Trinity's College Declamation Prize, in which he offered a panegyric of the recently deceased Master of Trinity College, William Whewell. In that oration, Clifford recounted an apologue that reflected the nature of knowledge generation as he had then come to view it by then. Knowledge, he argued, is the product of willingness on part of practitioners to contemplate alternative theories and to nurture non-standard views in respective fields of study. In his oration, Clifford narrated the following allegory:

> Once upon a time—much longer than six thousand years ago—the Trilobites were the only people that had eyes; and they were only just beginning to have them, and some even of the Trilobites had as yet no signs of coming sight. So that the utmost they could know was that they were living in darkness, and that perhaps there was such a thing as light. But at last one of them got so far advanced that when he happened to come to the top of the water in the daytime he saw the sun. So he went down and told the others that in general the world was light, but there was one great light which caused it all. Then they killed him for disturbing the commonwealth; but they considered it impious to doubt that in general the world was light, and that there was one great light which caused it all. And they had great disputes about the manner in which they had come to know this. Afterwards another of them got so far advanced that when he happened to come to the top of the water in the night-time he saw stars. So he went down and told the others that in general the world was dark, but that nevertheless there was a great number of little lights in it. Then they killed him for maintaining false doctrines: but from that time there was a division amongst them, and all the Trilobites were split into two parties, some maintaining one thing and some the other, until such a time as so many of them had learned to see that there could be no doubt about the matter (Clifford 1886, 8).

Clifford's metaphorical account of the few fish willing to offer alternative hypotheses was a reference to the early generation of symbolical algebraists (of which Whewell had been a member). His use of the term "Trilobite," sounding as it does like a fossil record, was not arbitrary, either. Clifford's claim that the Trilobites lived long before six thousand years ago was intended to convey an evolutionary message, and it was meant to poke fun at those traditional church-based accounts of the age of the Earth. Clifford's philosophical shift from High Churchman to Darwinist acolyte was underway.

His philosophical transformation was no simple matter of swapping beliefs, however. As Pollock recalls, Clifford had become acquainted with Catholic theology soon after he arrived to Cambridge, demonstrating a thorough knowledge of St. Thomas Aquinas by the end of his first year. In matters of theological debate, Clifford had initially adopted a Catholic position and he even maintained it "with extreme ingenuity, not unfrequently [sic.] adding scientific arguments and analogies of his own" (Clifford 1886, 23). As a first year undergraduate, Clifford believed there was such a thing as theological insight (i.e. divine grace in Christian theology), which could give practitioners a sense of certain knowledge, even if that knowledge lies beyond the realm of experimental or scientific and observable data. That said, Clifford had already expressed distaste for "natural theology" and other Protestant attempts to scientifically justify God or religious belief through science. By his third year, however, his quasi-Catholic view of things had broken down, as well. Clifford began to favour the popular Darwinist sentiment then flooding the university's halls of residence. As Pollock recalls,

> For two or three years, the knot of Cambridge friends of whom Clifford was the leading spirit were carried away by a wave of Darwinian enthusiasm: we seemed to ride triumphant on an ocean of new life and boundless possibilities. Natural selection was to be the master-key of the universe; we expected it to solve all riddles and reconcile all contradictions (Clifford 1886, 24).

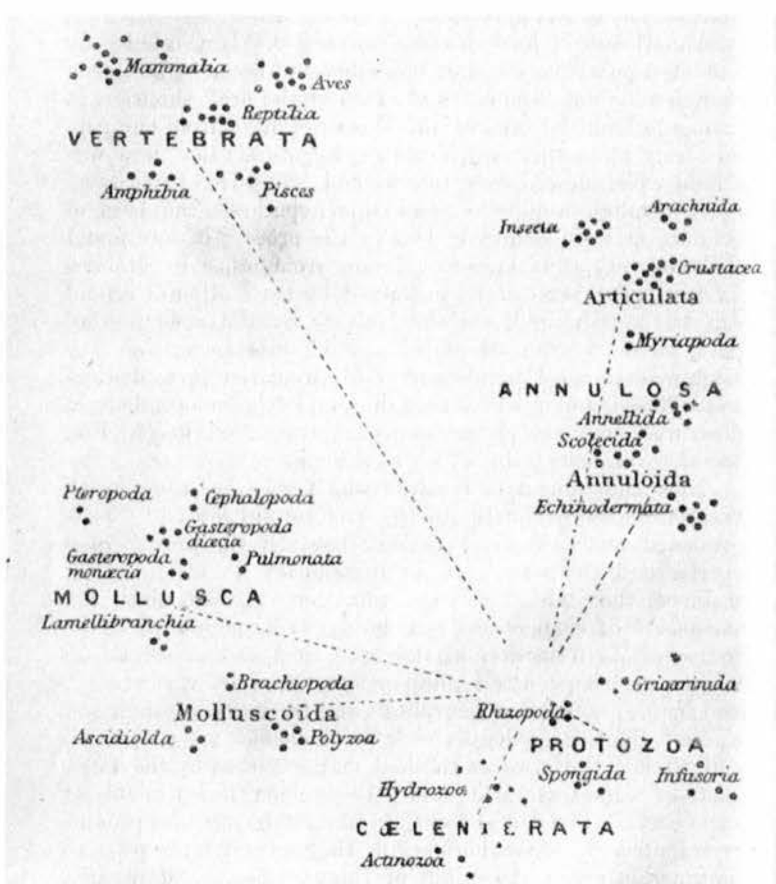
Darwinism was an ethical guide governing practice as much as it was an explanatory framework accounting for origins. It combined, "The exactness of the utilitarian with the poetical ideals of the transcendentalist," Pollock writes. Of the young men then partaking in this socio-philosophical phenomenon, Pollock adds, "We were not only to believe joyfully in the survival of the fittest, but to take an active and conscious part in making ourselves fitter" (Clifford 1886, 25).

Clifford expressed this mandate in more than a few public ways. In a talk delivered to the Royal Institution on March 6th, 1868, he associates his developing socio-philosophical views with his nascent opinions on "mental development." Entitled "On Some of the Conditions of Mental Development," the lecture formed Clifford's inaugural speech as a Trinity fellow. In his opening speech, Clifford claims the brain is constantly in a state of "change". He states,

> You will see that it is not even true that a character remains the same for a single day: the mind, leaves its mark, infinitely small it may be, imperceptible in itself, but yet more indelible than the stone-carved hieroglyphics of Egypt (Clifford 1886, 50).

The "character" of a person is the "history of the entire previous life of the individual"—a history that is continually in a "state of change." Clifford likens this to the change in the orbit of any given planet, which, he says, is seemingly constant but which, in fact, undergoes constant fluctuation as the planet travels through space. Clifford concludes all we can know of the constantly fluctuating nature of the brain is presented in the characteristics of the people we observe. There is, in other words, no constant "Law of force" that predetermines how an organism will develop nor how a planet will orbit.

Yet, if this is the case, and if all occurrences—biological or physical—are the result of random changes, then how can society ever delineate between good and bad changes?, Clifford wonders. The answer is found in the "Evolution-hypothesis." The "Evolution-hypothesis" is similar to Darwinism, he says, though it is not specifically "tied down" to the views of Darwin himself. Rather, something like a Darwinist account of the mind's material changes accounts for the development of new traits, including the capacity to entertain new ideas. Clifford admits he is assuming the growth of an individual can be treated in the same manner as the growth of a species. He is supposing that "the race of crabs has gone through much the same sort of changes as every crab goes through ... in the course of its formation in the egg" (Clifford 1886, 57). However, Clifford is keen to point out certain past "errors" adopted by others who had previously sought to advance the "evolution-theory." One of those errors is the claim that species, and thus individual members of a species, go through continual changes such that they can be organized into a "continuous chain, from the highest to the lowest [and] that the transition is gradual all through, and that nature makes no jumps" (Clifford 1886, 58). The Linnaean system, in other words, is a flawed one. Clifford argues it is so difficult to organize the development of species or their mutual relations that even a diagram (see below), such as the one Herbert Spencer had offered in his *Principles of Biology* (1864), cannot adequately represent the arrangement of existing organisms in a line or chain.



(Clifford 1886, 58)

The most that can be said is that the "Evolution-hypothesis"

> represents a *race* of animals or plants as a thing slowly changing: and it also represents these changes as connected by fixed laws with the action of the surrounding circumstances, or, as it is customary to say, the environment (Clifford 1886, 60).

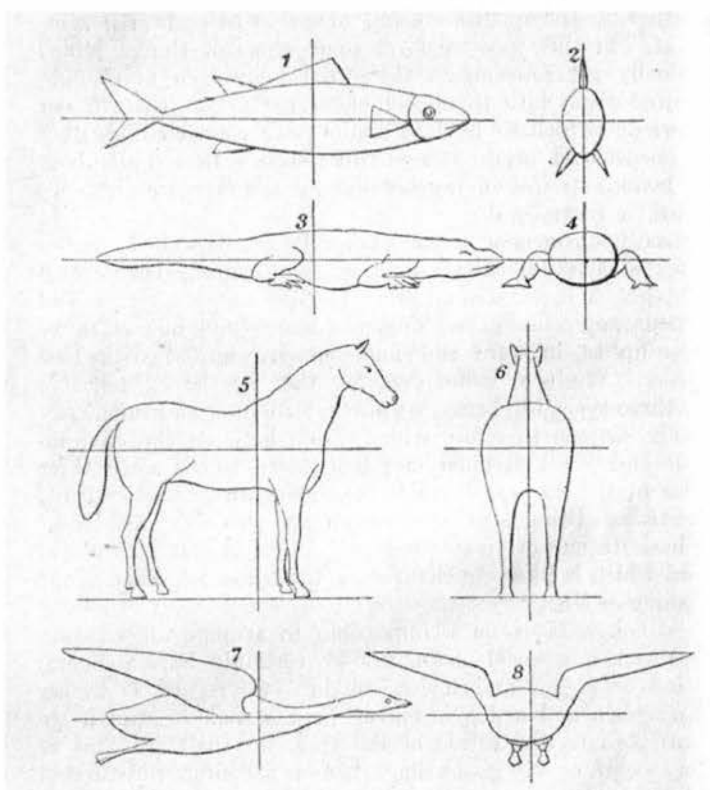
Clifford explains that the sorts of evolutionary changes that can occur are of two types: direct and indirect. The "direct" action of the environment—the climate and other environmental demands on our bodies—is clear enough. "Indirect" change is less obvious, though it is just as important. It is termed "natural selection" and, according to Clifford, it involves those traits that best allow for reproduction. Traits that hinder reproduction within a species constitute a "force" of nature that selects out the weaker organisms, "So that nature gradually weeds out all those forms which are not suited to the environment, and thus tends to produce equilibrium between the species and its surrounding circumstances" (Clifford 1886, 61). The results of these two types of change—direct and indirect—can be categorized into three groups: change in size; change in structure (or shape); and change in function.

Like the body, the brain undergoes evolutionary transformations, too. "Change in size" occurs with the acquisition of new knowledge, Clifford argues, suggesting that new brain matter increases with respect to surface size as new knowledge is acquired. "Change in structure" also occurs over time, as "ideas, which were only feebly connected, are aggregated into a close and compact whole." In other words, new neural connections come into existence as humans habituate themselves to, and learn about, particular relations between objects and ideas. Clifford writes,

> The ideas of several different qualities, for instance, which we never thought of as connected with each other, are brought together by the qualities being found to exist in the same object. In this way we form conceptions of things, which gradually get so compact that we cannot ... separate them into their component parts. Portions of our knowledge which we held as distinct are connected together by scientific theories; images which were scattered all about are bound up into living bundles by the artist, and so we find them rearranged (Clifford 1886, 63).

Lastly, "change in function" involves a process by which "men acquire faculties by practice." As Clifford concludes, "Without any conscious seeking, you must know how often we wake up, as it were, and find ourselves gifted with new powers" (Clifford 1886, 63). All three types of evolutionary change are evident in the case of the human mind, and all three are occurring on a continual basis.

This evolutionary understanding leads naturally to the ethical and moral query: What sort of change is the best sort of change for the mind to undergo? Clifford critically suggests that in asking "What is that attitude of mind which is likely to change for the better?" we must first ask "What is the meaning of *better*?" In other words, what changes lead to a better situation for the species or individual within a particular environment (Clifford 1886, 63)? Clifford's response is that changes for the "better" might be those changes that "differentiate" an individual from its surroundings, or those changes that "integrate" the individual with its surroundings (the image below indicates animals "integrating" and "differentiating," according to Clifford).



(Clifford 1886, 64)

Both processes can be beneficial (or detrimental) to a species, or an individual member of a biological group, depending upon circumstances. For instance, the first sign of human "consciousness," Clifford says, is the perception and recognition of difference (i.e. differentiation). When a "child's eyes follow the light, this colourless homogenous universe splits up into two parts, the light part and the dark part," he explains (Clifford 1886, 65). This differentiating process is important for the survival of the child. Yet, so too is integration. "Integration with the environment means close correspondence with it; actions of the environment are followed by corresponding actions of the animal," which allow the organism to survive within its environmental circumstances (Clifford 1886, 65).

The combined processes of integration and differentiation encompass the following possibilities: a separation of parts; a connection of parts; a separation from the environment; a closer correspondence with the environment; a separation from other individuals; a greater degree of sociality.

Taking these processes and applying them to mathematics, Clifford further argues the "Evolution-hypothesis" accounts for the development and acceptance of new theories in mathematics and natural philosophy. As our minds engage in the diverse processes of differentiation and integration, we find:

> more and more of our ideas are put outside of us and made real, [and] our minds are continually growing more and more into accordance with the nature of external things; our ideas become truer, more conformable to the facts; and at the same time they answer more surely and completely to changes in the environment; a new experience is more rapidly and completely connected with the sum of previous experiences (Clifford 1886, 67).

Importantly, he adds,

> The action of these two laws taken together does in fact amount to the creation of new senses. Men of science, for example, have to deal with extremely abstract and general conceptions. By constant use and familiarity, these and the relations between them, become just as real and external as the ordinary objects of experience; and the perception of new relations among them is so rapid, the correspondence of the mind to external circumstances so great, that a real scientific sense is developed, by which things are perceived as immediately and truly as I see you now (Clifford 1886, 67).

Thus, the "best" changes are not those that are forced by nature onto the organism, but rather "those produced by the spontaneous action of the organism" (Clifford 1886, 69). By encouraging spontaneous change within oneself, the human organism remains "plastic"—able to adapt ever more effectively to changing circumstances.

In citing examples from Huxley and Seeley that describe organisms in the seeming middle ground between "birds and reptiles," such as pterodactyls, Clifford explains that plasticity in size, structure and function is of the utmost importance in the long-term development and survival of a species (Clifford 1886, 70). By analogy, the human mind should seek to assimilate new and unusual ideas and it should also work to differentiate old, staid theories that have become dogmatic, uncreative and uniformly believed. Clifford states,

> If scientific, [the mind] must not rest in the contemplation of existing theories, or the learning of facts by rote; it must act, create, make fresh powers, discover new facts and laws. And, if the analogy is true, it must create things not immediately useful. I am here putting in a word for those abstruse mathematical researches which are so often abused for having no obvious physical application. The fact is that the most useful parts of science have been investigated for the sake of truth, and not for their usefulness (Clifford 1886, 70).

Clifford had in mind, here, those critics of Riemann, Helmholtz and non-Euclidean geometrical ideas in general, who had argued that the conceptual tools of non-Euclidean geometry were "useless" (this criticism will be discussed in greater detail later). Clifford publicizes the value of those supposedly "useless" mathematical and scientific speculations in his lecture on the "Evolutionary hypothesis" by issuing a clarion call to his fellow (young) Cambridge mathematicians, and Trinity college students. He states,

> If the mind is artistic, it must not sit down in hopeless awe before the monuments of the great masters, as if heights so lofty could have no heaven beyond them. Still less must it tremble before the conventionalism of one age, when its mission may be to form the whole life of the age succeeding. No amount of erudition or technical skill or critical power can absolve the mind from the necessity of creating, if it would grow. And the power of creation is not a matter of static ability, so that one man absolutely can do these things and another man absolutely cannot; it is a matter of habits and desires. The results of things follow not from their state but from their tendency. The first condition then of mental development is that the attitude of the mind should be creative rather than acquisitive: or, as it has been well said, that intellectual food should go to form mental muscle and not mental fat (Clifford 1886, 71).

Clifford's usage of the terminology of "differentiation" and "integration" reveals just how intertwined his views on the foundations of mathematical knowledge had become with emergent metaphors in the world of Darwinist Britain. For Clifford, the invocation of those terms was not arbitrary. His claim that the generation of mathematical knowledge is akin to the process of generating knowledge more broadly, and that, in both instances, the composition of the mind determines the nature of what is known, indicates a profound level of conventionalism shaped both by his mathematical education and his exposure to Darwinism. For Clifford, there is no direct perception of *à priori* universal rules or truths. There are only the perceptions obtained by a mind that is in constant evolutionary flux. Thus, all mathematical knowledge is in flux.

Following his inaugural speech, Clifford went on to produce a series of minor papers on the "theory of polars" and the "power of spheres." His most important fellowship contribution, however, came in the form of a 34-page unpublished paper, which his later mathematical biographer, Robert Tucker, entitled "On the Theory of Distances" (1869).[109] This is the first of Clifford's works in which knowledge of the geometry of Hermann Günther Grassmann (1809-1877) is mentioned. In it, Clifford indicates he had read an account of Grassmann's *Ausdehnungslehre* (1844) as published in *Crelle's Journal* in previous years. Grassmannian algebra constituted an aspect of the symbolical algebraic terrain of mathematical knowledge that Clifford engaged with near the end of the end of the 1860s. It is important, therefore, to offer a brief historical account of Grassmann's algebra so as to describe certain unique aspects of that terrain.

A brief account of Grassmannian algebra

Grassmann's father, Justus Günter Grassmann (1779-1852), had been involved in crystallography studies. Those studies created an environment replete with resources that his son, Hermann, used to re-formulate the "geometry of extension," for which the younger Grassmann later gained renown (Erhard Scholz, 1991). One of those resources included the concept of "intellectual construction," which was central to the elder Grassmann's philosophy of mathematics, as derived from Friedrich Schelling's *Naturphilosophie* (Marie-Luise Heuser, 1991). Justus Grassmann's life works largely

[109] Clifford's editor, R. Tucker, ascribed the year 1869 to the paper, which seems reasonable given that Clifford gave notes of the "preliminary" section of the paper to Olaus Henrici (1840-1918) at that year's BAAS, although sections of the unpublished paper may have been written subsequent to that date.

focused on geometry. In particular, they focused on the development of what he considered to be a new research field that would mediate between natural philosophy and mathematics—namely, combinatorial geometry. He used the notion of "intellectual construction" to categorize all mathematical knowledge into three groupings: arithmetic, geometry, and combinatorics. "Combinatorics" referred to the generation of dissimilar or "unequal" things from one another. This stood in contradistinction to arithmetic, which considered the generation of similar things from one another (e.g. numbers). "Intellectual construction" served as a defining motif in Justus's geometry textbooks.

Justus Grassmann also considered geometry to be based upon the two elements of length and direction. Length represented a geometrical characteristic of the object, while direction constituted a "combinatorial" aspect of it. By disregarding direction (i.e. the combinatorial aspect of length), it is possible to reduce all geometry to the study of one line. If we consider the objects used in combinatorics to be equal, then all combinatorics reduces to arithmetic. Justus viewed Euclidean geometry as having done precisely this, because it focused on length (i.e. "magnitude"), and ignored direction. However, Justus wanted a geometrical system in which *position* is privileged over length. He pled, therefore, for an axiom-free geometry; one in which all the notions of the geometry in question are generated by means of "synthesis"—i.e. construction.

The young Hermann Grassmann, therefore, found himself navigating through specific "terrains of knowledge" largely constructed by his father, as he worked to develop his own formal algebraical system. Those terrains included his father's mathematical philosophy of "intellectual construction," which served as the starting point for Hermann's own mathematical epistemology, and which Hermann expounds upon in the introduction to his *Ausdehnungslehre*. But Grassmann's "terrains of knowledge" also included the world of 19th-century German mathematics, which had been transformed in profound ways by Wilhelm von Humboldt's (1767-1835) educational reforms in earlier decades. Those reforms were aimed at avoiding rote learning and encouraging constructive thought. Another terrain that Grassmann navigated through includes the theological discourse of Friedrich Schleiermacher (1768-1834), which had been passed on to him through his theology training in Berlin, and which focused on the symmetrical division of elements into polar opposites.[110] Placed within these overlapping environments, the young Grassmann developed a system of directed magnitudes in *n*-dimensional space that he later called the "calculus of extension." Grassmann's work on "construction" and higher dimensionality in algebra is the outcome, therefore, of his conceptual navigations through mathematical, educational and philosophical terrains of knowledge (Lewis 1992).

The first two chapters of Grassmann's *Ausdehnungslehre* (1844) are devoted to the addition and multiplication of directed lines ("vectors" in Hamiltonian terms; "Strecke" in Grassmannian vocabulary).[111] In Grassmann's geometry, the product of two displacements (or directed lines) is a directed parallelogram. Because the multiplication of two directed lines involves a process by which one directed line segment is translated across a space delineated by a second directed line, the "product" traces out a continuous surface across a particular plane. And because both component lines have direction, so too does the resultant parallelogram surface. Grassmann introduces here the notion of a directed area. The plane area thus produced can be multiplied further, i.e. translated across a third vector. The resulting projection produces a parallelepiped, which has direction associated with volume.

At times, Grassmann relies upon geometrical imagery to help his readers understand the simple processes of construction underway here. But he soon departs from geometrical imagery when moving beyond the three dimensions of a directed volume. For Grassmann, geometrical imagery is only a heuristic device. The system is ultimately algebraical in nature and it can be extended to include *n*-dimensional spaces. Grassmann therefore makes the claim that, "Every vector of a system

[110] For an account that challenges the role Schleiermacher played in Grassmann's mathematical philosophy, see Schubring (1996).

[111] For a recent English translation of Grassmann's *Extension Theory* see Grassmann (2000).

of the m^{th} order can be expressed as the sum of *m* vectors, which belong to the *m* given independent manners of change of the system" (Crowe 1967, 69). Grassmann insists that geometrical images are not necessary for the establishment of formal meaning in his algebra. Geometrical imagery is of heuristic, not foundational, value. Grassmann, therefore, pushes farther in his text with a purely "formal", i.e. symbolical, version of directed-line multiplication in order to describe those products that lay beyond three-dimensions.

By the time Grassmann had written his *Ausdehnungslehre*,[112] he had come to view geometry as a subsidiary realm of mathematical thinking. As he wrote in the "Introduction" to his work,

> It had for a long time been evident to me that geometry can in no way be viewed, like arithmetic or combination theory, as a branch of mathematics; instead geometry relates to something already given in nature, namely, space. I also had realized that there must be a branch of mathematics which yields in a purely abstract way laws similar to those in geometry, which appears bound to space. By means of the new analysis it appeared possible to form such a purely abstract branch of mathematics; indeed this new analysis, developed without the assumption of any principles established outside of its own domain and proceeding purely by abstraction, was itself this science (Crowe 1967, 64).

Unlike many of his Kantian German contemporaries, Grassmann did not privilege the "intuition" of geometrical relations. Rather, he reiterates throughout the *Ausdehnungslehre*, the strength of his system lies in its purely "formal" (read symbolical) framework. "All principles that express views of space are entirely omitted, and consequently the beginnings of the science [are] as direct as those of arithmetic," he writes, adding "the limitation to three dimensions is omitted" (Crowe 1967, 64).

In sum, Grassmann's *Ausdehnungslehre* presents mathematical rules and mathematical products as constructed entities—not as truths about spatial ontology. As Grassmann explains, "Up to now one could not go beyond three directions; however in the pure theory of extension the number of directions can increase to infinity" (Crowe 1967, 66).

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[112] The full title of the work is *Die lineale Ausdehnungslehre, ein neuer Zweig der Mathematik dargestellt und durch Anwendungen auf die übrigen Zweige der Mathematik, wie auch auf die Statik, Mechanik, die Lehre vom Magnetismus und die Krystallonomie erläutert* (1844). It sold poorly. A letter from his publisher, sent in 1876, indicates that 600 copies were left unsold by 1864 and they were used as "waste paper"; the remainder, "a few odd copies," had been sold with the exception of one copy left for the publisher's library (Crowe 1967, 65).

Recall that one of the "terrains of knowledge" upon which Grassmann trod in his effort to develop his *n*-dimensional geometry of directed magnitudes was the theological philosophy of Schleiermacher, as expressed in that author's *Dialektik* (1839) (Lewis 1977). The *Dialektik* is a compilation of Schleiermacher's lectures at Berlin University from 1811 to 1831, which Grassmann attended from 1827 to 1830. In his book, Schleiermacher argues that mathematics is divided into the "real" and the "formal"—a dichotomy replicated to some extent in Grassmann's 1844 text between geometry and symbolical algebra. For Schleiermacher, the "real" includes those sciences whose content is based upon that which is "existent" (i.e. observable and inductive facts), while the "formal" includes those sciences that have as their objects concepts that are purely intellectual in nature. Of this latter group, "truth consists in the correspondence between the thought processes themselves," Scheiermacher argued (Crowe 1967, 65). Grassmann viewed his own algebraic system as a "formal" intellectual construction along the lines of Scheiermacher's definition. Although his algebraic system was, he noted, applicable to certain geometrical interpretations, it was ultimately independent of spatial truths in geometry.[113]

In his "General Theory of Forms," for example, Grassmann highlights the importance of establishing general, symbolical equivalences that apply to all branches of mathematics. Similar to the manner in which Peacock had argued for the "Principles of the Permanence of Equivalent Forms," Grassmann contends that formal algebraic rules can be intellectually generated, rendered internally logical, and deemed universally valid without reference to natural philosophy or empirical findings. The value of such construction is located solely in the operational relations manifest between the symbols in equations. As Grassmann claims,

[113] Invoking his father's previous terminology, Grassmann categorized mathematical objects into "discrete" and "continuous" (a division that Clifford later repeated in his own works). He also divided the results of mathematical operations into "equal" and "same". In his *Ausdehnungslehre*, Grassmann offers an account of "continuous, different mathematics," because within multi-dimensional space (which he considers "different", in that it invokes differing dimensions) points vary continuously (e.g. a varying point produces a line).

> By the general theory of forms, I mean that series of truths which relate to all branches of mathematics in the same way, and thus presume only the general concepts of equality and difference, connection and separation (Crowe 1967, 67).

Unlike his English counterparts, Grassmann envisioned the establishment of a mathematical hierarchy in which equivalent forms constitute a meta-level of mathematical thought, beneath which all other forms of mathematics branch out into subsidiary categories.

Recall that for Peacock and his colleagues, the categories of normal versus symbolical algebra simply indicate a bifurcated world in mathematics—two different branches of mathematical inquiry, with neither subsidiary to the other. Those branches are simply divided by the process of abstraction involved in each. For Grassmann, a hierarchy does necessarily emerge. He contends,

> The general theory of forms should thus precede all special branches of mathematics. Since however that branch does not as such exist yet, and since we cannot omit it without entangling ourselves in useless ramblings, we have no choice but to develop this subject insofar as we need it for our science (Crowe 1967, 67).

Grassmann viewed himself as the developer of an overarching meta-mathematics.

In creating this meta-level of symbolical analysis, Grassmann introduces the notion of "forms", which are purposely devoid of content. Grassmann first introduces the symbols a and b as related by the general equivalence,

$$a^\frown b = b^\frown a.$$

He also introduces,

$$(a^\frown b)^\frown c = a^\frown (b^\frown c) = a^\frown b^\frown c.$$

The "forms" are (a, b, c) and they are commutative according to whatever operation $^\frown$ signifies. The definition is a "synthetic connection," Grassmann explains. Having established such a synthetic connection, the mathematician can then go on to create "analytical connections" in which the connection of two forms occurs "in such a way that when the resultant form is synthetically connected with one of the original forms, the other original form results." In other words, if $\smile$ is taken to mean the analytical connection whereby $(a \smile b)$ connected to b results in a, one can write symbolically that:

$$(a \smile b) \frown b = a.$$

When forms obey this formal connection, they are the equivalent of traditional addition and subtraction. Grassmann's aim was to present the well-known processes of addition and subtraction as mere instantiations of more universal mathematical rules. Thus, arithmetic would become a sub-branch of an over-arching meta-mathematical domain of formalism.

Grassmann goes on in his text to develop a series of other "synthetic" and "analytical" connections. He concludes by noting similar forms (i.e. forms of the same order, such as two points or two vectors) generally result in forms of a similar order, at least when they are combined using the two formal relations noted above. When, however, forms of the same order are combined using universal synthetic connections to represent multiplication, or analytical connections, such as the one symbolized here:

$$\frac{a \mp b}{c} = \frac{a}{c} \mp \frac{b}{c},$$

which represents division, the results often differ in "kind" (i.e. in order). For example, vectors—as directed lines— are forms of the first order. Their product, however, is a form of the second order (i.e. a directed area). By allowing his "contentless" forms to adopt any value (e.g. numbers, points, vectors, or directed areas), Grassmann is able to generate 16 different types of symbolical connections, or equivalences, using the formal rule for the multiplication of forms stated above.

In his 1844 work, Grassmann also develops more fully his notion of "outer" and "inner" products in the multiplication of vectors. For Grassmann, the "outer product" of two vectors indicates the process by which one vector travels along the path dictated by another vector, thereby tracing out an oriented area. In algebraic terms, Grassmann defines this "connection" as one in which the base elements of the system (in this case a vector) are represented by $e_1, e_2, e_3 \dots e_n$ and where the following equivalences hold:

$$e_x e_y = -e_y e_x,$$

$$e_x e_x = e_y e_y = 0,$$

$$e_x(e_y + e_z) = e_x e_y + e_x e_z.$$

Grassmann's "outer product" allows for the generation of all sorts of "elements" at increasingly higher orders. For instance, he considers the multiplication of a plethora of vectors, $a.b.c.d.e \dots$, such that vector a travels along vector b, and the resulting directed area travels along vector c, and that resulting directed volume travels along vector d (resulting in a 4^{th}-dimensional directed "volume"), and so forth. Meanwhile, Grassmann's "inner product" relates "distances to fixed directions." It is the result of multiplying the lengths of two vectors by the ratio of the angle that separates them. The product is a non-directed number (i.e. "0", if the two vectors are perpendicular to one another). When the elements in question are two directed lines that lie in the same line, the "inner product" is simply the result of multiplying their respective magnitudes. In such instances, the "inner product" of vector multiplication coincides with normal multiplication in arithmetic. Grassmann thus produces two types of multiplication for elements within his formal system. The fact the "outer product" stops being analogous to conceivable geometrical representations once the products of the elements extend beyond three dimensions leads Grassmann to label the system "extensive algebra." He defines the system as one composed of rules that govern the behaviour of contentless formal symbols.

Due, in part, to the fact that Grassmann's works were received poorly and barely read, and due in part to the fact that among those German mathematicians who did attempt to read his work there was a pre-existing bias against the notion of a constructed meta-mathematics that could serve as a formal governor for all branches of mathematical inquiry, Grassmann's ideas were not adopted by any of his contemporaries in the 1840s. They were, however, criticized heavily. A revealing example of the sort of criticism lobbed Grassmann's way is found in a letter from E.F. Apelt (1812-1859), Professor of Philosophy at Jena, to A. Möbius, the German mathematician. In that letter, Apelt asks in typical Kantian form,

> Have you read Grassmann's remarkable *Ausdehnungslehre*? It seems to me that a false philosophy of mathematics is at the bottom of it. The essential character of mathematical knowledge, that it is intuitive, seems to be excluded from it completely. An abstract theory of extension such as Grassmann wishes, can be developed only from concepts; but the course of mathematical knowledge is found not in concepts, but in intuition (Lewis 1977, 108).

Apelt's concern was that Grassmann had constructed new mathematical entities based upon internally justified algebraic rules only. Appeals to the intuition of *à priori* geometrical "truths" are nowhere apparent. This alone set more than a few mathematical readers against Grassmann's work. In addition, Grassmann was not a mathematician by training. He had studied theology and philology at the Berlin University. He did not operate within any network of academic mathematicians. This too was an important factor resulting in the poor reception of his work. Grassmann did not have the professional legitimacy to advance his system through any institutional channels or the ability to institutionalize the approach by teaching it to classes of students, as Tait and Kelland later did with their quaternions in Edinburgh.

Yet, though Grassmann's work received little recognition or publicity in Germany, or elsewhere for more than 20 years after it was published, Hamilton did mention it in the "Preface" to his 1853 *Lectures,* as well as in letters to De Morgan. Hamilton had heard of Grassmann's text in 1852, while he was in the midst of writing his "Preface." Out of concern for priority rights, he procured a copy of the *Ausdehnungslehre* and began to work through it. His review was initially favourable, as he wrote to De Morgan on January 31st, 1853:

> I have recently been *reading* (and it is curious that sometimes, when otherwise in mental activity, I seem to myself unable to read a page, or almost a sentence of German) more than a hundred pages of Grassmann's *Ausdehnungslehre*, with great admiration and interest ... But it is curious to see how narrowly, yet how completely, Grassmann failed to hit off the Quaternions (Crowe 1967, 86).

Evidently, Hamilton sought to place Grassmann within the same tradition of mathematical development that he viewed himself as engaged in. But after his initially positive review, Hamilton came to view the German's contributions as a potential threat to his own status as progenitor of a non-commutative algebraic vector system. He quickly wrote to De Morgan a few days later stating,

> I am not quite so enthusiastic today about Grassmann as I was when I last wrote. But I have read through nearly all of what I could procure of his writings ... Grassmann is a great and most German genius; his view of *space* is at least as new and comprehensive as mine of *time*; but he has not anticipated, nor attained the conception of, the *quaternions* (Crowe 1967, 86).

Hamilton notes Grassmann's "inner product" has "much analogy to my *'scalar parts'* of a quaternion," and his "outer products" bears resemblance to Hamilton's *"vector parts."* Furthermore, "If the notion of combining them had occurred to him, *he might* have been led to the quaternions," Hamilton wrote, "but those he seems to me to have altogether failed to perceive" (Crowe 1967, 86).

Partly through Hamilton and Tait's recognition of Grassmann's work in their respective quaternion publications, and partly through the publication of sections of Grassmann's system in *Crelle's Journal* in the 1860s, more British practitioners became acquainted with the rudiments of the German's non-commuting vector system two decades after it was initially published.[114] Clifford was among them. He repackaged and reissued those small segments of Grassmann's work that he had become familiar with through *Crelle's Journal* and Hamilton's *Lectures.* In his paper on "distances," for example,

[114] See Crowe (1967) for an account of how Grassmann failed to garner support for his mathematical claims until just before his death in 1877.

Clifford invokes Grassmannian notation in using single large letters, such as A, B, C, to represent straight lines in a plane, and single small letters, such a, b, c, to represent points. Two large letters put together, as in AB, refers to the point of intersection of the lines A and B; two small letters put together, as in ab, refer to a line joining the points a and b. The equation $ABC = 0$ means the lines A, B, C meet at one point (a use of Grassmann's "outer product"), while the equation $abc = 0$ means that a, b, c lie in one line (a use of Grassmann's "inner product"). Clifford adds some extensions of his own to this notational system, which he finds "convenient." While an equation can be represented by its point-coordinates (x, y, z), where the degree of the equation is the "order" of the curve, Clifford finds it useful to represent the same equation in "contravariant, tangential, or line-coordinates" (ξ, η, ζ), where the degree of the equation is the "class" of the curve. Clifford claims his new Grassmannian-inspired notation can be used to investigate relationships between geometrical entities, such that "absolute values" for (xyz) or $(\xi\eta\zeta)$ are no longer needed. Their ratios can be discussed without reference to any absolute magnitude.

In the same paper, Clifford discusses the metric properties of figures in plane geometry which, he says, depends upon certain "circular points at infinity" denoted by i, j. His intention here to highlight the properties of figures in spherical geometry "upon the imaginary circle at infinity" (as denoted by O_2 or o_2). In so doing, Clifford combines aspects of Hamilton's vectors with Cayley's "Absolute" and Grassmann's directed-line multiplication. His aim is to offer algebraic expressions for "the distance of a point from a conic given tangentially" and the "distance of a line from a conic given by its points" (Clifford 1882, 132). Two different geometrical definitions can be obtained for each of these distances, where the "ratio" of the distances in question is a quantity that Clifford refers to as the "*distance of the curve from the absolute.*" The mathematicians' task is to algebraically obtain that ratio, and then translate it into a geometrical analogues. This is not a simple task, Clifford notes. In the case of a sphero-conic, the ratio of the two distances of a point or line is a quantity independent of the point or line. "But," Clifford writes, "I have as yet obtained no geometrical definition of it"

(Clifford 1882, 134). As with many of Clifford's productions up until 1870, this paper was left incomplete. Yet, it serves as an early historical indication of Clifford's navigations through that "terrain of knowledge" described here as Cambridge's symbolical algebraic geometry in the mid-1860s, which included, for Clifford at least, early (if incomplete) exposure to certain aspects of Grassmannian analysis.

Clifford as a fellow, 1868-1870

During his years as a Cambridge fellow, Clifford was under financial and professionalizing pressure. He needed to establish himself as a respected member of Britain's mathematical community so as to gain employment at one of the country's universities. To do so, he had to develop his professional career through publications. At the same time, he had to demonstrate his lecturing abilities. Thus, in addition to producing draft papers and small published articles, Clifford focused his fellowship on developing his teaching profile. As part of his socio-professional commitments, we find Clifford delivering lectures to women students at South Kensington in the spring and summer of 1869.

Throughout that course, Clifford explicates the bases of arithmetic and algebra, and he introduces nascent notions in non-Euclidean and vector geometry. He also teaches his students about "boundaries," indicating that boundaries distinguish between two adjacent regions of space—"one inside and one outside" (Clifford 1882, 628). The surface of any body belongs to both regions of space. "Congruency" is therefore defined by the boundaries of the objects in question; it means that a given body can fit the same region of space at all times. Furthermore, all geometrical entities—including the most basic particles of space—are bounded entities. A point is the intersection, or boundary, between two lines; a line is the "intersection," or boundary, between two surfaces, and so on into higher dimensional space.

Throughout the ten lectures, that Clifford delivers to his female students, geometry figures prominently and geometrical definitions are offered in primarily projective terms. Clifford describes a line as the path—i.e. the "locus of points"—that any given point produces as it moves through a plane. He describes a surface as the path of a line moving through space. He spends an entire lecture (i.e. Lecture Three) discussing the difference between "projective" and "non-projective" properties of a body. "Projective" properties are defined as those characteristics that remain constant "in the shadows of the figure," while non-projective properties are variable. In Lecture Four, Clifford explains projective "proportions," stating that if "four quantities are proportional, and the first is greater or less than any fraction of the second, the third is greater or less than the same fraction of the fourth" (Clifford 1882, 631).

The Lectures, as a whole, however, are not focused on geometry as distinct from algebra or symbolical analysis. Rather, the lectures focus on a symbolical approach to geometry. Clifford's lectures at South Kensington, therefore, constitutes a basic introduction to concepts in symbolical algebra and symbolical geometry. In other words, embedded in this simple course of lectures is a mathematical-philosophical outlook in which algebraic vocabulary constitutes a primary mode of expressing geometrical relationships. In Lecture Five, for example, Clifford introduces the "first principles of calculation" in which he elaborates upon the concept of "operation." "*Numbers* may be changed into other numbers by the operation of addition, subtraction, multiplication and division," he explains, preparing his students for the more complicated conceptions of "operation" and "transformation" that follows. Clifford then introduces the concepts of commutative and distributive multiplication (with regards to normal numbers). He notes these algebraic properties do not hold for all mathematical objects. As a practice problem, at the end of Lecture Five, Clifford asks students to "Write in symbolic language..." a given geometrical problem. Thus, Clifford sought to train his students in the same manner he had been trained. He emphasized the use of symbolical analytical methods in understanding geometrical relations, including the motion of geometrical entities, such as points, lines and surfaces in spaces of more than three dimensions. And although Clifford did teach his students about the Pythagorean Theorem, the properties of a circle, and the "shadows of a circle" (or conics), the South Kensington women were also expected to understand that "imaginary" solutions to curves are reasonable outcomes when the curves in question are situated within a plane of infinite directions.

By the end of the 1860s, Clifford's navigations through symbolical algebra and symbolical geometry were also being defined by another overlapping terrain—namely, Riemannian geometry. In his lecture, "Of Boundaries in General," delivered also in 1869,[115] Clifford demonstrates his emerging allegiance to a new foundational philosophy, as manifest in the empirical mathematical claims of Bernard Riemann (1826-1866). Clifford tells his audience to "forget that you have ever lived until this moment." He says,

> I want you not to believe a word I say, unless you can see quite plainly at the moment that it is true; and I shall try only to say such things as you can quite easily verify at once while you sit there. That is what I mean by asking you to forget that you have ever lived until this moment: for geometry, you know, is the gate of science, and the gate is so low and small that one can only enter it as a little child (Clifford 1879, 127).

Clifford explains that all bodies take up space, and in so doing they establish boundaries between "inside" and "outside." The separating element between those two regions is the "surface" of the body, which belongs simultaneously to the inside and the outside of a body. The fact there is no dividing entity between the "inside" and "outside" of any particular body allows for "continuity" in the physical structure of the universe. He explains,

> The idea expressed by the word *continuous* is one of extreme importance; it is the foundation of all exact science of things; and yet it is so very simple and elementary, that it must have been almost the first clear idea that we got into our heads (Clifford 1879, 134).

Simply put, "continuity" means that one "cannot move this thing from one position to another, without making it go through an infinite number of intermediate positions" (Clifford 1879, 134).

[115] Clifford delivered the other three essays in that collection at the Town Hall in Shoreditch in 1874. They were collectively published with the present lecture in a book entitled *Seeing and Thinking* (1879).

The notion of the "infinite" is often a sticking point, Clifford concedes. It is "a dreadful word, I know," but it generally means "without any end." It is this concept that, applied to space, justifies the notion of "continuity" itself, which stipulates that the motion of any body does not experience gaps in its own existence (otherwise, the motion would be "discontinuous"). "Continuity" is implied in the notion of any "movement," given that movement depends upon the infinite nature of space. When something moves through space, an infinite number of new surfaces are created in the process. In geometry, the notion of "congruency" emerges from this fact. Two "regions of space" are congruent when "a thing which exactly fills one of them can be made exactly to fill the other by moving it, without changing its size or shape." If the surfaces of the two regions overlap, they are identical. "Congruency" is the quality of two bodies that demonstrates they have the same size and same shape in different regions of space.

Using the combined notions of "congruency" and "continuity," Clifford presents the geometrical concepts of solid space (or volume), surface (or plane), line and point as "boundaries" between two regions of space and also as projective productions. Though a point has no dimension, it nonetheless divides space between where the point is and where it is not. If the point moves (continuously) across space, it produces a line, which is another surface between two regions of adjacent space. If that line then moves (continuously) across space, it creates a plane, which is yet another boundary between one region of space and another. If that surface moves (continuously) across space, it generates a solid volume, which is yet another boundary between the "inside" and "outside" regions of space. To use technical language, we could say a line is the "locus of the successive positions of a moving point," and we could make similar claims with regards to the other projected geometrical objects. The implication is that such projective objects, and their concomitant boundaries, can be extended to exist also in higher dimensional spaces, where they would constitute boundaries in *n*-dimensional spaces.

Clifford then proffers an account of his newly developing stance on the "empirical" nature of symbolical analysis. He explains there is no grand significance to the meaning of a technical term such as "locus." As with symbolical abbreviations, technical terms such as "locus" have real, empirical content, even though their use often suggests otherwise. He explains,

> I have laid some stress on this [the meaning of the term "locus"], because it seems to be a fair opportunity for warning you of a very serious danger: the danger of thinking that there is any mystery in a technical term. So long as you use it merely to save time and trouble, as an abbreviation, namely, for other simple words or phrases which everybody can understand, a technical word will be useful and harmless. But directly you begin to think that there is some hidden and mysterious meaning in it, which cannot be expressed in simple ordinary words that everybody could understand, there is no end to the nonsense that it will help you to think and talk. And when I have been using technical words, and am not quite sure whether I have been talking nonsense or no, I have one very safe way of finding out. I translate the whole thing into English, that is to say, into short easy words of Saxon origin (Clifford 1879, 144-145).

Unlike symbolical algebraists of the early- to mid-century, such as Peacock and De Morgan, or formalists such as Grassmann, Clifford did not consider symbolical mathematical equivalences as necessarily worthy in and of themselves. Rather, by the late-1860s, he had begun to elevate the empirical (including the geometrical) to the level of symbolical analysis in terms of inherent worth.

Thus, by the end of his fellowship, Clifford's training in Cambridge-style symbolical analysis had been overlaid not only by the analytical works of Grassmann, but also by the new "terrain of knowledge" composed by the empirical conceptions found in the works of Riemann and other "non-Euclidean" geometers. To clarify, note that Clifford's idiosyncratic twist to the issue of mathematical foundations appears in his argument that all mathematical forms of knowledge that can be spoken of, whether they are spoken of in "words of Saxon origin" or in symbolical terms, must have an empirical meaning. When such meaning cannot be located, the user is "talking nonsense." Such nonsense in mathematical symbolism had been generated in the past when mathematical practitioners pursued their craft without consideration for the natural philosophical or geometrical content filling the symbolatry used. Therefore, Clifford tells his listeners, they "must not imagine that the Latin word *locus*, as used in geometry, means anything more or less than the English word place" (Clifford 1879, 145).

Even the seemingly basic concept of "number" needs to be deconstructed so that misconceptions based on mystified notions of mathematical objects are done away with. "Number" refers to discrete entities—two apples, five points, eight people. By contrast, a "continuous" entity, such as a line, cannot be broken down into such discrete elements, as it is not composed of individual parts. A line is not a mere aggregate of points. No matter how many points we line up adjacent to one another, they will never equate to a "line," he explains. "The failure to make a line does not mean that you have not taken a large enough number, but that *number itself* is essentially inadequate to make points into a line," he states. "So you must be very careful to remember that a line is a different thing from the aggregate of all the points upon it; the points are on the line, but they are not the line itself" (Clifford 1879, 147). By extension, space is not made up of points or discrete parts, nor is a solid made up of an accumulation of distinct lines. The "projection" of geometrical objects is altogether different from the discrete accumulation, or "repetition," of parts. This highlights the difference in "kind" between geometrically continuous mathematical objects, such as lines, surfaces and space, and discontinuous entities, such as "number". Symbolism in analytical mathematics has, in the past, obscured these fundamental differences by representing all objects as undifferentiated and undefined symbolical entities.

Clifford invokes Riemann's account of geometrical extension as "singly," "doubly," and "multiply" infinite to clarify his point. The number of points on a line is "singly infinite," he explains, because it can only vary in one manner. Given that the number of points on a surface "is twice as infinite as the number of points on a piece of line," a plane surface is "doubly" infinite. By extension, the number of points in a solid is "triply" infinite. In brief, a point on a line can vary in one direction; thus, it has one variation. A point on a surface can vary in two directions; thus, it has two variations. A point in a solid can vary in three directions; thus, it has three variations. This terminology helps to identify position in space, Clifford explains. To identify a point on a line, one piece of information is required—namely, a description of the distance of the point from the end of the line, or from some other arbitrary point of origin. On a surface, two pieces of information are required—namely, a description of the point from two axes. In a solid, three pieces of information are required—a description of the point's distance from three arbitrary axes. In sum, discrete entities can be counted and quantified, while geometrically continuous entities can only be measured and located according to relative points of origin. This difference in "kind" (i.e. in spatial extension) of geometrical entities means they require new symbolical representations in their treatments—representations that may not, and need not, map onto the symbolical algebraic representations used for discrete entities (i.e. arithmetic).

This latter point indicates Clifford's Peacockian-inspired symbolical training in that the construction of new mathematical rules for new, or different "entities," is entirely justified and even laudable. However, Clifford's conclusion also represents those new terrains overlapping his symbolical and analytical training. In a revealingly critical tone, Clifford makes an allegorical reference to the views of those early symbolical algebraists for whom universal algebraic relations were worthy whether they had any geometrical analogue or not. He states,

> You may think it is beneath the dignity of human nature to spend all this time in contemplating the size and shape of a piece of wood. Very well; it is written in the fifteenth chapter of the Koran that when Adam was created all the angels were commanded to worship him. But Eblis, the chief of them, refused, saying, 'Far be it from me that am a pure spirit to worship a creature of clay.' And for this refusal he was shut out for ever from Paradise. Now the doom of Eblis awaits you if you fail to give due reverence to these little obvious everyday things—things that are true of every stone that lies on the pavement, of every drop of rain that falls from heaven, of every breath of air that fans you. Like him, you will find with astonishment that the creature of clay which you despise is the Lord of Nature and the Measure of all things, for in every speck of dust that falls lie hid the laws of the universe; and there is not an hour that passes in which you do not hold the Infinite in your hand (Clifford 1879, 156).

The allegorical reference to that which is made of "clay" refers to Clifford's attempt to rehabilitate the notion of the "empirical" knowledge in mathematics. While Cambridge mathematicians had already begun to use the linguistic dichotomy of "pure" versus "applied" mathematics, Clifford viewed the dichotomy as a false one. The "dust," or physical manifestation of "laws," is not a fallen version of the pure ideal of symbolical mathematical truth. Clifford issues a clarion call enjoining his fellow British practitioners to improve upon their lofty mathematical theorizations by studying the nature of the geometrical in order to justify their symbolical manipulations.

From 1870 to 1879, Clifford increasingly sought to defend the view that those algebraic rules that could be deemed to be "true" and "universal" were those that had a physical or geometrical underpinning. Clifford's developing philosophy of mathematics included the notion that mathematics is the result of inductive, empirical observations, rather than a rationalist abstraction or experimentalist *application* to physical and geometrical circumstances. In Clifford's lecture, "On Theories of the Physical Forces," delivered at the Royal Institution on February 18th, 1870, for instance, he argues the notion of "force" is presented as an outdated one. The continuous movement of bodies through space is an observable "fact"—not just a matter of symbolical "calculation." However, humans are limited in terms of their "causal" knowledge. Any appeal to "force" is a mistaken attempt to ascribe cause to that which is merely descriptive. As Clifford states,[116]

> Why does the moon go round the earth? When the Solar system was nebulous, anybody who knew all about some one particle of nebulous vapour might have predicted that it

[116] Indicating his familiarity with Humean skepticism, Clifford contends perceptual knowledge is problematical in that something as evident as "continuity" in space could be nothing but an illusion. Like a "wheel of life", in which a discrete series of images spinning around renders a discrete series of images seemingly continuous, our perception of continuity may in fact be the result of our inability to perceive discrete motion in space. Clifford states: "Here then is an apparently continuous motion which is really discontinuous; and moreover there is an apparently continuous perception of it which is really discontinuous—that is, it seems to be gradually changed, while it really goes by little jumps...and the question inevitably presents itself—is not every case of apparently continuous perception really a case of successive distinct images very close together" (Clifford 1879, 78). Although Clifford operates based on the assumption that continuity does exist, he does so only because a discontinuous world would have no room in it for *explanation* of any sort. It would not be possible, for instance, to associate any event with a pre-existing phenomenon, because discontinuities in space and time allow for infinite possible causes. In a discontinuous universe, in other words, dynamical processes could not be mathematically described, given that the entire framework of the calculus, which measures changes in motion in space over time, is fundamentally dependent upon the assumption of continuity both in space and time.

> would at this moment form part of the moon's mass, and be rotating about the earth exactly as it does. But why with an acceleration inversely as the square of the distance? There is no why; the fact is probably equivalent to saying that the continuous motion of one body is such as not to interfere with the continuous motion of another. If once so, then always; the cause is only the fact that at some moment the thing is so,—or rather, the facts of one time are not the cause of the facts of another, but the facts of all time are included in one statement, and rigorously bound up together (Clifford 1879, 84).

Phenomena have causes, but humans are not necessarily privy to those causes. The best we can do, is to generate models for any given phenomenon. Each model, however, can only describe what is empirically observed. To assume the inverse square law explains gravity, rather than just describes it, is to be misled.

Clifford expands on these views in a lecture to the Cambridge Philosophical Society delivered in the same year, in which he hypothesizes about the "fact" of rigid body motion. In that lecture, he associates the "why" of motion with Riemannian geometry. He argues,

> Riemann has shewn that as there are different kinds of lines and surfaces, so there are different kinds of space of three dimensions; and that we can only find out by experience to which of these kinds the space in which we live belongs (Clifford 1882, 21).

Although the axioms of plane geometry are true "within the limits of experiment on the surface of a sheet of paper, we know that the sheet is really covered with a number of small ridges and furrows." The total curvature of the sheet of paper is not zero. Clifford cites Riemann in saying that

> although the axioms of solid geometry are true within the limits of experiment for finite portions of our space, yet we have no reason to conclude that they are true for very small portions; and if any help can be got thereby for the explanation of physical phenomena, we may have reason to conclude that they are not true for very small portions of space (Clifford 1882, 22).

Clifford absorbs some of those Riemannian empirical-geometrical notions to offer a unique account of good mathematical practice on his own terms. He contends, for instance, that there is a possibility these "speculations may be applied to the investigation of physical phenomena." Specifically, Clifford hypothesizes,

1) That small portions of space *are* in fact of a nature analogous to little hills on a surface which is on the average flat; namely, that the ordinary laws of geometry are not valid in them.
2) That this property of being curved or distorted is continually being passed on from one portion of space to another after the manner of a wave.
3) That this variation of the curvature of space is what really happens in that phenomenon which we call the *motion of matter*, whether ponderable or ethereal.
4) That in the physical world nothing else takes place but this variation, subject (possibly) to the law of continuity (Clifford 1882, 21).

For Clifford, the non-flat nature of the physical universe may be the factual cause of the varied natural phenomena that are sometimes observed empirically to be discontinuous events (e.g. action-at-a-distance "forces," such as gravity).

By the end of 1870, Clifford had come to the conclusion that "geometry" itself is a physical science—an empirical art induced from observable facts that despite the fact it is symbolically expressed. Clifford's navigation through the terrain of Riemannian geometry had provided him with the conceptual resources required to talk about geometry as an *à posterior* science rather than an *à priori* intuition. As Clifford explains to his students at Trinity College in the last year of his fellowship,

> Geometry is a physical science. It is concerned with the sizes and shapes of objects, and the positions which they may occupy. The doctrines which it teaches on these subjects are derived from experience extended by hypotheses so as to become precise (Clifford 1882, 524).

Those hypotheses are threefold. The first two Clifford ascribes to Riemann; the last one he ascribes to Leibniz. Collectively, they include the hypothesis of continuity, which states that surfaces take up no room, so there is no space between adjacent bodies; the hypothesis of rigid motion, which states that as a body moves through space, it does not change shape or size; and, the hypothesis of infinite extent, which states that a point travelling along a line will never come back upon itself, nor would a line travelling along a plane. Assuming these three hypotheses hold, meaningful geometrical measurements can take place. For example, in analyzing the position of a point on a straight line, one can take a fixed point o, and a fixed length oa in a given direction from o, and identify the position of a point p on the line, by determining the ratio of op to oa and the side of o on which p is located. If μ denotes the ratio in question, then $op = \mu . oa$. Clifford explains μ can be understood to be a command to perform the following action: "Change the value of oa in the ratio of 1 to μ." The equation describes the fact that, in performing this action upon oa, one obtains the value of op, where op and oa can be regarded as "quantities of motion"—i.e. symbolical representations of continuous movement through space, as opposed to symbolical representations of discrete entities (Clifford 1882, 525). "Quantities of motion" are fundamentally *geometrical* and empirical in nature, Clifford concludes; they are based upon assumptions of continuity and infinite extent in space, even though their geometrical behaviour can be summarized in discrete symbolical form.

For example, the geometrical behaviour of line segments justifies the symbolical rules used, Clifford explains. The "quantity of motion," which carries a point from the position a to another position b, is a vector. It can be symbolized by ab. That vector is said to be equal to another vector cd "when b is on the same side of a that d is of c, and at the same distance from it," such that the lines have to have equal lengths and they must lie in the same direction (Clifford 1882, 525-526). The addition of such geometrical entities can be represented symbolically as:

$$ab + bc = ac,$$

or,

$$ab + bc + ca = 0.$$

If we keep in mind the fact that the symbols represent vectors, then we can determine that the ratio of two vectors is the "operation which changes the second into the first."

In Hamiltonian language, Clifford describes the same operation as containing two parts: "A *tensor* or stretching part which merely alters the length of the vector or the quantity of motion, and a *versor* or turning part, which either *preserves* the direction of motion or *reverses* it" (Clifford 1882, 526). If μ is the ratio of the "quantities of motion" in the vectors ab and cd, then:

$$\frac{ab}{cd} = +\mu \text{ if the vectors are in the same direction,}$$

and,

$$\frac{ab}{cd} = -\mu, \text{if the vectors are in different directions.}$$

The "operation -1" therefore causes the vector to reverse direction, which is the same thing as turning twice about a right angle. To turn the vector by only one right angle is to "halve" the operation above, though not in the sense as halving a discrete quantity.

Rather, "This operation of turning a vector through a right angle is denoted by the letter i." If aa', bb' are two equal lines that bisect one another at right angles at the point o, then we can describe their geometrical relationship to one another symbolically by stating,

$$oa' = -oa,$$

$$ob' = -ob.$$

and,

$$ob = i.oa,$$

$$oa' = i.ob,$$

$$ob' = i.oa' = -i.oa,$$

$$oa = i.ob'.$$

Therefore,

$$i^2 = -1.$$

Clifford concludes by explaining the symbolical representation of a complex entity, such as $x + iy$ (or $r(cos\phi + i\ sin\phi)$ more generally), describes vectorial behaviour in space. Clifford underscores the fact that vectors do exist and they behave in particular ways. The symbolical rules describing their behaviour emerges inductively from observing their geometrical traits. In idiosyncratically applying his Cambridge training in symbolical analysis, Clifford concludes that although symbolical rules are conventional, they are useful only when they describe things that can be empirically observed in geometry.

Non-Euclidean geometries in Britain

The history of non-Euclidean geometry in Britain is, in fact, a history of non-Euclidean geometr*ies*, given that none of the geometries in question, or their concomitant philosophical and scientific commentaries, were introduced in Britain in anything like a coherent package of accepted ideas. The terrain of non-Euclidean geometries that Clifford trod upon was a varied and diverse one. It exposed him to the works of Nicolai Lobachevsky[117] (1792-1856) and Riemann, as well as the physiological, biological and philosophical arguments accompanying those works, as manifest in debates between Hermann von Helmholtz (1821-1894) and J.P. Land throughout the mid-1870s.

In his late 19th-century retrospective account of the rise of non-Euclidean geometry, the mathematician George Bruce Halstead (1853-1922) has dichotomized the history of non-Euclidean geometry into two opposing views. The first was advocated by the "Anti-Euclideans," which included mathematicians such as M.L Bertrand of Geneva, Legendre, M. Vincent, and Ed. T. Dixon, who tried "to convict Euclid of imperfection by offering short proofs of his celebrated Parallel-Postulate." The second was advocated by practitioners who were "the true Non-Euclideans," including Lambert, Janos Bolyai (1802-1860), Lobatschewsky [sic.], Riemann, Helmholtz, Sophus Lie (1842-1899), Felix Klein (1849-1925), Clifford and Cayley, all of whom were "ardent admirers of Euclid," but all of

[117] There are many variations on the spelling of Nicolai (sometimes Nicolas) Ivanovich Lobachevsky. I have chosen to use the spelling adopted in Halsted's short biography of the mathematician (Halsted 1894).

whom were also "makers of two companion geometries, called usually Lobatchewksy's and Riemann's" (Halsted 1894, 150). Halsted's account is a Whiggish categorization of the place of actors in the development of non-Euclidean geometries, especially as they practiced their respective crafts in Great Britain. His bifurcation of geometrical history into these two categories of practitioners assumes concerns over the legitimacy of the parallel postulate and the construction of alternative geometrical models were necessarily distinct fields of inquiry. For someone such as Clifford, however, this was not the case. Both queries also motivated actors such as Lobachevsky, Bolyai and Riemann.

In his canonical account of the history of non-Euclidean geometries, Roberto Bonola (1955) has offered another account of non-Euclidean geometry. Bonola views new geometers such as Lobachevsky, Bolyai and Riemann to be practitioners who developed their respective concepts as the direct result of geometrical queries posed by their predecessors. This unbroken line of inquiry, Bonola argues, resulted in three types of non-Euclidean geometry: hyperbolic, elliptic, and parabolic (Bonola 1955, 199). A linear account of geometrical history is particularly evident in Bonola's account of "Clifford parallels." Here he writes that Clifford wrote within a tradition that had queried the nature of Euclid's parallel lines, where Euclid's parallels possess three properties: coplanarity, no common points, and equidistance. By ignoring the last property, one could simply construct the geometries of Gauss, Lobachevsky, and Bolyai. One finds that, in so doing,

> The parallels which correspond to [the new geometry] have very few properties in common with the ordinary parallels. This is due to the fact that the most beautiful properties we meet in studying the latter depend principally on the [equidistance] condition. For this reason we are led to seek such an extension of the notion of parallelism, that, so far as possible, the *new parallels* shall still possess the characteristics, which, in Euclidean geometry, depend on their equidistance (Bonola 1955, 200).

From Bonola's point of view, Clifford chose to develop a concept of parallels that lauded equidistance but which ignored the first two properties stated above. For instance, in his "Preliminary Sketch of Bi-quaternions" (1873), Clifford writes that parallels ought to be defined as straight lines in either the same or differing planes, but of which the points of one are always equidistant from the points of the other. Two cases arise as a result. When the lines lie in the same plane there is nothing other than the instantiation of ordinary Euclidean geometry. When the two lines do not lie in the same plane, the parallel lines can only reside in Riemannian space (or elliptic space)—that is, space in which the internal angle sum of a triangle is greater than 180°. Clifford summarized this finding by saying that if two straight lines are equal and parallel, and their extremities are joined, the result is a "skew parallelogram" (Bonola 1955, 206). On Bonola's view, Clifford's contribution to non-Euclidean geometry is the direct result of his engagement with the parallel postulate problem, which was presented to him as a coherent set of concerns over the properties of parallels.

Clifford's conceptualization of parallel lines in "elliptic" space was not, however, a direct response to the long-standing problem of how to define parallels. Nor was it a direct response to the question of how to develop differing systems when the various properties of Euclidean parallels are tampered with. Rather, Clifford's 1873 paper on bi-quaternions was the combined outcome of his navigations through those varying terrains of knowledge identified earlier, only one of which included the vocabulary and conceptual resources provided to him by Lobachevsky, Bolyai and Riemann. Clifford's engagements with non-Euclidean geometries were not limited to the terms of pre-existing discourses on the problem of "parallels". Nor were those discourses offered to him in the form of a pre-packaged problem with a predefined set of potential responses. As Gray (1979) has demonstrated, the ability to draw on analytical language in the 19th-century changed the manner in which geometrical problems were approached, discussed and analyzed. Not least among those "problems" was the famed parallel postulate in Euclidean geometry. Although this point is "glossed over in the expositions which treat the history [of non-Euclidean geometry] as belonging to 'pure' geometry and foundations," the use of analysis played a crucial role in providing new resources to describe the potential behaviour of geometrical objects. The works of Lobachevsky and Bolyai are important precisely because they were "thoroughly analytic," Gray contends (Gray 1979, 243). Those two mathematicians rendered the formulae of spherical trigonometry independent of the axiom of parallels; they defined a surface in space that differs from a plane in that its "parallel lines" extend into three-dimensional space. Those practitioners also constructed formulae for triangles that hold true on their hyperbolic surfaces, and which are analogous to formulae for spherical surfaces; and they concluded from their own formulae that the angle sum of a triangle on such a surface is less than $180°$.

Bolyai's contributions were first published in his father's *Tentamen*, a work that emerged in the early 1830s. Lobachevsky's works were first published in Russian in small publications in Kasan; later, in 1840, he published a larger text in German. In part, Bolyai was interested in seeking out the commonalities that existed between Euclidean and non-Euclidean geometries, while Lobachevsky was interested more in the elaboration of those non-Euclidean properties that he had generated himself. Neither author's works were widely adopted at the time, partly due to their respective locations on the periphery of European institutions (Lobachevsky was a mathematician in Kasan, while Bolyai was the son of a Hungarian mathematics teacher). The medium each author used to disseminate his respective works is also revealing. Lobachevsky published his early works in the *Kazan University Messenger* in 1829 and 1835. The *Messenger* was not a widely circulated publication. A truncated version of his text was later published in a French translation in 1837, but it too was largely ignored. Lobachevsky then published a German text, *Geometrische Untersuchungen zur Theorie der Parallellinien* (1840), which constituted the summation of a course of lectures he had delivered at Kasan University. However, in it, the author chose not to discuss the three-dimensional non-Euclidean figures he had discussed in his other, less circulated works.

Bolyai's work, meanwhile, appeared as a Latin appendix to his father's text, *Tentamen.* The *Tentamen* itself focused on the foundations of geometry, arithmetic, algebra and analysis. Farkas Bolyai (1775-1856) was not an unknown figure; through his studies at Jena and Göttingen in previous years he had become a friend of Carl Friedrich Gauss (1777-1855), the well-connected and well-known German mathematician. Consequently, Gauss did, in fact, read the young Bolyai's appendix and, in a letter to his father, praised the work as reflecting some of his own sentiments on the matter of the parallel postulate (including the possibility of alternative geometrical models). Yet, Gauss's moral support was not enough to generate professional recognition for the young Bolyai's efforts. Bolyai's work remained relatively unknown in Britain until the 1860s (for a detailed account of Bolyai's work, see Gray (2003)).

In their respective efforts, both Lobachevsky and Bolyai perceived themselves to be doing something in the geometry of empirical space. Lobachevsky, for example, called for astronomical experiments to empirically verify the structure of space (Gray 1979, 249). Though this explains little to nothing about why the works of these two authors were ignored for so long, it does help to explain why practitioners, such as Clifford, found Bolyai and Lobachevsky's works so useful, given that the British terrains upon which they operated in later years were ones in which an increasingly dominant experimentalist community of laboratory scientists had re-evaluated the empirical origins of knowledge. Clifford's own mathematical navigations had already led him to adopt non-standard techniques in measuring motion in space, i.e. quaternions and Grassmannian formalism. The adoption of non-standard metrics in geometry would not have seemed radically out of place.

Noteworthy also is the fact that alternatives to the Euclidean account of parallel lines had also entered into British discourse in the 1860s, including Philip Kelland's 1863 lecture to the Royal Society of Edinburgh, entitled "On the Limits of our Knowledge respecting the Theory of Parallels."[118] In that paper, Kelland recounted a publication in *Crelle's* journal some years earlier, entitled "Imaginary or Impossible Geometry," which had discussed the possibility of a geometry in which the angle sum in a triangle was less than two right angles. Kelland was referring to Lobachevsky's book.

[118] Richards (1988) cites the author as being "John Kelland". However, there is no record of a John Kelland at the University of Edinburgh. The citation in the *Transactions of the Royal Society of Edinburgh* merely states "Professor Kelland." It was, in all likelihood, the professor of mathematics, Philip Kelland, who delivered this paper to the society.

Although Kelland had not read the original book, he did find that he was unable to refute the impossibility of the "acute angle hypothesis" on his own. Nonetheless, he argued the limits of the hypothesis were too narrow to take seriously and thus of insignificant value (Richards 1988, 71-72). More prominently, Arthur Cayley published a "Note on Lobachewsky's [sic.] Imaginary Geometry" in the *Philosophical Magazine* (1865). In his paper, Cayley called Lobachevsky's work "curious," concluding that he would like to see the full working out of a geometrical interpretation based upon the notion that the angle sum of a triangle could be less than two right angles. This was likely the first paper that exposed the young Clifford (who was still a student of Cayley's at Cambridge at the time) to the notion of non-Euclidean geometries.

The burgeoning of interest in non-Euclidean geometries that occurred in Britain in the latter half of the 1860s was due, however, to the emergence of a series of other publications. The first was Gauss's correspondence with the astronomer H.C. Schumacher (published in volumes from 1860 to 1863). In his letters, Gauss had written to Schumacher in 1844 mentioning Lobachevsky's work. He also noted that he had contemplated similar ideas decades earlier. Gauss's professional support, now issued in a published form, helped to legitimatize Lobachevsky's little-known mathematical speculations, sparking new interest in Britain and Europe more generally. A few years later, Guillaume-Jules Hoüel (1823-1886) published his French translation of Lobachevsky's work in *Études Géométriques sur la Théorie des Parallèls* (1866). One year later, Riemann's *Habilitationsvortrag*, which had originally been delivered to the faculty at the University of Göttingen in 1854, was published in its original German as "Über die Hypothesen, welche der Geometrie zu Grunde liegen" in the *Abhandlungen der Konglichen Gesellschaft der Wissenschafter zu Göttingen* (1867). By the mid-1860s, therefore, a slew of "new" literature featuring non-Euclidean geometry had emerged in both French and German, along with two smaller notices by Kelland and Cayley in English.

This quasi-legitimization was further bolstered in a series of interpretive accounts set out by Sylvester, who mentioned higher dimensional spaces and Riemann's 1866 paper in his 1869

Presidential Address to Section A of the BAAS. Meanwhile, Helmholtz, who had been active publishing popular accounts Gauss, Lobachevsky, Houel and Riemann's geometrical ideas in German journals, followed up by publishing his popular account, "The Axioms of Geometry," in English in *The Academy* starting in 1870. Helmholtz then played a prominent role in popularizing Riemannian geometry in Britain through a series of articles published in *Mind.* Clifford followed suit soon after, invoking Riemannian and Lobachevskian claims in his work with elliptic geometrical models in his papers on bi-quaternions. Along the way, Clifford also offered lectures to the BAAS, the Sunday Lecture Society, and the Metaphysical Society in which he elaborated upon the physiological justifications for non-Euclidean geometries.[119] Thus, by the early 1870s, a series of publications and speeches across Britain had highlighted, criticized and lauded "non-Euclidean" geometries.

Recall that Clifford had engaged with Riemann's "elliptical" geometry and *n*-tuple manifolds as early as 1869. An account, therefore, of Riemann's geometry is useful in explaining why this was the case. In the 1850s, it was Riemann's mentor, Gauss, who had encouraged the young acolyte to prepare a text on geometry for his *Habilitationsschrift* (possibly Gauss had Bolyai and Lobachevsky's works in mind at the time). In his *Habilitationsschrift*, Riemann talked about the "darkness" in geometrical analysis that had spanned centuries. He had in mind the particular cases of surfaces with constant positive curvature (i.e. the exterior surface of a sphere). By contrast, the regular Euclidean plane is a surface of zero curvature. It is flat in all directions. The interesting comparative characteristic to note about surfaces of zero curvature, such as a plane surface, and surfaces of constant curvature, such as the surface of a sphere, is that any object described upon either of those surfaces can be moved about with no alteration to its size or shape. No distortion occurs as a triangle on a plane is moved about the plane. And no distortion occurs when a triangle on a constantly curved surface is moved along that surface. From Riemann's perspective, the issue is a geometrical one in that one's "metric", i.e the system of measurement being used, must be able to distinguish between flat and

[119] For an account of the immediate response—in particular the critical responses—of authors such as William Stanley Jevons to popular accounts of geometry see Richards (1988).

non-flat surfaces when the person carrying out the measurement is limited to the surface in question. It is simple to observe such differences between a flat plane and a spherical one when the observer observes the surfaces from a third dimension, i.e. external to the surfaces in question. It is not as simple to observe those differences when the observer is located upon the surface in question, Riemann explains. In spaces of constant curvature, for instance, the sum of all triangles is determined when the sum of one triangle is determined.[120]

Clifford's engagement with Lobachevsky's work, meanwhile, first occurred in 1870 only a little later after his initial reading of Riemann. Clifford wrote from Trinity College on April 2nd, 1870, to say:

> Several new ideas have come to me lately: First I have procured Lobachevski, 'Études Géométriques sur la Théorie des Parallels'—a small tract of which Gauss, therein quoted, says: L'auteur a traité la matière en main de maître et avec le véritable esprit géométrique. Je crois devoir appeler votre attention sur ce livre, dont la lecture ne peut manquer de vous causer le plus vif plaisir ... It is quite simple, merely Euclid without the vicious assumption, but the way the things come out of one another is quite lovely (Bonola 1955, 8).

Clifford reproduces Lobachevsky's claims in a short set of lecture notes used for his mathematics classes at UCL. In those notes, Clifford lists a series of characteristics (15 in total) that are common to both Euclidean and the Lobachevskian geometries. The properties include items such as "two straight lines cannot cut in two points" and "a straight line may be produced indefinitely" (Clifford 1882, 531). Thereafter, Clifford offers an in-depth discussion of hyperbolic spaces in a series of lectures delivered in 1873 and entitled "Philosophy of Science."

[120] The history of the constant "k" is one that deserves more attention. By looking at the manner in which that constant was introduced in Lobachevsky and Bolyai's works, the manner in which their respectively interpreted that symbols, and the manner in which later practitioners *used* that symbolical concept in attempting to discuss problems of important to their own fields of research, the historian may better be able to offer a social history of geometrical conceptualizations from the 1820s to the 1870s. Eugenio Beltrami (1835-1899) was able to take this Riemannian concept of curvature and further mathematize it (something Riemann was limited in doing due to the public nature of his *Habilitation* (Gray 1979). The imaginary sphere with radius $\frac{i}{k}$ is the surface of constant curvature that has a "curvature" determined by $K\ (curvature) = (\frac{i}{k})^2 = \frac{-1}{k}$ (Gray 1979, 246).

Empirical mathematics in the 1870s

Clifford's navigations through Cambridge's symbolical algebra, including his exposure to Grassmannian algebra, and his brief encounters with Riemann and Lobachevskian geometry, as well as his early exposure to the "terrain" of Victorian Darwinism, had produced a complex topology of interests that motivated the young mathematician's choices. Those "terrains" were manifest, for instance, when, just over a year after arriving to teach at UCL, Clifford delivered a lecture to the BAAS, in Brighton, on August 19th, 1872, entitled, "On the Aims and Instruments of Scientific Thought." In that lecture, the "applied mathematician" argues there is an important epistemological difference existent between highly technical computations and "predictions." Building a bridge, and expecting that it should stand based upon past experiences of bridge-building, does not constitute a prediction. It is, rather, the outcome of a skilled calculation and repeated practice. "Scientific prediction" requires speculation of truly unknown circumstances. Clifford claims,

> The difference between scientific and merely technical thought ... is just this: Both of them make use of experience to direct human action; but while technical thought or skill enables a man to deal with the same circumstances that he has met with before, scientific thought enables him to deal with different circumstances that he has never met with before (Clifford 1886, 88).

True "prediction" centres on speculation of the unknown, not repetition of the known. The exemplar here is Hamilton's account of canonical refraction in certain crystals, Clifford says, noting that the Irish mathematician came to make his "prediction" by considering the ramifications of Fresnel's theory with regards to the perception of crystals from particular directions. No previous knowledge of such refraction had existed. Predictions are to be lauded, therefore, because they generate genuinely new knowledge.

Yet, Clifford admits predictions are also always hampered by the same limitations that hamper technical measurements more broadly—namely, the limits of human perception. The degree to which humans can measure events accurately depends upon their ability to perceive. Scientific instruments also depend upon human perceptions in being calibrated. Even measuring macroscopic phenomena, such as gravity, can be problematical. Two falling bodies do not fall towards the centre of the Earth at exactly the same speed (due to factors such as gravitational pull, location in space, the tides of the sea and the positioning of the sun, the moon and the other planets, etc.). The deviation between respective gravitational speeds is too miniscule for humans to perceive, Clifford argues. The "theoretical" meaning of "exactness" appeals, therefore, to a sort of super-human observational capacity. Clifford states,

> The practical meaning [of "exact"] is only very close approximation; *how* close, depends upon the circumstances. The knowledge then of an exact law in the theoretical sense would be equivalent to an infinite observation (Clifford 1886, 94).

Historically, mathematicians have claimed "theoretically exact" knowledge exists in geometry and mechanics. "If this had been said to me in the last century, I should not have known what to reply," Clifford concludes.

However, by 1872, Clifford tells his audience, no such belief is any longer tenable. Rather, the foundations of geometry had been thoroughly criticized by two mathematicians, including Lobachevsky and "the immortal Gauss." Here, Clifford cites Hoüel's 1866 French translation of Lobachevsky's German text, *Geometrische Untersuchungen zur Theorie der Parallellinien* (1840). He also cites Gauss's letter to Schumacher (Nov. 28, 1846), in which the German mentions his recent encounter with Lobachevsky's geometry (a decade after having been exposed to Bolyai's account), and in which he praises the work as creative and original. Clifford states Lobachevsky and Gauss's works had recently been "extended and generalised" by Riemann and Helmholtz.[121] He cites Hoüel's 1866-67 translation of Riemann's *Ueber dis Hypothesen welche der Geometrie zu Grunde liegen,* as it had appeared in the *Annali di Matematica* (this being the same paper Clifford would later translate into English in 1873). And he refers to Helmholtz's English account, "The Axioms of Geometry," as

[121] In this lecture, Clifford nowhere mentions Bolyai or Beltrami.

published in *Mind.* The conclusion that mathematicians ought to draw from these varied reconsiderations of geometrical foundations is that

> although the assumptions which were very properly made by the ancient geometers are practically exact—that is to say, more exact than experiment can be—for such finite things as we have to deal with, and such portions of space as we can reach; yet the truth of them for very much larger things, or very much smaller things, or parts of space which are at present beyond our reach, is a matter to be decided by experiment, when its powers are considerably increased (Clifford 1886, 94).

Clifford is keen to "make as clear as possible the real state of this question," because, "it is often supposed to be a question of metaphysics, whereas it is a very distinct and simple question of fact" (Clifford 1886, 94).

There is a difference, he explains, between "exactness" and "universality." What is universally perceived is not necessarily "exactly" true. The deviation of perceived measurement from any particular theoretical (i.e. exact-truth) claim might be "inconceivably small, which no experiment could detect." Yet,

> Between this inconceivably small error and no error at all, there is fixed an enormous gulf; the gulf between practical and theoretical exactness, and, what is even more important, the gulf between what is practically universal and what is theoretically universal (Clifford 1886, 95).

A law is "practically universal" when it is as exact, or more exact, than all measurements obtained experimentally. A law is "theoretically universal" when it is exact in all instances. This latter definition is, in many ways moot, Clifford argues, because "this is what we do not know of any law at all."

Clifford accepts, therefore, that mathematical-scientific laws can be established, even though, at root, they are only ever fantastic approximations and never absolute models. Furthermore, in that infinitesimal region between exactness and error, there resides an infinite number of possible alternative explanations of the particular phenomenon being explained. The laws of gases, electricity, and magnetism, he says, have all been treated in "statistical" ways, such that any observed measurement is dependent upon the average number of molecules behaving in any particular manner. Clifford argues even "the law of gravity" is dependent upon the statistical behaviour of molecules in space. In other domains of science, too, we find that we must constantly revise our notion of "exact" and adopt a more conventional approach to that which we deem to be an observed and measured "fact" of the past. In a vehemently-worded diatribe against absolutist accounts of exactness and *à priori* intuitive truth (including mathematical truth), Clifford boldly tells his audience:

> When people are hopelessly ignorant of a thing, they quarrel about the source of their knowledge. Accordingly many maintained that we know these exact laws by intuition. These said always one true thing, that we did not know them from experience. Others said that they were really given in the facts, and adopted ingenious ways of hiding the gulf between the two. Others again deduced from transcendental considerations sometimes the laws themselves, and sometimes what through imperfect information they supposed to be the laws. But more serious consequences arose when these conceptions derived from Physics were carried over into the field of Biology. Sharp lines of division were made between kingdoms and classes and orders; an animal was described as a miracle to the vegetable world; specific differences which are practically permanent through all time; a sharp line was drawn between organic and inorganic matter. Further investigation, however, has shown that accuracy had been prematurely attributed to the science, and has filled up all the gulfs and gaps that hasty observers had invented. The animal and vegetable kingdoms have a debatable ground between them, occupied by beings that have the characters of both and yet belong distinctly to neither. Classes and orders shade into one another all along their common boundary. Specific differences turn out to be the work of time. The line dividing organic from inorganic, if drawn to-day, must be moved to-morrow to another place; and the chemist will tell you that the distinction has now no place in his science except in a technical sense for the convenience of studying carbon compounds by themselves (Clifford 1886, 96-97).

Thus,

> When we say that the uniformity which we observe in the course of events is exact and universal, we mean no more than this: that we are able to state general rules which are far more exact than direct experiment, and which apply to all cases that we are at present likely to come across (Clifford 1886, 97).

Clifford concludes the entire structure of our scientific and mathematical measuring apparatus is based upon our fallible and inaccurate perceptions. Physiology indicates humans are not equipped to perceive every source of information, or every aspect of physical reality. The eye is regarded as the ultimate "optical instrument of human manufacture." Yet Helmholtz, "the physiologist who learned physics for the sake of his physiology, and mathematics for the sake of his physics," has declared, "If an optician sent me [a human eye] as an instrument, I should send it back to him with grave reproaches for the carelessness of his work, and the demand the return of my money" (Clifford 1886, 100). We might assume based on the works of Darwin, Spencer and Wallace that the human eye is in the process of improving due to the effects of natural selection over time; thus, there is no guarantee of the accuracy of its measurements.

This poor level of perceptive ability had already led to the production of multiple contradictions and paradoxes in science and mathematics, Clifford further explains, drawing on Riemann's similar claim in prior years. Starting with Kant, a false dichotomy was established between bounded space and infinite space. Given that a boundary is that which divides two portions of space, the entire inquiry into whether space has a boundary or not is a "contradiction in terms." Empirical studies might tell us whether space is finite or composed of a certain number of cubic miles of distance. In such cases, there is no reason to assume that scientists will not be able to determine an approximate size of the universe in the future. If, on the other hand, it is not finite, then knowledge of that "would be quite different from any knowledge we at present possess." Clifford adds the "contradiction" between infinite and bounded arises from a false sense of absoluteness and exactness in the "laws of geometry." He cites explicitly the overturning of that dichotomy as evidenced in Riemann's account of "unboundedness" and "infinite extent" (two concepts that will be explored shortly in Clifford's translation of Riemann's paper).

Clifford tells his fellow Association members there is no reason to think new experiences or empirical knowledge will not shed some light on these matters, though our conclusions will be only ever "practically exact:"

> We may ask if there is any piece of matter so small that its properties as matter depend upon its remaining all in one piece. This question is reasonable; but we cannot answer it at present, though we are not at all sure that we shall be equally ignorant next year. If there is no such piece of matter, no such limit to the division which shall leave it matter,

> the knowledge of that fact would be different from any of our present knowledge; but we have no right to say that it is impossible (Clifford 1886, 107).

Similarly, when speaking about the "infinite extent of space" as something that cannot, at present, be perceived,

> we may reply that this is only natural, since our experience has never yet supplied us with the means of conceiving such things. But then we cannot be sure that the facts will not make us learn to conceive them; in which case they will cease to be inconceivable (Clifford 1886, 107).

Clifford defends the physiological conceivability of non-Euclidean geometries and the empirical nature of mathematical-geometrical knowledge in general—this being his personal interpretation of the *Habilitation*. The degree to which Clifford absorbed that viewpoint as part of his own professional identity is evident at the end of 1872, when he issued a mathematical article in the *Proceedings of the London Mathematical Society* (December 12th, 1872), entitled "Geometry on an Ellipsoid." That paper discusses the construction of ellipsoids and their basic properties, including the "lines of curvature" of the ellipsoid. It is an exploratory piece that helps Clifford to pave the symbolical way toward further developing an algebraic system that can represent rigid body motion in elliptically curved spaces. It is, in other words, an exploratory piece that indicates his navigations through the varying "terrains" outlined so far were leading him towards the construction of biquaternion mathematics.

Clifford's efforts to professionalize

By the early-1870s, Clifford had become a well-known public speaker on varied matters of scientific interest, as well as a proponent of newly emergent concepts in geometry. Yet, he had not produced a university textbook or any other major publication of note. As with Tait's appeal to Hamiltonian quaternions and his concomitant hope that it would launch his publishing career in the previous decade, Clifford appealed to what he perceived to be an emerging symbiosis between the algebra of quaternions and the geometry of Riemann in an effort to launch a boldly unique research program. In a series of lectures entitled "The Philosophy of the Pure Sciences," delivered to the *Royal*

Institution in March 1873, Clifford expounds upon the philosophical foundations for this new mathematical program. In his lecture, Clifford describes the untrustworthiness of visual perceptions. "When you move," he tells to his audience members, "I seem to see you go *continuously* from one position to another through an infinite series of intermediate positions." But regardless of whether one feels one is perceiving continuity, the electrical-neural stimulus that is actually transmitted from eye to brain is a discontinuous one. Clifford claims,

> the sensitive portion of my retina, which receives impressions, is not itself a continuous surface, but consists of an enormously large but still finite number of nerve filaments distributed in a sort of network ... All I can possibly have seen therefore at any moment is a picture made of a very large number of very small patches, exceedingly near to one another, but not actually touching (Clifford 1886, 183).

In other words, our perceptions are composed of two parts. The first is the physical "sensations" that stimulate our eyes, muscles and other nerve endings. The second is the "imagined" part, which allows us to fill in the gaps and perceive the world continuously.

The question to pose, of course, is how does one know when one is filling in the gaps "*rightly*"? If part of our perceptions are imaginative, could not the imagination fill in the gaps of the universe incorrectly? To answer that query, Clifford invokes both the empirical notion of geometrical knowledge, as advanced by Riemann, along with the Darwinist conceptual tools he had adopted, to argue the imaginative filling-in of our perceptual gaps does not occur randomly—it is based, rather, upon learned (both empirical and evolutionary) experiences. He states,

> In the first place, out of pictures I have imagined solid things. Out of space of two dimensions, as we call it, I have made space of three dimensions, and I imagine these solid things as existing in it; that is to say, as having certain relations of distance to one another. Now these relations of distance are always so filled in as to fulfill a code of rules, some called common notion, and some called definitions, and some called postulates, and some assumed without warning, but all somehow contained in Euclid's *Elements of Geometry.*

Clifford continues,

> I sometimes imagine that I see two lines in a position which I call parallel. Parallelism is impossible on the curved picture of my retina; so this is part of the filling in. Now whenever I imagine that I see a quadrilateral figure whose opposite sides are parallel, I always fill them in so that the opposite sides are also *equal.* This equality is also a part of the filling in, and relates to possible perceptions other than the one immediately present. From this example, then, you can see that the fundamental axioms and definitions of geometry are really certain rules according to which we supplement or fill in our experience (Clifford 1886, 185).

Euclidean axioms and postulates train people to intuit flat space and specific sorts of parallel lines. This is not necessarily representative of the patchwork of reality that we observe discontinuously. When we perceive motion occurring in space over time, we fill in our perceptions using the assumed "laws of kinematic" or "the pure science of motion," which we have been trained to believe in. Add to this the "continuity of things," which is another rule "according to which we fill in our experience," and it becomes clear that our minds have been trained to organize sensory data in particular and non-arbitrary ways. Thus, Clifford argues, we come to impose a certain level of uniformity upon the experiences we perceive.

Clifford then associates his claims with those of Locke and Hume who, he says, had advanced similar theses. For Locke, our sensations are aggregates of experience, which we naturally bundle together by experience. For Hume, "causation" is nothing other than a perceived series of events to which we ascribe uniformity. Those British philosophers, Clifford claims, justify the view that "the supplement of experience is made up of past experience, together with links which bind together perceptions that have been accustomed to occur together" (Clifford 1886, 190). Thus, all of our knowledge claims and all of our "objective" verifications of their truth are fundamentally experiential in nature. This viewpoint has caused historical controversy in the past, he writes. "After being staggered for some time by Hume's explanation," Kant eventually responded that not all knowledge can be experiential for "the axioms of mathematics are absolutely and universally true." According to Kant, Clifford says, no experience could possibly have informed our understanding of mathematical axioms. However, everyone in the audience had likely had the experience of concluding that the angle sum of all the triangles they had ever encountered is the same—namely, 180°. Likely, Clifford's listeners felt as though they had known something "which could not possibly be derived from experience" (Clifford 1886, 191). At the time Kant was writing, that sentiment had been a general one. "If a man felt absolutely sure that two straight lines perpendicular to the same line would never meet, however far produced, he could not maintain against Kant that all knowledge is derived from experience," Clifford explains, adding that Kant assumed that our statements about "Space and Time" are statements about the phenomena themselves.

The Kantian approach was popular for a time, adopted and promulgated by people such as Whewell, in his *Philosophy of the Inductive Sciences* (1840). Whewell used the Kantian viewpoint to counteract the emergent views of John Stuart Mill (1806-1873), who had argued geometrical images can be painted in the imagination in a way that is "equal to reality." Clifford paraphrases Mill in saying,

> This, in the first place, enables us to make (at least with a little practice) mental pictures of all possible combinations of lines and angles, which resemble the realities quite as well as any which we could make on paper; and in the next place, make those pictures just as fit subjects of geometrical experimentation as the realities themselves.

Furthermore,

> The foundations of geometry would therefore be laid in direct experience, even if the experiments (which in this case consist merely in attentive contemplation) were practiced solely upon what we call our ideas, that is, upon the diagrams in our minds, and not upon outward objects. For in all systems of experimentation we take some objects to serve as representatives of all which resemble them ... Without denying, therefore, the possibility of satisfying ourselves that two straight lines cannot enclose a space, by merely thinking of straight lines without actually looking at them, I contend that we do not believe this truth on the ground of the imaginary intuition simply, but because we know that the imaginary lines exactly resemble real ones with quite as much certainty as we could conclude from one real line to another. The conclusion, therefore, is still an induction from observation (Clifford 1886, 195-196).

Clifford appeals to Mill as justification of his own view that geometrical knowledge is fundamentally inductive in nature. It is experiential. Though Mill believed our experiences justify a belief in Euclidean geometry as true of space, Clifford argues the Millian viewpoint holds even though alternative geometrical experiences are clearly possible.

Contrary to Kant, and based on a contemporary interpretation of Mill, Clifford argues we should consider statements about the world to be utterances about the perceiver, "because these statements *are* about me." The uniformity in our perceptions derives from the fact that it is always the same "*me*" doing the perceiving, "or at any rate it is a *me* possessing always the same faculties of representation," he concludes (Clifford 1882, 193). Clifford used Locke, Hume and Mill as antidotes to Whewell and Kant to legitimize his engagement with empiricism in mathematics. The symbolical algebraists of the early century had largely sidelined the importance of empirical foundations (though De Morgan and Hamilton had sought to re-elevate the "empirical" to some degree). And they had used Locke to do so.

Clifford's response was to revivify Locke as an empiricist and to invoke other key British figures (i.e. Hume and Mill) to open up the Pandora's Box of geometrical possibilities in space, thereby allowing him to advance a Riemannian and Helmholtzian-inspired account of the universe's ontology. Indicative of this fact is Clifford's claim that he seeks to go beyond Mill. He notes that experience "tells me that my mental images of geometrical figures are faithful representations of those realities *which are within the bounds of experience*." He wonders, however, "What is to tell me that they are faithful representations of realities that are beyond the bounds of experience?" Answering his own question, Clifford argues, "Surely no experience can tell me that" (Clifford 1886, 197). Thus, although geometrical knowledge is induced from experience, certain knowledge still lies beyond perception altogether. Clifford's objective is to provide justification for those research programs that sought to invoke geometrical models, such as Riemann's elliptic model and Lobachevsky and Bolyai's hyperbolic models, to present new possibilities at distances far beyond anyone's perceptive capabilities. Such speculation should be encouraged, Clifford claims, as events "beyond the limits of experience" could be described in an infinite number of ways. Clifford therefore seeks to use empirical knowledge as a foundation, such that symbolical algebra alone is not enough. But he hopes also to abstract beyond the immediate and perceptible realms of inductive truth.

To do so, Clifford appeals to Herbert Spencer (1820-1903), who had recently demonstrated that knowledge is the result of one's evolutionary "self." In other words, one's knowledge claims are the outcome of one's physiological and intellectual composition, and the intellectual and physiological composition of one's ancestors, which began "with the first molecule that was complex enough to preserve records of its own changes" (Clifford 1886, 198). All of those changes had been handed down through "hereditary descent," and they have affected the manner in which we "perceive" geometrical truth. The "doctrine of 'forms of intuition'" has some truth, though not the truth ascribed to it by Kant or Whewell. Rather, the only *à priori* aspect of knowledge is the biological preconstruction of our neurological selves, much of which is determined by our hereditary position within the evolutionary schema. The most common experiences humans have, including that of time and space, have had an effect upon the common structures of their brains, Clifford explains. Those experiences have created neurological connections that, over time, have come to support perceptions of uniformity, continuity and certainty. In typically Victorian tones, Clifford describes the resulting state of human affairs by quoting from Spencer's *Principles of Psychology* (1870). "The effects of the most uniform and frequent of these experiences," he says,

> have been successively bequeathed, principal and interest; and have slowly mounted to that high intelligence which lies latent in the brain of the infant—which the infant in after-life exercises and perhaps strengthens or further complicates—and which, with minute additions, it bequeaths to future generations. And thus it happens that the European inherits from twenty to thirty cubic inches more brain than the Papaun. Thus it happens that faculties, as of music, which scarcely exist in some inferior human races, become congenital in superior ones. Thus it happens that out of savages unable to count up to the number of their fingers, and speaking a language containing only nouns and verbs, arise at length our Newtons and our Shakespeares (Clifford 1886, 109).

Clifford links this evolutionary picture to the ability of humanity to engage with modern geometrical knowledge. "The doctrine of evolution itself forbids me to admit any transcendental source of knowledge," he writes,

> So that I am driven to conclude in regard to every apparently universal statement, either that it is not really universal, but a particular statement about my nervous system, about my apparatus of thought; or that I do not know that it is true (Clifford 1886, 200).

Whereas the early algebraists had sought to abstract beyond the "empirical" by producing universal symbolical equivalences, which they then argued reflected the mind of God, Clifford seeks to abstract beyond the "empirical" by hypothesizing alternative spatial models, the reasonability of which, he argues, is representative of the evolutionary state of the "self." Clifford reminds his audience of the claims made both by Berkeley and Helmholtz, regarding the complexity of interpreting sensory images as received through the eye. Both authors had argued the retina is a problematical component of human physiology as it does not transmit images directly to the brain. For Clifford, the structure of the eye is an indication of the sometimes poor nature of evolutionary outcomes, which affects knowledge generation.

To exemplify the sort of incomplete outcomes of the "eye," Clifford considers the "postulates of the science of space," offering herein his own account of the story of Euclid's ascent to "authority" for the past 22 centuries. Clifford first draws an analogy between the effect of Copernicus who, he says, introduced the science of the "here and now," as opposed to infinite space and infinite distance, and Lobachevsky, who, he says, did the same for geometry. In both the Copernican and Lobachevskian cases, "The knowledge of Immensity and Eternity is replaced by knowledge of the here and now" (Clifford 1886, 213). Prior to Lobachevsky, the "postulate of superposition" had long reigned authoritative. It is the belief that a body with a particular shape occupies the same amount of space anywhere in space. Lobachevsky was among the first, Clifford says, to query the truth of this postulate and to suppose that at infinite (or just very large) distances, a shape may occupy a different amount of space. The effect of Lobachevsky "and his successors" was so profound that "the geometer of today knows nothing about the nature of actually existing space at an infinite distance," he says. The geometer can no longer assume that "all parts of space are exactly alike" (Clifford 1886, 223). Riemann then "accomplished the task of analyzing all the assumptions of geometry ... showing which of them were independent." Clifford concludes,

> This very disentangling and separation of them is sufficient to deprive them for the geometer of their exactness and necessity; for the process by which it is effected consists in showing the possibility of conceiving these suppositions one by one to be untrue; whereby it is clearly made out how much is supposed (Clifford 1886, 228).

Absolutist claims have no place in modern geometry, Clifford concludes. Geometry is, rather, an empirical craft predetermined only by the evolutionarily-determined neurological setup of the geometers in question.

Clifford's account of Riemannian geometry, 1873

By the mid-1870s, Clifford was well-versed in the geometrical theories of Lobachevsky and Riemann. But it is important to describe in detail the precise manner in which Clifford engaged with those authors, and in particular Riemann, whose works appear as explicit influences on Clifford's later bi-quaternion—so much so that I have identified "Riemannian and non-Euclidean geometries" as a specific "terrain" that overlapped in profound ways with the other environments through which Clifford was navigating in the 1870s.

The most important artifact to note in this regard is Clifford's 1873 translation of Riemann's seminal paper, "On the hypotheses which lie at the bases of geometry." Written as part of his German *Habilitation*, that paper investigated the origins of geometrical knowledge as Riemann had come to view it in the mid-1850s within the Göttingen world of German mathematical practice. Riemann had sought to expand upon the limits of geometrical and analytical possibility to include multiple dimensions, introducing along the way a new vocabulary to categorize the notions introduced therein. For instance, in Clifford's translation, Riemann writes,

> It is known that geometry assumes, as things given, both the notion of space and the first principles of constructions in space. She gives definitions of them which are merely nominal, while the true determinations appear in the form of axioms. The relation of these assumptions remains consequently in darkness; we neither perceive whether and

> how far their connection is necessary, nor, *a priori*, whether it is possible (Clifford 1882, 55).

Clifford had adopted a similar stance in his earlier arguments, as we have already seen in his 1869 paper on the "Space-theory of matter." The nature of geometrical "assumptions" (or axioms and postulates) cannot be known for those regions of space resident beyond the bounds of human perception, Clifford had argued. In his translation of Riemann's paper, Clifford demonstrates his continued and renewed interest in these Riemannian claims, especially with regards to the *origins* of mathematical knowledge, i.e. geometrical knowledge.

In the translation, Riemann claims one of the reasons for which mathematicians had failed to query relations in space is that they had not considered the possibility of "multiply extended magnitudes," including "space-magnitudes." By starting with the notion of "multiply extended magnitudes," the concept of a three-dimensional space (or a space of three extended magnitudes) emerges as just one particular instance in an infinite number of possible space types. The notion of multiple extensions does away with the assumption that the standards of measurement (i.e. the metric relations) that may hold in the case of three-dimensions will necessarily hold in other types of extended space. The problem, as far as Riemann sees it, is to determine which measure relations hold within specific kinds of space. This, he argues, is a "matter of fact," which, "like all matters of fact" are "not necessary, but only of empirical certainty." In other words, the metric system that should be used in any given space (i.e. extended space) is determined through empirical analysis of the type of space within which the measurement is taking place. However, given that such empirical analysis is uncertain, due to the fact that the measurer cannot extricate herself from the space she is measuring, Riemann concludes any metric assumption one makes is ultimately a "hypothesis." At best, mathematicians are only able to,

> investigate [the] probability [of the metric system], which within the limits of observation is of course very great, and inquire about the justice of their extension beyond the limits of observation, on the side both of the infinitely great and of the infinitely small (Clifford 1882, 56).

According to Clifford, Riemann proffers here an account of *n*-fold extended magnitudes to explain that the worth of any metric system is relative to the type of space it is meant to be measuring. In so far as our definitions allow for "continuous" and "discrete" magnitude, it is possible to talk of continuous or discrete manifoldness. Discrete manifolds are everywhere; they include collections of all sorts of discrete entities (i.e. a set of balls). Mathematicians are able to found theories of discrete magnitudes upon definitions of equivalences within the given manifold. If the manifold includes the elements of whole numbers, for instance, then normal arithmetic forms a theory of measurement for that particular manifold. "Continuous manifoldness," on the other hand, is a less common observation. "Quantity" relates to counting, because one can count discrete entities, while, "quanta" refers to measurement. While we can identify "quantity" in discrete manifolds relatively easily, finding "quanta" in continuous manifolds is more difficult. One cannot count continuous manifoldness, one can only measure and compare it. Such "measure consists in the superposition of the magnitudes to be compared," Riemann explains. "It therefore requires a means of using one magnitude as the standard for another." Where there is no standard, two magnitudes can only be compared when one is a part of the other, in which case the mathematician can only determine "the more or less and not the how much." Riemann concludes,

> The researches which can in this case be instituted about them form a general division of the science of magnitude in which magnitudes are regarded not as existing independently of position and not as expressible in terms of a unit, but as regions in a manifoldness (Clifford 1882, 57).

"Position" is, therefore, crucial. Given that the manifoldness of a given space in which objects are located is determined by the metric relations between those objects in that region, objects in differing regions of manifoldness may very well require differing metric relations, as they cannot be compared in any direct manner.

In order to describe how these *n*-ply extended spaces are constructed, Riemann uses projective geometry in a manner akin to how Grassmann had described the projection of algebraic forms in multiple dimensions in his *Ausdehnungslehre*.[122] In the case of a continuous manifoldness, a simply extended manifoldness will be that in which,

> continuous progress from a point is possible only on two sides, forwards or backwards. If one now supposes that this manifoldness in its turn passes over into another entirely different, and again in a definite way, namely so that each point passes over into a definite point of the other, then all the specializations so obtained form a doubly extended manifoldness. In a similar manner one obtains a triply extended manifoldness, if one imagines a doubly extended one passing over in a definite way to another entirely different; and it is easy to see how this construction may be continued (Clifford 1882, 58).

The determination of position in a given *n*-ply extended manifoldness is, therefore, dependent upon *n* determinations of "quantity" (i.e. *n*-position measures). In a flat *n*-fold, the curvature of the space is zero at all points and in all directions. This means that any given measure-relations within that manifold holds throughout space regardless of direction. "Manifoldness whose curvature is constantly zero may be treated as a special case of those whose curvature is constant," Riemann observes. The common characteristic shared by manifolds with constant curvature is that "figures may be moved in them without stretching," Riemann writes. "For clearly figures could not be arbitrarily shifted and turned round in them if the curvature at each point were not the same in all directions" (Clifford 1882, 65). In other words, Euclidean space is the limiting case for all other uniformly curved elliptic spaces.

In a section entitled "Application to Space," Riemann then states the determination of measure-relations in any space of "an *n*-fold extent" allows for the determination of metric properties elsewhere in that space (assuming flatness at the most fundamental level). Yet, those properties always remain uncertain and only ever approximate. Indeed, "the question how, in what degree, and

[122] It is likely that similar notions of "projection" were passed on to Riemann via Gauss and other practitioners in Germany at the time.

to what extent these assumptions are borne out by experience," remains to be resolved. In Clifford's translation, Riemann writes,

> In this respect there is a real distinction between mere extensive relations, and measure-relations; in so far as in the former, where the possible cases form a discrete manifoldness, the declarations of experience are indeed not quite certain, but still not inaccurate; while in the latter, where the possible cases form a continuous manifoldness, every determination from experience remains always inaccurate: be the probability ever so great that it is nearly exact. This consideration becomes important in the extensions of these empirical determinations beyond the limits of observation to the infinitely great and infinitely small; since the latter may clearly become more inaccurate beyond the limits of observation, but not the former (Clifford 1882, 67).

Riemann differentiates between "*unboundedness,*" which relates to "extent relations," and "*infinite extent,*" which relates to "measure-relations." The German argues that generations of mathematicians had relied upon the assumption that space is an unbounded, three-fold manifoldness. Even if this is an empirical reality, and not mere assumption, the "infinite extent" of space by no means follows from its status as unbounded: "If we assume independence of bodies from position, and therefore ascribe to space constant curvature [as opposed to assuming flatness], it must necessarily be finite provided this curvature has ever so small a positive value" (Clifford 1882, 68). In such a situation, we would obtain "flat manifoldness;" objects could move about with no change in their shape, despite the fact the space is analogous to the surface of a sphere—i.e. it is "finite".

Questions regarding the infinitely great simply lie beyond the possibility of human observation, Riemann concludes, although this is not so with the realm of the infinitely small. In fact, "It is the exactness with which we follow phenomena into the infinitely small that our knowledge of their causal relations essentially depends" (Clifford 1882, 68). Riemann invests hope in the development of the infinitesimal calculus and its application to mechanics, which he considers to be fruitful starting points for the "natural sciences," as those sciences "are still in want of simple principles for such constructions," as they seek "to discover the causal relations by following the phenomena into great minuteness, so far as the microscope permits" (Clifford 1882, 68). Riemann hypothesizes that, because perceived curvature at the astronomical level is zero, it is likely that constant curvature prevails in the universe. However, he notes this is only a hypothesis, and wherever we cannot assure ourselves of constant curvature, then movement (or the measure of movement) is necessarily position-dependent, as curvature in different regions of space might vary arbitrarily, including in spaces of "the infinitely small." For Riemann, the validity of geometrical claims is ultimately dependent upon discovery made in another science—namely, that of "physic".

Riemann's notion of "continuous manifoldness," in which "quantity" is a "measure relation" (i.e. a comparative ratio), added to the contours of Clifford's intellectual terrains, infusing them with cogent notions of "empirical" mathematics. Clifford came to view legitimate mathematical knowledge as that which is generated alongside developing geometrical perceptions and experiences. In an article entitled "Preliminary Sketch of Biquaternions," Clifford advances this point of view in a professional arena. The piece was produced in the same year that Clifford issued his translation of Riemann's paper. It was published in the *Proceedings of the London Mathematical Society* (June 12th, 1873). In that paper, the young British mathematician expounded upon Hamiltonian quaternions in conjunction with Riemannian conceptions of space and the "theory of screws," as advanced by Robert Ball (1840-1913), then Andrews Professor of Astronomy (and Hamilton's successor as Royal Astronomer of Ireland). In his account, Clifford represents the need for a more powerful algebra—one that can allow for the symbolical manipulation of "wrenches" and "screws" in spaces of multiply-extended dimensions. The first line of Clifford's bi-quaternion text is revealing in this regards. He states,

> The *vectors* of Hamilton are quantities having magnitude and direction, but no particular position; the vector AB being regarded as identical with the vector CD when AB is equal and parallel to CD and in the same sense (Clifford 1882, 181).

Vectors are, however, insufficient, Clifford explains, given that Riemann had demonstrated the importance of "position." A simple translation of a rigid body can be described using Hamiltonian vectors, since "all of the particles in the rigid body move through equal distances along parallel straight lines in the same sense." In many cases, however, "It is necessary to consider quantities which have not only magnitude and direction, but *position* also" (Clifford 1882, 181). The rotational velocity of a rigid body always occurs about a particular axis, but equal rotations about two parallel axes should not be considered as "equal," which is what Hamilton and Tait had assumed.

The difference between a system that takes "position" into account and one that does not, Clifford explains, is manifest in the "geometric calculus" used to describe it. He writes,

> In studying the motions of a particle or the composition of couples, the only construction required is that of the "force-polygon" and the theory involved is that of the addition of vectors; but in the static or kinematic of solids we require in addition the construction of the "link-polygon" and there is involved the theory of the involution of lines in space, or the linear complex (Clifford 1882, 181-182).

A *"vector"* can be associated with the notion of a *"translation"*—i.e. a "quantity" denoted by a simple motion. Meanwhile, Hamilton and Tait's "quaternion" is a ratio of two vectors and it possesses its own magnitude and direction. A *"rotor,"* short for "rotator," extends the "quaternion" by including magnitude, direction and position. The simplest type of "rotor" motion is a rotation about a particular axis. While Hamilton and Tait's "quaternion" is an operator that translates and rotates, Clifford introduces his notion of "rotor" as a more general extension of the Hamiltonian quaternion. A "rotor" can described a wider variety of rotations (i.e. it can describe a greater degree of dimensional variation in rotational direction). "A rotor," Clifford says, "will be geometrically represented by a length proportional to its magnitude measured upon its axis in a certain sense." In other words,

> A rotor AB will be identical with CD if they are in the same straight line, of the same length, and in the same sense; i.e. a vector may move anywise parallel to itself, but a rotor *only* in its own line (Clifford 1882, 182).

Clifford's rotors have magnitude, direction and a particular position within three-dimensional space.

Appealing then to Ball's notions of "wrenches" and "screws,"[123] Clifford explains rotors are only partially analogous to Hamilton's work. This is because, in Hamilton's work, the addition of two vectors results in another vector. Only in certain circumstances, however, does the addition of two rotors lead to a third rotor. If two original rotors lie in different planes, they may never intersect in their line of action. Therefore,

> The composition of two forces whose lines of action do not intersect, or of two rotation-velocities whose axes do not intersect, gives rise to a system of forces on the one hand, and the most general velocity of a rigid body on the other (Clifford 1882, 182).

As Ball had already demonstrated, a system of "forces" can be reduced to one single force, P, and a couple, G, whose plane is perpendicular to the line of action of the force (i.e. the "central axis"). Ball described this system of forces as a "*wrench*" about a certain "*screw*," where the axis of the screw, is the central axis. The pitch of the screw action is the ratio of the couple to the force, or $\frac{G}{P}$. By analogy, the velocity of a rigid body rotating in space can be represented as a rotation-velocity, ω, which travels about a certain axis, combined with a translation-velocity, v, along that same axis. Ball had termed this combined velocity as "*twist-velocity*" about a certain screw, where the axis of that screw is the axis of rotation. By analogy to the system of forces mentioned earlier, the pitch of that axis of rotation is the ratio of the translation-velocity to the rotation, or $\frac{v}{\omega}$.

For Clifford, this new symbolical terminology refers to new geometrical objects, or "forms." He writes:

> A *screw* is here a geometrical form resulting from the combination of an *axis* or straight line given in position with a *pitch* which is a linear magnitude. A *wrench* is the association with this geometrical form of a magnitude whose dimensions are those of a

[123] Ball explains in his textbook, *The Theory of Screws: a study in the dynamics of a rigid body* (1876), that his "theory of screws" was mostly developed in 1871 and 1872, during which time he published his developing ideas in the *Philosophical Transactions of the Royal Society of London* and the *Transactions of the Royal Irish Academy*. Ball expresses thanks to the previous works of Chasles, Poinsot and in particular the linear geometry of Plücker for providing him with the mathematical resources to tackle the problem of rotational motion in space. He also thanks the personal involvement of his friend, the mathematics professor in Munich, Felix Klein (1849-1925), in helping to bring the present text to fruition. Relatively little is known about Ball, though Ball's work served as an invaluable store of conceptual resources for Clifford's development of bi-quaternions.

force; a *twist*-velocity the association of a magnitude whose dimensions are those of an angular velocity (Clifford 1882, 183).

Clifford concludes, therefore, that

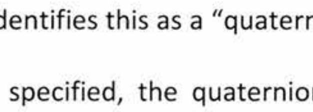
> just as a vector ("translation-velocity" or "couple") is magnitude associated with direction, and as a rotor ("rotation-velocity" or "force") is magnitude associated with an axis; so this new quantity, which is the sum of two or more rotors ("twist-velocity" or "wrench"), is magnitude associated with a screw ... I propose to call this quantity a *motor*; the simplest type of it being the general motion of a rigid body (Clifford 1882, 183).

The sum of rotors is a "motor,"[124] and a "motor" constitutes a generalization of Hamilton's quaternion system, by extending it symbolically to allow for a greater degree of variation among the directions and rotating magnitudes in question.

To clarify, Clifford explains a quaternion is the ratio of two vectors, "or the operation necessary to make one into the other." If the vectors AB and AC start from any arbitrary point A, then:

> AB is made into AC by turning it about an axis through A perpendicular to the plane BAC until its direction coincides with that of AC, and then magnifying or diminishing it until it is of the same length as AC (Clifford 1882, 183).

Fig. 23.



(Clifford 1882)

The ratio of these two vectors is the combination of an "ordinary numerical ratio with a *rotation;*" Clifford identifies this as a "quaternion" composed of a tensor part and a versor part. Since the point A is not specified, the quaternion's spatial positioning is entirely arbitrary. It is deemed to be "completely specified" by its angular magnitude and the direction of its axis alone. In symbolical terms, the quaternion can therefore be represented as:

[124] In particular cases, Clifford says, it might "degenerate" into either a "rotor" or even, simply, a "vector" (Clifford 1882, 183).

$$\frac{AC}{AB} = q.$$

This refers to an operation performed on AB such that it converts it into AC. Symbolically,

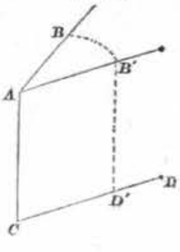
$$q.AB = AC,$$

where the axis of the quaternion (the axis of rotation) is perpendicular to the plane of the vectors (i.e. perpendicular to BAC).

This highlights the fundamental limitation of Hamilton's quaternion mathematics, Clifford argues: "A quaternion can *only* operate upon a vector which is perpendicular to its axis." If AF is a vector not in the plane BAC, then the symbolical expression $q.AF$ "is absolutely unmeaning." It is "very important," Clifford emphasizes, "to remark that so long as AF means a $vector$ not perpendicular to the axis of q, the expression $q.AF$ has no meaning at all" (Clifford 1882, 184). Hamilton's system requires extension so that vectors that are not coplanar can also be compared. It is this extended system that Clifford identifies as his own system of "rotors," "motors" and "bi-quaternions."

Clifford explains that one must first imagine a straight line that meets at right angles with the axes of any two rotors.

Fig. 24.



(Clifford 1882)

If AC is the shortest distance between the rotors AB and CD, then AB can be converted into CD in three steps. The first step is to turn AB about the axis AC in order to bring it into the position of AB', parallel to CD. The second step is to slide it along that axis into the position CD'. The last step is to "magnify or diminish it in the ratio" of CD' to CD. The first two operations together form a twist about a screw with the axis AC, with a pitch determined by, $\frac{AC}{circ.meas.of\ BAB'}$. The ratio of two rotors—i.e. that which converts the first rotor into the second—is a combination of a numerical ratio with a "twist" (Clifford 1882, 185). In rotor mathematics, the "twist" is defined by a definite screw, and it is specified when both its angular magnitude and the screw (including the direction, position and pitch of the screw) are provided.

In drawing an analogy to Hamilton's system, Clifford explains "that just as the rotation (versor) involved in a quaternion is the ratio of two directions, so the twist involved in the ratio of two rotors is really the ratio of their axes" (Clifford 1882, 185). As with quaternions, Clifford also acknowledges his system of "rotors" is limited in its range of activity. A "*tensor-twist*" can only operate upon two rotors that meet at right angles to the axis linking them. Clifford therefore works to extend his own system further, by arguing that if we let t denote

> the operation which converts AB into CD, so that $t = \frac{CD}{AB}$, and $t.AB = CD$; then if EF be any other rotor which meets AC at right angles, the expression $t.EF$ will have a definite meaning, viz., it will mean a rotor obtained by sliding EF along a distance equal to AC, turning it about AC as axis through an angle equal to BAB', and altering its length in the ratio $AB:CD$. But if EF be a rotor not meeting AC, or meeting it at any other than a right angle, the expression $t.EF$ will have no meaning whatever (Clifford 1882, 185).

The question, therefore, is whether the ratio between "motors" is restricted in the same way that the ratio between two "rotors" and two vectors are restricted (i.e. restricted to particular planes of action). When two motors have the same pitch, the answer is simple: their ratio is a tensor-twist. The "tensor" is the ratio of their magnitudes and the "twist" is the ratio of their axes. Where the pitches are not the same, however, the solution is more complicated.

Recall that every motor is composed of a rotor part and a vector part. Its pitch is determined by the ratio between those two parts. By combining "a suitable vector with a motor ... we may make the pitch of it anything we like, without altering the rotor part" (Clifford 1882, 186). Converting motor A

into motor B, requires that we let B' be a motor that has the same rotor part as B and the same pitch as A, and that we let $B = B' + \beta$, where β is a vector parallel to the axis of B. In that instance, the ratio is represented as: $\frac{B}{A} = \frac{B'}{A} + \frac{\beta}{A}$. Given that $\frac{B'}{A}$ is a tensor-twist, t, it is possible to write: $\frac{B}{A} = t + \frac{\beta}{A}$. The last step is to find an operation that converts the motor A into a vector β. To do this, Clifford introduces a symbol "whose nature and operation will at first sight appear completely arbitrary, but will be justified." The symbol ω, when applied to any motor, reduces that motor "into a vector parallel to its axis and proportional to the rotor part of it" (Clifford 1882, 186). In other words, ω changes a rotation about an axis into a translation parallel to that axis.[125] In symbolical terms, $\omega A = \alpha$, where α is a vector. If the ratio $\frac{\beta}{\alpha}$ is a quaternion, q, such that $q\alpha = \beta$, the quaternion can be rewritten as $\beta = q\alpha = q\omega A$, which means $\frac{\beta}{A} = q\omega$, and, $\frac{B}{A} = t + q\omega$. This series of manipulations demonstrates the "ratio of two motors may be expressed as the sum of two parts, one of which is a tensor-twist, and the other is ω multiplied by a quaternion" (Clifford 1882, 187). Clifford notes the ratio between differing motors is not restricted in any way. The system is open to an infinite number of variations in space, where the general expression for a motor is $\alpha + \omega\beta$.

Extending the system further, Clifford then notes that to express the ratio between two motors, $\alpha + \omega\beta$ and $\gamma + \omega\beta$, or $\frac{\alpha+\omega\beta}{\gamma+\omega\beta}$, one must revert to symbolical analysis alone to invoke an arbitrary and "meaningless" symbolical term. If we let $\frac{\gamma}{\alpha} = q$, then $q(\alpha + \omega\beta) = \gamma + \omega q\beta$. The symbol $q\beta$ "has at present no geometrical meaning; for, in general, the rotors α, β, γ will not be coplanar and cannot therefore be operated on by the same quaternion q," Clifford admits. However, by invoking the "calculus of quaternions," all those quantities can be expressed in terms of three rectangular unit rotors through the origin. As such, the expression $\frac{\delta - q\beta}{\alpha}$ "will be a perfectly definite quaternion r." Although the equation $r\alpha = \delta - q\beta$ is like the equation $q(\alpha + \omega\beta) = \gamma + \omega q\beta$, in that it is

[125] It follows from this definition that, if ω is made to operate twice upon any motor, the effect would be to reduce that motor to zero: $\omega^2 A = 0$.

"purely literal and devoid of meaning," it is possible to manipulate these expressions by introducing the operator ω once more. If one adds "ω times the first equation to the second,"[126] it is possible to algebraically obtain $(q + \omega r)(\alpha + \omega\beta) = \gamma + \omega\delta$, such that the ratio $\frac{\alpha+\omega\beta}{\gamma+\omega\beta}$ can be symbolized by the simple expression $q + \omega r$. This latter expression is what Clifford terms a "biquaternion." Although there are difficulties with the interpretation of a "biquaternion"—i.e. it does not denote the sum of all the geometrical operations that Clifford says can be applied to the motor $\alpha + \omega\beta$—it does denote a basic concept that Clifford hoped would eventually constitute a meaningful extension to Hamilton's quaternions.

In summing his first bi-quaternion efforts, Clifford offers his readers a classification table that organizes the concepts he relies upon. The table is historically revealing in that it indicates the place that Clifford gives his biquaternions vis-à-vis Hamilton and Tait's quaternions.

Geometrical Form	Quantity	Example	Ratio
Sense on st. line	Vector on st. line	Addition or Subtraction	Signed Ratio
Direction in plane	Vector in plane	Complex quantity	Complex Ratio
Direction in space	Vector in space	Translation, Couple	Quaternion
Axis	Rotor	Rotation-Velocity, Force	Twist
Screw	Motor	Twist-Velocity, System of Forces	Biquaternion

(Clifford 1882, 188)

Biquaternions appear as the final and ultimate operation—the generalized form of a system of operations that measure rigid body motion in space.

Having thus categorized his bi-quaternions, Clifford then invokes Felix Klein's terminology to distinguish between "parabolic" geometry (the geometry of Euclidean space with no curvature), "elliptic" geometry (a geometry of Riemann), and "hyperbolic" geometry (the geometry of Lobachevsky and Bolyai). Clifford identifies his future aim as that of applying his "biquaternion" tool to Riemannian "elliptic" geometry. Clifford first defines the basis of his geometrical terminology. A

[126] Clifford also assumes the distributive law.

"unicursal space," he says, is a particular sort of algebraic space in which "every system of values" corresponds in general to "one point," and in which "every point in general" corresponds to "one system of values." In some cases, certain points correspond to an infinite number of values (i.e. they correspond an infinite number of coordinates that satisfy a given equation). In other cases, certain value-systems correspond to a locus of points in space. Projective geometry is an instance of this latter situation. It is based upon the relations existent between point-equations and their resulting loci-values in space.[127] The "*metric* geometry of space," which is attributable to Cayley (especially as presented in his "Sixth Memoir on Quantics" (1859)), is a theory of projective relations between fixed geometrical forms and all other geometrical forms, Clifford writes. It is a theory of the invariant relations that hold between certain fixed algebraic forms and all other algebraic forms. The "fixed form" in Cayley's system is what Cayley termed the "absolute." To determine the types of projections occurring in a particular space, the mathematician requires an equation for the "absolute" elements in a given system, whether they refer to points, lines or plane-coordinates.

There are three cases, Clifford explains, which constitute possible models accommodating such observed space: "elliptic geometry," in which all the of elements of the absolute are imaginary; "hyperbolic geometry," in which all of the elements of the absolute contain no "real straight lines" (i.e. "real points situated on the other side of the surface are called ideal" in this instance), and, "parabolic geometry," in which the surface "degenerates into an imaginary conic in a real plane," and "the points of the absolute are mere points in the (real) plane of this conic" (Clifford 1882, 191). Clifford's personal interest is in the first of the spaces. Within a space deemed "elliptic," by virtue of its intrinsic curvature, a twist-velocity of a rigid body necessarily has two axes. A motion of translation along any axis in elliptic space constitutes a similar phenomenal event to that of a

[127] These loci correspond to linear equations between coordinates represent planes, and their intersections are lines. Clifford notes this is a "purely projective definition" and these loci "are not necessarily *flat* planes and *straight* lines in the metrical sense."

rotation about a polar axis (Clifford 1882, 193).[128] Clifford links the concept of elliptic space, as an empirical possibility, to "bi-quaternions," as a metrical tool that can generate measure-relations in such spaces.

In Clifford's view, Cayley's projective geometry, Riemann's elliptic geometry, and Hamilton's quaternions go hand-in-hand. Clifford provides the following account to explain how that is. First, he relates Hamiltonian vectors and quaternions to his own notion of "rotors":

> A fixed point being chosen as origin, let three lines perpendicular to one another be drawn through it, and let three unit-rotors having these lines as axes be denoted by the symbols i, j, k. Then every rotor through the origin will be denoted by an expression of the form $ix + jy + kz$, where x, y, z are scalar quantities, or the ratios of magnitudes. The symbols i, j, k shall have also another meaning; viz., each shall signify the rotation through a right angle about its axis of any rotor which meets that axis at right angles. When they are performed on rotors passing through the origin, these operations satisfy the equations $i^2 = j^2 = k^2 = ijk = -1$, by the ordinary rules of quaternions; and it is easy to see that the same equations hold good when the operations are performed on rotors not passing through the origin (Clifford 1882, 194).

Clifford notes, however, that

> the compound symbol $ix + jy + kz$ is also to have an analogous secondary meaning; viz, a rectangular rotation about the axis of the rotor which it previously denoted, combined with a tensor $\sqrt{(x^2 + y^2 + z^2)}$. It can operate only on a rotor which meets its axis at right angles. This being so, the ratio of any two rotors through the origin is a *quaternion* of the form $q \equiv w + ix + jy + kz \equiv w + \rho$, say. The axis ρ of this quaternion is perpendicular to the plane of the two rotors. If α be a rotor through the origin and q a quaternion, the product qx can be formed according to the Hamiltonian rules of multiplication, and is in general a quaternion r. In this general case the equation $qx = r$ can only be interpreted by giving to α its *secondary* meaning; and the translation of this statement into words is as follows;--If a rotor be capable of being successively operated upon by the rectangular versor α and the quaternion q, the final result will be the same as if it had been originally operated upon by the quaternion r. If, however, the axes of q and α are at right angles, the scalar part of r will be wanting, and we may write the equation $qx = \rho$. This equation is now susceptible of a *primary* interpretation; viz., the quaternion q operating on the rotor α produces the rotor ρ; although the *secondary* interpretation does not cease to be true (Clifford 1882, 194-195).

Clifford insists upon an empirical view of the matter. He states:

[128] As Clifford explains, "a twist-velocity is compounded of rotation-velocities about tow polar axes; say these are θ, ϕ. Then the motion may be regarded either as a twist-velocity about a screw whose pitch is $\frac{\phi}{\theta}$ and whose axis is the first axis, or about a screw whose pitch is $\frac{\theta}{\phi}$ and whose axis is the polar axis" (Clifford 1882, 193).

> With such conventions, the two sides of the equation $(q+r)s = qs + rs$ (in which q, r, s are quaternions) have always the same meaning when both are interpretable; which is what is meant by saying that the distributive law holds good for these symbols (Clifford 1882, 195).

Hamiltonian quaternions and a Grassmannian sense of abstract "forms," which describe geometrical phenomena in spaces of more than three-dimensions, along with a Riemannian "elliptical" spatial model in mind, led Clifford to develop the nascent elements of this unique account of metrical relations in non-flat space.

In the same year that Clifford issued this first brief account of bi-quaternions, he also delivered a series of lectures to his UCL class focusing on "motion". Clifford's editor, Robert Tucker, entitled those notes "Syllabus of Lectures on Motion". In his lecture, Clifford provides his students with a clear account of what he took to be the study of "Kinematic." It is, he says, "the science which teaches how to describe motion accurately, and how to compound different motions together, without considering the circumstances under which motions take place." Clifford talks about "rigid" body motion, the simplest kind of which is a translation. In a translation, the size and shape of the given body do not change. A "strain" is another sort of motion, but one in which there is a change in size or shape. Bodies that change in size or shape are "elastic" bodies, in contradistinction to the "rigid" bodies above. Complementing "kinematics", however, is the science that teaches one about the circumstances under which "particular motions take place." Here, Clifford uses Tait and Thomson's language of "energy" in referring to scientific inquiry that analyzes the "Law of Force." "If its results are expressed in terms of force," then the science is called "Dynamics". But if the results of the inquiry are expressed in terms of "energy" then it is the science of "Energetics." "Energetics" is divided into "statics," which explores circumstances under which bodies are at rest, and "kinetics," which explores the circumstances under which bodies are in motion. Static and kinetic studies of "rigid bodies" are dealt with in relation to the study of "particles," Clifford explains, while studies of "elastic bodies" are dealt with in relation to "Elasticity."

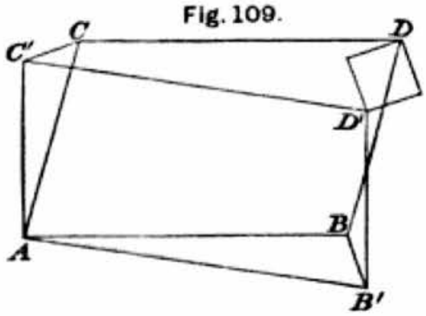
Clifford focuses here on "rigid body kinematics." He provides students with a clear definition of a "translation" of a rigid body. For example,

> If two bodies A and B are in motion, the motion of B is said to be compounded of the motion of B relative to A. The proposition can be similarly stated as: Translations represented by the sides of a parallelogram compound together into translation represented by the diagonal (Clifford 1882, 517).

Clifford explains, "A translation is a particular kind of vector, and the composition of translations is equivalent to their addition as vectors." That composition satisfies the "law" (i.e. the mathematical equivalence) that, $\alpha + \beta = \beta + \alpha$, where "uniform rectilinear motion" (i.e. that in which "equal spaces are traversed in equal times") is represented by $\rho = \alpha + \beta t$. Clifford also accounts for "velocity" in vectorial form, explaining that velocity has both magnitude and direction. Therefore, "If each of two motions has a velocity at a certain instant of time, the motion compounded of them has a velocity which is compounded of their velocities by the rule of addition of vectors" (Clifford 1882, 522). Clifford invokes Hamiltonian language here with regards to the addition of vectors. Notably, this is also an example previously discussed by Tait in his 1867 textbook on quaternions. In his own account of it, Clifford describes the following scenario:

> Let AB and AC be the given velocities; complete the parallelogram ABDC. Let also AB', AC' be the mean velocities during an interval which ends at the given instant; if the parallelogram AB'D'C' be completed, we know that AD' is the mean velocity of the resultant motion. Now the interval may be so chosen that for it and all shorter ones included in it BB' and CC' are each less than half of any proposed length; and therefore DD', which is their vector-sum, less than the proposed length. Consequently AD is the velocity of the resultant motion at the given instant (Clifford 1882, 522).

Fig. 109.



(Clifford 1882, 522)

Though the example mimics Tait's textbook example from the previous decade, Clifford does not discuss quaternions further in this series of lectures, and nowhere does he cite Tait.

On the conventional nature of symbolical representations

In Clifford's "Further note on Biquaternions," which constitutes Clifford's class notes from 1873 to 1876, the English mathematician continues to build upon his nascent notion of bi-quaternions as it applies to rigid body motion. He begins by explaining the symbolical nature of the simple arithmetic statements that "twice three are six" and "six is the product of two and three." Although both statements are symbolically represented by the mathematical statement $2 \times 3 = 6$, they refer to two different mathematical phenomena. On one interpretation,

> 3 is a concrete number of things, say three marbles, while 2 is not a number but an operation, namely, the operation of doubling; and we may read the equation "doubling three marbles makes six marbles" (Clifford 1882, 385).

The entities manipulated are existent, tangible things—namely, "marbles." The operation refers to an empirically-observable event. On a second interpretation, however, nothing tangible or particular is being manipulated. Rather, the mathematical expression above represents a general and universal abstract rule that applies to anything at all. As Clifford explains,

> The second interpretation regards 2 and 3 as abstract numbers, and affirms the existence of a third number 6 having a definite relation to them which it is convenient to study, this number so related being called their product; and various meanings given to the numbers 2 and 3 may lead to various concrete interpretations of the formula (Clifford 1882, 385).

Either interpretation "may be extended to other things besides numbers," Clifford adds, noting his intention is to extend these two interpretations such that "all three symbols," including $2, 3,$ and 6, are regarded as "symbols of operation." We would then read the formula as saying that "doubling the triple of anything makes the sextuple of it." If we were to approach the problem in this manner, an equation such as $abc = d$ could always possess two meanings:

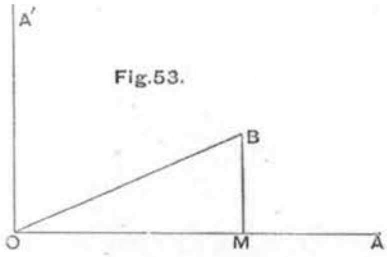
1. a times b times c things makes d things;

2. a times b times c times anything makes d times that thing (Clifford 1882, 386).

The last symbol could be regarded either as a "concrete number," or a "symbol of operation," though the other symbols are regarded as symbols of operations only.

In Clifford's second "extension," the symbols are considered not as "concrete numbers", but as "steps" in space. This requires giving a "double meaning" to the signs $+$ and $-$. The first meaning indicates the "direction" of the "step", so that $+3$ is three steps forward (i.e. addition) while -3 is three steps backwards (i.e. subtraction). When these symbols are "attached to an operation performed upon steps," Clifford explains, "they mean *retaining* and *reversing* respectively." The equation $(-2)(+3) = -6$ has two meanings. It can mean "doubling a step of 3 forward and reversing it [to make] a step of 6 backwards," or it can mean "to triple a step and retain its direction, then to double and reverse it," which "is the same as to sextuple and reverse it." Clifford explains these directed steps can be regarded as "vectors on a straight line along which all numbers are supposed to be ranged." Furthermore, "By exchanging numbers for continuous quantities we may deal in this way with all vectors in a straight line." In all equations, "we may regard the *last* symbol in every term as either a vector or an operation" (Clifford 1882, 386). This interpretive malleability stems from Clifford's symbolical analytical training, but it is coloured by his need to interpret the resulting equivalences in terms of geometrical (read continuous) forms.

But to advance his overall claim—that all meaningful mathematical symbols should have a geometrical analogue—Clifford embarks upon a more detailed discussion of "*imaginary* or *impossible* quantities." Assuming the law of addition of vectors in a plane holds, and can be symbolically represented as $AB + BC = AC$, imaginary "quantities" can be interpreted as the "operators which convert one vector into another" (Clifford 1882, 386).



(Clifford 1882)

The symbol I denotes an operator that causes the vector to which it is applied to turn counter-clockwise through a right angle, such that $I.OA = OA'$. Furthermore, given the following ratios, $a = \frac{OM}{OA}, b = \frac{MB}{OA'}$, a and b being ratios of vectors in a line as just defined, it is possible to write that $OB = OM + MB = a.OA + b.OA' = (a + bI)OA$, where $I^2 = -1$. Every expression of the form $a + bI$ is the ratio of two vectors. And every vector in the plane, can be represented by $a.OA + b.OA'$, if we assign proper values to a and b. To be concise, the mathematician can write $OA = j$, $OA' = k$. As a result, $Ij = k$ and $Ik = -j$. Thus, we find

$$(a + bI)(cj + dk) = (ac - bd)j + (ad + bc)k.$$

This indicates "two classes of expressions," Clifford writes. The first includes those expressions in which the last symbol in every product is a vector and "all the others" are ratios of vectors. The second includes those expressions in which all the symbols represent ratios of vectors. It is important to observe symbolically that $cj + dk = (c + dI)j$. This observation allows us to generate the "useful convention" that *j* is "to be understood as written after every term; so that the complex symbol $c + dI$ will now mean *either* a ratio of two vectors as above, or the vector $cj + dk$" (Clifford 1882, 387). Clifford explains,

> This artifice amounts to taking a definite vector as the unit, and representing all others by means of their ratios to the unit. The success of the artifice depends on the fact that the product of two such ratios is another ratio of the same kind (Clifford 1882, 387).

Clifford proposes extending this interpretation to vectors in space, such that the operation that makes one vector into another is of the form $a + bQ$, where Q "turns through a right angle *in the* *plane of the two vectors.*" Q only operates upon vectors within a particular plane. The "variety" of Q is therefore "doubly infinite," to put it in Riemannian terminology, as Clifford does. The symbol Q can be represented as a sort of "handle or axis of unit length perpendicular to the plane." Thus, the compounded operation represented by bQ turns through a right angle and increases the vector by a ratio of $1: b$ (where the operation has an axis of length b).

If Q and R represent two such compound operations, and if we assume there is a vector α on which they both operate (where this vector constitutes the intersection of the planes of those compounded operations), then $Q\alpha + R\alpha$ represents a vector at right angles to α itself, Clifford says. If S is the rectangular "versor" that converts α into $Q\alpha + R\alpha$, such that $S\alpha = Q\alpha + R\alpha$, then the axis of S is obtained by adding the axes of Q and R as if they were vectors. To represent this action, Clifford writes $S = Q + R$, such that, $(Q + R)\alpha = Q\alpha + R\alpha$. "By the law of formation of $Q + R$," Clifford explains, it appears that $P + (Q + R) = (P + Q) + R = (P + R) + Q = etc.$, which represents a "rectangular versor whose axis is the vector-sum of the axes of P, Q, R." This can be symbolised as $P + Q + R$, even though the equation, $(P + Q + R)\alpha = P\alpha + Q\alpha + R\alpha$, does not "admit of interpretation in general, because there is no vector α which is capable of being operated on by P, Q, and R" (Clifford 1882, 388).

Symbolical analysis drives Clifford's account of vectorial operations here, although Clifford is keen to also highlight the need for empirical interpretation along the way. The lack of interpretive reality underpinning $(P + Q + R)\alpha = P\alpha + Q\alpha + R\alpha$ does not hinder his analysis; it pushes it further. Clifford insists interpretations for such vectorial operations can be determined when the meaning of the symbols being used is extended to more dimensions. Every rectangular versor, he explains, can be represented by the symbolical form $xI + yJ + zK$, where IJK constitutes the "three rectangular versors whose axes are the unit-vectors ijk." As a result, there are two "kinds" of "complex quantities" to consider. The first is "vectors" of the (Hamiltonian form) $\rho = ai + bj + ck$, and the second is "quaternions" of the form $q = w + xI + yJ + zK$. In addition, the product of any number of quaternions is another quaternion, where the units IJK are multiplied according to the (Hamiltonian) rules:

$$IJ = K = -JI, KI = J = -IK, JK = I = -KJ, I^2 = J^2 = K^2 = -1.$$

Because we cannot multiply a vector by a quaternion "in general," $q\rho$ will only be meaningful if ρ is perpendicular to the axis of q—that is, when $ax + by + cz = 0$. Even then, Clifford adds, we cannot find the value of $q\rho$ directly by multiplication (even though we have at our hands the formulaic identities of $Ij = k = -Ji, Ki = j = -Ik, Jk = i = -Kj$), because the symbols Ii, Jj, and Kk are "unmeaning" in themselves. It is only when we assume they have the "same value" that the result of the direct multiplication, $q\rho$, is interpretable.

Importantly, the "artifice" by which, "in the geometry of two dimensions the two kinds of complex quantities were reduced to one, is not applicable here." Although we can symbolically write $ai + bj + ck = (a + bK - cJ)i$, and thus represent every vector by its ratio to the unit i, "it no longer remains true that the product of two such ratios is another ratio of the same sort." We might "attain the desired reduction by a simpler method," Clifford argues. The mathematician could use the symbols ijk in a "double sense," as both vectors and versors. When ρ is regarded as a rectangular versor, the product qp can be obtained as the result of direct multiplication, as in $(w + xi + yj + zk)(ai + bj + ck)$, where ijk stands for what *IJK* had previously stood for. An expression such as $pq\rho$ is "always interpretable," when all the symbols are regarded as operations on vectors. And it is "sometimes interpretable" when ρ is regarded as a vector (i.e. when it is perpendicular to the axis of pq and "provided we make the formal assumption that $i^2 = j^2 = k^2 = -1$"). Clifford draws the conclusion the "artifice" by which,

> one symbol is made to do duty for two meanings is the same in quaternions, which deal with three dimensions, and in scalars, which deal with one dimension. Namely, the signs $+$ and $-$, which are originally unit-*vectors,* indicating the direction of a step forward or backward, receive the additional meaning of *versors*, retaining or reversing the direction

> of a vector; just as the symbols ijk mean vectors originally, and afterwards are made to mean versors too (Clifford 1882, 390).

In complex numbers, the "artifice is essentially different," where the "*product of two vectors,* has very different geometrical relations to its components," Clifford writes. One can algebraically demonstrate that all "geometric algebras" that deal with an odd number of dimensions resemble scalars and quaternions in this regards, while those dealing with an even number of dimensions resemble complex numbers. Consider, for instance, that the versors IJK can be represented on "great circles of a sphere." In such a set-up, those versors could be regarded as steps on the surface of the sphere; if understood in this manner, their ratios lead to the "whole theory of quaternions." On this interpretation, vectors can only be represented by "points on the sphere supposed to have definite *weights* attached to them, proportional to the length of the corresponding vectors." This, Clifford says, is a geometric algebra of three dimensions "interpreted by means of a space of two dimensions which has constant positive curvature, namely the surface of a sphere" (Clifford 1882, 390).

Drawing a link to the geometric studies of Riemann, and Helmholtz's physiological studies of non-Euclidean geometry, Clifford then argues:

> In the same way, we may interpret the algebra of a space of four dimensions, which cannot be imaged, by means of a space of three dimensions having constant positive curvature, of which a clear mental picture may be formed (Clifford 1882, 390).

Though the mental imagery is difficult, Clifford tells his students to consider "any vertical line" and a series of horizontal planes that cut through the line at right angles. In "ordinary Euclidean geometry," he says,

> These planes intersect on the *horizon*, which is a straight line infinitely distant. In the geometry of a space of constant positive curvature, or *elliptic* geometry, the horizon is at a certain finite distance in all directions from the vertical line with which we started; it belongs to that particular line, which is called its *polar*, and is not the same for all vertical lines. Although it appears to be a great circle when viewed from the

> neighbourhood of its polar, yet if we were to go to it and examine it we should find it straight (Clifford 1882, 390).

Furthermore,

> Points of it which are in opposite directions from a point on the polar are really identical; and every straight line in this space resembles a circle in being of finite length, so that if we travel far enough along it we shall arrive at our starting point. Every straight line has a polar line, which is the intersection of all planes at right angles to it (Clifford 1882, 390-391).

Clifford offers his students an example. Take a "very small circle on a sphere" and suppose that it expands "keeping always the same centre." Initially, the circle will be concave inside and convex on the outside. Once the expansion goes far enough, the circle will become a great circle of the sphere, which is essentially the same shape on both sides. That is, it is "straight so far as the surface of the sphere is concerned." In Euclidean space,

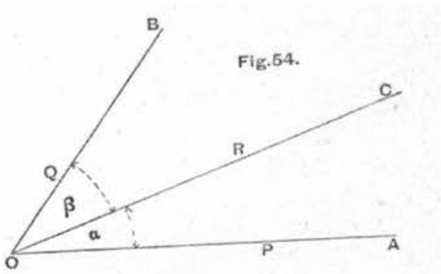
> If we take a sphere and suppose it to expand, keeping always the same centre, it will continue to be concave inside and convex outside so long as it is finite; but when the radius has become infinite, the inside in one direction is the same as the outside in the opposite direction, opposite points being identical; thus the sphere is of the same shape on both sides, or is a *plane* viz., the plane at infinity(Clifford 1882, 391).

In elliptic space,

> just as in geometry on the surface of a sphere, this takes place for a *finite* length of the radius, not for an infinite length; for every point there is a sphere having its centre at that point, which is also a plane. Or, which is the same thing, every point has a polar plane which is the locus of all points situated at a certain distance from it; this distance is called a *quadrant*. So also every plane has a certain point in the plane. All lines and planes perpendicular to the plane pass through its pole, and conversely. The polar lines of all lines in the plane pass through its pole, and so do the polar planes of all points in the plane (Clifford 1882, 391).

Whenever two lines are polars of one another, each point on one line is distant exactly one quadrant from every point on the other. The polar planes of all points of one line pass through the other. In other words, "every line which is at right angles to one meets the other, and conversely" (Clifford 1882, 391).

In his 1873 account of bi-quaternions, Clifford had preliminarily offered a "geometric algebra" that was adapted to fit this form of "elliptic geometry of space." In his interpretation of quaternions on the surface of a sphere, rectangular versors are represented by quadrants of great circles, such that a versor accompanied by a tensor, symbolized by xi, can be represented by an arc AB measured on the great circle i, where $\tan AB = x$. As a result, AB "differs from a vector in a plane in a most important way," Clifford explains. "For while a vector in a plane is unaltered by being moved parallel to itself in any direction, AB can only be slid along its great circle, and must not be moved out of it." Similar quantities in the elliptic geometry of three dimensions are "quantities represented by a length marked off on a certain straight line, which are unaltered when the length is slid along the line but not in any other case" (Clifford 1882, 392). These are "*vectors having position*." Whereas a vector (in Hamiltonian terms) represents the translation-velocity of a rigid body, "which is everywhere the same," these new position-dependent vectors represent the "*rotation-velocity* of a rigid body, which is about a certain definite axis." It is for this reason, Clifford explains, that he has termed these position-dependent vectors "rotors." Rotors, he recalls, are added together according to the law of composition of forces and rotations.



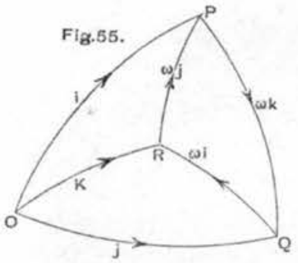
(Clifford 1882)

In other words, if a rotor P along OA were added to a rotor Q along OB, the result gives a rotor R along OC, making the angles α, β. Meanwhile, OC is in the plane of OA, OB, and $P{:}\,Q{:}\,R = \sin\beta : \sin(\alpha+\beta)$, which determines the position and magnitude of the resultant. Clifford says the parallelogram construction used in Hamiltonian vector addition does not work in elliptic geometry, "where the plane AOB is the same as that of the surface of a sphere when opposite points are regarded as identical." No parallelogram of "forces" can be drawn. Hamiltonian algebra requires extension to higher-dimensionality.

Given that two great circles always meet one another, any two rotors have a single rotor which is their "resultant or sum." In three dimensions, this is "not the case," Clifford explains, because in three dimensions, the axes of the two rotors might not meet, in which case their sum is not equal to another rotor. "We may however find two other rotors which have the same sum," although that sum may occur in an "infinite number of ways." Of those, he says, "one is of the greatest importance, namely, that in which the axes of the two rotors are polars of one another" (Clifford 1882, 392). If each rotor is viewed as representing a rotation about an axis, each of the rotations is equivalent to a translation along the other axis. In Clifford's geometric algebra of biquaternions, "rotation about a vertical line is translation along the horizon, and *vice versa,*" where the resultant of two rotations can be compounded into one "screw motion" about either of the axes, and this "describes the most general motion of a rigid body."

Recall that Clifford had previously called this general motion a "*motor*." He adds here that every motor is the "sum of two rotors which do not meet;" thus, every motor "has two axes which are polars of one another." To illustrate the behaviour of motors, the mathematician can draw three planes at right angles through a point O, where unit rotors along their intersections are denoted by ijk. Any rotor through the point O is denoted by $ai + bj + ck$, and the ratio of two rotors takes the form $w + xi + yj + zk$, where ijk refer to rectangular versors whose axes are the rotors ijk. The extension here is not difficult to understand: "In fact, we are merely applying the results of quaternions to vectors passing through a fixed point and their ratios," Clifford says (Clifford 1882, 393).

Clifford adds another extension to the system by arguing that ω can be introduced to denote the operation that "converts any rotor into an equal rotor along the polar line of its axis."



(Clifford 1882)

The result is that $\omega i, \omega j, \omega k$ symbolise rotors that lay along the lines of intersection of the polar planes of O, with the three rectangular planes through O. It is "easy to see," Clifford claims, that any rotor can be "resolved into two"—one which passes through O, the other of which lies in the plane PQR. The former, he says, is compounded of ijk, while the latter is composed of $\omega i, \omega j, \omega k$. The symbolical expression for the sum of a number of rotors (i.e. a motor) is, therefore,

$$ai + bj + ck + \omega(fi + gj + hk) = \alpha + \omega\beta.$$

Clifford then extends the system even further by noting that if the versors ijk are "allowed to operate, not only on rotors through O which meet their axes, but on any rotors which meet them at right angles," then the following algebraic identities can be derived from the resulting geometrical behaviour: $i(\omega j) = \omega k = \omega ij$; $j(\omega k) = \omega i = \omega jk$; $k(\omega i) = \omega j = \omega ki$. These identities indicate that "motor" operation is commutative with regards to the symbols i, j, k. Furthermore, we find that $\omega i. \omega j = k, \omega k. \omega i = j, \omega j. \omega k = i$, from which one can determine $\omega^2 = 1$. The result of this last identity leads Clifford to the "very important consequence" that every motor $\alpha + \omega\beta$ can be written in the equivalent forms $(\xi + \eta)\alpha + (\xi - \eta)\beta$ or $\xi(\alpha + \beta) + \eta(\alpha - \beta)$, where $\xi = \frac{1}{2}(1 + \omega), \eta = \frac{1}{2}(1 - \omega)$ (Clifford 1882, 394). The upshot of these symbolical manipulations is to demonstrate that the ratio of any two motors can be represented by the symbolical form $\xi p + \eta q$, or, "which is the same thing," Clifford says, $s + \omega t$, assuming $2s = p + q, t = 2p - q$ and that p, q, s, t are all quaternions. The combination of two such extended and generalized quaternions is another way of representing what Clifford terms a "biquaternion."

In a presentation to the London Mathematical Society delivered in the same year, Clifford locates his biquaternion system in relation to a historical lineage of geometric algebras. He identifies those historical versions of geometric algebra as forming the basis of his own inquiries. Clifford tells the Society the following works had led up to his own formation of biquaternions:

1806 Argand, *Manière de représenter les quantités imaginaires.*
Buée, *Mém. Sur les qu. Imag.*
1827 Möbius, *Barycentrischer Calcul.*
1831 Gauss [Letters].
1834 Peacock, *Doctrine of Operations in Algebra.*
1843 Hamilton, *Quaternions.*
1844 Grassmann, *Lineale-Ausdehnungslehre.*
1845 Saint-Venant, *Multiplication of vectors.*
1848 Kirkman, *Pluquaternions and Homoid Products.*
1853 Cauchy, *Clefs Algébriques.*
1862 Grassmann, *Ausdehnungslehre.*
1870 Pierce, *Linear Associative Algebra* (Clifford 1882, 397).

This list is interesting for a number of reasons. It indicates Clifford's efforts to familiarize himself with the various texts (European and British) that discuss some aspect of "geometric algebra" as he had come to define it. It also indicates Clifford's view that Hamiltonian quaternions constitute only one in a series of geometrical and algebraic analyses of geometry. Not surprisingly, Clifford nowhere refers to Hamilton's "genius" or Hamilton as the progenitor of directed line concepts and operators, nor does he mention Tait. Revealingly, however, he does identify his "bi-quaternions" as an obvious extension of concepts in higher-dimensionality. In 1827, the Barycentric Calculus had represented a point by a complex number, he writes. Grassmann then demonstrated that one can regard the symbol ab as the "ordinary symbol for a line joining two points, as the nature of a product." The algebra of that system is distributive, and Grassmann's "extensive quantities" emerge when ab is both a line and a product, such that $aa = 0$ (i.e. $(a_1\iota_{1+}a_2\iota_2)^2 = 0$, which requires that $\iota_1\iota_2 = -\iota_2\iota_1$; such that $ab = -ba$). Hamilton's system extended further that those two accounts, he argues. In the "theory of Quaternions," the symbols ijk are used as multipliers. They "represent not things but operations of turning," Clifford explains. Therefore, $i^2 = -1$, not 0. Those multipliers can be viewed as vectors and, as vectors, they represent the geometry of the plane which passes through the ends of ijk.

Clifford elaborates upon the similarities manifest between Hamilton and Grassmann's respective systems. The Grassmannian algebra "will be reproduced if we attend only to the *vector* part of the binary products, and the *scalar* part of the ternary" in Hamilton (Clifford 1882, 397). Importantly, "physical considerations" lead the mathematician to regard i^2 as a scalar rather than when i is first regarded as a vector (not a versor). In such cases, i can be equivalent to either $+1$ or -1. Clifford then translates Hamilton's quaternion notation into Grassmannian forms. First, he explains, the quaternion symbols satisfy the equation $ijk = -1$, and this, combined with the "assumption" that ρ^2= scalar, gives all of the "laws of multiplication" of quaternions. If one stipulates that $\omega = \iota_2, \iota_2, \ldots \iota_n$, then in the case that $n = 3$ (where we assume n units, $\iota_1 \iota_2 \ldots \iota_n$, such that ${\iota_s}^2 = +1$, and $\iota_r \iota_s = -\iota_s \iota_r$), we may take ω to be a scalar. Clifford draws a "very important" distinction here between the cases where n is odd and the cases where n is even. In the former case, ω is commutative with the symbols ι, or $\omega\iota = \iota\omega$. In the latter case, $\omega\iota = -\iota\omega$. Hence, when n is odd, ω acts as a scalar; when n is even, it acts as a vector. By making "$\omega =$ to a scalar in the former case we are conveniently representing two different things by the same symbols, because they have the same laws of combination" (Clifford 1882, 398). Thus, one reduces the algebra to 2^{n-1}units when n is odd, and whenever n is even the symbol ω has an even order. If we restrict the system such that $n = 3$, the algebra produces quaternions "at once." Meanwhile, when n is even, the symbol ω belongs to the even algebra which contains 2^{n-2} terms. When $n = 4$, the even algebra produces "biquaternions," along with its general expression, $q + \omega r$, where q, r are quaternions.

At this stage in his career, Clifford was still reading Grassmann through Hermann Hankel's (1839-1873) version. Hankel, he says, presents the "extensive quantities" of Grassmann as "alternate numbers." Those numbers are symbols that possess the property of "polar multiplication," by which $ab = -ba$, and therefore whose square vanishes, such that $a^2 = 0$. This system conveniently serves to represent "the projective geometry of n dimensions." In plane geometry, if we allow the symbols $\iota_1, \iota_2, \iota_3$ to represent three points, then we find the expression $a = a_1\iota_1 + a_2\iota_2 + a_3\iota_3$ represents a point which is the "centre of inertia of masses a_1, a_2, a_3." If the products $\iota_2\iota_3, \iota_3\iota_1, \iota_1\iota_2$ are understood to mean those lines joining the fundamental points, then the product of ab represents the line joining the points a, b.[129] In a like manner, the ternary product, $abc = \begin{vmatrix} a_1, & a_{2,} & a_3 \\ b_1, & b_2, & b_3 \\ c_1, & c_{2,} & c_3 \end{vmatrix} \iota_1\iota_2\iota_3$, is proportional to the area of the triangle abc. It therefore vanishes when the three points are collinear.

Hamilton's system of quaternions differs from Grassmann's extensive algebra in that, first, the squares of the units do not equal zero—instead, they equal -1. Secondly, the ternary product is also made equal to -1. The "interpretation" is at the same time extended to three dimensions, Clifford says, but that extension nonetheless suffers from a "restriction." Whereas the alternate units represent "*any* three points in a plane, and the system deals primarily with projective relations, Hamiltonian units represent three vectors at right angles, and the system is the natural language of metrical geometry and of physics" (Clifford 1882, 399). As Clifford had previously mentioned in his 1873 paper, Hamiltonian quaternions are limited to planes that are normal to one another. Clifford appeals to the Grassmannian system to determine whether there is any sort of analogue between the Hamiltonian rule that $\iota_1\iota_2\iota_3 = -1$, and the behaviour of ω when ω when $\omega = \iota_1\iota_2 \dots i_n$. His aim in so doing is to determine the value of ω^2, and his analysis leads him to conclude there are "four classes of geometric algebra" according to the value of $\omega\omega$ and its associated product $\omega\iota$. These classes of algebra are variously defined by whether $\omega^2 = +1$ or $\omega^2 - 1$, and whether $\omega\iota = \iota\omega$ or whether $\omega\iota = -\iota\omega$ (Clifford 1882, 401).

In his classification of algebras, Clifford indicates that no contradiction, or fight for priority, can exist between the Hamiltonian and Grassmannian systems, let alone any other previous historical

[129] That is, $ab = (a_1\iota_1 + a_2\iota_2 + a_3\iota_3)(b_1\iota_1 + b_2\iota_2 + b_3\iota_3) = (a_2b_3 - a_3b_1)\iota_2\iota_3 + (a_3b_1 - a_1b_3)\iota_3\iota_1 + (a_1b_2 - a_2b_1)\iota_1\iota_3$.

versions of what he took to be proto-algebraic geometrical systems. Rather, in the tradition of Cauchy and Klein, Clifford identifies the common characteristics unifying those diverse sets of mathematical analyses, and he highlights the malleable and interpretative nature of these varied symbolical algebras. In his effort to "classify," by highlighting common characteristics within diverse algebraic systems, Clifford also makes clear the degree to which evolutionist beliefs in the need to "integrate" diverse ideas in order to generate evolutionarily advanced modes of thinking motivates his mathematical work.

By 1878, for instance, Clifford states that he finally had the chance to read Grassmann's *Ausdehnungslehre* directly, as opposed to relying upon Hankel's translation in *Lectures on Complex Numbers,* or Grassmann's shorter accounts in *Crelle's Journal*. He says that, upon reading the *Ausdehnungslehre* directly, he wished to "express my profound admiration of that extraordinary work, and my conviction that its principles will exercise a vast influence upon the future of mathematical science." Clifford acknowledges the overarching superiority of the Grassmannian approach, and locates Hamiltonian quaternions and his own "bi-quaternions" in relation to Grassmann's "extensive algebra." Grassmann's is the simplest of all three systems, he explains, noting that by placing quaternions and bi-quaternions within the Grassmannian system, he is able to generate "a generalization of them, applicable to any number of dimensions; and a demonstration that the algebra thus obtained is always a compound of quaternion algebras which do not interfere with one another" (Clifford 1882, 266).

As an example, Clifford states that in the past he had used the term "polar multiplication" (i.e. a term recommended by Sylvester) to indicate the product ab has opposite properties at its two ends, such that $ab = -ba$, while "ordinary or commutative multiplication" is mere "scalar" multiplication. The former corresponds to Grassmann's "outer product," which can produce negative areas, and the latter corresponds to Grassmann's "inner product," which can produce non-directed magnitudes only. If one considers a system of n units, $\iota_1, \iota_2, \dots \iota_n$, such that the multiplication of any two units is polar ($\text{i. e.}\, \iota_1\iota_2 = -\iota_2\iota_1$), one can interpret these elements as points lying in a flat space of $n - 1$ dimensions. A binary product is therefore a unit length measured on the line joining the two points ($\iota_1\iota_2$) being multiplied, while a tertiary product is the unit area measured on the plane through the three elements. By extension, in spaces of three dimensions, one may take the product of four elements (i.e. points), such as $\iota_0, \iota_1, \iota_2, \iota_3$, where the latter three points lay at infinite distances from the first point, ι_0.

To understand this process, we must recall there are two interpretations to the notation of any given product. When we state that $2 \times 3 = 6$, we can regard 6 as a number "derived from the numbers 2 and 3 by a process in which they play similar parts," in which case both 2 and 3 are concrete numbers that are operated upon, or "we may regard it as derived from the number 3 by the operation of doubling," in which case 2 signifies the operation of doubling and 3 is a concrete number. The Grassmannian system is based upon the former interpretation, such that one symbol represents a particular operation and the other symbol represents a concrete thing. Here emerges the conventionalist-empiricist motif running throughout Clifford's mathematical researches. In the Grassmannian system, he writes, a line is regarded as the product of two points and a parallelogram as the product of its sides—meaning that the "two factors are things of the same kind and [they] play similar parts" (Clifford 1882, 267). In a quaternion equation such as $q\rho = \sigma$, where ρ and σ are vectors, the quaternion q is an "operation of turning and stretching" that converts ρ into σ—it is "a thing totally different in kind from the vector ρ," Clifford writes. In order to bring it into the Grassmannian system, the factors q and ρ must be interpreted as being of the "same kind." The way to do this is to view ρ as a special case of a quaternion. It must be viewed as a rectangular versor. Yet, "in that case the expression does not receive its full meaning until we suppose a *subject* on which the operations ρ and q can be performed in succession" (Clifford 1882, 268). Clifford does this by recalling in quaternion mathematics, i, j, k represent rectangular versors that "turn a figure through a right angle in the three co-ordinate planes respectively." If either versor is applied twice to the same figure, the result is that it turns the figure through two right angles (i.e. it reverses the figure). The following identities represent that phenomena: $i^2 = j^2 = k^2 = -1$.

Clifford translates those versors into Grassmannian notation, using the four elements, $\iota_0, \iota_1, \iota_2, \iota_3$. Suppose, he says, that i turns the line $\iota_0\iota_2$ into $\iota_0\iota_3$; j turns $\iota_0\iota_3$ into $\iota_0\iota_1$; and k turns $\iota_0\iota_1$ into $\iota_0\iota_2$. The "turning of $\iota_0\iota_2$ into $\iota_0\iota_3$ is equivalent to a translation along the line at infinity at $\iota_2\iota_3$," Clifford explains. Hamilton's notation can be translated into the more general Grassmannian terms. The symbolical result of this manipulation is that ${\iota_2}^2 = -1$. Similarly, ${\iota_1}^2 = {\iota_3}^2 = -1$. The Hamiltonian identities (i.e. Hamilton's rules of multiplication) can all be translated into Grassmannian terms as follows:

$$jk = \iota_3\iota_1 . \iota_1\iota_2 = \iota_2\iota_3 = i$$

$$ki = \iota_1\iota_2 . \iota_2\iota_3 = \iota_3\iota_1 = j$$

$$ij = \iota_2\iota_3 . \iota_3\iota_1 = \iota_1\iota_2 = k$$

$$ijk = \iota_2\iota_3 . \iota_3\iota_1 = \iota_1\iota_2 = -1.$$

Thus, "to bring the quaternion algebra within that of the *Ausdehnungslehre*, we have to make the square of each of our units equal to -1," as pointed out by Grassmann, Clifford writes.

Clifford's ultimate opinion is that the quaternions do not answer to the notion of "Elementargrösse," as described in the *Ausdehnungslehre*, but rather to the binary products of those elements, "from which supposition, as we have seen, the laws of their multiplication follow at once." This conception of a product, as derived from factors of the same kind (i.e. as the product of two vectors), had led to the gradual development in "Hamilton's mind" of replacing the units i, j, k with the more general symbols S and V, Clifford notes. To explain the laws of multiplication using those later symbols, however, one must draw on a theory of "Erganzung"—namely, the representation of the area ij by a vector k perpendicular to it. The explanation of this "case," Clifford says, "is by no means so easy." But, he adds,

> It is instructive to observe that the distinction between a quantity and its "Ergänzung," i.e. between an area and its representative vector, which, for some purposes it is convenient to ignore, has to be reintroduced in physics. Thus, Maxwell specially distinguishes the two kinds of vectors, which he calls *force* and *flow*, and which in fact are respectively linear functions of the units and of their binary products (Clifford 1882, 269).

In other words, Hamilton finished the job only partly. It remains for physicists to finish it completely.

Clifford, meanwhile, locates his own bi-quaternion extensions with the generalized Grassmannian system. In so far as we have regarded i, j, k as rectangular versors that operate on the quantities $\iota_0\iota_1, \iota_0\iota_2, \iota_0\iota_3$, and in so far as these quantities are taken to refer to unit lengths measured anywhere on the axes in the positive direction, they have magnitude, direction and position. Thus, they are what Clifford had earlier termed "rotors," as distinguished from vectors, which have magnitude and direction but no position. Clifford sums up the difference in saying,

> A vector is of the nature of the translation-velocity of a rigid body, or of a couple; it may be represented by a straight line of given length and direction drawn *anywhere*. A rotor is of the nature of the rotation-velocity of a rigid body, or of a force; it belongs to a definite axis (Clifford 1882, 269).

A vector can, therefore, be represented as the difference between two equally weighted points. Vector ab can be written as $b - a$, and the symbols $\iota_1, \iota_2, \iota_3$ refer to unit vectors along the axes. The versors, i, j, k, will operate upon those vectors in the same way they operate on the rotors $\iota_0\iota_1, \iota_0\iota_2, \iota_0\iota_3$. In elliptic or hyperbolic geometry of three dimensions, the four points, $\iota_0, \iota_1, \iota_2, \iota_3$, must be taken as the vertices of a tetrahedron self-conjugate with regards to the absolute, such that the distance between each pair is a "quadrant". The product of any even number of linear factors will be of the same form—namely, that of a biquaternion, which can be "exhibited" symbolically as follows. If $\omega = \iota_0\iota_1\iota_2\iota_3$, then $i = \iota_2\iota_3$, $j = \iota_3\iota_1$, $k = \iota_1\iota_2$, and $\omega i = i\omega = \iota_1\iota_0$, $\omega j = j\omega = \iota_2\iota_0$, $\omega k = k\omega = \iota_3\iota_0$, $\omega^2 = 1$. In sum, the product of an even number of factors greater than two will always be a linear function of $1, i, j, k, \omega i, \omega j, \omega k$. It can be expressed in the symbolical form (of a linear equation) as $q + \omega r$, where q and r are quaternions. The multiplication of ω with i, j, k is scalar, although its multiplication with $\iota_0, \iota_1, \iota_2, \iota_3$ will be polar. The "effect" of multiplying a system by ω is to change that system into its polar system with regards to the absolute.

Hamiltonian quaternions, Grassmann's algebra, and Riemannian geometry are brought together into one system that indicates the uncertainty of empirical observations of the behaviour of rigid bodies in space and the necessary conventionalism of symbolical manipulations. The chief classification of geometric algebras is according to those of "odd and even dimensions." The geometry of an elliptic space of n dimensions is the same as the geometry of the points at an infinite distance in a flat or parabolic space of $n + 1$ dimensions. In other words, "The theory of *points* and *rotors* in the former is the same as that of vectors and their products in the latter," Clifford writes (Clifford 1882, 271). Clifford therefore concludes,

> The algebra of four units, leading as above to biquaternions, is either that of points and rotors in an elliptic space of three dimensions, or of vectors and their products in a flat space of four dimensions. All geometric algebras having an even number of units are closely analogous to it (Clifford 1882, 271).

In Clifford's view, the symbolical representation of these varying "geometric algebras" helps to highlight their underlying connectivity. Clifford "integrates" the diverse systems of geometric algebra that he has identified through his own classification system, which relies upon the generalized formalism of Grassmann. Unlike Grassmann, however, Clifford's aim—as evidenced by his reference to Riemann's work—is to highlight the role of these algebraic systems in characterizing the nature of space itself, as well as the physical phenomena that take place within it. Clifford has analytically linked the world of flat, Euclidean space to the infinite worlds of higher-dimensional spaces, including non-flat and (especially) positively curved spaces. The analytical similarities between the systems imply new physiological and epistemological quagmires, as humans are not always able to easily distinguish between differing spatial models, especially when living within them, despite the availability of "exact" measuring tools.

Luciano Boi (1992) has argued that Clifford's uniqueness lay in his combination of Hamilton's quaternions, Grassmann's algebra, and non-Euclidean (especially elliptic) geometry. His grand idea, Boi states, is that algebra constitutes a means of interpreting geometric phenomena and that for each algebraic operation there exists a geometric analogue. Boi is right to place emphasis on Clifford's idiosyncratic combination of these conceptual resources. In addition, Clifford emphasizes the perceptual equivalence of rotation about an axis at an infinite distance with that of a simple translation upon a positively curved surface. Yet, more than Boi (1992) realized, this latter interpretation was informed by Clifford's interests in the physiology of perception, and the evolutionary limitations of human observers. It is to this terrain of knowledge that we turn now.

Clifford's Darwinism

A great deal of space has been devoted already to discussing the fact Clifford drew upon resources provided by evolutionary theorists, who had gained popularity in Britain following the publication of Darwin's *Origin of Species* (1859).[130] The "terrain of knowledge" that emerged from those theories, and the lobbying efforts of their advocates, constituted an "evolutionist" intellectual environment, as opposed to a purely "Darwinist" one. "Evolutionism" was a social, political and philosophical movement that can be distinguished in important ways from Darwin's own theory of natural selection (Bowler 1989). The two most important aspects of Darwin's evolutionary theory, as stated in the *Origin of Species*, included the notions that variations in a population are random and that local environments determine which variations survive to reproduce in future generations. Darwin's proposed theory was, in fact, too radical for many of his contemporaries, as it denied an overall guiding force behind organic development. It left human destiny open to the randomness of mutation and other arbitrary environmental forces. Specifically, *human* characteristics were the result of environmental responses, rather than divine traits. Relatively few Victorian scientists, or social and political commentators, were actually "Darwinist" as a result (Bowler 1984). While it is the

[130] Clifford also drew from nascent developments in logic, as propounded by Stanley Jevons, as he worked to develop his largely speculative and incomplete theory of "mind stuff" in the late 1870s.

case that many scientists in the 1860s adopted some notion of "evolution," they often did so from within the perspective of "design" or with hopes in mind for a liberal transformation of society towards a more progressive whole. "Progressionism" became a Victorian religion of sorts, imbuing social and political discourse with the idea of positive transformation through the improvement of individuals. From the progressionist standpoint, moral, ethical, and political changes were taking place constantly, and they were all headed towards an improved social order.

Clifford's navigation through this "terrain" of progressionist discourse was a mediated and unique one. He adopted the idea that random variations play a role in developing new capacities and new human traits. He also came to believe that changes in the structure of the mind take place over time and can lead to the capacity for new thought. In his lectures in 1874, collectively entitled "Body and Mind," and delivered to the Sunday Lecture Society, Clifford explicitly states that such variations in the structure of the human brain have allowed for the improvement of scientific theories and the conceptualizations of new geometries. The body, including the brain, is in a constant state of flux and this change has evolutionary consequences with regards to the sophistication with which we engage in mathematical and scientific speculations. In extending his 1868 notions of "integration" and "differentiation," Clifford claims humans can advance the evolutionary process themselves by encouraging processes of change within the grey matter of their individual brains; they do this by exposing themselves to ever new stimuli, including new scientific and mathematical accounts (Clifford 1886, 244-273).

By the mid-1870s, Clifford's empiricist-evolutionist beliefs constituted a distinct motif that ran throughout his general and specialist discourses. He sculpted a nascent philosophy of empirical and evolutionary mathematics as he navigated through mid-Victorian accounts of Hume, while at the same time absorbing the increasingly bold lessons of spontaneous evolution. In response to criticisms of the incompleteness and imperfection of empirical-evolutionist views, for instance, Clifford contended the critics had effectively highlighted the very nature of knowledge itself. It is fundamentally incomplete and fundamentally human. As Clifford wrote to Pollock in 1874,

> I hope you have seen Sidgwick's remarks (I think in the *Academy*); he points out that to prove Hume insufficient is not to do much in the present day. It should, I think, be brought out clearly that if we pay attention only to the scientific or empirical school, the theory of consciousness and its relation to the nervous system has progressed in exactly the same way as any other scientific theory; that no position once gained has ever been lost, and that each investigator has been able to say 'I don't know' of the questions which lay beyond him without at all imperiling his own conclusions. Green, for instance, points out that Hume has no complete theory of the *object*, which is of course a very complex thing from the subjective point of view, because of the mixture of association and symbolic substitution in it; and in fact I suppose this piece of work has not yet been satisfactorily done. But it seems merely perverse to say that the scientific method is a wrong one, because there is yet something for it to do; and to find fault with Hume for the omission is like blaming Newton for not including Maxwell's Electricity in the *Principia* (Clifford 1901, 58).

In his 1874 lecture to the Sunday Lecture Society, entitled "First and Last Catastrophe," Clifford elaborates upon Maxwell's molecular theory of matter and then proceeds to deconstruct Maxwell's concomitant claim that the molecular theory proves creationism and destroys evolutionism. The crux of the molecular theory is that all matter is made up of identical particles that possess energy, Clifford states. Clifford agrees with Maxwell, and the northern cohort of energy scientists, who had argued that all oxygen particles are constituted identically, regardless of when or where in the universe they exist. Maxwell had argued that they cannot be the product of "natural" formation or evolution. Their sameness bears the stamp of a Creator who had created atomic entities in exact likeness with one another.

Clifford's response maintains this molecular theory while at the same time invoking a boldly evolutionist outlook to deconstruct Maxwell's metaphysical spin. Clifford laments the fact that someone so eminent as Maxwell had given his support over to such anti-evolutionist claims, as Maxwell's authority in science would lead many people to agree with the Creationist viewpoint. Clifford tells his audience:

> [Maxwell] said that because these molecules are exactly alike, and because they have not been in the least altered since the beginning of time, therefore they cannot have been produced by any process of evolution ... I was to consider whether the evidence we have to prove that these molecules are exactly alike is sufficient to make it impossible that they can have been produced by any process of evolution (Clifford 1901, 238).

By referring to various molecular experiments from the past, including those carried out by "Dr. Graham, late Master of the Mint," which had measured the rate at which different gases mixed together, Clifford states the rates are found to nearly coincide each time, experimentally justifying the molecular theory. However, such experiments only prove that all molecules of particular gases are "nearly" the same given that "if there is any difference it is too small to be perceived by our present means of observation." In other words,

> Evidence of that sort can never prove that they are exactly of the same weight. The means of measurement we have may be exceedingly correct, but a certain limit must always be allowed for deviation; and if the deviation of molecules of oxygen from a certain standard of weight were very small and restricted within small limits, it would be quite possible for our experiments to give us the results which they do now (Clifford 1901, 242).

Clifford goes on to cite a series of spectroscopy experiments carried out by Lord Rayleigh, among others, which had implied that perhaps molecules of a particular sort are not in fact all of the same size. The upshot of those experiments is to highlight the fact that Maxwell's molecular theory in no way discounts evolutionary theory of organic or inorganic objects. Clifford contends:

> I do not understand myself how, even supposing we knew that they were exactly alike, we could infer for certain that they had not been evolved; because there is only one case of evolution that we know anything at all about—and that we know very little about yet—namely, the evolution of organized beings. The processes by which that evolution takes place are long, cumbrous, and wasteful processes of natural selection and hereditary descent. They are processes which act slowly, which take a great lapse of ages to produce their natural effects. But it seems to me quite possible to conceive, in our entire ignorance of the subject, that there may be other processes of evolution which result in a definite number of forms—those of the chemical elements—just as these processes of the evolution of organized beings have resulted in a greater number of forms. All that we know of the ether shows that its actions are of a rapidity very much exceeding anything we know of the motions of visible matter. It is a possible thing, for example, that mechanical conditions should exist according to which all bodies must be made of regular solids, that molecules should all have flat sides, and that these sides

> should all be of the same shape. I suppose that it is just conceivable that it might be impossible for a molecule to exist with two of its faces different (Clifford 1901, 250).

Clifford concludes, however, that

> we have nothing definite to go upon [to determine] what the shape of a molecule is, or what is the nature of the vibration it undergoes, or what its condition is compared with the ether; and in our absolute ignorance it would be impossible to make any conception of the mode in which it grew up … In our present ignorance all we have to do is to show that such experiments as we can make do not give us evidence that it is absolutely impossible for molecules of matter to have been evolved out of ether by natural processes (Clifford 1901, 251).

Similar to Maxwell's appropriation of legitimate science in the name of speculative religious philosophizing, "physicalists" such as Thomson, Tait, Stewart, and Murphy had used the principles of the "conservation" and "continuity" of "energy" to defend their view that some catastrophic beginning to the universe had occurred in the past. Clifford argues that such theorizing is absurd as,

> It is not according to the known laws of nature, it is according to the known laws of conduction of heat, that Sir William Thomson is speaking; and from this we may see the fallacy of concluding that if we consider the case of the whole universe we should be able, supposing we had paper and ink enough, to write down an equation which would enable us to make out the history of the world forward as far forward as we liked to go; but if we attempted to calculate the history of the world backward, we should come to a point where the equation would begin to talk nonsense we should come to a state of things which could not have been produced from any previous state of things by any known natural laws. You will see at once that that is an entirely different statement (Clifford 1901, 256).

Clifford contends the conservation of heat is not the only process that goes on in the universe. To assume that the dissipation of "energy" has a finite past when mapped out backwards, and then to conclude that its finite origins constitute the catastrophic moment of Creation, is to assume that the conservation (and dissipation) of energy constitute the only processes in the universe that determine the development of organic and inorganic matter.

Clifford rejects such accounts outright, just as he rejects the concomitant religiosity associated with those thermo-dynamical claims. He says that such a view,

> Depends upon the same assumption that the laws of geometry and mechanics are exactly and absolutely true; and that they will continue exactly and absolutely true for ever and ever. Such an assumption we have no right whatever to make (Clifford 1901, 264).

Clifford's public attack on the religious musings of the northern scientists of "energy" would have found a happy home in mid-Victorian Britain. Indeed, despite its underlying design tones, one of the offshoots of the rise of "progressionism" in Britain was the increasingly public nature of scientific discourse imbued with religious (or anti-religious) themes. Actors such as Huxley, Bishop Wilberforce, Tyndall, Francis Galton, Gladstone and Clifford all thought there was a direct conflict between science and religion (Turner 1993). Sermons that criticized the egotism of scientists rang out in British churches, while journals and newspapers issued attacks on the ignorance and authoritarianism of the clergy. The rise of mid-century "empiricism" in Britain can be linked to those popular advocates of secularism and science such as Huxley, Tyndall, Dalton and Henry Maudsley, who argued religious stipulations and metaphysical constraints were hindering the full development of science in the country. In defense of Darwin's *Origins of Species,* for instance, Huxley famously declared "Extinguished theologians lie about the cradle of every science as the strangled snakes beside that of Hercules" (Turner 1993, 173). It is not surprising, then, to find the avowedly agnostic Clifford devoting pages upon pages in his lecture series "Body and Mind" to claims made by Huxley and other agnostic criticisms of inexplicable "causes"—i.e. claims that god-like notions lurk behind theologically-inspired science.

Huxley's neologistic use of the word "agnosticism" to identify like-minded secularists, as well as Tyndall's blatant attack on Tait and Thomson's religious views in previous years, constituted two anti-religious precedents that Clifford followed upon. Clifford actively sought to engage the public in matters of religious criticism throughout the 1870s. He did so by identifying the empirically an conventionally malleable nature of scientific and mathematical knowledge. By the mid-1870s, Clifford's engagement in such matters had become increasingly adversarial. Questions about the nature and status of geometry were not distinct from questions about the nature and status of God.

The publication of Stewart and Tait's *The Unseen Universe* (1875) spurred him to issue a frenzied diatribe against the proper place of God in science, which, in Clifford's view, was nowhere.

On the other hand, religious scientists, such as Maxwell, viewed the lamentable rise in agnosticism as being linked to the transfer of authority from church to universities had degraded the overall quality of intellectualism in the country. As Maxwell stated in 1873,

> It is simply this, that while the numbers of our professors and their emoluments are increasing, while the number of students is increasing, while practical instruction is being introduced and text-books multiplied, while the number and calibre of popular lectures and popular writers in Science is increasing, original research, the fountain-head of a nation's wealth, is decreasing (Turner 1973, 175).

The expansion of professional scientists and professional bodies in newly emerging disciplines paralleled the wider dispersion of science through the emergence of an increasing number of small laboratories across the country and the rise of popular science discourse. Science was being produced for mass consumption. The rise of mid-Victorian Whig organizations, such as the Society for the Diffusion of Useful Knowledge, added to the dilution of specialist knowledge among laypeople. Reverence that had once been given "to priests and to their stories of an unseen universe," was now being transferred "to the astronomer, the geologist, the physician, and the engineer" (Turner 1973, 175). For devout believers, such as Maxwell, Tait and Stewart, this diffusion of access to scientific knowledge could have a negative impact on legitimate sources of authoritative knowledge. Though not necessarily seeking to grant absolute authority back to the church, scientists such as Maxwell, Tait and Stewart did aim to shift it away from the unfettered sciences of secularity, i.e. Darwinism, to venerable, God-fearing sciences such as thermodynamics.

One of the vehicles by means of which both supporters and antagonists of the new secularist-evolutionist discourse advocated their respective causes was the journal *Mind,* edited by the Scottish philosopher George Croom Robertson (1842-1892) and financed by Alexander Bain. In its first issue, appearing in January 1876, the editor notes that the historical lack of an English journal of philosophy or psychology is "not surprising" given that the professions of philosopher and psychology were only then beginning to gain wider recognition. Robertson writes,

> The signs ... that mental science and philosophy have for some time past been cultivated with a more single-minded endeavour, and that the class of those who are specially interested is growing steadily larger, are neither few nor uncertain (Robertson 1876, 2).

It is revealing of the state of popularized discourse regarding mathematics that one of the first debates to unfold in the pages of this Scottish journal was that between Helmholtz and J. P. Land on the issue of non-Euclidean geometry. In his paper, "The Origin and Meaning of Geometrical Axioms," published in *Mind* (July 1876), Helmholtz opened with the bold claim that he would discuss "the philosophical bearing of recent inquiries concerning geometrical axioms and the possibility of working out analytically other systems of geometry with other axioms than Euclid's" (Helmholtz 1876, 21). Helmholtz describes the traditional basis of mathematics as that of axiomatic rules—statements of truth about mathematical systems. But, he asks, where do those axioms come from? He wonders,

> What is the origin of such propositions, unquestionably true yet incapable of proof in a science where everything else is reasoned conclusion? Are they inherited from the divine source of our reason as the idealistic philosophers think, or is it only that the ingenuity of mathematicians has hitherto not been penetrating enough to find the proof? (Helmholtz 1876, 302).

The problem with the axioms of geometry, Helmholtz explains, is that "everyday experiences become mixed up as apparent necessities of thought" (Helmholtz 1876, 302). Recent geometrical investigations had come to new and profound conclusions "by means of the purely abstract methods of analytical geometry," he argues. Yet, the results of those methods are based upon the analysis of experience.

Here Helmholtz offers an account of two-dimensional worms. He tells his readers to suppose that there are two-dimensional beings that live and move on the surface of a solid body. He continues,

> We will assume that they have not the power of perceiving anything outside their surface, but that upon it they have perceptions similar to ours. If such beings worked out a geometry, they would of course assign only two dimensions to their space. They would ascertain that a point moving describes a line, and that a line in moving describes a surface. But they could as little represent to themselves what further spatial construction would be generated by a surface moving out of itself, as we can represent what would be generated by a solid moving out of the space we know ... Now as no sensible impression is known relating to such an unheard-of event as the movement to a fourth dimension would be to us, or as a movement to our third dimension would be to the inhabitants of a surface, such a "representation" is as impossible as the "representation" of colours would be to one born blind, though a description of them in general terms might be given to him (Helmholtz 1876, 304).

Those surface-beings would be able to draw out lines—not necessarily straight lines, but the "straightest" lines possible (also known as "geodetic" lines, he explains)—which would bring out their analogy with the straight line in a plane. If all beings lived in this way, their geometry would be the same as our "planimetry" (plane geometry), Helmholtz concludes. They would hold that only one straight line is possible between two points and that through a third point not lying on that line only one line could be drawn parallel.

However, what if our intelligent beings were to inhabit the surface of a sphere?, Helmholtz asks. In that instance, their perceptions would differ in that between any two points at polar opposites on the sphere, there would be an infinite number of geodesic lines connecting them. Thus, these sphere-surface dwellers would find their geometrical axioms differ from those of the plane-dwellers. For instance, the sum of the triangles on the surface of the sphere would always be greater than two right angles. Consider now the instance of egg-surface dwellers. Helmholtz explains that in such instances, the rules governing geometry would be radically different from those of the plane-dwellers and those of the sphere-dwellers. Whereas in the latter two cases, constant curvature (or zero curvature) of the surface means shapes remain congruous to themselves as they move through space, the egg-dwellers would experience changes in shape and size as they move about the surface of the egg, from the ends where the curvature is greater to the sides of the egg-surface, where the curvature is less.[131] The upshot of these examples is to argue that geometrical axioms are founded upon empirical perceptions.

Helmholtz concludes it is perceptually impossible for us to know whether we are living as beings on a constantly-curved surface or as beings on a flat surface, without experimenting in either case to determine whether the rules of geometry, as we suppose them to hold, do in fact hold throughout space in all directions. By the 1860s, Helmholtz had become deeply involved in the developing science of thermodynamics in Scotland. He was a personal friend of Thomson and Tait's (he often visited Thomson and even travelled aboard his yacht on vacation). He was also a scientific colleague. Helmholtz's views in *Mind* are revealing of the "terrains of knowledge" he himself was treading upon in his engagements with non-Euclidean geometries. A study of Helmholtz's cultural context lies beyond the remit of this chapter, but it is interesting to note his overall view at the time is that geometrical knowledge requires an intimate acquaintance with developments in physics. He writes,

> I would again urge that the axioms of geometry are not propositions pertaining only to the pure doctrine of space. As I said before, they are concerned with quantity. We can speak of quantities only when we know of some way by which we can compare, divide and measure them. All space-measurements and therefore in general all ideas of quantities applied to space assume the possibility of figures moving without change of form or size. It is true we are accustomed in geometry to call such figures purely geometrical solids, surfaces, angles and lines, because we abstract from all the other distinctions physical and chemical of natural bodies; but yet one physical quality, rigidity is retained. Now we have no other mark of rigidity of bodies or figures but congruence, whenever they are applied to one another at any time or place, and after any revolution. We cannot however decide by pure geometry and without mechanical considerations whether the coinciding bodies may not both have varied in the same sense (Helmholtz 1876, 319).

The debate that follows with Land over the course of the following two years centres on these empiricist claims. Land argues that the "rigidity" ascribed to geometrical figures has nothing to do with "physical quality." Being a "rigid body" is a part of the geometrical definition of the object, Land writes. Thus, it is impossible for a geometrical body to lose its rigidity in the same way that an India-

[131] Helmholtz defines "the measure of curvature" as being the "reciprocal of the product of the greatest and least radii of curvature." According to Gauss, for a figure to move about a surface without changing shape, this measure must be constant for all measures of the radii (Helmholtz 1876, 305).

rubber ball could be flattened (Land 1877, 44). Helmholtz's response is to argue that, in defining an object as "rigid", one already assumes Euclidean space. Thus, all geometrical notions that assume rigidity are assuming Euclidean space *à priori* (Helmholtz 1878).

The debate between Helmholtz and Land raged on in other venues. Clifford, for instance, engaged in similar debates as he offered popular lectures on the physiological reasonability of unconventional mathematical knowledge. He foreshadowed the end of *à priori* appeals to truth. No longer would mathematics—and certainly not Euclidean axioms, or Kantian assumptions about space and time—serve as models of God's incontrovertible universe. As part of his anti-religious, pro-empirical view of mathematical epistemology, Clifford chose to attack Stewart and Tait's *Unseen Universe*. Recall that Stewart and Tait had sought to save God by arguing that the conservation of "energy" and the principle of "continuity" together reveal there is some realm of existence for our body's energy (i.e. our soul) after death. In his review of their book, published in the *Fortnightly Review* (June 1875), Clifford writes,

> Our authors assume, as absolutely self-evident, the existence of a Deity who is the Creator of all things. They must both have had enough to do with examinations to be aware that "it is evident" means "I do not know how to prove" (Clifford 1886, 176).

The hope for deathlessness, or life after death, is a mere "shrinking from death," Clifford writes. It is the hope embodied in all non-active lives of those people who fear that no good can be done in the here and now and that no improvement or progress can be had, such that the deathbed always appears a better option than living, toiling, or working for change. Clifford states,

> However vividly I recall the feelings of pain and weakness, it is the life and energy of my present self that pictures them; and this life and energy cannot help raising at the same time combative instincts of resistance to pain and weakness, whose very nature it is to demand that the sun shall not go down upon Gibeon until they have slain the Amalekites (Clifford 1901, 271).

It is perhaps this feeling of youth, the love of youthful life, or the fear of losing it that leads certain practitioners—such as Stewart and Tait—to long for an ever-after, he concludes.

In their book, Stewart and Tait attempt to account for the dissipation of energy by suggesting that entropy allows for the passage of lost potential energy into some other "unseen" domain of the universe. Clifford rejects the notion that the conservation and dissipation of energy leads to anything like the need for an alternative, "unseen" universe, where dissipated energy serves some ultimate purpose. In deeply empiricist tones, Clifford argues,

> The laws of motion and the conservation of energy are very general propositions which are as nearly true as we can make out for gross bodies, and which, being tentatively applied to certain motions of molecules and the ether, are found to fit. There is nothing to tell us that they are absolutely exact in any particular case, or that they are everywhere and always true. If it were shown conclusively that energy was lost from the ether, it would not at all follow that it was handed on to anything else. The right statement might be that the conservation of energy was only a very near approximation to the facts (Clifford 1901, 291).

Not only are thermodynamical principles approximations, but those principles change and evolve over time, Clifford contends. There is nothing to prevent the ongoing, spontaneous evolution of the physical universe in the same way that Darwinism had revealed the ongoing, spontaneous evolution of the organic universe.

In a paper published in the popular journal *Nineteenth Century* (October, 1877), Clifford further argues that evolutionary theories constitute the only legitimate approach to understanding mathematical, or scientific knowledge-generation. He writes,

> It is true that we can no longer think of conscience and reason as testifying to us of things eternal and immutable. Human nature is no longer there, a definite thing from age to age, persisting unaltered through the vicissitudes of cities and peoples. Very nearly constant it is, practically constant for so many centuries; but not constant through that range of time which it practically concerns us to know about and to ponder. But, on the other side, what a flood of light is let in by this very fact, not only on human nature, but on the whole world. It is impossible to exaggerate the effect of the doctrine of evolution on our conception of man and of nature. Suppose all moving things to be suddenly stopped at some instant, and that we could be brought fresh, without any previous knowledge, to look at this petrified scene. The spectacle would be intensely absurd. Crowds of people would be senselessly standing on one leg in the street, looking at one another's backs; others would be wasting their time by sitting in a train in a place difficult to get at, nearly all with their mouths open and their bodies in some contorted, unrestful posture. Clocks would stand with their pendulum on one side. Everything would be disorderly conflicting in its wrong place. But once remember that

> the world is in motion, is going somewhere and everything will be accounted for and found just as it should be. Just so great a change of view, just so complete an explanation, is given to us when we recognise that the nature of man and beast and of all the world is changing ... The silly maladaptations in organic nature are seen to be steps towards the improvement or discarding of imperfect organs (Clifford 1901, 282-283).

A year after issuing these claims, Clifford followed up with his account of Rudolph Virchow's (1821-1902) talk on the theoretical uncertainty of certain aspects of evolutionary theory, such as the descent of man. In a lecture later published in the *Nineteenth Century* (1878), Clifford recounts the jubilee meeting of German naturalists and physicians at Munich in 1877, where Virchow had argued evolutionary theory should not be taught dogmatically, as many elements of various evolutionary notions had yet to find justification in paleontological or geological evidence. Yet, despite the many gaping holes in the evidential picture, Clifford nonetheless enjoins his readers to,

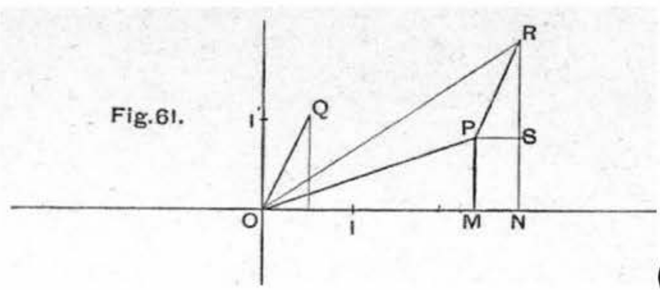
> not allow any dishonest person to persuade you to *not* believe strongly in the doctrine of evolution, because Virchow has admitted that certain parts of it are not yet absolutely proved. It is one thing to believe that a doctrine is false, and quite another thing to admit a theoretical doubt about it, I say a theoretical doubt, because it is a doubt founded on the necessary imperfection of all human knowledge, and not on any practical defect of the evidence (Clifford 1901, 319-320).

In issuing his evolutionary-themed views of development, change and knowledge-generation, Clifford relied upon the institutional conduits of community halls, popular reviews, and professional journals. He also utilized the lecturing space of his classrooms as channels through which he delivered his empiricist-evolutionist view of scientific and mathematical knowledge.

One particular set of lecture notes existent from Clifford's UCL days indicates the degree to which these empiricist-evolutionist views shaped his mathematical discourses, especially with regards to quaternions.[132] In his notes, Clifford outlines the fact symbols such as $+$ and $-$ have geometrical content. They indicate the augmentation or diminishment of a magnitude, if we assume we are operating in one-dimension only. Clifford then seeks to offer a geometrical or spatial rendering of

[132] This series of lectures formed the basis for Clifford's unfinished manuscript for the *Common Sense of the Exact Sciences* (1885), edited by Karl Pearson, the bulk of which was reworked and even written by Pearson himself. The first two chapters of that book were Clifford's own, and they map on to the lecture notes discussed here.

every symbol he uses to teach his students. The upshot of his lesson is that much symbolical algebra is merely short-hand for data gathered by geometrical induction. Symbols such as $+2$ and -3 are conventional tools that indicate steps along a number line, forward or backwards. In arithmetic, we can consider $+2$ to be a positive number and -3 to be a negative number. However, arithmetic deals only in discrete quantities. In geometry, $+2$ indicates the continuous length of 2 units combined with a $+$ direction along an axis in a particular direction. These symbols indicate nothing other than "steps in space." In the terms of vector geometry, the symbol $+$ in a plane indicates a step in a plane. The equation $OP = OM + MP$ refers to the "step" from O to M followed by a "step" from M to P, the resulting series of which is equally represented by the "single step" OP.



(Clifford 1882)

Hamiltonian vector addition, subtraction, multiplication and division demonstrate the nature of quaternions as operators that initiate a series of "steps in space," Clifford writes, adding that Grassmann's "outer product" is a "step" too, as an area is considered to be the product of two vectors (two directed steps) of the same kind, and a volume is considered to be a product of three vectors of the same kind. Thus, there is an empirically meaningful way (i.e. a geometrical way) to speak about negative areas and negative volumes.

In his lectures to students, this distinction between the Hamiltonian approach and the Grassmannian approach is not posited as problematical, but rather as terminological. Directed areas and volumes can be represented symbolically, Clifford explains, but their legitimacy derives from the geometrical content of those symbols. Hamiltonian "versors", enable "us to represent any one in terms of three units at right angles to each other," where (x, y, z) are components of length in the Hamiltonian vector $xi + yj + zk$. In this sense, both Grassmannian and Hamiltonian algebras are simple matters of combined "steps in space," some of which involve translations, some of which involve rotations, and none of which are nullified or delegitimized by their non-commutative nature.[133] In sum, Clifford presents his students with the view that mathematical knowledge is empirically-founded—it is based upon the experience of "steps in space," which are themselves observational concepts that depend upon the evolutionary status of the observer.

Clifford's view of the fundamentally conventional and empirical nature of mathematical knowledge—and especially quaternions—motivated him, in part, to teach the topic to his undergraduates at UCL. Clifford was convinced the complex symbolical representations of quaternions could be rendered easily comprehensible by appeals to "steps in space"—i.e. the empirical content of quaternions. Those lessons formed the basis for Clifford's 1878 *Elements of Dynamic, An Introduction to the Study of Motion and Rest in Solid and Fluid bodies, Part I: Kinematic*—a text written and published before Clifford's death. This latter text constitutes Clifford's first textbook. The *Dynamics* reflects Clifford's appeal to students, as well as his need to financially sustain himself by producing textbooks that his students would buy for use at UCL. It is to this last "terrain of knowledge" that we turn now to see how Clifford's quaternion presentations were contoured by the institutional framework within which he operated.

University College, London—a secularist tradition in urban education

Alongside the country's emerging mechanics institutes, and groups such as the Society for the Diffusion of Useful Knowledge, the University College, London (UCL) emerged as an early-century attempt to diffuse higher knowledge to a greater proportion of Britain's middle classes. This process was promulgated, in part, by a reaction against traditional church-university relations, which had governed accessibility to the country's grandest universities for centuries. UCL was established with

[133] For Clifford's account of "strain" see Clifford (1882, 510-513); for Clifford's account of the vector function used so extensively by Tait, see Clifford (1882, 514).

the explicit objective of sidestepping religious tests and faith-based oaths required at other institutions, both in practice (as at Cambridge) and by statute (as at Edinburgh). The University College was, therefore, a child of the Industrial Revolution (Taylor 1967). As the middle-class came to represent a greater cross-section of local communities, industrial merchant-men, businessmen, and technicians were gaining increased social recognition. Non-conformism was on the rise, and although neither Oxford nor Cambridge had dropped their respective requirements to have students (and graduates) subscribe to the Thirty-Nine Articles of Religion that defined the Anglican Church, UCL's Deed of Settlement, as signed on February 11th, 1826, declared:

> The object of the said Institution is the advancement of literature and science by affording to young men residing or resorting to the Cities of London and Westminster, the Borough of Southwark, and Counties adjoining to either of the said Cities, or to the said Borough, adequate opportunities for acquiring literary and scientific education at a moderate expense (Taylor 1967).

A new and distinct textbook industry consequently cropped up in London, where the need for university-level books to be used by students who had arrived to school less well-prepared than their peers at Cambridge, meant that Clifford, as professor of mathematics, faced a needy market. Given Clifford's own impoverished circumstances (he eventually died with only £400 to his name), the motivation to generate textbooks for entry-level university students was a powerful driving force. Furthermore, the nature of UCL's curriculum, based as it was upon the Scottish liberal arts tradition, meant that texts in "pure" mathematics—i.e. Tripos-style mathematical training—would have failed to meet student needs appropriately. It is for this reason Clifford crafted the *Dynamics* as an omnibus textbook that could have provided students with textual material for a series of courses in mathematics, mechanics and natural sciences. Parts I, II, and III of the *Dynamics* were completed in full and published before Clifford died; part IV was left unfinished at Clifford's death, and was published posthumously with relatively few edits.

In the *Dynamics,* Clifford presents his joint Hamiltonian-Grassmannian approach to directed magnitudes as set out within a conventionalist-empiricist (and, thus, evolutionist) approach to the generation of mathematical knowledge. The first chapter begins with a recapitulation of the lecture on "steps." Clifford writes,

> Just as geometry teaches us about the *sizes* and *shapes* and *distances* of bodies, and about the relations which hold good between them, so Dynamic teaches us about the changes which take place in those distances, sizes, and shapes (which changes are called *motions*), the relations which hold good between different motions, and the circumstances under which motions take place (Clifford 1878, 1).

"Motions" are complicated things, Clifford writes; if one tries to describe the motion of one person on a moving train, one must describe first the motion of the train, second the motion of the person's body, third the change in muscle shape as the person's body contracts as he walks through the train, and so forth. To avoid such complications, Clifford writes, "We deal with the simplest motions first, and gradually go on to consider the more complex ones."

Pedagogically-minded, Clifford first defines his terms. A body that is so small that it has no differing parts is called a "particle." The only motion it is capable of is a translation. A "rigid body" is a body that does not change its shape when moved about. It can be moved in two ways—via translation (a change in its place), or via rotation (a change in its orientation). In pure translations, every straight line in the body remains parallel to its original position. When that is not the case, it indicates that a rotation has taken place. A motion in which there is both a translation and a rotation at the same time is "like that of a corkscrew entering into a cork" and it is called a "*twist*." Lastly, an "elastic" body is that which can change in size or shape. It can experience motions called "strains." The science that describes these various motions is termed "kinematics," and it can be subdivided as follows (Clifford 1878, 2):

Kinematic of	Points or particles	Translations
	Rigid Bodies	Rotations and Twists
	Elastic Bodies	Strains

When a body changes in motion, the cause of that change is generally talked about as though it is a "force." The science that describes the changes in motion due to forces is called "dynamics," although the present textbook considers various changes in motion without reference to the "forces" at play. Thus, the *Dynamics* seeks to speak about motion as "compositions of steps," by appealing to geometrical constructions and algebraic manipulations alone.

In a section entitled "Product of Two Vectors," Clifford presents both the Hamiltonian account of vector multiplication and the Grassmannian account. He explains that two vectors multiplied together can result in either a scalar product or a directed area. That is:

> The area of the parallelogram $abcd$ may be supposed to be generated by the motion of ab over the step ac, or by the motion of ac over the step of ab. Hence it seems natural to speak of it as the *product* of the two steps ab, ac. We have been accustomed to identify a rectangle with the product of its two sides, when their lengths are only taken into account; we shall now make just such an extension of the meaning of a product as we formerly made of the meaning of a sum, and still regard the parallelogram contained by two steps as their product, when their directions are taken into account. The magnitude of this product is $ab.ac \sin bac$; like any other area, it is to be regarded as a directed quantity.

Furthermore,

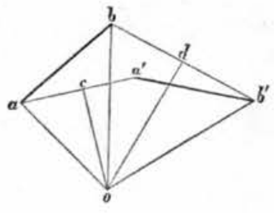
> Suppose ... that one of the two steps, say ac, represents an area perpendicular to it; then to multiply this by ab, we must naturally make that area take the step of translation ab. In so doing it will generate a volume, which may be regarded as the product of ac and ab. But the magnitude of this volume is ab multiplied by the area into the sine of the angle it makes with ab. This kind of product therefore has the magnitude $ab.ac \cos bac$; being a volume, it can only be greater or less; that is, it is a *scalar* quantity.

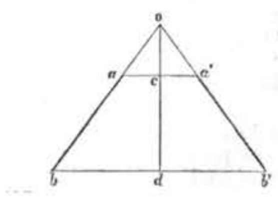
The important difference between these two products is that the direction, or sign, of the area in the first instance depends upon "the way it is gone round; an area gone round counter-clockwise is positive, gone round clockwise is negative." If $V.ab.ac = \text{area } abcd, \text{then } V.ac.ab = area\ acdb$; therefore, $V.ac.ab = -V.ab.ac$, while the scalar products can be represented in Hamiltonian terms as $S\alpha\beta = S\beta\alpha$.

Having accounted for the two possible products of multiplication of vectors, Clifford discusses "rotations," which he says can be considered to be the "steps of a rigid body" through space. He clarifies a significant equivalence established in his 1873 paper on biquaternions. He repeats there are two "kinds" of motion of a rigid body. "The first," he says, "is the motion of a body sliding about on a plane (e.g. a book on a table), which may be completely described by specifying the motion of a moving plane on a fixed plane." The second "is the motion of a body, one point of which is fixed; which in practice is secured by a ball-and-socket joint, and which is most conveniently studied under the form of the sliding of a spherical surface on an equal spherical surface." This second "kind" of motion is important to the study of dynamics, because of the dual nature in which it can be interpreted empirically. Clifford explains,

> When the centre of a sphere is very far away from the surface, the surface approximates to that of a plane. Thus, the frozen surface of still water is approximately spherical, with its centre at the centre of the earth. In this way we may see that the first of our two motions is only a limiting case of the second, in which the fixed point is an infinite distance off (Clifford 1878, 118).

In other words, "every change of position in a plane sliding on a plane may be produced either by translation or by rotation about a fixed point located at an infinite distance.





(Clifford 1878, 119)

Lastly, Clifford discusses velocity-systems, strain-steps and strain-velocities. Most of his discussion is presented in vector and quaternion form, although always with the caveat that as complicated as the "motion" might seem, it is only ever a composition of simple "steps" including translations, rotations and strain-steps. In all of these instances, Clifford emphasizes the possibility of "dual interpretations". Where one observes simple translations in space of three-dimensions, one may also be observing the rotation of a rigid body about a distant central axis. At an astronomical level, perceivers are incapable of recognizing any difference between these two ontological constructs. Mathematically there is a difference in terms of the geometrical model being analyzed. Yet, there is no distinguishing phenomenal factor that an observer located upon the surface in which the motion is taking place could use to distinguish between the two geometrical possibilities.

Apart from the small discussion on directed products, Grassmannian analysis does not actually appear prominently in the *Dynamics*, and the biquaternions do not appear at all. This is not an indication that Clifford had abandoned the more formal aspects of Grassmannian analysis, or that his bi-quaternion system—as presented in generalized form in his 1873 and 1876 papers—had been left to the wayside. Rather, it is a clear indication that the *Dynamics* is an introductory textbook written for students at UCL, rather than a treatise written for an advanced mathematical community of practitioners. It is meant to generate income for Clifford and his family, while at the same time satisfying the need for a textbook that students in Clifford's UCL classes could use meaningfully.

Given that Clifford died only a year later, and in a state of penury so severe that his colleagues had to generate a survival fund for his wife, Lucy, and their children, the motivation to produce such a beginner's texts would have been a compelling one. The fact the book was aimed at a newly emerging market of urban university students also helps to explain why Tait's lightly veiled accusation of plagiarism emerged a few years later. Tait was concerned about a potential loss in income as the author of the primary introductory text in quaternion and vector mathematics. Clifford's text was more readable and more elementary than both of Tait's quaternion books, and it reflected the increasingly popular view of "empirical" and "evolutionary" mathematics. If popular, it could possibly lead to Clifford's posthumous name being credited with the development and advancement of vector mathematics, especially if his textbook were to be adopted and used by Tripos coaches—a phenomenon that Tait and Kelland had failed to initiate in with their own works in past years.

Conclusion

Thus, we have seen that Clifford trod upon multiple "terrains" throughout his engagements with quaternion mathematics. Like Tait, Clifford had adopted a foundational philosophy in symbolical algebra by virtue of his Cambridge education. In Clifford's case, however, that symbolical algebraic education was imbued with lessons on projective geometry, as put forth by Cayley, and, most importantly, exposure to the formalism of Grassmann. That symbolical algebraic training taught Clifford that mathematical analysis can drive research in new directions, including new geometrical directions. However, Clifford's symbolical algebraic "terrain" developed as another "terrain"—namely, Victorian Darwinism—was developing underneath it. Clifford's early exposure to Darwinist discourse led him to view mathematical "truths"—symbolical or otherwise—as the product of the evolutionary status of the human mind. Through the processes of "integration" and "differentiation," the grey matter of human brains had come to deem certain mathematical claims as "true." In Clifford's view, such truth was only ever temporal—fundamentally dependent upon the physiologically incomplete status of the "eye" and "mind."

With those Darwinist notions contouring Clifford's navigations, his approach to quaternions was unique indeed. Clifford engaged with quaternions both as symbolical algebraic tools in the newly forming field of symbolical geometry, as well as instantiations of "empiricism" in mathematics. Clifford viewed himself as revivifying a venerable tradition in hunting for the empirical content—i.e. the "steps in space"—which gave meaning to operational tools such as quaternions and his own bi-quaternions. Clifford's publications on the matter were later defined by his need to generate wealth via textbooks, as well as his belief that, as empirically-founded artifacts, quaternions could be explained as basic “steps in space” to even the mathematically untrained, middle-class boys populating his classroom at UCL. In sum, in explaining why Clifford used quaternions as he did, the historian must appeal to those terrains that he trod upon in order to understand the specific conceptual resources that equipped him to make his movements, and which imbued his choices and actions with meaning, given that they provided a space within which Clifford could view his own uses of quaternions as reasonable and, even, natural.

Conclusion

Tait and Clifford were contemporaries. They were colleagues. They were competitors. Though the Tait-Clifford relationship is nowhere near as antagonistic in the history of science as some other duos have been in the past (consider Hobbes and Boyle, Newton and Leibniz, or even Whewell and Peacock), their mathematical engagements diverged in important respects. Both actors were educated into the Cambridge school of mid-century symbolical analysis, but each mathematician trod upon a variety of contoured conceptual terrains, which provided him with the resources needed to engage with the particular artifact known as "quaternions" in unique and distinct ways. Those "terrains" allowed both Tait and Clifford to view their respective engagements as meaningful attempts to generate new knowledge claims. The relative terrains navigated by Tait and Clifford should suggest to the reader that unique knowledge claims are based on the contingent factors that shape the conceptual topology within which practitioners find meaningful reasons to behave in particular ways. An actor's engagement with an artifact, such as quaternions, is not predetermined by previous uses, although previous uses can form part of a "terrain" navigated by the actor, constituting part of the "meaning" infused in the terrain itself. But, like a trekker moving through a diverse landscape, an actor's engagements with an artifact can occur from an infinite number of possible directions and result in an infinite number of possible outcomes.

In Chapter One, I explored the foundational ground set out by the tradition of symbolical algebra in Britain or, better put, the philosophy of symbolical algebra, which embodied a methodological approach towards mathematical practice (i.e. succinct, symbolical, and devoid of empirical content) as well as a metaphysical view about the nature of mathematical identities and manipulations (i.e. that symbolical equivalences are universally true, though conventional in construction). Expressed poignantly in Peacock's "principle of the permanence of equivalent forms," the symbolical algebraic "philosophy" and its various interpretations and justifications created the basis for more specific terrains that would manifest themselves in Tait and Clifford's later mathematical outputs. Both actors were trained into the belief that mathematical research can be driven by symbolical manipulation alone, although, due to the contouring influence of other "terrains", neither actor remained satisfied with that approach. "Symbolical algebra" was, therefore, both a tool for mathematicians to use, i.e. the "how", and the "why" for much of their mathematical practices. For actors such as Tait and Clifford, symbolical algebra constituted an entirely justifiable, reasonable and meaningful way to approach new problems in the first instance.

In Chapter Two, I explored a brief period in Hamilton's life in order to demonstrate why the Irishman chose to recast his research in quaternions in the light of the symbolical algebraic discourse then institutionalized at Cambridge. Hamilton had always used the tools of symbolical analysis in his research, but he had often sought to identify his "couples" as the "Science of Pure Time" and to grant them *à priori*, ontological content rather than view them as mere symbolical equivalences. Hamilton's choice to adopt the conventionalist-speak of the British symbolical algebraists of the 1850s signaled a discontinuity in his approach to knowledge. In his 1853 "Preface," Hamilton painted himself the colour of Peacockian algebra. This was, in part, motivated by his desire to gain renown within the Peacockian-inspired community of practitioners in Cambridge, and thus satisfy the institutional requirements of Trinity College, Dublin, and it was motivated, in part, by Hamilton's personal hopes at revivifying his own reputation as the producer of great Irish science—a reputation that he felt required the kudos and support of English, Protestant practitioners.

In Chapter Three, I explored Tait's engagements with quaternions, from the early-1850s to the early-1880s in order to account for why it is that Tait sought to present them as symbolical tools, why he recast them as thermodynamical tools in the 1860s, and why he lauded their simplicity and "paucity of symbols" (i.e. their efficiency). Tait had started out as a Scottish Presbyterian student at Cambridge, where he absorbed the philosophy of Peacock's symbolical algebra as it had been institutionalized in the university's newly reformed curriculum in the early-1850s. After he graduated Senior Wrangler, Tait's initial research outputs reproduced meaningful accounts of symbolical analysis that he had absorbed during his navigations through his educational "terrain of knowledge". Tait's later navigations through Belfast mathematics, where he received support from Andrews, and his navigations through the world of natural philosophy at the University of Edinburgh, overlaid his symbolical algebraic outlook with resources stemming from thermodynamic and Presbyterian conceptions of "dissipation" in the universe and avoidance of "waste" in moral life. His movements through the institution of a Scottish university also generated poignant interests in wealth-generation, where textbooks could be used to attract potential students to fee-based lectures. When overlaid, these terrains constituted a complex topology upon which Tait chose to engage with quaternions. The historian is well-placed, therefore, to explain that Tait engaged with quaternions in the ways he did because those varied terrains equipped him with the tools (i.e. symbolical algebraic techniques and specific vocabularies) to do so and because they rendered his choice of engagement meaningful, both as an approach and as an output.

In Chapter Four, I explored Clifford's engagements with quaternions, from the mid-1860s to the end of the 1870s, in order to account for why it is that Clifford sought to present them as symbolical geometrical tools that were fundamentally "empirical" in their foundations and "conventional" in their universality. For Clifford, all knowledge is shaped by the human state of evolution. Thus, knowledge claims are the representation of our imperfect evolutionary status with regards to intellect and perception. Mathematical knowledge is no different. As Clifford moved through the terrain of symbolical algebra at Cambridge in the mid-1860s, he learned to apply symbolical techniques to the analysis of geometrical properties; his encounters with Riemannian and Lobachevskian geometries came to influence him so deeply that they also formed another terrain, upon which he began to judge other scientific, mathematical and even physiological claims. Clifford approached Hamiltonian quaternions from the perspective of non-Euclidean geometries, which led him to attempt to generalize quaternions into biquaternions—generating, thereby, new symbolical geometrical tools that he felt could be used meaningfully within spaces of multiple dimensions.

Many of Clifford's research outputs along the way were rendered meaningful by virtue of the institutional environments within which he was operating. His initial papers were highly technical—they were published in mathematical journals based at Cambridge. But once at UCL, Clifford's quaternion outputs took the form of popular lectures, entry-level lectures to students (both female and male), and beginner textbooks that placed complex symbolical analysis, such as quaternions, within the domain of empirically-simple "steps in space."

Following the advice of Shapin and Schaffer (1985), this author has indeed tried to "get on with it" in order to present this historical account of Tait and Clifford's respective encounters with, and uses of, quaternions mathematics. Clearly, each actor approached the matter of "quaternions" from differing directions and with differing sets of conceptual resources in hand. As a result, their mathematical outputs reflected alternative understandings of symbolical algebraic knowledge, as well as alternative views on the place of humanity in the universe, the moral duties of humans, the evolutionary status of human perception, and professionalization.

The comparative study provided here has outlined four terrains of knowledge that describe Tait and Clifford's respective engagements with quaternions. The aim has been to explain not only "how" they engaged with quaternions—i.e. the symbolical techniques used—but, more importantly, "why" they engaged with quaternions. Mathematical knowledge generation does not happen on its own. It is not driven by an internal force unique to its domain. Mathematical claims are generated by actors navigating through particular terrains infused with conceptual resources. The history of mathematics is, therefore, the history of people doing things with the contingent resources available to them. In sum, mathematical knowledge cannot be separated from the terrains upon which it is rendered meaningful, engaged with, and made into artifacts for use by other actors. Simply put, mathematics is terrain-based, social knowledge.

Appendices

Appendix One

A brief note on quaternions—a summary overview

A summary overview of Hamilton's quaternion mathematics is useful here to orient the reader to the 700-page account that Hamilton provides in his *Lectures* (1853). Working within a tradition that sought to ascribe meaning to imaginary numbers, Hamilton relied upon Jean- *Argand's* (1768- 1822) method of representing imaginary numbers as directed lines in space. In his paper, "On Conjugate Functions and on Algebra as the Science of Pure Time," Hamilton developed an extension of Argand's method. He argued that although $a + bi$ could be used to represent a complex number in space (where a and b are real number components of the complex number), the expression $a + bi$ is not a "sum" in the same sense that $a + b$ is a sum of two quantities. The problem, Hamilton states, is that complex sums assume that a quantity (a) can be added to a directed line (bi), which it cannot. The things are two of a different kind, Hamilton argues. The expression $a + bi$ does not represent an arithmetic addition; it represents an ordered geometric couple in space, where (a, b) constitutes the end point of a directed line emanating from a given origin. According to Hamilton, if $a + bi$ and $c + di$ are two complex numbers, then the following identities hold (I use Kline's notation here) (Kline 1972, 776):

$$(a, b) \pm (c, d) = (a \pm c, b \pm d),$$

$$(a, b) \cdot (c, d) = (ac - bd, ad + bc); \text{ and}$$

$$\frac{(a,b)}{(c,d)} = \left(\frac{ac + bd}{c^2 + d^2}, \frac{bc - ad}{c^2 + d^2}\right).$$

The sum, difference, product and quotient of two couples result in another couple.

Although the concept of "directed lines" had long been established in the works of mathematicians seeking to represent the addition of forces in space, and although mathematicians such as Wessel, Argand and Gauss had already used the complex representation of directed lines $(a + bi)$ to "add forces" (so that if two forces are represented by the directed lines $(1 + 2i)$ and $(2 + 3i)$, the combined force is represented by the single line $(3 + 5i)$), there was no obvious way to extend that addition of forces to more than two dimensions. Such an extension would have been a useful artifice for any natural philosopher who sought to find the resultant force on a body acted upon by forces emanating from different directions. Hamilton had set as his task the resolution of this problem—namely, to determine a three-dimensional complex algebra that could be used to geometrically describe the motion of bodies in space when acted upon by varying forces.

Hamilton believed that to represent the sum, difference, product and quotient of directed lines in three-dimensional space, i.e. "triplets," a four-termed number would be required. His solution was to develop a number of the form $a + bi + cj + dk$, where "a" is the scalar part of the number and $bi + cj + dk$ is the vector part of the number. Together, the scalar and vector parts form a "quaternion"—a four-termed number that, when applied to another directed line in space, causes a change in the vector's size (i.e. its magnitude, or scalar part) as well as a change in its direction (i.e. its vector part). The three real-number parts of the vector component are the three Cartesian coordinates of the point *P*, while the i, j, k components represent a directed unit along three axes. Quaternions can be subjected to the following algebraic operations: addition, subtraction, multiplication and division, as long as the i, j, k components follow the rules stated earlier for multiplication. For instance, Hamilton says, a quaternion **q** is represented by $q = w + xi + yj + zk$, where $w, x, y,$ and z are real numbers. He uses the notation $q, q^{'},$ and q'' to represent different quaternions so that, if q and q' are two quaternions of the form $q = w + xi + yj + zk$ and $q^{'} = w^{'} + x^{'}i + y^{'}j + z^{'}k$, then we can define the following basic algebraic operations:

$$q + q^{'} = (w + w^{'}) + (x + x^{'})i + (y + y^{'})j + (z + z^{'})k,$$

$$q - q' = (w - w') + (x - x')i + (y - y')j + (z - z')k, \text{ and,}$$

$$qq' = (ww - xx' - yy' - zz') + (wx' + xw' - yz' - zy'')i + (wy' - yw' - zx' - xz')j + (wz' - zw—xy' - yx')k.$$

The rules of multiplication for the imaginary parts of the quaternion further indicate that, if one has two quaternions **p** and **q**, and $\mathbf{p} = 3 + 2\mathbf{i} + 6\mathbf{j} + 7\mathbf{k}$ and $\mathbf{q} = 4 + 6\mathbf{i} + 8\mathbf{j} + 9\mathbf{k}$, then $\mathbf{pq} = -111 + 24\mathbf{i} + 72\mathbf{j} + 35\mathbf{k}$, whereas, $\mathbf{qp} = -11 + 28\mathbf{i} + 24\mathbf{j} + 75\mathbf{k}$. In other words, the multiplication of quaternions is not commutative $(\mathbf{ab} \neq \mathbf{ba})$ (Kline 1972). That multiplication is, however, "associative" (i.e. $(qq')q'' = q(q'q'')$) (Wilkins 2005, 240).

Hamilton also defined a "conjugate" quaternion, using the symbol Kq to represent the conjugate (defined in Cartesian terms as $Kq = w - xi - yj - zk$). And he used the symbol Tq to represent the "modulus," or what he later termed the "tensor" (after 1846). The "modulus" of the quaternion (i.e. the length of the quaternion) is defined as $Tq = \sqrt{w^2 + x^2 + y^2 + z^2}$. Various relationships can then be determined to hold true between the conjugate of the quaternion, **q**, and its tensor, Tq. For example, Hamilton finds that $q(Kq) = (Kq)q = (Tq)^2$. This algebraic relationship means that a quaternion multiplied by the conjugate of itself is equal to the conjugate of the quaternion multiplied by the quaternion (i.e. the multiplication of these two entities commutes). The product of this operation is equal to the tensor of the quaternion squared. On Hamilton's account, all of these operations can be interpreted "synthetically"—i.e. geometrically—as systems of forces acting upon points and directed lines in space.

In his *Lectures*, Hamilton also introduces a differential operator, known as "nabla", symbolized by ∇. Hamilton introduces the "nabla" differential operator on page 610 of his *Lectures*, in which he offers a semi-Cartesian trinomial version of the analytical tool. The defined as: $\nabla = \mathbf{i}\frac{\partial}{\partial x} + \mathbf{j}\frac{\partial}{\partial y} + \mathbf{k}\frac{\partial}{\partial z}$.[134] When a scalar point function $u(x, y, z)$ is operated upon by "nabla," the result is a vector that varies

[134] A name first suggested by William *Robertson Smith* (1846-1894), because of its resemblance to an Assyrian harp.

from point to point—that is, it represents the rate of increase of u in space. When "nabla" is applied to a vector point function, it produces a quaternion.

Tait later raised concerns about the "nabla" operator in his correspondence with Hamilton; Hamilton failed to elaborate upon ∇ any further in his *Elements* (1866) or in his correspondences with Tait throughout the same decade. As a result, the operator is barely mentioned in Tait's first quaternion textbook, *An Elementary Treatise on Quaternions* (1867). By the 1870s, however, "nabla" does come to dominate Tait's discussions with other mathematicians, such as James Clerk Maxwell (1831-1879), and it comes to constitute an important topic in the former's *Introduction to Quaternions* (1873).

Appendix Two

Tait's navigations away from Hamilton

Tait did not adopt all of Hamilton's notational specifics, or his specific definitions and interpretations, with regards to quaternions. For instance, in his work on differentials of quaternions, and in his correspondence with Tait, Hamilton interpreted differentials (such as dx and dy) not as infinitely small quantities (which was a common interpretation among post-Cauchian British mathematicians at the time), but rather as ordinary finite quantities (Wilkins 2005, 245). This caused no little amount of puzzlement for Tait, who had been trained in the Cambridge-influenced symbolical analytical curriculum of the 1850s. Such divergences between Hamilton and Tait became more prominent in the 1860s, just as Tait was becoming a more widely-recognized authority in his own right in relation to the science of "energy". At the same time, Hamilton was nearing the end of his productive life, and becoming less of a valuable colleague than he would have been to the young and newly graduated Tait in the late-1850s. Thus, we find in Tait's quaternion publications, throughout the 1860s, a clear willingness to make more bold and ideological statements with regards to quaternion mathematics and its role in practical, efficient and productive mathematics, even when those accounts were not necessarily in line with Hamilton's own views.

Upon arriving to Edinburgh, for instance, Tait wrote to Hamilton of the "powers" embedded in the "tremendous engine" of quaternions, to which the "great secret ... seems to be the *utter absence of artifice* and the *perfect simplicity and naturalness* of the original conceptions" (Knott 1911, 139). The terminology reflects the industrial motifs surrounding Tait in Scotland. "Power", "work" and "engines" were metaphors that served as standards of good science for the Scotsman—they were not, however, the metaphors favoured by Hamilton, an élite Protestant Irishman. From 1862 onwards, the correspondence between the two mathematicians thus began to wane and Tait turned his attention towards courting his old Cambridge colleagues and fellow mathematicians. Recall that

Tait's initial aim in the late-1850s had been to have his quaternion works "taken up" at Cambridge. Yet, the pressures of developing and legitimizing thermodynamics in the 1860s meant that Tait's textbook efforts in quaternions had given way to laboratory work. As Tait wrote to Maxwell, the difficulties associated with pursuing both the science of "energy" and quaternion mathematics simultaneously were, at times, insurmountable. Thus, Tait enjoins the mathematician-astronomer to advance his own quaternion publications down South so as to keep the field alive. In a letter written in 1864, for instance, Tait tells Maxwell,

> I am much obliged by your very kind note just received...
>
> Five years ago, Messrs Macmillan & Co. advertised for speedy publication an "Elementary Treatise on Quaternions" by me; but, as my good friend Sir W.R. Hamilton thought that it might possibly interfere with his forthcoming "Elements of Quaternions" I withdrew it—and have published only the few articles I recently sent you—all of them with *his* approval.
>
> I had no idea that you had been engaged in preparing such a work; and I merely write to say that I shall be most happy if you will persevere in your intention of publishing an elementary volume on the subject. In fact the papers I have sent you contain nearly the *whole* of my researches in the *elementary* part of the theory. I have an immense store of work in MSS relating to its higher applications—but unfit for an elementary treatise.
>
> Since I projected the treatise I have ceased to be a Professor of Mathematics; and with private experiments and the ordinary preparation for the work of my class, I feel that I have barely time enough to contribute my fair share to the "Treatise on Natural Philosophy" which Thomson and I have undertaken. And, as this Treatise is certain to extend to *three* volumes at least, of which (after two years work) not even *one* is yet published, I feel that it may be years before I shall be in a position to write on Quaternions in a carefully considered popular style. I am sure that my old friend Macmillan would be delighted to have the chance of substituting your name for mine in the advertisement, which he has been hopelessly repeating for some years.
>
> But the consent of Sir W.R. Hamilton is absolutely necessary to anyone undertaking the work (Knott 1911, 141-142).

Tait had, in other words, become heavily involved in other fruitful ventures—namely, the development of the science of "energy" and its legitimization within the wider British network of natural philosophy. Within that matrix, quaternions had slipped to become a peripheral issue in large part due to the publishing strictures imposed on Tait's research by Hamilton. For the still young Cambridge-graduate, the need to publish and to establish primacy in a particular field was of paramount importance. Tait's inability to do so with quaternions meant that, in an intellectual balancing act of costs and benefits, Tait favored thermodynamics—a more profitable and publicized endeavor in many respects.

In the meantime, a key point of contrast had emerged between Hamilton and Tait over the question of the relative use and importance of quaternions. For Hamilton, the usefulness of quaternions stemmed from their ability to expand upon the foundations of symbolical algebra by including non-commutative symbolical equivalences. For Tait, the relative relevance and usefulness of quaternions stemmed from their potential applications to dynamics (thermodynamics in particular) and to natural philosophy more generally. By the mid-1860s, quaternions had become another natural philosophical tool for Tait; they had become a hand-maiden to science. This outlook was reflected in Tait's use of the linear vector function, which he viewed as a means of expanding upon his theory of "strains"—a theme outlined in Tait and Kelland's *Introduction to Quaternions* (1873).

Following a long discussion with Maxwell on the Hamiltonian operator "nabla", ∇, Tait also included a thorough account of the Hamiltonian operator in the second edition of his *Elementary Treatise on Quaternions* (1875). In a letter dated November 7th, 1870, Maxwell highlighted the theme by querying Tait about the results of ∇ acting on scalar and vector functions. Maxwell wrote humorously, though with a note of seriousness, of the possibility of describing "twists" and "twirls" to generate "convergence" values for a vector affected by the Hamiltonian operator. He stated,

> Dear Tait,
>
> $$\nabla = i\frac{d}{dx} + j\frac{d}{dy} + k\frac{d}{dz}.$$
>
> What do you call this? Atled?
>
> I want to get a name or names for the result of it on scalar or vector functions of the vector of a point.
>
> Here are some rough hewn names. Will you like a good Divinity shape their ends properly so as to make stick?

(1) The result of ∇ applied to a scalar function might be called the slope of the function. Lamé would call it the differential parameter, but the thing itself is a vector, now slope is a vector word, whereas parameter has, to say the least, a scalar sound.
(2) If the original function is a vector then ∇ applied to it may give two parts. The scalar part I would call the Convergence of the vector function, and the vector part I would call the Twist of the vector function. Here the word twist has nothing to do with a screw or helix. If the word *turn* or *version* would do they would be better than twist, for twist suggests a screw. Twirl is free from the screw notion and is sufficiently racy. Perhaps it is too dynamical for pure mathematicians, so for Cayley's sake I might say Curl (after the fashion of Scroll). Hence the effect of ∇ on a scalar function is to give the slope of that scalar, and its effect on a vector function is to give the convergence and the twirl of that function. The result of ∇^2 applied to any function may be called the concentration of that function because it indicates the mode in which the value of the function at a point exceeds (in the Hamiltonian sense) the average value of the function in a little spherical surface drawn round it.

Now if σ be a vector function of ρ, and F a scalar function of ρ, ∇F is the slope of F

$$V\nabla.\nabla F \text{ is the twirl of the slope which is necessarily zero}$$

$$S\nabla.\nabla F = \nabla^2 F \text{ is the twirl of the slope which is necessarily zero}$$

$$S\nabla\sigma \text{ is the twirl of } \sigma.$$

Now, the convergence being a scalar if we operate on it with ∇, we find that it has a slope but no twirl.

The twirl of σ is a vector function which has no convergence but only a twirl ...

What I want is to ascertain from you if there are any better names for these things, or if these names are inconsistent with anything in Quaternions, for I am unlearned in quaternion idioms and may make solecisms. I want phrases of this kind to make statements in electromagnetism and I do not wish to expose either myself to the contempt of the initiated, or Quaternions to the scorn of the profane (Knott 1911, 143-144).

In 1871, Maxwell wrote to Tait again to encourage him to push further with his quaternion demonstrations. Maxwell himself viewed quaternions as much more than just short-hand for longer Cartesian accounts of particular problems. Maxwell wrote:

The unbelievers are rampant. They say "show me something done by 4nions which has not been done by old plans. At the best it must rank with abbreviated notations."

You should reply to this, and no doubt you will.

But the virtue of the 4nions lies not so much as yet in solving hard questions, as in enabling us to see the meaning of the question and of its solution, instead of setting up the question $x\ y\ z$, sending it to the analytical engine, and when the solution is sent home translating it back from $x\ y\ z$ so that it may appear as A, B, C, to the vulgar.

There appears to be a desire for thermodynamics in these regions...

Yours truly,

$$\frac{dp}{dt}.$$ [135]

In the same decade that Tait began to correspond with Maxwell on quaternions, he also began to correspond with Arthur Cayley (1821-1895) on the "nabla" operator. In Tait's "Note on Linear Differential Equations in Quaternions," published in the *Proceedings for the Royal Society of Edinburgh, Scientific Papers,* the Scotsman stated a solution to the problem of extracting the square root of a strain (i.e. a linear function). In a letter to Cayley dated February 28th, 1872, Tait noted that, as a result of that article, he wished

> to introduce [the quaternion account of strains] into the new edition of our first volume on Natural Philosophy—but [Thomson] objects utterly to Quaternions, and neither of us can profess to more than a very slight acquaintance with modern algebra—so that we are afraid of publishing something which you and Sylvester would smile at as utterly antiquated if we gave our laborious solutions of these nine quadratic equations (Knott 1911, 152).

Cayley responds by stating that the "question may be solved very simply by means of the theorem in my memoir on Matrices," which had been published a decade before (Knott 1911, 153). Tait responds a month later saying that, although he recognized "many of Hamilton's properties of the linear and vector function" could be converted into matrices, he was not entirely convinced of the method. Tait also acknowledges his lack of eminence in employing newly developed symbolical techniques. He notes, for instance, that he might not be the best prepared to fulfill Cayley's request for an article on quaternions for the BAAS, as a result of his lack of contemporary algebraic knowledge. Thus, he asks to be relieved of the task by having Clifford take up that duty instead.

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[135] While Thomson was often referred to by the northern British scientists and their network of colleagues as T, and Tait as T', Maxwell represented himself as $\frac{dp}{dt}$. It is one expression, Knott says, for the Second Law of Thermodynamics: $\frac{dp}{dt} = JCM$, where J is Joule's equivalent, C is Carnot's function, and M the rate at which heat must be supplied per unit increase of volume, the temperature being constant (and where JCM also happens to be the initials for James Clerk Maxwell).

Yet, although it seems here that Tait is subordinating his skills to those of Cayley, his correspondence with the mathematician continues throughout the 1870s and into later decades, during which time Tait reverts to reasserting his mastery and skill in quaternion mathematics. He rejects, for instance, Cayley's matrix approach. In 1874, following Tait's publication of a paper entitled "Orthogonal Isothermal Surfaces," Tait and Cayley discuss the ultimate meaning of one solution provided therein. In their correspondence, Cayley notes that for "$r = const.$" to represent a family of orthogonal surfaces, r (as a function of x, y, z) has to satisfy a particular partial differential equation of the third order. The equation Tait had presented in his published paper (i.e. $d\sigma = uqd\rho q^{-1}$) had to be the equivalent of that partial differential equation, Cayley argued. Tait disagreed. Whereas Cayley viewed quaternions as a "pocket map"—the formulae being concise, though they described nothing other than the same underlying concepts that mathematicians were familiar with already—Tait felt quaternions pointed to new physical structures, rather than just new forms of symbolical equivalences. Thus, a matrix approach reduced the "power" of quaternions to mere symbolical manipulation.

In 1884, Sylvester was also brought into the correspondence. Sylvester commented on the solution to the quaternion equation $aq = qb$. At the time, Sylvester had already published his general solution for a linear matrix equation. In generalizing the quaternion, he had developed a solution that differed from Tait's. The correspondence between Tait, Cayley and Sylvester led Tait to return to his original solution, as presented in 1867 textbook on quaternions, and to revise it significantly, invoking, this time, Cayley's help. He took Sylvester's lead to present a thoroughly analytic approach to the study of quaternions in general. In the mid-1880s, therefore, quaternions were re-interpreted within a Cayley- and Sylvester-inspired tradition in late-Victorian symbolical algebra and analytics, which engendered a renewed emphasis on moving away from geometrical and natural philosophical concerns.

However, after having consulted with Cayley and Sylvester on a number of points related to the third edition of his *Quaternions* (as published in 1890), Tait wrote to the mathematicians to renew his reservations about the analytical treatment of quaternions. In a letter written on August 28th, 1888, he told the mathematicians:

> Since I returned to Edinburgh, I have been considering more closely the question of the new edition of my *Quaternions* and looking up specially Sylvester's papers in the *Comptes Rendus* and the *Phil. Mag*. It seems to me from my point of view (which I think is that of Hamilton) that all these things, excellent and valuable as they are, are not *Quaternions* but developments of *Matrices*. As I understand Hamilton's quest, it was for a method which should *supersede* Cartesian methods, wherever it is possible to do so. Hence i, j, k, and their properties, though they were the stepping stones by which Hamilton got his method, are to be discarded in favour of α, q, ϕ etc.: and no problem or subject is a *fit* one for the introduction of Quaternions if it necessitates the introduction of Cartesian Machinery...
>
> The conclusion from this seems to be that I ought, instead of inserting your contributions in the text of my book as it stands, make a new chapter "On the Analytical view of Quaternions" (or some such title) in which they will form the spinal column. Therein will naturally assemble all the disaffected or lob-sided members, which are not capable of pure quaternionic treatment but which are nevertheless valuable, like the occipital ribs and the anecephalous heads in an anatomical museum (Knott 1911, 158-159).

Cayley replied a week later, indicating his continued disagreement with Tait on the matter. The mathematician wrote:

> I ... have not yet written out two further notes which I should like to send you for the new Chapter—which (I take it kindly) you do *not* compare with the Chamber of Horrors are Madame Tussaud's ... I need not say anything as to the difference between our points of view; we are irreconcilable and shall remain so: but is it necessary to express (in the book) all your feelings in regard to coordinates? One remark: I think you do not give your symbol ϕ a sufficiently formal introduction: it comes in incidentally through a particular case, without the full meaning of it being shown (Knott 1911, 159).

Tait's response followed a month later:

> I don't know that my view of coordinates is very different from yours, though my sight is vastly inferior. But I can see pretty clearly in the real world, with its simple Euclidean space, by means of the quaternion telescope. Witness a paper of Thomson's which I have just seen in type for the next *Phil. Mag.*; where three *pages* of formulae can easily, and with immense increase of comprehensibility, be put into as many *lines* of quaternions (Knott 1911, 159).

In his response, Cayley continued to wonder whether "geometrical proofs" were "independent of anything that is distinctly Quaternions, and depend only on the notion of $ix + jy + kz$, with i, j, k as incommensurable imaginaries not further defined?" These concerns would go on to occupy Tait well into the next decade.

An account of those occupations is beyond the remit of this study, though an historical study of Tait and Cayley's discourse on the matter constitutes fertile grounds for further research. The aim here has been to highlight that, well into the 1880s, Tait was navigating through what was an offshoot of the symbolical algebraic tradition at Cambridge, now manifest in the universalizing claims of Cayley and Sylvester. Ultimately, for Tait, the quaternion obeyed certain laws, and through various transformations it could come to represent a plethora of physical phenomena. For Cayley and Sylvester, the quaternion was merely a symbolical tool that could be played with. Both Cayley and Sylvester saw quaternions in relation to matrices—a topic that remained of little interest to Tait until his death in 1901.

Appendix Three

The "science of energy" and a shifting in worldview

In a letter (dated November 1, 1877) written to his former Belfast colleague, Thomas Andrews, Tait declares:

> We all heartily join in wishing you and yours many happy new years. We are all well, but *very* busy—I at Physics, the rest at *skating*! ... I am delighted to hear that you are getting on so well with your high pressures. I often wish I were back again in Belfast. True I had more lecturing to do, and less pay, but I had a great deal more leisure for private work. In fact I have barely time for any private work during the winter session now-a-days.
>
> However, I have got some students who are able and willing to work and I have handed over my apparatus to them to make the best of it. At present I am entirely engaged with "l'effet Thomson" if you know what that is—the so-called specific heat in electricity in different conductors, which I think I have proved both experimentally and theoretically to be proportional to the absolute temperature. This has led me to construct a thermometer depending on two separate thermoelectric circuits working against one another, so as to give galvonometric deflections *rigorously* proportional to differences of absolute temperature through all ranges till the wires melt. I hope to get the specific heats and melting points of various igneous rocks, &c., &c., true to a very few degrees (Knott 1911, 76-77).

From 1870 to the 1880s, thermoelectric investigations dominated work in Tait's Physical Laboratory. He instructed his students, Cargill Gilston Knott (1856-1922) and C.E. Greig, to spend a winter in the laboratory investigating the thermoelectric properties of 20 different metals pairs. Those experiments formed the basis of the "First Approximation to the Thermoelectric Diagram," an experiment that involved heating oil to a temperature of nearly 300°C. Tait was also involved in working with higher temperatures to demonstrate the fact that differing temperatures produce differing thermoelectric properties in iron and nickel (Knott 1911, 77). Tait had determined that, with nearly all of the pairs of metals he had tested up to their melting points, the "thermoelectromotive force" of the metal in question could be represented by a "parabolic function of the difference of the temperatures of the junctions." Yet, when iron or nickel is introduced into the metallic pair, that rule breaks down. Tait concluded that "between particular limits of temperature the parabolic law is satisfied, so that the relation between electromotive force and temperature can be fairly well represented by a succession of three parabolas with quite different parameters" (Knott 1911, 77-78). From 1860 to 1880, in other words, distinctive features of the "energy of science," especially with regards to electromotive force, manifested themselves in Tait's daily research and his personal correspondences.

Tait's preoccupations with thermo-dynamic and electro-motive practices hearkened a significant shift in world view more broadly—from Enlightenment-inspired metaphors of "balance" and "equilibrium" to unidirectional views of dissipation and degradation in the universe. Newton's "System of the world," as it had been termed, formed the basis for the Enlightenment view of equilibrium and celestial balance. Centripetal force (gravity) and centrifugal force (inertia) reached a state of equivalence such that the orbits of the planets maintain their respective paths infinitely and continuously. The natural philosophy of Antoine-Laurent Lavoisier (1743-1794) had also made the balance of chemical equations a conceptual foundation for natural philosophy. Post-enlightenment mathematicians, such Lagrange and Laplace, placed "balance" and "equilibrium" at the centre of their algebraic developments throughout the 18th and early 19th centuries. In Lagrange's *Mécanique Analytique* (1788), for instance, the universe is governed by action-reaction balances such that dynamics became subordinate to an overall natural philosophy of statics. Early British advocates of Lagrange and Laplace, including Herschel and Babbage, adopted this Enlightenment picture of the universe. Progress in the material, temporal world derived from rational control over matter via knowledge of the universe's fixed and static laws. In addition, the progression of time did not affect the composition of material existence. Consider, for instance, Herschel's thesis in his *Preliminary Discourse on the Study of Natural Philosophy* (1830). Herschel argued that, although society can progress through experimentation, nature itself remains fundamentally static and atemporal.

Whereas early symbolical algebraists embraced Enlightenment notions of "balance" and "equilibrium" in their conceptualizations of mathematical and natural philosophical processes, Victorian energy scientists—in particular Thomson and Tait—transformed those religiously- and socially-inspired notions of "progress" into natural philosophical claims about "dissipation" in thermodynamics. Thomson and Tait's *Treatise on Natural Philosophy* (1867), for instance, elaborated upon three thermo-dynamical laws, which collectively embraced all aspects of "energy" science. Those laws dictated that usable energy (i.e. "potential energy", which can be converted into work by humans) is always dissipating. The culturally-rooted notions of "work," "waste," and "efficiency" were thus translated into thermo-dynamical principles of conservation and dissipation. Northern energy scientists catalyzed a transformation away from the Enlightenment worldview of balance by focusing on the linear "progress" and inevitable "dissipation of energy". The moral upshot of this new natural philosophy was that constant human ingenuity is required to harness energy before it ultimately dissipates and is rendered utterly unusable and irretrievable. From the point of view of actors such as Thomson and Tait, the harnessing of this dissipating potential energy constituted nothing less than the moral obligation of man to fulfill his place in the kingdom of God (Wise and Smith 1989a; 1989b; 1990).

Appendix Four

Treatise on the Dynamics of a Particle (1858; 1865)

Tait and Steele's *Dynamics of a Particle* (1858) represents a characteristically "Cambridge" production in the sense that the book is written as a textbook for examination-minded students, with problems and solutions included at the end of the chapters. The bulk of the questions, and their attached "hint" solutions, derive from the Senate House (Tripos) examination and the College examinations at Cambridge then in place. In the second edition of the book, published in 1865 (years after Steele's death), Tait shifts his attention and philosophical outlook away from Cambridge and towards his and Thomson's developing "Natural Philosophy"—something that had been entirely unknown to Steele in the previous decade. In the new edition, Tait recollects that when he had written the book with Steele in 1858, he had not read Newton's "admirable introduction to the *Principia*" in its original Latin. He had relied upon English and French treatises on mechanics. As a result, certain pages were "erroneous" and had "cost [him] almost as much labour and thought as the utterly disproportionate remainder of my contributions to the volume." He writes:

> I cannot but ascribe this result, in part at least, to the vicious system of the present day, which ignores Newton's Third Law of Motion, though constantly assuming it (tacitly) as an axiom; and erects Statics upon a separate basis from Kinetics, thereby necessitating several additional Physical Axioms, the splitting of Newton's Law into two, and the introduction of a so-called *Statical* measure of Force (Tait 1865, viii).

Tait starts by acknowledging that his previous work seemed to be passé and "*not* the form in which such a treatise ought to be written." The 1858 text was meant to provide "advantages to the student whose sole object in reading is to pass an examination" (Tait 1865, ix).

In consulting with Cambridge coaches, such as Isaac Todhunter (1820-1884), Tait notes he had refrained from revising the new edition as thoroughly as he had wished to do, because Todhunter had told him the 1858 text was still well-suited for the Tripos. Yet, Tait's collaboration with Thomson on thermodynamics was a dominant theme in the natural philosopher's life. Not surprisingly, it crept into Tait's edited passages on "dynamics," where he indicates that properly speaking dynamics is "thermo-dynamics".

Recall that the Scottish energy scientists had been engaged in a long-winded struggle for legitimacy and primacy in their thermo-dynamical theories. As part of that project, they hunted for a British progenitor of "energy" concepts. They found him in the form of Newton. "The Conservation of Energy must be in Newton somewhere if we can only find it," Tait had told Thomson (Knott 1911, 191). The two authors thus reread the *Principia* in its original Latin, eventually falling upon a part of that treatise that served their needs. Tait and Thomson reissued Newton's Laws of Motion as the foundation for their own laws of dynamics. They also adopted Newton's definition of "force" as measured by changes in a body's motion to show that the composition and resolution of concurrent forces follows naturally from the second law of motion. As a result, Tait exclaimed he had "rediscovered Newton for the world" (Knott 1911, 190). In the second edition of the *Dynamics,* Tait invokes the concept of "energy" and argues he has revised the text so it serves as a precursor for his and Thomson's treatise on thermodynamics (unpublished at the time). The link between "thermodynamics" and Newton's "Laws of Motion" are evident, the authors claim, as both consider mass, density, particle, force, momentum, vis viva, kinetic energy and the measurement of force.

The shift towards neo-Newtonianism in Scotland was already apparent by the time Tait had moved to Edinburgh in 1860. In his inaugural lecture at the university that year, Tait tells his audience,

> That godlike mortal, as Halley does not scruple to call him, who, finding the very laws of motion imperfectly understood, in a few years not only gave them fully and accurately, and devised a mathematical method of almost unlimited power for their application, but explained most of the phenomena of the Solar System including Tides, Precession and Perturbations (though this is but one part of his contributions to Natural Philosophy)—and who was only, after repeated solicitations, persuaded that he had anything worthy to offer the world, will remain to all time the beau-ideal of magnificent genius and

devoted application, alike unstained by vanity and unwarped by prejudice (Knott 1911, 206).[136]

Thus, in his *Dynamics,* Tait talks of "the definition of work done by a force" and Newton's laws of motion as a "Parallelogram of Forces" in conjunction with "energy" and the laws of thermodynamics.

Remarkably, nowhere in his revised text does Tait mention vectors or quaternions, nor does he mention Hamilton. There are two reasons for these omissions. Even in 1865, Tait would have been reluctant to publish any introductory account of quaternions, especially as related to the addition of forces in the form of couples. Recall that Hamilton had stipulated he wanted to publish his own version of an introductory account of quaternions before Tait. Hamilton had already berated Tait in their private correspondences for having promised such an account in his inaugural lecture in 1860. Tait's reservation in issuing even a preliminary account of vector addition in his revised *Dynamics* was, therefore, the result of his hyper-awareness to issues surrounding priority claims in mid-Victorian science and Hamilton's potential admonishment were he to breach the Irishman's trust. Given Tait's sensitivity to these issues, he would have been reluctant to irk so vocal a supporter as Hamilton had been, by usurping the latter's position as being the first to publish an introductory account of quaternions. Secondly, despite its revisions, the *Dynamics* was still aimed at students studying for the Tripos examination. By the mid-1860s, the Tripos did not include anything related to quaternion mathematics. Thus, Tait chose to prudently ignore quaternions in his exam-minded text, given that it would not have been taught by coaches anyhow. And for coaches such as Todhunter, the worth of any given text lay solely in its applicability to current Tripos exams, not in its propagation of new techniques or philosophies.

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[136] In the same address, Tait defines the nascent concept of "energy" in linking it to Newton's old notion of "force". He explains, "When we talk of the *Conservation of Force* as a principle in Nature, it is to be carefully noted that we do not mean force in the ordinary acceptation of the word—and, indeed, the principle is now better known as the *Conservation of Energy*. As this is a matter of very considerable moment I shall treat of it with a little detail. *Energy* may be *Actual* or *Potential*. Actual Energy belongs to moving bodies ... Potential Energy .... belongs to a mass or a particle in virtue of its *position* ... Supposing that you have now an idea as to the meaning of these two terms, I give the principle of the *Conservation of Energy* as it has been put by Professor Rankine to whom these terms are due ... In any system of bodies, the sum of the potential and actual energies of the bodies is never altered by their mutual action" (Knott 1911, 206).

That is not to say, however, that the *Dynamics* was philosophically inert. On the contrary, it did contain some ideologically-driven statements, including claims about what constitutes correct and proper theorizing in natural philosophy, at least according to Tait. For instance, Tait argues he had been dissuaded from introducing a Newtonian fluxional notation in his treatment of particle dynamics due to the fact Newton's fluxional notation was no longer a central or significant part of the Tripos examination. Yet, for Tait, this was a regrettable dissuasion. He claimed,

> When the general equations of motion of a system have to be treated, in the beautiful manner by Lagrange, a partial use of [Newton's fluxions] is absolutely necessary. Newton's idea of Fluxions was purely Kinematical; and, in fact, the fundamental ideas of the Differential Calculus are essentially involved in the most elementary considerations regarding velocity. It is also to be observed, that, whenever we write $f'(x)$ for the differential coefficient of $f(x)$, we are really employing the principal feature of Newton's notation, though in a form somewhat more expressive than his (Tait 1865, x).

In Chapter II, on Newton's Laws of Motion, Tait introduces the "parallelogram of forces" as a means of describing Newton's second law. He further states,

> When any forces whatever act on a body, then, whether the body be originally at rest or moving with any velocity and in any direction, each force produces in the body the exact change of motion which it would have produced if it had acted singly on the body originally at rest (Tait 1865, 41).

The consequence of this law is that two forces acting upon a given body can produce a resultant motion, that can be described using the geometric resultant of a parallelogram representing the two initial forces. Tait uses this opportunity to laud Newton as the precursor to "energy" science. And he writes of Newton's third law (i.e. that every action has an equal and opposite reaction) that the great British figure had laid "the foundations of the abstract theory of *Energy*, which recent experimental discovery has raised to the position of the grandest of known physical laws." Newton discerned the application of such a theory to mechanics, which "in modern English phraseology, [is] the rate at which the agent works." Though James Watt (1736-1819) had introduced the practical "work" unit of horse-power, or "the rate at which an agent works when overcoming 33,000 times the weight of a pound through the space of a foot in a minute," a century, Newton's implied unit is still more useful, Tait argues. What Newton "implied," Tait says, is the rate of doing work "in which the unit of energy is produced in the unit of time" (Tait 1865, 46). Of course, Newton "implied" no such thing, given the concept of "energy" is a quintessentially 19th-century production. However, Tait's reconstruction of Newtonian mechanics characterizes the degree to which he saw thermodynamics forming the next big British moment in science.

Within that formation, Tait no doubt saw himself playing an important role, both as a scientist engaged in the development of the theory, and as a key propagator of the "discoveries" of the northern group. Tait's self-positing alongside Newton is developed further in Tait and Thomson's *Treatise on Natural Philosophy* (1867). In their personal correspondences it is evident that Tait was the driving force behind the *Treatise*'s production. His aim was not only to produce a university textbook that would be used in his natural philosophy classes in Scotland, which might serve to introduce thermo-dynamical questions into the Tripos exam, and which might serve as a manifesto declaring the pre-eminence and scientific superiority of the science of "energy" in Britain. The *Treatise* opens with a declaration of Newton's wisdom in having defined "natural philosophy" as the investigation of laws in the material world—investigation, often mathematical in nature, that is preceded by "observation, classification and description" (Tait and Thomson 1867, v). Modern natural philosophy relies upon "principles derived directly from experiment," though they are principles that "lead by mathematical processes to interesting and useful results, for the testing of which our most delicate experimental methods are as yet totally insufficient." Newton had foreseen the contemporary development of mathematical-physics. The authors write,

> One object which we have constantly kept in view is the grand principle of the *Conservation of Energy*. According to modern experimental results, especially those of Joule, Energy is as real and as indestructible as Matter. It is satisfactory to find that Newton anticipated, so far as the state of experimental science in his time permitted him, this magnificent generalization.
>
> We desire it to be remarked that in much of our work, where we may appear to have rashly and needlessly interfered with methods and systems of proof in the present day

> generally accepted, we take the position of Restorers [of Newton], and not Innovators (Tait and Thomson 1867, vi).

The "Restorers" claim they are revivifying a tradition in mixed and applied mathematics that had been lost through the decades of symbolization and rigorization of mathematics prominent at Cambridge. To restore the greatness of Newton was also to associate the grand concepts of the science of "energy" with the venerable Newtonian tradition.

Revealingly, the symbolical approach employed by Thomson and Tait, not only in this text but throughout their natural philosophical texts, belies the mythical nature of their historical reconstruction. Both Thomson and Tait used symbolical analysis in the mathematization of their "energy" science. They were not geometers of the Newtonian sort. Tait and Thomson both engaged in symbolical algebraic analysis according to the accepted rules of the day. The fact the authors introduced Lagrange's "generalized Cöordinates" in their second chapter on Newton's Laws of Motion in order "to complete the chapter" indicates the degree to which this was the case. Thus, it was neither Newton's mathematics, nor the Newtonian "tradition" they were restoring. Rather, they were displaying a bravura account of the developing field of energy science, which was heavily dependent upon 19^{th}-century symbolical algebra, but which could be glossed over by mid-Victorian and Scottish sentimental histories and claims to Newtonian revivification.

Appendix Five

Presbyterian Culture

The "Disruption" of 1843, led by Thomas Chalmers (1780-1847), a Scottish preacher and theologian, resulted in a split in religious culture across the north of Britain. Unlike Anglicanism, in which highly structured and aristocratic social divisions governed daily life, affordable university education in Scotland meant that much of the Scottish Kirk's staff stemmed from families of small farmers and small-scale entrepreneurs who could wield much less financial clout than the industrial middle-class families that sent their sons to Cambridge. The governing structure of the Presbyterian Church also differed from its Anglican analogue in that Scottish Kirk congregations were represented in the Kirk's General Assembly, and it was—ostensibly—the congregations that controlled the election of the Kirk's yearly Moderator and the appointment of ministers. The Moderator was elected by the Assembly to serve a 12-month term.

Contrary to the Kirk's statutes and mandate, however, patronage and politics went hand-in-hand in Scotland (Fry 1987). By the 1820s, "moderate" Presbyterians had largely allied themselves with the academic élite of Scotland's university cultures. That alliance displayed the untruth of the democratic ethos engendered in the Presbyterian governing system, as aristocrat Scottish patrons effectively dominated the election of moderators and ministers, despite the fact that control ought to have laid solely in the hands of the General Assembly and its constituent congregations. Throughout the 1830s, various parishes protested the cronyism embedded in the Kirk's election processes; the rising level of aristocratic patronage in church politics led to a "crisis" in allegiance spearheaded by Chalmers in 1843.

Chalmers led the vanguard of protesting congregations that had ceded from the established church to univocally found the Free Kirk of Scotland. At the time of the Disruption, Chalmers was the chair of Divinity at the University of Edinburgh. He had published his most ardent defense of natural history and natural philosophy in the 1830s. Chalmers' sermons were severe in their account of the world of man. Life, for Chalmers, was inherently immoral and degraded. The world was an "arena of moral trial," an "imperfect state" not able to accommodate much welfare or well-being (Hilton 1991, 80). Furthermore, Chalmers defined "natural history" as the description of static categories, such as kingdoms and classes. In so doing, he highlighted the designed nature of the contemporary universe and he focused on suffering as a natural part of a fallen human existence.

According to Chalmers, natural philosophy could explain progress in *time*. It could reveal the law-like transformations of the universe that scientists observe. In Chalmers's view, natural philosophy can explain both observable phenomena and insensible phenomena. Problematically, however, natural philosophy cannot describe the original arrangement of organic and inorganic life; nor can it account for how those arrangements would re-emerge if ever the known laws of natural philosophy were destroyed entirely (say, by an act of God). Thus, natural philosophy always provides an incomplete view of the universe. Chalmers claimed, "The laws of nature may keep up the working of the machinery—but they did not and could not set up the machine." Furthermore:

> For the continuance of the system and of all its operations, we might imagine a sufficiency in the laws of nature; but it is the first construction of the system which so palpably calls for the intervention of an artificer, or demonstrates so powerfully the fiat and finger of a God (Chalmers 1836-1842, 1: 222-225).

In other words, natural philosophy cannot serve as the basis for natural theology. For Chalmers, human toil is a necessary part of life. Suffering is a virtue and a gateway to heaven. Charity was to be avoided as inappropriate tampering with the divine design of the universe. On this view, patronage is an indication of moral laziness and profligacy—the sort that lead to eternal condemnation rather than a virtuous ever-after.

Chalmers's querulous protests, along with other criticisms of patronage and aristocratic creep within the Scottish Kirk, ultimately resulted in a serious rift between congregations. The Scottish church divided into the Established Kirk and the Free Kirk. The Kirk's schism split sympathies across the spectrum of Scottish social, political, economic and academic life. The five Scottish universities were largely staffed by moderates, who saw themselves as representatives of the ideals of the Scottish Enlightenment. They played a formative role in shaping the nature of Scottish religion by producing the bulk of the clergy. Strong ties between the Moderate voices of the Scottish Kirk (and their allied resources in aristocratic patronage), and the universities of the nation, led to direct attacks from Chalmers and the Free Kirk followers.

For their part, the Free churchmen proposed an evangelical interpretation of life as an ongoing manifestation of humanity's decline. The lackluster performance of Scottish universities in the first half of the 19^{th}-century was used as proof of this fact. The Free Kirk followers pointed to the irrelevance of Scottish moral philosophy, natural philosophy and educational curricula in Britain more generally as further indication of the nation's degradation. The country was no longer producing great names, such as Adam Smith (1723-1790) or Joseph Black (1728-199), nor was it providing solutions to the ongoing social problems of urban decay and deprivation witnessed daily on the streets of Edinburgh and Glasgow. An indication of the radicalized nature of religious life across Scotland is evident in the General Assembly of the Church of Scotland's 1835 national "day of humiliation." The Evangelical Party decried the "demerits of this our day" in religious instruction, family worship and church attendance. Following the Disruption, the Pastoral Letter of the Free Church in 1846 lamented "the prevailing ignorance and practical heathenism of large masses of the people." Those authors concluded "God seems a stranger in the land." And in 1851, concerns over rising secularism, or at least rising church absenteeism, reached a peak when 17 presbyteries petitioned the General Assembly of the Free Church to counteract the "Spirit Destitution of the Land." The Synod of Glasgow and Ayr issued the following statement in response:

> Whereas irreligion, neglect of the means of grace, atheistic contempt of God, and antichristian error are alarmingly prevalent over all the land, but more especially wheresoever large masses of the population are drawn together: Whereas this lamentable state of things is so rapidly increasing as to be almost already beyond the ordinary means of cure, and threatens soon to obliterate the very appearance of Christianity amongst us and thus to bring down the judgments of God on a people so

privileged as we have been ... We overture the General Assembly of the Free Church of Scotland to take the subject into its consideration (Enright 1978, 401).

Indeed, as late as 1863, the *The North British Review* argued that, while the religious "condition" in Scotland was better than that found in England, the "sunken classes are the irreligious classes" and they still marked the landscape (Enright 1978, 401).

The evangelical fervour of the Free Kirk fuelled debates over moral obligation and religious action throughout the 1840s and 1850s. A new-found biblical literalism swept across the country's university campuses in those decades. Biblical literalism of the Free Church increased to accommodate the negative reaction of its members to the "materialism" of the anonymous *Vestiges of the Natural History of Creation* (1844) (Hilton 1991, 24-26). Following Chalmers's death in 1847, a severely literalist reading of the Bible became popular among young university students, colouring the manner in which natural philosophy and mathematics were viewed by Free Kirk members more generally.

The Disruption would come to have profound effects on Tait's later professional career. One way in which it did so was through the choices made by James Thomson. When the senior Thomson had arrived to Glasgow in the 1830s, with his young sons James and William in tow, the Scottish colleges had largely alienated themselves from the poverty-stricken, urban populations of the overcrowded town centres. Tory-supporting moderates, who enjoyed academic and social privileges from their positions at the university, were reluctant to reform the university to allow for more democratic access to paid positions or even to alter curricula so as to better suit and serve the circumstances of the cities. For instance, in the year of the Disruption, the Principal of the University of Glasgow, Duncan Macfarlane (1771-1857), was also Moderator of the Scottish Kirk. Macfarlane led his fellow moderate colleagues in Glasgow in a protest against any changes to their privileged status. Through Macfarlane's efforts, college moderates were able to hold on to their administrative control at the university as well as their hefty incomes, derived from annual land rents. Indeed, for years after the Disruption, the status quo remained untouched.

Yet, the changing nature of the student body, which was becoming increasingly evangelical and Free Kirk-friendly, exerted indirect pressure on the moderate class of professors and academic staff at the universities. Decreasing student numbers (which led to a drop in salaries among university staff), was particularly problematic. Young students were being drawn away to newly established Free Kirk educational institutions (such as the Edinburgh Free Church College). The need to attract students in the new era of religious schism drove a series of reform efforts, led in large part by the latitudinarian Glaswegian professor of mathematics, James Thomson, and his reformist colleague astronomer, John Nichol. With Thomson and Nichol leading calls for change, Macfarlane's efforts were short-lived. A significant change in teaching staff took place over the course of the latter half of the 1840s, evidenced by the fact that, by the end of that decade, nearly half the votes in the College belonged to self-identified reformers and non-Moderates. As a result, the university was under severe internal pressure to abolish its religious tests, which had long ensured that new professors had signed the Westminster Confession of Faith professing faith and commitment to the established Kirk in Scottish Presbyterianism (Smith 1989, 23).

To spearhead his efforts, Thomson allied himself with Norman Macleod (1812-1872), a former Arts student at Glasgow and divinity student at Edinburgh who had returned to Glasgow as leader of Glasgow's Peel Party in the late-1830s. In so doing, Macleod situated himself within a community of reformers who opposed traditional Tory moderates. But unlike the evangelical Chalmers, Macleod was not a Free Churchman. He believed that humans had a role to play in improving the lot of humanity (Macleod 1876). In 1851, Macleod became the Church of Scotland minister for Glasgow's Barony parish. In that position, he vocally attacked the elitism and authoritarian control that traditional moderates still brandished in the Scottish universities. In 1856, Macleod addressed the General Assembly of the Church of Scotland, arguing that the old guard required reform. Macleod's speeches created a standard for reformist rhetoric; as a result of his diatribes, increasing numbers of non-sectarian liberals were appointed to posts within the university. For instance, Rev. John Caird (1820-1898) was appointed professor of theology in 1862; he later became principal of the university in 1873. Caird's non-sectarian views had long been known. In his sermon, "The Christian's Heritage," published in 1858, Caird argued:

> The history of the Church but too often exhibits the strange anomaly of a religion of love producing the keenest haters, and a gospel of peace engendering strifes and animosities more bitter than the disputes and rivalries of the profane ... The Christians at Corinth had quarreled with each other on the merits of their respective teachers—each party boasting of the pre-eminent wisdom or eloquence of its own head, and contemning the gifts of his supposed rivals...In the pursuit of wealth it may be natural, however culpable, to begrudge another his gains, or to be elated at our own; for wealth is a limited good...But with respect to spiritual good ...These belong to that class of blessings which possess the qualities of universality and inexhaustibleness. The light of the sun is not the less bright to me that it beams at the same moment on millions of my fellow-men (Caird 1858, 246-249).

With such views in hand, Caird became a clear contender for an appointment at the university given that Thomson, Macleod and Nichol were, by that time, in control of the choice.

These religious events in Scotland coloured the workings of the later scientists of "energy" in important ways. In 1848, for instance, the deeply religious, though latitudinarian, Thomson declared, "as yet much is involved in mystery with reference to [the] fundamental questions of natural philosophy" (Smith 1989, 101). Chalmers had been a close acquaintance of the Thomson family for years, and he had spoken vehemently throughout the 1830s about the nature of the universe to tend towards disruption and decay. Though neither the elder nor the younger Thomson shared Chalmers's Calvinist gloominess, Thomson's 1851 statement that "everything in the material world is progressive" evoked images of decay. Thomson had defined "progressive" as movement towards dissipation. That phrase, uttered in the context of his first installment of the "Dynamical Theory of Heat," as presented to the Royal Society of Edinburgh on March 17th, 1851, suggested a theoretical approach to "energy" that had been informed by more than just the experimental results of the Thomson brothers, the young Rankine, and Joule. Thomson noted, for instance, the convincing investigations of Clausius and Rankine, which had indicated a dynamical theory of heat at play, in which heat and work could be converted into one another. He declared those investigations to be among the best means of developing theories of motive force, as those investigations demonstrated work could be produced from systems that went from high heat to low heat. They also indicated that one could not recapture work done or energy lost. Thus, Thomson concluded, "Everything in the material world is progressive." In other words, energy flows in one direction only and humans can choose to capture it and use it efficiently, or let it go to waste and allow it to dissipate all the more quickly. Thomson's moral claim was that humans are born with the duty to capture divine resources in the form of "potential energy" and use it to do useful and meaningful work—i.e. the work of God.

Thus, the northern scientists—inspired largely by Thomson's latitudinarian attitude—forged a middle-ground between doomsday scenarios and the positive interventions possible on part of humanity. Macleod, for his part, encouraged the northern scientists in their compromising efforts. On Macleod's interpretation, natural laws govern phenomena, but the teleological end of the universe is envisioned and encapsulated in the dissipation of energy. Although theirs was not as severe an account of the death of the universe as Chalmers's moral condemnation of the entire world had been, the northern British scientists did view energy dissipation as a form of natural degradation. They belonged to a "post-evangelical culture," as Smith (1989) has labeled it, in which human beings "in their moral and material actions, had an obligation to aspire to the perfections of nature and of Christ" (Smith 1989, 318n). The developing "science of energy" thus attempted to counter atheist materialism, as embodied in Darwinism in the South, and biblical literalism, which was still prominent among practitioners in the North.

In later years, Tait diverged from this latitudinarian and compromising stance by issuing increasingly bombastic and explicit religious claims. In part, he was motivated to be explicit in his metaphysical views by the threat of Darwinism, as he perceived it. His religiousness also came to represent, however, a subtle opposition to his colleague, Thomson, who had remained adamantly private in religious practice. By the 1870s, Tait found support in the like-minded opinions of his fellow energy scientist, the Scotsman Balfour Stewart (1828–1887), a natural philosopher who, like Tait, had been schooled at the University of Edinburgh in the radical days of the 1840s. Those two Presbyterians chose to express their frustrations with evangelicalism and secularism through the issuance of two highly charged publications. Much to the chagrin of Thomson, Tait and Stewart first publicized their world view in *The Unseen Universe* (1875). Following a severe intellectual hammering from Clifford and other secular critics, Tait and Stewart issued a second account of their religious-scientific picture of the universe in *A Paradoxical Universe: Sequel to the Unseen Universe* (1878). Despite the criticism from secular social theorists and mathematicians in the South, and a displeasure among northern thermo-dynamics scientists, such as Thomson, both books proved to be wildly popular. By 1880, *The Unseen Universe,* and its profoundly religious message about morality in scientific discourse, had already gone through six editions.

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