Abstract: In 2010 Reinhart and Rogoff published “Growth in a Time of Debt”, a wildly successful macro-economics paper. The conclusions made in that paper turned out to be based on a mistaken analysis of the available data. My intention here is to explain how they went wrong, without cleaving to mere worries about how to look at the data. I cover a number of different issues, and arrive at an account of the conceptual details of the paper, which emphasises the “fragility” of the “systematic relationship” the paper purports to have discovered. I discuss the question of “induction” and argue that the paper goes beyond induction anyway, which leads me to conclusions about how causation is built into economic theory, and how such building in requires more careful attention than the authors of “Growth in a Time of Debt” (Reinhart, Rogoff 2010), and their critics, attempted.

This essay has three parts. The first describes what a c-theory is and addresses how building a c-theory differs from building different kinds of economic theory, here will be introduced the notion that a c-theory is limited as a basic induction from a dataset, without predictive or explanatory power. The second part will attempt to show that this worry is at least partially wrong, a c-theory has particular features which delineate it from and set it out as more sophisticated than mere numbers on a spreadsheet; a c-theory will be shown to have at least limited causal content. Furthermore, these can be characterised as virtues that a c-theory possesses over and above a mere collation of data, however, at the end of this section I will introduce the worry that this does not mean that c-theories are as predictive or explanatory as their advocates would like them to be. In that section I introduce the “optimistic” account of such science, as especially favoured by Hasok Chang and Eran Tal (Tal 2016) (Chang 2012), and contrast it with my own more pessimistic view. In the third section I will discuss the fragility of a c-theory, and present an argument that c-theories have only a limited place in economic theory.

1a. C-theories and their cousins

A c-theory is a particular kind of economic theory which does not discuss causation. To clarify, I will be using the term “c-theory” loosely, and be agnostic as to whether the term “theory” really applies in that usage, and c-theories will come in for criticism with respect to the authenticity, so to speak, of any claims that a c-theory is a ‘genuine’ theory. Let “c-theory” therefore be a placeholder for the kind of paper written by economists which does not discuss causation in its pages. C-theories are contrasted with a- and b- theories, both of which discuss causation, but in different ways.

An a-theory is somewhat agnostic about the specific causation built into the theory, and uses what the theorist regards as a sufficiently plausible causal assumption to explain existing data, and makes predictions on that basis. A cardinal example of a successful a-theory is “consumption smoothing” as articulated by Milton Friedman in his “Permanent Income Hypothesis”: “consumption smoothing” makes the assumption that households rationally anticipate macro-economic trends by, for example, saving in anticipation of oncoming periods of low-income, and draw conclusions and make decisions about how to consume at time x or y which causally affect overall or aggregate consumption in an economy.

A b-theory goes the other way, using statistical tools to infer specifically what relevant variable is the "cause" of some economic event, relying less on the a priori plausibility of the causal story it tells. In contrast to e.g. Friedman's account of consumption smoothing, the causal variable is not a plausible assumption, but a specific account which falls out of statistical analysis. Good examples of b-theories are to be found in the history of "The Credibility Revolution" (Angrist, Pischke 2010) in macro-economics, where increased computational power, more complete micro-economic data, and more sophisticated statistical tools gave economists the chance to attempt to actually nail down what variable was doing the causing out there in the real economy. By contrast a-theorists have perforce to accept that their causal variables are significantly coarser, and more reliant on the plausibility of a fiction: Friedman was building on the same rough method as Keynes, who in the absence of b-theoretical analysis spoke in a speculative manner of "animal spirits" in the minds of economic actors (e.g. households) for which he did not have a satisfactory explanation but which were a postulate of his account of persistent disequilibrium.

C-theories are notable for not, at least not explicitly, being interested in such questions. Instead a c-theory takes what is known about one, two, or a thousand economies, and establishes a putative regularity or correlation (I will mainly use these terms synonymously, unless otherwise specified) in the data. My main example is Reinhard and Rogoff's (R-R's) Growth in a Time of Debt, the most important contribution of which is its attempt establish that beyond a certain level of public debt[1], GDP slows or becomes negative across a variety of economies over a breadth of space, time, and circumstance. It does so without discussing a plausible causal mechanism, or attempting to establish by statistical analysis what the causal mechanism is behind this correlation.

Such a theory has interesting features. For example, in the absence of putative causal explanation such a theory somewhat resembles the ideal science of causal republicans who prefer to replace such a supposedly naive concept as "causation" with demonstrable mathematical association between observations as Russell speaks of with respect to Newtonian physics. On the other hand, c-theories could be a blank slate: in the absence of a causal story proposed its author, a reader may be in a position to impose such a story on the data. Which makes this a good time to properly introduce my main example.

1b. A c-theory in more detail, with induction

R-R's Growth collated data on levels of public debt from a variety of economies over 200 years, growth in Gross Domestic Product (GDP). They summarise their, methods, aims, and (most important) findings thusly:

[1] "Public debt" is in *Growth…* understood to be "gross central government debt" (R-R 1), i.e. all of the debt owned by the government at any one time, but this does not include debt "carrying a government guarantee", I agree with the reader that this latter phrase could have been clarified better.

“...we exploit a new multi-country historical dataset on public (government) debt to search for a systemic relationship between high public debt levels, growth and inflation. Our main result is that whereas the link between growth and debt seems relatively weak at “normal” debt levels, median growth rates for countries with public debt over roughly 90 percent of GDP are about one percent lower than otherwise; average (mean) growth rates are several percent lower.”

R-R’s “systematic relationship” was, in their paper, a relationship without causal explanation. Indeed they described the uniformity of the above relationship between debt and growth across different economies and times as “Surprising[...]” but presumably nonetheless compelling. That it is surprising but compelling implies one or both of two propositions: first, that R-R find results which don’t discuss causation scientifically worthwhile; second, that surprising results can overturn prior intuitions in the absence of such discussion. R-R considered their conclusions in Growth… to be of scientific importance even without themselves proffering an a- or b-theory about those conclusions.

Building a c-theory such as Growth… means finding data from whatever sources are available in order to discover whether a “systematic relationship” exists. If the data do not appear to align with each other, then presumably no such “systematic relationship” exists. The first thing to do is, as R-R do, to make observations, or in this case to draw on existing observations. The next thing to do is to do something like build a spreadsheet, which is what they did. The data need to be weighted for significance and confounding factors in order to make them comparable. For example, two governments may at one time hold at first glance a certain amount of debt as a ratio to GDP-growth. However, consider that one country only held that debt for a brief period, compared with a period twenty times that long for the second. It therefore follows that if, as seems plausible, the nature of the relationship between government debt and GDP growth may depend in part on the length of the time during which the government has been borrowing (medium-term vs short-term borrowing), that this further consideration should be accounted for, and weighted in the spreadsheet. In Growth… R-R are careful to emphasise that it is build-up of debt over years which is one of their primary concerns, and they weighted different data-points accordingly. For example, they claim, against critics, that their weighting of one of the datapoints from New Zealand is appropriate, and does not reflect a short-term crisis due to extraneous and unique circumstances which might render that datapoint not comparable with the others.

Still, the philosophical heritage here is decidedly Humean. The goal is to produce an internally consistent account of the available observations mediated by their consistency with each other and with potential confounds. In building their c-theory, R-R took their new dataset and checked whether the data could be related to each other sufficiently coherently to permit their declaration that they had inductively discovered a regularity, and in their case the spreadsheet and entailed weighting was the intermediary between the dataset and the c-theory.

Dataset -> spreadsheet - c-theory

In Humean style, R-R had discovered, by comparing whether their relations of observations, and ideas prompted by and working on those observations, that economies on the macro level consistently behaved in a certain manner where the relationship between public debt and GDP-growth was concerned, therefore predicting that as one expects to find the Sun rising tomorrow, so too one expects if one looks hard enough, to find that above a ratio of 90% public debt to GDP, GDP-growth will slow accordingly. Hume would likely call this “customary”: not justifiable by pure reason, but sufficiently inductive to countenance. Further analysis was still needed to make the case, however. Aside from worries about how to weight data between individual countries, they also group different countries according to reflections on their different states of economic development, in order to bolster their conclusions by showing that if you group economies as “developed” and “emerging market” economies, the data work out differently for each group. The word “surprisingly” turns up again, prefaced this time with the word “perhaps”, because the authors find it “perhaps surprising” that the same ratio between GDP-growth and debt shows up in their developed economies as it does in their less developed “emerging market” economies. This will become worth considering where I attempt to show how such groupings fit into the theory-ladenness of Growth...

Note that the work involved in analysing the spreadsheet when it comes to these groupings is of a different order than it was when weighting was the main theoretical consideration. Whereas before the issue of weighting different numbers was done in a bottom-up manner, in order to produce consistent, precise, and accurate mathematical analyses on a spreadsheet, now the distinction between “developed” and “emerging market” economies is applied from the top down, both to satisfy the intuition that economies in different states of development will behave differently, and also to reflect apparent differences in the data. Here the attempt to identify a correlation goes in the other direction.[2] This has the effect that once the data are organised into two different groupings, the public debt and GDP-growth correlation still exists, but other measures, such as of inflation, diverge between the two groups. The correlation between the two groups also falls apart in other places because emerging markets experience, for one, different and faster growth under desirable conditions. Up to the golden 90% debt to GDP ratio there is some divergence, in that “emerging market” economies tend to experience faster growth, and after the 90% debt-GDP ratio, in undesirable conditions emerging market economies experience worse problems with rising inflation[3].

Building this particular c-theory therefore involves not just low level comparison of data-points divided by time, geography, and circumstance; but also broader distinctions between groups of economies which are divided according to a common and pre-existing vocabulary of

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[2] Interestingly, the authors here point back to the apparently more data-complete working version of the paper for “the interested reader”, since for reasons of space they have not in the final version included the complete table of data for emerging market economies.

[3] Here R-R offer a titiliting glimpse of a b-theory, suggesting as a plausible reason for this phenomenon “fiscal dominance”, i.e. the dominance of fiscal policy in an economy without the sophisticated government-independent monetary bodies such as the Bank of England which in developed economies nowadays take the primary role in regulating the business cycle - since fiscal interventionism as a policy tool is associated with high inflation, and the **causes** of this are relatively well-known.

“developed” and “emerging market” economies. Readers are expected to be familiar with these terms and understand how they work, knowing that “emerging market economies” and “developed economies” have different conditions for debt, GDP-growth, and inflation. However, although independent of the bottom-up considerations about how to weight the numbers for different countries are independent of such top-down groupings, these top-down groupings of countries still correspond to an acausal account of which countries belong in which group in the analysis, R-R contest with some justification that although the distinction between the two groups is coarse, it still reflects differences in the data.

1c. The notion of an “acausal map”.

I want to summarise this section by using the metaphor of a “map”, specifically an “acausal map”. An acausal map is a representation of something like Lewis’s notion that a “bare particular” can be expressed by its place in space and time. Without even asking how one variable relates to another, if Growth… is a good c-theory, it represents a dataset by showing that at this time, and at this place, growth was at a certain level and debt was at a certain level. It then proceeds to show that in a second, third, and so onth place growth and debt were also at a certain level. We could draw up a map of the Earth which identified at which time and place debt and GDP-growth coincided at the 90% limit, and without reference to causal explanation plausibly show that the identified regularity exists. Sure, we wouldn’t have a causal explanation for why these numbers converge, but we could represent the convergence both on bottom-up terms, by making sure that the temporal and geographical differences are accounted for, and top-down terms, by showing that those temporal and geographical differences produce a distinction between two groups. Just as cartographers are concerned with simply drawing a map of the world, but need to do compare and assess available data to arrive at a good enough version of the shape of a coastline, and borders between countries, without worrying about how they got there, a c-theory could just be the upshot of the same kind of theorisation: it’s up to the geographers to work with those maps to come up with things like causal explanation.

This is motivated by considering causal republicanism, as I raised eariler. The c-theory approach reminds me especially of Russell’s assertion about pre-relativistic physics that it is not necessary to know how an object gets from A to B, so long as one has a mathematical account of what A and B are at any one point in space and time in order to establish that the universe behaves according to certain rules, laws, or regularities (take your pick). If I observe that any one object appears to be have x, y, z… properties, and even if my observations are imperfect, I can develop a theory, which does not rely on any causal proposition (quasi-speculative in a-theories, investigative in b-theories). Newtonian physics, for example, relies not on causation, but on whether a prediction matches data. If it does not, then either the data or the theory are compromised, without causation ever entering into it. We build a map of sorts describing the perihelion of Mercury, building in ideas like “gravitation”, without ever asking what it is that made Mercury have a perihelion in the first place. Building a c-theory, as with any theory, involves intricate cartographical work which involves the sorts of assessments as described above, but does not prima facie impute causation. It is possible to impose judgements on the data without

appearing to invoke causation, as with Russell's account of physics. The terms employed, such as "GDP" and "public debt" appear agnostic or even atheistic about causation. At the very least, causal explanations can come later.

The worry here, therefore, is that without causal explanation a c-theory is just an explication of data which involves some grimly methodological theorising at both the bottom-up and top-down level in order to make the data comparable according to existing terminological standards. Those standards are employed, be they the standards for assessing "public debt" or "gravitation", but as long as they are employed properly, no causal account of how the measures "public debt" or "gravitation" came to produce the measures that they did is required. It is, for the time being, sufficient that they map out the state of play in time and space (and, one might broach, society).

But it has been worried by some philosophers, and many economists, that scientific theory should and does aspire to better than cartographical explanation. For example, it seems as if economists who want to use a c-theory to make predictions - which R-R certainly do, given their warning that governments should not be borrowing to spend their way out of the ongoing Global Financial Crisis in 2010 - they should be doing more than merely inducting that public debt has caused GDP-growth issues across time and space. No matter how accurate their forecasts, the question "why is this an effect?" lingers, because, contra naive Humeanism, economics is general concerned with supplying causal explanations and discussing causation more broadly.

Bear in mind that R-R compare data not just between different governments separated by space, time, and circumstance, crudely defined, but across states whose constitutions wildly diverge from each other, and even their analysis between different developed economies includes data from economies separated by 200 years of the development of, for example, the concept of debt, the concept of currency, and the concept of a state itself. The Bank of England was only instituted as independent of government in the 1990s, and the realisation that American monetary policy was the major factor in the Great Depression came decades after significant debate about whether or not a central bank should even exist. The analysis which demonstrated monetary policy's role in The Great Depression explicitly relied on attempts to b-theoretically isolate causation, and those efforts were re-itigated by Ben Bernanke in his attempts to deal with the GFC as chairman of The Fed. Under these circumstances, economists have long sought specific causal explanations for why such and such a correlation should be such and such a way, in the manner of sceptics who worry that "correlation is not causation".

2a. Grounds for going beyond induction

In section 1 I showed that a c-theory is not just a spreadsheet but also showed how the operations made on the spreadsheet related back to existing terms of art and the c-theorist can draw up a "map" of how the measures which those terms are used to represent ("GDP", "public debt" etc.) can be drawn up. In this section I aim to show that such a map can be read, and that this fact entails causal content for the theory which that map represents. This entails a few

conclusions about at least my chosen c-theory, in Growth… loosely summarised in this list. To be clear, this is a rough guide for what will follow laying out some key issues in what will become more complicated:

(1) That the theory-ladenness of the terms employed entails that the theory has causal content, even if that goes unacknowledged in the paper describing it
(2) That this theory-ladenness is assumed by the c-theorist, such that the theory is taken at face value by a sufficiently expert writer or reader of the paper, without asking for explication
(3) That this theory-ladenness is sometimes taken as a "ticket", such that it is taken as read that the causal bases are covered, and the c-theory fits neatly enough into existing theory that no causal assumptions or analyses need to be taken into account, in spite of the general atmosphere of economic science which encourages economists to attempt to discuss causation
(4) Given (1-3), a c-theory carries epistemically normative content: a c-theory is the sort of theory you have reasons to believe in if you think that causation is important, because even though it might lack an explicit causal account of the phenomena it describes, it still has causal content by its theory-ladenness
(5) Even given (4) a sceptical worry arises, where the implicit causal content of the c-theory does not suffice for a warrant in believing in the c-theory. It is too easy to believe that just because a c-theory intuitively carries causal content, that that content adequately matches what is really going on in reality

There are two different concerns here. Points (1-3) address facts about my chosen c-theory: in spite of acausal appearances, Growth… actually includes causal content, even if only implicitly. Points (4-5) address the question whether what is addressed in (1-3) justly motivates belief in the conclusions of Growth… These two concerns are inter-related, but separate, and the difference should be kept in mind in what comes next. The former refers to whether or not there is causal content, the latter refers to whether the existence of that content sufficiently motivates agreement with the c-theory's conclusions.

In this section I will draw on two recentish and popular accounts, in Eran Tal (Tal and Hasok Chang, of the inter-relationship between these two ideas. I think a better account can be supplied where it comes to Growth..., but both Tal and Chang insightfully grasp how the theory-ladenness of (1-3) can motivate optimism where (4-5) are concerned (Tal . Both authors emphasise that one should not be dogmatic about putting assessment of the theory front and centre at the expense of attention to the details of why that theory has been accepted. They both argue that there is nothing wrong with accepting that a theory is imperfect, and the reasons for choosing a theory, or a version of a theory, could be indissolubly sociological and/or ad hoc, rather than ideally rational, but still warranted. As such, they provide a fruitful base for analysing c-theories, where ad hoc and sociological interpretations of causation are easily imposed. Where they argue for optimism in their own case-studies, I will argue for pessimism about mine, but build on their insights in doing so.

First, however, I make the case that c-theories imply some causal causation (1-3), and then build on Tal and Chang (4-5), although I will litter my discussion with references to both by way of explanation.

I will deal with three issues: first, a c-theory is not just a collation of data but a participant in a holistic process of theorisation; second, a c-theory is "ticketed" for a community of scientists by its skillful employment of relevant terms of art; third, a c-theory is theory-laden, in the sense that it succeeds or fails by its relationship to existing theory. The first issue is broadly metaphysical in the sense that it is concerned with what the theory "is"; the second is broadly sociological because it concerns the meaning of terms, and the implications of that for a scientific community; the third is epistemological, a c-theory qua "knowledge" must have some fit to existing theory.

2b. Doing better or worse than induction, specifically

First is the issue of how terms of art like "GDP" contain causal content. In order for R-R to make their case, they needed to use terms like "GDP", and these terms don't arrive in their analysis ex nihilo. Indeed, as Tal and Chang both point out, a measure like "GDP" is inseparable from theorisation. The theory cannot exist without the measure and vice versa. Whether such a term is apposite for analysis is reliant on the historical development of the term as a term of analysis. The term "GDP" is only an apposite term because in debate about that term it has been found to accurately reflect explanations elsewhere than in the c-theory. In the case of Growth… it is clear that in drawing their map of GDP-debt ratios R-R were concerned with characterising what growth and debt are and have been. A c-theory does not explicitly prescribe any causal nature for the concepts "GDP" or "public debt", but the author of a c-theory can also not characterise those concepts without at least implicit deference to the historical development of either concept. At least implicitly, when an author uses either concept, and assesses their relationship to each other, they are invoking existing theorisations (for example of the a- and b- kinds) which were backed by their explanatory power which is why they are being used now.

In the development of either concept, a causal explanation has generally been debated, and fiercely. The nature of, for example, "GDP-growth" is tied to a- and b-theories of how GDP growth happens. In order to correctly measure "GDP-growth", economists frequently assess what the underlying causes are, in order to arrive at a more accurate and precise understanding of what they mean when they employ the term "GDP-growth", and do so in order to arrive at some unity, such that even where disagreement exists between competing theories, the grounds exist for intelligent disagreement, and those economists are communicating meaningfully. Therefore the nature of "GDP-growth", as a number, has a concept attached, and can only be cartographically built into the c-theory if some degree of understanding exists about what "GDP-growth" means. This has the result that "GDP-growth", as more than a mere number, carries for example the explanatory content of the a- and b- theories which discuss causation that conceptually precede its employment in a c-theory. The employment of the

concept “GDP-growth” in Growth… is implicitly related back to the macro-economic theory which gives its meaningfulness whenever it is employed: it cannot but implicitly carry some causal content if it is employed meaningfully.

Here is an example: GDP-growth explains what is going in an economy in the sense that it relates further back to estimates of that economy’s capacity to produce goods. These estimates rely on measurements of what is produced, either good by good (calculation-in-kind) or by aggregating the total number of goods and their worth in currency as measured by the standards that obtain in contemporary economic science[4]. Further, those estimates rely on data collated and reported by, for example, accountants or government officials. Sometimes they rely on reports by firms themselves, as well as by actuaries and accountants and so on from outside those firms who assess them. At the same time those same assessors rely on GDP-growth data from other assessors to make their own projections about what they are going to do, and adjust their own predictions accordingly. The system is, to borrow a phrase from George Soros, “reflexive”, in that drawing a map which represents “GDP-growth” is not the act of one cartographer but a collective act of back and forth between different and even competing cartographers, each with only partial knowledge of the state of the economy, who all are trying to arrive at the best possible estimation of what the term “GDP-growth” means, often with causal explanations being part of the content of that assessment. Certainly particular assessors have in mind their preferred causal explanations when they make their assessments, even if only implicitly.

There is the analogy with Tal. For Tal, the meaningfulness of the term “second” as a term of art for the measurement of the passage of time is reliant on a reflexive relationship between different measurements made by different, similar but not identical, machines and the engineer-scientists who maintain and measure with them. As Tal notes, those who look after the machines which are compared with each other use their own ad hoc ideas about what might be going right or wrong, and make intuitive adjustments accordingly. As with Tal’s account of the measurement for the passage of a “second”, the attempt to make the term “GDP-growth” meaningful is an issue of engineering, with central banks working together with accountants, firms, and so on, back and forth, to test each other’s intuitions and practices in order to arrive at a meaningful account of the concept “GDP-growth”. Similarly, Chang assesses the meaningfulness of the term “temperature” and concludes that it’s coherence is reliant on the ongoing attempts in the 18th century to bring the concept into a wider theory of what, for example, Boyle’s Law entails, where the formulation of Boyle’s Law was reflexively also reliant on a sufficiently coherent account of the meaning of the term “temperature”, and on experimental attempts by his experimental chemists to make the idea of “temperature” work as a measurable quantity. For GDP and GDP-growth, everybody involved in measuring what any one GDP-growth figure is contributes to the process of making the term “GDP-growth”

[4] Economists generally do a form of “spot-check”, since nobody can measure goods produced in totality. Various measures are employed and related back and again to one another, and checked again, rather in the manner that Tal highlights when it comes to measurements of time.

meaningful, because “GDP-growth” is not just a stand-alone word, but a part of a theory and ongoing process of theorising.

Understood thusly, the term “GDP-growth” employed in Growth… means something only if it at least implicitly relies on the underlying causes of GDP-growth within a community of understanders. Without those underlying causes being a part of the theory grounding the term “GDP-growth”, no such term would exist, or at least be meaningful and communicable. Consequently, the term has some causal content, in virtue of the fact that nobody should deny that in employing a measure such as GDP-growth one is employing more than a raw acausal number, because without that background nobody could meaningfully interpret what her interlocutor was saying. This, on the Tal-Chang axis at least, a somewhat (4-5) issue, and could be reason for optimism about the utility of the term. It licences (a) the idea that a c-theory employing it is more than acausal and (b) that it is backed by an ongoing endeavour to make the theory itself meaningful.

Second is the notion of a “ticket” and its relationship to skillful employment of a term. Skillful employment of a term, i.e. knowing what it means, can be a “ticket” for credulity, which is to say that skillful employment of a term, and properly using it in analysis credits the employer with a degree of expertise, and therefore trust. While this may seem like an uninteresting concern, the skillful employment of terms warrant acceptance of a c-theory by a scientific community is something which should be paid attention to, and has been paid attention to, in macro-economics - especially when it comes to causation. In the case of Growth… it would not have been possible for the paper to have been accepted as widely as it was were it not for the skill of the economists involved in assessing what “GDP-growth” was in their dataset.

This returns to the issue of doing bottom-up analysis of the dataset R-R employed. Economists are expected not only to know what “GDP-growth” means, but to be able to apply that concept to data in the proper manner, and to assess which numbers can be meaningfully incorporated into that concept and how. When an economist finds a “surprising” result, it is understood that they have done so on the basis of not only their best understanding of the term, but on the basis of the theorisation which the term entails i.e. on the basis that their analysis comports either (a) with the implicit entailed meaning of the term drawn up in the first part of this section or (b) that it undermines some conceptual (perhaps causal) assumption made in the common understanding of how the concept “GDP-growth” works. Deciding which of (a) or (b) is the case means doing further work to see whether we should reject existing theory or re-adjust our conceptual apparatus in light of new facts emerging.

Third is the epistemological question of how and whether a c-theory fits existing theory. This is distinct from the foregoing questions in that it is concerned not with working outwards from the conditions which would set up a c-theory as being in a relationship with wider scientific practice, but asks the question whether or not it does, and makes demands on a c-theory which it has to meet in order to be justifiably accepted into a wider scientific corpus. “Tickets” turn up here as well, since economists do not deal with an idealised “view from nowhere” on economic matters,

but each individually participate in research via their own partial knowledge and judgement, as already discussed with respect to Tal and Chang. As such, the epistemological grounds for accepting a theory may rely, to even only a limited extent, on the degree to which the available tickets meet acceptable rather than ideal epistemological standards in the absence of ideal rationality, theory, and information (or data). In explaining this aspect, I will draw on what I have said in the first and second parts immediately foregoing this part, and consider that the epistemological question is parasitic on the metaphysical and sociological considerations therein. Explaining the epistemological question of fit to existing theory relies on appreciating what I have already said about the meaning of terms and their skillful use within a scientific community.

A c-theory can fit into wider theory in more ways than one. For example, it can confirm or disconfirm existing observations. I spoke earlier of the “meaning” of a term, whereas here we are talking about fitting existing observations. While the conclusions of Growth… were “surprising”, in the sense that if accurate it disconfirmed existing expectations, another example suggested to me, Hicks’s notion of “stylized facts”, intends to provide a basis for future theory by assessing general observations which are said to have been already been made. By capturing those observations, without immediately imputing causation, “stylized facts” provide a ground for future research. They may be confirmed or disconfirmed by future investigation, but they provide a foundation. In direct contrast to Growth… they are supposed to be the opposite of surprising from the perspective of the competent economist, and so represent the state of current theory up to that point.

In this sense, a c-theory of type G (Growth…) intends to fit a future explanation of its data, whereas one of type H (Hicks), intends to fit what is accepted within wider theory at the time. Both positions can motivate optimism about the acausal nature of c-theories. So long as a theory isn’t a meaningless clutter of data, and can be expertly interpreted, we have in significant part satisfactorily responded to the worry about c-theories being mere agglomerations of data as outlined in notes (1-3) above. By adding a further feature, that c-theories aspire to have epistemological fit, in spite of known limitations, it seems we have also satisfied the condition in note (4). So long as the terms are well understood, and meaningful, and theories which use them aspire to be truth-seeking, we should be alright. This is at least my read on the idea that c-theories are more than just spreadsheets, in meeting the conditions outlined in notes (1-4), a c-theory is both more than a spreadsheet, and also induces normative content, if you are an optimist, for believing in it as a model. A further comparison with Chang and Tal therefore follows, given that I take both to be, broadly, optimists about scientific investigation even given limits on the ability of a theory to reflect reality.

For both Chang and Tal, errors in one’s scientific machinery aren’t the be all and end all of whether your theory fits existing theory, such that if a theory makes mistakes it can still be fitted to important measurements by, for example, ad hoc processess. In essence, the problem of putting together a theory, and a wider systematisation of that theory, isn’t grounded in whether or not you’ve picked the right answer. Instead, to use my own term, theories are ticketed by

other features that they possess, even, as in Chang's example of phlogiston, they lack the sort of external justification that comes with being right. This goes beyond fallibilism in Chang's case, and becomes an account of theorising whereby the credulity of the scientist to a wrong theory is licenced by its relationship to existing data, and to potential fruitfulness (even if abandoned). In Tal's case, scientists should not be impugned for attempting to fit data together by responding to unexpected developments in an ad hoc manner, because it is not a reasonable demand to make on a scientist that they should not be subject to unforeseen engineering problems which aren't always recognisable when one takes a birds-eye view of the theorisation itself and assesses it by its formal virtues.

Taking Tal and Chang's assessments of scientific practice seriously, if we provisionally extend their insights from chemistry and physics to economics, the upshot for c-theories is that, as with the concerns about the meaning of terms, there is a space open for c-theories where they are licenced by their attempts to fit data, and to do so on the basis that their terms at least partially reflect reality, and can be understood within a community of experts. A c-theory can be "surprising" or it can be deliberately unsurprising, and still the warrant for believing in a c-theory rests outside the perfection of its fit to what has gone before, so long as it is earnestly truth-aspiring.

2c. Sceptical worries

My sceptical worry is that the foregoing is an incomplete account of Growth… as a c-theory, and I believe that, while Tal and Chang's contributions to pragmatic optimism are accurate when it comes to notes (1-3), even up to (4), the conditions that I have set out in section 2 do not licence the optimistic belief that such a theory can be more than an interpretation of a dataset, which interpretation happens to have causal content. Instead, as in the condition noted in (5), a c-theory having these features can easily fail, in spite of satisfying the conditions I laid out. As such, I argue that one should be sceptical of such c-theories, and should not credit, or ticket, them just because they possess these features.

I have said that there are good reasons to regard c-theories as, essentially, theory-laden, but in a positive way. It would not be possible to produce Growth… as if it were a mere collation of data, or the observation of a mere regularity, and even better, because Growth… made sense of a dataset, it's attempt to fit existing causal theory would be a positive whether or not it confirmed or disconfirmed that theory.

My concern is that even in meeting the foregoing conditions, Growth… was still a successfully bad paper.

Growth… itself turned out to have been a badly produced paper. Three years after its publication, first as an NBER working paper, and later in the American Economic Review. A PhD student at Amherst was given the routine task in 2013 of replicating a paper and chose Growth… on the understanding that such an important paper would not be hard to replicate

from R-R's own spreadsheet. However, he was unable to replicate R-R's results from their own data. Interest in the paper has since been focused on that mismanagement, but my worry is that the background for focusing on that mismanagement is a spurious credulity motivated by the three conditions I layed out for a c-theory's being more than a dataset in the previous section.

Much has been said about whether R-R should have more ably handled their data, but almost nothing has been said about whether, for example, "tickets" genuinely ticket such credulity towards the theoriness of a theory in economics.

As I understand them, both Chang and Tal largely leave open the question of how a theory can go wrong. As I have said, they advance a now popular and appealing view of scientific progress whose optimism is grounded in an idea of 'muddling through', so to speak. The sceptical worry is therefore that this is incomplete, and a better understanding of scientific theory would include an account of why a c-theory could fail totally. I believe I have shown that Growth… meets their standards for 'good' science in this section, and so in the following section I aim to show that they leave something important out.

3a. Further conditions: the ticket that exploded.

In this section I want to argue that Adrian Currie's "fragile-systems" approach to historical science better satisfies the sceptical worry I have laid out, and better explains how a c-theory like Growth… can go so wong, in spite of meeting the conditions I laid out in the previous section, with notes (1-4) explaining the basics, and note (5) motivating the sceptical worry. The reader will note as well the explication of those three conditions I set up as first, making a c-theory more than a mere collation of data and second, relating back to Chang and Tal's optimistic account of scientific investigation. (Currie 2018).

Currie's approach, which he also characterises as "optimistic", differs from Chang and Tal's optimism about e.g. ad hoc procedures for fixing unexpected problems in that his optimism is grounded in affirming the importance of theory, rather than affirming the importance of extra-theoretical concerns. For Currie, a "historical" and therefore supposedly "parochial" science can still have predictive power, so long as it relates back to existing theoretical concerns such as those I have addressed in section 2. Currie explains this in terms of the relationship between physics and archaeology or geology, pointing out that, under very limited conditions, scientists are able to go further than merely describing how a "trace" - i.e. a piece of evidence - came to be discoverable by an archeologist, and are able to use experiment to figure out what, for example, a fossilised tooth tells us about the likely structure of the skeleton of the animal of which it is an artefact.

Without going into too much detail, the analogy is clear: a c-theory like Growth… is an account of historical "traces" which are theory-laden. In the same way that an archaeologist can draw conclusions about the skeletal structure of an extinct animal by drawing conclusions from the fit of what is known to physics, an economist who is trying to work out what level of public debt

could create a drop in GDP-growth would do something similar and, analysing from what terms are known to contemporary economists, as described in section 2, could draw conclusions from a historical dataset.

The idea that a c-theory is a “fragile system” has the advantage over the explanation given in section 2 because it allows for criteria which explain how it can go wrong, whereas in section 2 we only saw how a theory can go well. For a fragile system, it is explicit that it only predicts or explains under very limited circumstances. Whereas in section 2 we only saw how a c-theory could be better than a mere collation of data, introducing the concept of fragility allows us to explain how Growth… could have been wrong even if its data were good. A fragile systems theory only works if futher criteria are met.

Are the relevant examples causally comparable? I.e. are the reasons why such and such GDP-growth and such and such public debt arose at the same time in one place the same reasons why it arose in another place and at another time? This is the sort of criterion imposed on a c-theory by the notion of its being a fragile-system, which is not imposed by the conditions heretofore discussed. Contra Chang and Tal, the ad hoc reasonings on limited evidence, and in the face of engineering problems about how to analyse the data, some degree of cynicism is appropriate about how exactly the data were analysed. In the case of Growth… even if R-R had done their due diligence they would only have turned out to be right, not because they’d expertly treated of the data, but because they had recognised the limited conditions under which their conclusions could have been true. At least this would be the case if Currie’s “fragile systems” concept accurately captured c-theories as in Growth...

Tal and Chang’s assessment, which favours improvisation and recognises the fact that scientists cannot always theorise ideally, fails to capture this insight. Economists generally desire causal content, and c-theories do not fail to have that content. However, a c-theory like Growth… even if it is truth-aspiring (as suggest by note (4)), does not meet the condition laid down by the notion of its being a “fragile system” that it should recognise the sceptical worry of note (5) that it could be wrong if applied to circumstances which were not causally comparable. The “ticket” concept I noted earlier explodes[5] if we have reason to allow that economists can go disastrously wrong even though they might the conditions for producing a theory which has causal content.

3b. Or, alternatively, the conclusion of this paper: The concept of “distance”

A c-theory can meet all the conditions laid out heretofore without meeting this further condition that it should not just have causal content, but be causally comparable with comparable data if the conditions laid out in Section 2 are right. Even if the data which motivated Growth… had been analysed right, the absence of an attempt to explain why the data appeared to match up

[5] With due respect to William Burroughs

so correctly could have put it at what I would call a “distance” between the theorisations to which it is related by the aforementioned and the theorisations which it attempts.

All of the connections to existing theoy, the meaning of terms etc. that I have described rely on whether or not the theory itself eventually is sufficiently close to what has gone before for its predictions to be intelligible in that context. Introducing the concept of “fragility” means introducing the possibility that any such c-theory could be be set apart from its peers by the question not only whether it meets the conditions laid out above, but the degree to which it meets those conditions. A c-theory like Growth… exists at a “distance” from other theories in that it goes further than them, without introducing new causal content of its own, even as it relies on that causal content.

The fact, therefore that Growth… relied on a faulty analysis of the data, should not lead assessors to believe that it was just failures of data-management which led to its demise. Instead, Growth… should be seen as flawed because it implicitly relied on its ability to satisfy those conditions even while it existed at a distance from the implications of meeting those conditions, and was able to make implicit causal claims because of its status as a c-theory. R-R referred casually to their “surprising” results, relying on, for example, their expertise in understanding concepts like “GDP” or “public debt”. But these did not reliably ticket their conclusions by themselves. Growth… was “surprising” because the “systematic relationship” which it purported to discover was part of, but also contradicted expected claims about how this was caused, which should have been a check on the policy-makers who later made it famous. If historical science is largely about fragile, rather than robust, predictions, then economists should be live to the conceptual distance between the conditions they do their work on, and the conditions which would make that work true.

This or that c-theory might be an accurate reflection of the state of the economy, data-driven or otherwise. But reliance on the criteria of section 2 is insufficient to warrant belief in a c-theory, and c-theories demand circumspection.

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