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Constructing Parapsychology

a discourse analysis of the accounts
of experimental parapsychologists.

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This thesis is concerned with parapsychology as a field of experimental science. It is based on the discourse analysis of interviews with experimental parapsychologists, in which they provide accounts of their field, their own research practices and experimental outcomes. Drawing on literature from the fields of parapsychology and social studies of science, experimental parapsychologists are characterised as having an asymmetrical standing within science. Whilst they share with other experimental scientists (e.g. psychologists) many of their core assumptions and investigative methods, they differ significantly in how their phenomena, basic propositions and empirical expertise are actively disputed both outwith and within parapsychology. It is this asymmetrical standing, the disputed nature of the reality of their object and of the scientific justification of its existence that makes parapsychologists' accounts of their work particularly interesting to the exploration of discursive practices involved in the construction of what they do a doing science. Drawing both on literature relating to the "linguistic turn" in social studies of science, and on recent methodological developments in discourse analysis, this thesis puts forward that the analysis of parapsychologists' accounts provides a particularly rich insight into how scientific knowledge and practice are discursively accomplished. It thus focuses on how these parapsychologists produce meaningfully variable factual versions of what they do as 'doing science', and of their disputed object as a real phenomenon. The aims of the study were the following: a) to examine parapsychologists' own accounts of their field, research practices and experimental outcomes; b) to analyse how these accounts attend to normative versions of what 'counts' as science; and c) to analyse the discursive resources they use to achieve factual accounts of 'doing science'. The analysis of the data obtained from 20 interviews with experimental parapsychologists begins with the examination of how they constructed their field as a community, as a body of evidence, and as a field with a particular relationship to a standard view of science. The analysis was inspired by the thread of discourse analytic research which focuses on 'fact construction'. It shows how they orient to ideas of demarcation and constitute parapsychology as a field with characteristics that compromise the scientificity of their own knowledge and practice. It also shows how these parapsychologists attend to and manage the relationship between what they do and these compromising characteristics, by building them up as essential properties of the evidence for the phenomena (as essentially ambiguous), and even of *psi* itself (as essentially elusive). The construction of parapsychology as inherently problematic (i.e. a 'less than perfect' scientific field), allows these parapsychologists to constitute their research work as an almost heroic achievement. Regarding the participants' versions of their research practices, the analysis shows that they make these scientifically safe (e.g., by appealing to, and by presenting them as, in line with, ordinary versions of empirical research). The analysis further explores these parapsychologists' constructions of their practices as doing strict and extreme empiricism, with no assumptions, expectations, theoretical underpinnings or objectives. Their appeal to the primacy of facts, the doing of methodology, neutrality and the dispensability of theory and models, constitute versions of scientific inquiry that are hearably in line with a version of science as 'doing strict empiricism'. The analysis argues that the variety and extremity of these formulations constitute the extent to which the empirical quality of their research is oriented to by them as something that is not taken for granted (and thus needs to be accounted for). Paradoxically, this same extremity rhetorically breaches normative accounts of doing science, through the intense problematization of theory or expectations of any sort. The final focus of the analysis is the exploration of these parapsychologists' constructions of the outcomes of their own research, specifically their categories of *psi* and of anomaly. The analysis shows that, though both of these concern the central object and claim of parapsychology, the participants present radically different categories of each, which are functionally meaningful in relation to their versions of doing science. Overall, the thesis argues that these parapsychologists constitute a paradoxical discursive position in relation to normative accounts of doing science. On the one hand, they actively appeal to the primacy of evidence and empiricism. On the other hand, they construct a set of characteristics for their research object and evidence that compromise the rhetorical achievements of empiricism; also, the extremity of these accounts is such that this constructed empiricism is made into a remarkable rhetorically brittle account of scientific practice in parapsychology. Finally, the thesis discusses the implications of these arguments for parapsychology, namely, for the development of a reflexive and discursive thread of research within the field. It also examines the limitations of this approach and possible future research.

*For Pete
and for Bob.*

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'Now, what I want is, Facts.

Teach these boys and girls nothing but Facts. Facts alone are wanted in life. Plant nothing else, and root out everything else. You can only form the minds of reasoning animals upon Facts: nothing else will ever be of any service to them. (...)

Stick to Facts, sir!'

Charles Dickens, *Hard Times*.

Background and location of the thesis.

Introduction.

This thesis is concerned with the field of parapsychology as an experimental science. It is based on discourse analysis of experimental parapsychologists' own accounts of their field, research practices and experimental outcomes. It will examine how, in these accounts of what they do, their field and their object these parapsychologists construct the ostensible reality of parapsychology as a field of scientific research, of what they do as doing science, and of their research object as a real (although controversial and disputed) object of research. In using discourse analysis, the thesis will explore their accounts as interpretative accomplishments, which perform meaningful actions. This first chapter will provide a background to the concerns of the thesis in terms of relevant literature on conceptual and methodological issues (section 1.1), and then provide a both an outline of the empirical location of this thesis and its aim in contributing towards the field of parapsychology, and finally, a summary of the following chapters (section 1.2).

1.1 The conceptual and methodological background to the thesis.

The aim of this section is to provide a conceptual and methodological background for this thesis, and thus the context of, and justification for, its focus and method. In sub-section 1.1.1, it will focus on the work that has been developed in the area of the Sociology of Scientific Knowledge (henceforth SSK) on: a) the proposition that science and its accomplishments are products of social negotiation; and b) the specific area of the study of controversial scientific claims, and the processes through which such controversies are built up, maintained and brought to a possible close. In sub-section 1.1.2, it will focus on the analysis of parapsychology as a special case of scientific controversy, in Collins' (1983a:278) words, as a "fringe science". Finally, in sub-section 1.1.3, it will consider issues of

epistemology and methodology, focusing on the analysis of scientists' discourse as a way of understanding how science can be seen as an achievement of social negotiation.

1.1.1 Controversies and the accomplishment of scientific knowledge and practice.

The sociology of scientific knowledge introduced in the early 70's a new way of thinking about science. According to Pickering (1992:1) it differentiated itself from contemporary positions in the philosophy and sociology of science in two ways:

“First, as its name proclaimed, SSK insisted that science was interestingly and constitutively social, all the way to its technical core: scientific knowledge itself had to be understood as a social product. Second, SSK was determinedly empirical and naturalistic. Just how scientific knowledge was social was to be explored through studies of real science, past and present”. (Pickering, 1992:1)

Within the analysis of science as a social enterprise, some authors (namely Harry Collins and the scholars that concentrated at the University of Bath), concentrated on a microsocial approach and on exploring particularly instances of scientific controversy, aimed at displaying the “production of consensual knowledge as the outcome of ‘negotiations’ between social actors” (Pickering, 1992:1). The following discussion of these studies will attempt to provide a broad context for this thesis – the exploration of controversy in accounts of scientific action and knowledge. In seeking to probe into and represent the natural world, scientists' accounts scientific activity, including its products and practices, appeal commonly to ideas of stability, consensus or uniformity. This seems to be the “most readily available” (Pinch and Collins, 1984:521) view of science. It presents it as an “intellectual enterprise concerned with providing an accurate account of objects, processes and relationships occurring in the world of natural phenomena” (Mulkay, 1979:20), a world considered to be real and objective and not subject to “the preferences or intentions of its observers” (Mulkay, 1979:19). It is precisely this view that requires representations of the natural world to be unitary and stable, if they are to be considered true representations. However, disagreement and dissent between actors within or between fields of scientific research are also frequent. The analysis of such controversies can produce stories of scientific thought and action that are quite compatible with such a view of science, if the analyst focuses on how these are eventually closed by appeal to reason, nature or fact. However, an alternative analysis of these instances of controversy can also yield versions of science that are somewhat less compatible, if the analyst focuses on how these accounts of controversy often unpack and question the value of reason, nature or fact as effective and clear arbiters for their resolution. The closure of scientific controversies is, by no means, the inevitable endpoint of a dispute, and can be described as a negotiation over the value of outcomes and practices, rather than the victory of a right

version of nature over an erroneous one. The versions of science that these analyses of controversy produce point up the way in which scientists themselves make up what eventually “comes to count” (Collins, 1985:267) as the valid and true representation of nature. Contrary to the basic assumption of so called problem of demarcation, in which analysts seek to “identify unique and essential characteristics of science that distinguish it from other kinds of intellectual activities” (Gieryn, 1983:781), controversies have been used to propose an alternative approach to the constitution of “science”. As Pinch and Collins (1984) argue, from this perspective, controversies are instances where “nature’s voice appears indistinct” (p. 522), and where the constitutive actions of scientists become more apparent. Hence, the study of controversies was put forward by Collins and colleagues¹ is a valuable way of exploring the construction of scientific knowledge (Collins, 1983a:273).

Focusing on controversial aspects of science or on non consensual practices, these authors put forward that these are instances where it is not clear what should count as a successful outcome, a competent experiment or evidence for a model or claim (Collins, 1981b:34). Therefore, these are instances where there is no “one” answer to a research question. They provide, therefore, an ideal context for the analysis of the terms of negotiation (i.e. the norms of practice, properties of the phenomenon and skills of the scientist that are implicitly or explicitly appealed to in the construction of what counts as a valid process or outcome) that turn a possible answer into the answer (Collins, 1981a:4; 1983a:274; Pickering, 1981:64; Potter, 1996a:26). As Collins puts it (1985:6), the analysis of the made up character of scientific knowledge finds in the exploration of controversies a useful resource, in that controversies divert the analyst’s attention from seeing instances of scientific knowledge as finished products, to instead focus on the ways in which these are negotiated – with parts being built up and destroyed, and with appeals to different kinds of rules of how and why something is made into a scientific fact. These authors propose that these negotiations and their terms are kept out of traditional and formal accounts of scientific action and belief, but rather are akin to those found in any context of social interaction.

The analysis of scientific controversies has focused on a wide range of debates in a number of fields of science, and although the present account does not aim to provide a complete listing or overview of these studies, it will however focus on specific references selected by Collins’ (1983a) as important studies of scientific controversy. According to this author, studies concerning the analysis of scientific controversies can be split into two types regarding their ability to make visible different processes

¹ This initial section will focus mainly on the work of Collins and other authors who have been included within the analytical position within SSK referred to as “empirical relativism”. This position was mainly developed by Collins, who, as was said above, pioneered in the University of Bath a microsocal analysis of scientific processes and products (referred to as the Bath variant of SSK [e.g. Woolgar,1992:328]). The selection of this particular variant of SSK is intended here to provide a thematic context for this thesis, but not a methodological one.

“involved in the production of scientific knowledge” (p. 273)². The first type of studies focus on how scientific controversies, although potentially facing an endless regress of arguments over the validity of opposing arguments (Pickering, 1981:64), can nevertheless be effectively closed down. These studies have examined the way in which scientists and their social context of action put restraints and controls on scientific controversies, practically resolving them, thus, allowing the maintenance of accounts of scientific knowledge as a stable, uniform and consensual core of contents and methods. As an example, Collins (1983a) selects the work of Pickering (1981) on a controversy in experimental physics which “followed the contentious observation of a novel entity – a magnetic monopole” (Pickering, 1981:63). The crucial point from Pickering’s work is that, in relation to this specific controversy, there were readily available social and cultural constraints, i.e. “prior agreements concerning the possible [and impossible] contents of the world” (Pickering, 1981:66), which, in being appealed to via agreed (thus, correct) instrumental practice and the interpretation of phenomena, allowed the controversy to be closed down (Collins, 1981a:5). In this sense, Pickering (1981) viewed this debate as one in which the participants decided to locate the potential existence of the monopole within a fixed set of “socially-conceivable” concepts which were appealed to as “essentially static” (p. 89), and their maintenance provided the instructions with which both the interpretation of the phenomenon and, crucially, the experimental practices that produced it were assessed. The wider upshot from Pickering’s analysis is that these instructions may be looked at as, ultimately, becoming “‘tuned’ with a framework of conceptualisations of the natural world” (p. 88). Moreover, this analysis of the agreed socially conceivable aspects of the world as resources that constrict the terms of debate, makes available the possibility that such “tuning” implies a “flexibility of the empirical base of science” (p. 88), that goes beyond the idea of observation being theory-laden, and suggests a “radical incommensurability between the “natural worlds” of societies subscribing to different conceptual frameworks” (p. 89). The analysis of the debate thus points up questions about the construction of scientific knowledge that have less to do with controversy than with how these socially-acceptable interpretative resources come into being, i.e. how these agreements can be constructed and used.

The second type of studies identified by Collins focused on the analysis of scientific methods as a way of establishing what counts, and what does not, as a “proper addition to scientific knowledge” (Collins 1983a:273). In particular, they have challenged the idea that “formal ‘algorithms’ of science” (Collins,

² This selection and systematisation forms part of Collins’ exposition of three stages of a programme of research in the sociology of scientific knowledge, which the author called “empirical relativism” (Collins, 1981a:4, Collins, 1983a, Collins 1983b), mentioned in the footnote 1. Within these three stages, the first two will be focused upon in the next few paragraphs, but not the third, as it does not relate directly with the work developed in this thesis. According to Collins’ outline, this last stage concerns an analysis of the construction of scientific knowledge in relation to a social and political structure which transcends the particular realm of the claim or process under analysis (Collins, 1983b:96). For instance, this third subset of studies explores issues such as the influence of political, social and economical interests in the construction of a particular view of the natural world, or the way that scientific knowledge is used in debates which occur in a wider social context (Collins, 1983b:96).

1983a:273) – such as standards of experimental methodology, including instructions for controls or replication – are unchanging and self-sufficient criteria for the closing down of controversies. Examples of such studies are the controversy over the detection of gravitational radiation (Collins, 1975, 1981b, 1985), and what the author has referred to as an area of “marginal science” or “fringe science” (respectively Collins 1981a:4; 1983a:278), the examination of the controversy of parapsychological claims.

Collins’ work on the construction of the controversy surrounding the detection of gravitational waves spanned more than two decades³. Amongst other issues, Collins used the debate over the experimental detection of gravitational waves to explore the negotiated character of scientific validation processes (e.g. replication) commonly presented in formal scientific literature as independent and absolute arbiters of the empirical support for and, ultimately, the reality of a phenomenon. The controversy over the detection of gravitational waves, Collins described, opposed those that claimed having successfully detected (and thus obtained empirical support for the existence of) gravitational waves, and those that considered the results of such experimental processes to be artifactual (and thus not valid empirical support for existence of such waves). Although gravitational waves constituted a novel empirical finding to a community of physicists, it was nevertheless compatible with existing theory (Collins, 1981b:35; Potter, 1996:27). Collins proposed in his exploration of formal and informal accounts of this debate that what came to count as a “successful outcome”, “competent experiment”, and, in particular, a “replication of evidence”, were constructed through processes of “social negotiation” (Collins 1981b:34). In doing so, he pointed out that during this dispute there were no consistent “purely cognitive reasons that would ‘force’ scientists” (Collins, 1981b:34) to agree about what constitutes a competent experiment, a successful outcome, a replication and, ultimately, the nature of gravitational radiation itself. Rather, Collins put forward accounts of processes through which these taken-for-granted terms of assessment of scientific competence, validity and reality are disputed and negotiated and agreed upon by the relevant actors in the controversy. The importance of this extended series of studies on the detection of gravitation waves, and of the area of investigation as a whole, is not so much that it refutes the claim that scientific truth is determined by nature and experiment, but rather that it demonstrates the role of scientists in determining scientific truth through criteria and processes of which they ultimately determine and regulate.

A further example of studies of the interpretative flexibility of scientific methods and outcomes provides the link to the analysis of parapsychologists’ accounts of their research into ostensibly paranormal claims, and of parapsychology as a field of science. Collins (1983a) argues that areas of “fringe science” (p. 278) can be looked at as extreme cases of controversy in which, although authors

³ This summary will not attempt to provide an exhaustive overview of Collins’ research, but it will use its reference and brief description as an example of research which focused on the interpretative flexibility regarding scientific methods and outcomes.

have found features of remarkable similarity to “those typical of controversy in the hardest areas of science” (Collins 1983a:278), the processes of negotiation through which the credibility of the claims to knowledge is achieved are more explicit. As Collins (1983a) puts it, “the ferocity of argument in fringe sciences ensures that nearly every negotiating tactic is available for inspection” (p. 278). The upshot is that the claims, practices and overall positioning of such “marginal” or “fringe” sciences exaggerates the salience of the controversy, pointing up the types of argumentation, rhetorical practices or more general processes of social negotiation of the value of outcomes and experimental actions, and thus allow for a richer analysis of “how pieces of knowledge gain acceptance within science” (Collins, 1982:300). The next section of this chapter therefore focuses on the particular case of parapsychology.

1.1.2 Fringe science and experimental parapsychology.

Experimental parapsychology has been an area of particular interest within the sociology of scientific knowledge⁴. In this section, I will focus on three fundamental pieces of research work, one by Pinch (1979) and two by Collins and Pinch (1979 and 1982), all of which adopt an analytical position named empirical relativism. These authors explored how claims about the ostensible reality of paranormal phenomena, and the practices of parapsychologists, are made into, or not made into, part of the realm of science. As will be explored in greater depth in the next chapter, the objects of parapsychological research are not only formally constructed as novel objects, but are also commonly constituted as phenomena whose reality would potentially challenge much of what has come to count as scientific knowledge (Collins 1985:4). For that reason, parapsychology and its claims have come to be regarded as marginal (Pinch, 1979:343), and the practices with which parapsychologists explore these objects, and constitute them into experimental outcomes, became controversial in themselves. This marginality is still sustained despite their formal accounts drawing on traditional “touchstones” of scientificity – such as hypothesis testing, controlled experimentation or the replication of outcomes – which reflect accounts of practices in contexts of accepted scientific knowledge. In spite of this formal symmetry, what remains apparent is that parapsychological claims are often located, in the literature both within and outwith parapsychology, as potentially incompatible with the contents and practices of science (Pinch 1979:342). Although both the demarcation and incompatibility arguments in relation to parapsychology have been robustly challenged within its literature (Pinch, 1979:332), the scientific acceptability of parapsychology and parapsychological claims continues to be disputed (provoking heated debate particularly in academic contexts). Parapsychology remains, therefore, an exemplar of

⁴ Although there have been other types of study of parapsychology from perspective such as history of science or anthropology/cultural studies – e.g. Mauskopf and McVaugh, (1980), Hess (1993) – this section will delimit its review of empirical studies of parapsychology to those developed within SSK’s focus on controversial areas of science.

scientific controversy, and one in which the negotiating practices of participants engaged in the construction of acceptable scientific knowledge might fruitfully be studied (Pinch, 1979:330).

The body of literature regarding the analysis of parapsychology from this SSK perspective will be described next, focusing on the three main studies mentioned above. Overall, these studies examined how participants, in debates surrounding specific phenomena (e.g. metal bending), “establish among themselves, through their own actions and culturally-based interpretations, the existence/non-existence of parapsychological phenomena” (Mulkay et al., 1983:182). In other words, these authors have used the examination of parapsychology as a way of analysing demarcation practices that allows them a) to challenge the traditional view of demarcation as the result of the successful deployment of definitive and socially invariant criteria (e.g. experimental demonstration, replication of results), which can distinguish a scientific claim from a non-scientific one; and b) to demonstrate the social contingency of these demarcation practices, i.e. their description as social processes of judgement, manufacturing and negotiation, similar to other processes of interaction.

The first study to be examined here is that of Collins and Pinch (1979), which examined the processes involved in the construction of parapsychological claims, and parapsychology’s standing as a scientific field. The analysis was based upon the authors’ selection of two contexts of scientific action – the constitutive and the contingent forums (Collins and Pinch 1979:240). According to the authors, these forums include distinguishable elements that are made explicit in argumentation by proponents and critics of the field “over the acceptance of ‘scientific’ parapsychology” (Collins and Pinch, 1979:262), and allowed them to explore the relationship between the content of arguments and the context in which these were presented. Collins and Pinch (1979) describe the “constitutive forum” as including “theorising and experiment and corresponding publication and criticism” (p. 239-40) in settings that have been constituted as formal places for scientific action, such as peer-reviewed journals or conferences. The constitutive forum therefore refers to elements and contexts that traditionally have been appealed to as universal scientific imperatives. The “contingent forum”, on the other hand, is described as including “everything that scientists do in connection to their work, but which is not found” (Collins and Pinch, 1979:240) in the formal contexts of science, such as issues of professional or academic allegiances, or arguments about personal interests and characteristics, which are found in contexts of scientific action like informal discussion or popular journals (p. 240). This sort of typology is identical to the distinction that traditional accounts of scientific action and belief propose. There are, on the one hand, elements in science that make it into what it is supposed to be (e.g. proper experimentation and theorisation) and, therefore, are constitutive of science. On the other hand, there are other elements that provide an “add on” contextual influence in science (e.g. group allegiances or personal characteristics), which are not regarded as proper scientific knowledge and are, therefore, contingent to science. The authors seem to use this traditional distinction to undermine the assumptions on which it is itself based. As Mulkay et al. (1983) argue, “[Collins and Pinch] do this

by trying to show that actions undertaken in the contingent forum actually have a definitive impact on those scientific assessments whereby the existence/non-existence of parapsychological phenomena is constituted” (p. 184). Collins and Pinch (1979) try to demonstrate that “contingent actions do constitute scientific knowledge, and the constitutive actions are as much a social construct as anything else” (p. 241). In doing so, they make the point that the asymmetrical epistemological standing of these two types of arguments is insufficient to either warrant or downgrade the scientificity of parapsychological claims. They argue that all scientific actions and belief are, ultimately, better understood as social achievements, and thus “contingent”. Parapsychology’s standing as an area of scientific controversy is therefore an achievement of the negotiations between its proponents and critics in both forums, and their respective claims that “something” or “nothing” unscientific is happening (Collins and Pinch, 1979:262).

In the second study examining parapsychological claims, Pinch (1979) directed the question of demarcation away from the parapsychological claims themselves, and towards a commonly appealed to alternative “normal” hypothesis for paranormal phenomena – the “fraud hypothesis”. Pinch’s (1979) analysis sets up a symmetrical standing for two possible explanations for apparently successful outcomes in parapsychology experiments. The first would consider results to be the verification of the experimental reality of these ostensibly paranormal phenomena, while the second would regard them as the result of some sort of “normal” process, such as fraudulent duplication by the experimenters or the participants. These two explanations are commonly placed in a hierarchy of scientificity, in which the fraud hypothesis comes out as a better scientific explanation for successful results than the paranormal hypothesis (Pinch, 1979:330). The analysis of parapsychological outcomes by critics, both outside and within the field, commonly obtains this demarcation of *psi*⁵ from science through the use of invariants guarantors of scientific status of an explanation, and thus, of a phenomenon – i.e. repeatability, falsifiability and theoretical contextualisation. Returning to the same issues that concerned Collins’ work on the controversy over the detection of gravitational radiation, Pinch (1979) points these invariants back at the fraud hypothesis, and rhetorically tries out the same demarcation exercise that is usually applied to the *psi* hypothesis. He thus asks the question: “what is the scientific status of the fraud hypothesis?” (Pinch, 1979:334). In doing so, he illustrates the symmetry between the fraud hypothesis and the *psi* hypothesis in terms of their apparent scientificity, or, as Pinch’s concludes, their “lack” of it. Pinch’s paper is a clear illustration of how these frequently appealed to criteria for demarcation of what does or does not constitute science, “cannot provide the necessary independent basis required of a universal standard of rationality” (Pinch, 1979:342). Instead, the conventional unsymmetrical scientific standing of these hypotheses, Pinch argues, is necessarily bound up with the bits of knowledge that, in a particular scientific culture and time, seem plausible. Demarcation seems, therefore, to be an issue of contextual negotiation, where the cultural marginality

⁵ The term *psi* (literally, the Greek letter ψ) is used in experimental parapsychology as either a noun or an adjective, to identify both paranormal processes and paranormal causation.

of a claim has more to do with its perceived compatibility with existing knowledge than with “replication” or “experimental control”. Such solid and abstract guardians of scientific values (Pinch, 1979:343) are rather powerful rhetorical devices that “make visible” (Pinch, 1979:342) the implausibility of the *psi* claim or parapsychology as a “clear-cut case of non-science” (Pinch, 1979:343). As with Pickering’s analysis above, Pinch concludes that parapsychology’s constructed standing as a marginal field of science is related to the potential clash of its claims with “present knowledge” (Pinch, 1979:343). Thus, demarcation, Pinch concludes, needs to be understood as a highly contextualised negotiation, situated in a particular scientific (cultural) setting.

The third and final analysis of parapsychology from the perspective of empirical relativism to be considered here is Collins and Pinch’s (1982) investigation of parapsychological claims about “psychokinesis”⁶. The particular phenomenon that Collins and Pinch (1982) investigated was described as “paranormal metal bending”, this was especially topical at the time of this investigation because of Uri Geller’s purported spoon-bending feats, which came to be studied experimentally in the 1970’s (Collins and Pinch, 1982:4). This report of investigative work is singularly interesting as the authors note that it is partially based on their own participation as experimenters in parapsychological research into metal bending. The authors described their work as being concerned with the relationship between “cultures in modern science” (Collins and Pinch, 1982:1). They focused on the actions of physicists as they entered the “conceptual repertoire” (p. 1) of the community of parapsychologists, as they developed research into the area of metal bending, “an area which is not a generally accepted problem situation in physics” (Collins and Pinch, 1982:1). This analysis thus relied on the assumption that the physicists’ entry into the conceptual world of parapsychology could involve a logical clash between two paradigms – the physicists’ and the parapsychologists’ – in a similar way to “Kuhn’s elaboration of the idea of paradigm conflict” (Collins and Pinch, 1982:4). Having established a scenario of conflict, Collins and Pinch examined how, on the one hand, the incoming physicists and a pre-existing community of parapsychologists interacted to propose or oppose the reality of the phenomena. On the other hand, they analysed how the conflict or controversy was worked up and worked through in terms which, the authors argue, would again render inadequate “any conception of major scientific disagreement in purely cognitive terms” (Collins and Pinch, 1982:5). In particular, the authors’ analysis of scientific activity of these physicists (i.e. experimentation), based on their first hand participation in experimental work, provides a robust account of how and why experiments in themselves were unable “to legislate for the existence of any natural phenomena” (Collins and Pinch, 1982:125) and, therefore unable to close down the controversy over the reality of paranormal metal bending. They describe, in exhaustive (and occasionally self-reflexive) detail, how the value of “rigorous experimentation”, frequently appealed

⁶ Psychokinesis (the initials PK are commonly used), refers to the direct influence of the mind on a physical (living or not) system that cannot be entirely accounted for by the mediation of any known physical process (Thalbourne, 2003:98) – e.g. the dislocation or bending of objects, the alteration of the state of physical functions of an organism.

to in parapsychology as the basis for a symmetry claim between parapsychology and other disciplines (Morris, 1987), is not an invariant measure of validity, but rather is itself open to interpretative variability.

By focusing on the issue of demarcation, these studies attempted to show how “contingent actions do constitute scientific knowledge, and that these constitutive actions are as much a social construct as anything else” (Collins and Pinch, 1979:241). The authors use parapsychology as an exemplar of extreme controversy. Within this community they identify two sides – its proponents and its critics. The claims that parapsychologists put forward are described by Collins (1985) as claims that have been positioned outwith the “centre of orthodoxy” (p. 4) of science, and thus carry a potential challenging value for an existing repertoire of scientific knowledge. Among the main analytical points, it is possible to identify the authors’ emphasis on breaking down a version of scientific action and belief that is strictly determined by factors which are rule like, and which should stem from “nature’s demands”. In this sense, they put forward an analysis of science which emphasises its processes as inherently social, highly flexible and variable ways of constituting claims into science. What these authors called negotiation, seemed to be characterised as an ongoing flux of contestation and modification (Knorr-Cetina and Mulkay, 1983:11) from which experimental facts or the value of replication cannot be excluded. It is through these processes that “the meaning of scientific observations as well as of the theoretical interpretations tends to get selectively constructed and reconstructed in scientific practice” (Knorr-Cetina and Mulkay, 1983:11). The next section will explore how the construction of these social processes can be viewed from a perspective which provides a possible specification of just how scientists produce inherently social and variable versions of science. This perspective is the so called “turn to language” in social studies of science and, in the words of Shapin (1979, cited in Woolgar, 1981), provides a possible specification of “how, precisely, to treat scientific culture as a social product” (p. 366).

1.1.3 The turn to language – analysing scientists’ discourse.

This chapter has so far focused on the general study of controversial scientific claims (and the processes which build it up, maintain and bring it to a possible close), and the specific case of parapsychology as a “fringe science”. In summary, the studies referred to above argue: a) that the content and practice of science can be seen as social achievements in themselves; b) that what comes to count as science is the product of processes of social negotiation and interaction that are particularly clear in the study of controversial areas of scientific practice, such as parapsychology; and c) that these versions of what (and what does not) count as valid knowledge are inherently variable and

purposeful. The chapter now considers the methodological orientation of the thesis, and will focus on the analysis of scientists' discourse.

Knorr-Cetina and Mulkay (1983:11) suggest that the linguistic turn in the social study of science, provides a critical approach to the analysis of science as a social phenomenon. Such an approach takes the production of discourse as a crucial part of making science into what it is. The introduction of the analysis of scientists' discourse, by authors such as Mulkay, Gilbert, Potter, Woolgar and Ashmore, proposed that the study of versions of science as social and purposeful accomplishments should give methodological priority to the study of discourse, i.e. that how scientists speak or write about science should become a topic of analysis itself. This proposition was quite different from the view of discourse as a means to access scientific actions and thoughts, that used scientists' accounts as a resource which could be "assembled in various ways to tell the analysts' stories about 'the way science is'" (Mulkay, Potter and Yearley, 1983, p. 196). The analysis of scientists' discourse, according to these authors, requires an understanding of "how actors constitute the character of their actions primarily through the use of language" (Mulkay, Potter and Yearley, 1983:195). Accounting practices are thus looked at as scientific practices in themselves, rather than as indicators of experimental or cognitive practices which they purportedly represent. Thus, these authors argue, the way scientists speak or write about their research practices, outcomes and theories is crucial in securing a wider agreement that these products are robust, factual and authoritative or, alternatively, brittle, invalid and unreliable – i.e. in constituting their scientific character or not. The analysis of scientists' discourse seems to have been put forward as a challenge to the methodological assumptions and implications of existing social studies of science based on qualitative data⁷, such as the studies referred to above (Pickering, 1992:2). At the centre of this approach were three key problematic aspects of existing qualitative methodologies: a) their use of scientists' accounts of their thoughts and actions as literal and definitive versions of what really happens, or of the way things actually are; b) their interpretation of the variability in scientists' accounts as a problem that needs to be eliminated for the purpose of presenting an unitary version of what science is like; and c) their failure to take notice of the way this variability in accounts of scientific thought and action i) points up the impossibility of a one-to-one correspondence to events or thoughts beyond it, ii) are in themselves purposeful interpretations that are tailored to the context and the function which they were made for, and thus iii) constitute social practices whose analysis is fundamental to the examination of the social contingency of science. Each of these criticisms will now be explored in turn, with reference to one of the papers already discussed above (Collins and Pinch, 1979), and criticisms made of it by Mulkay, Potter and Yearley (1983).

⁷ Data obtained in interviews, participation in or observation of experimental settings.

In relation to the first of the key criticisms, Mulkay (1981:164) drew attention to the difficulty caused by the use of these accounts as definitive versions “*of the way things actually happen or actually happened*” (p. 164, italics in the original). According to the author, in approaching data in this way, the analyst’s job is to interpret the evidence in order to find the best version of these actions and beliefs, and to use these accounts as resources to analyse “the way that science is” (Mulkay et al., 1983:196). In doing so, Mulkay (1981:168) points out, analysts are accepting the participants’ statements at face value. In translating their accounts into conclusions about their real actions and thoughts, the analysts are “allow[ing] the participants to do the analysis” (p. 168) for them, as “what enough scientists say about [scientific] action comes to be taken as a literal description of action” (p. 169). Mulkay (1981:171), and later Mulkay and Gilbert (1982a:314), described the dependency of such approaches upon participants’ own versions of scientific action or belief as a form of “interpretative vassalage” to the “actors supposedly under investigation” (Mulkay and Gilbert, 1982a:314). They argued instead that the standing of the accounts that scientists offer (of their and others’ activities and thoughts) in interviews or interactions should be taken as interpretative accomplishments in themselves, rather than as neutral descriptions of what really goes on.

An example of this methodological objection is Mulkay et al.’s (1983) critique of Collins and Pinch’s (1979) analysis of the construction of “scientific” and “unscientific” parapsychology, in which the latter authors identified two discernable communities involved in the debate, “parapsychologists” and “orthodox scientists”. Mulkay et al. asked “from where did Collins and Pinch obtain their categories of ‘parapsychologist’ and ‘orthodox scientist’?” (Mulkay et al., 1983:185). The point that Mulkay and colleagues make is that both categories are taken directly from participants’ descriptions of the field. While such categories might be seen as interpretative achievements or rhetorical devices, to treat them as accurate descriptions of people in the field is unreflexive and lacks analysis. Such an approach ignores the variability and purposefulness of the use of categories (Mulkay et al. 1983:187). For example, Mulkay et al. take issue with the way that the category of “orthodox scientist” is adopted literally from parapsychologists’ descriptions of opponents of the field and, in doing so, glosses over the pejorative category bound implications of term “orthodox” (i.e. the potential unwillingness to respond in a “open minded manner” to unusual, but nonetheless valid knowledge claims) (Mulkay et al, 1983:186). In this sense, Collins and Pinch’s analysis incorporated uncritically some of the terminology and interpretative practices of the parapsychologists in their study. In doing so, they departed from a fundamental analytical commitment of their approach: in overlooking the categorisations in themselves, they overlook a layer of social negotiation that constitutes, in this case, communities of scientists.

The second key criticism rests upon the observation of variability in scientists’ accounts of their thoughts and actions. In using accounts as ways of getting at actions and thoughts in themselves, there is an assumption of correspondence or representation, which then seems to imply that “for analytical

purposes, there is one best version of particular actions and beliefs” (Mulkay, 1981:164-165). However, it is apparent that scientists can and do produce diverse and even discrepant versions of scientific action and thought. The variability in accounts of science is found not only among different scientists, but also in the same scientist’s accounts in different contexts of text and talk (such as interviews, conference papers, journal papers, or coffee-break discussions), or even in different moments of the same interview (Mulkay and Gilbert, 1982a:312). This poses an obvious problem: if accounts are representations of an independent reality, but there are various versions of this reality, which version is the real one, and, how does one go about choosing it? Some analysts’ approach to this problem is producing a version of “best fit”, i.e. a composite version of events which coordinates and merges the different accounts produced by participants (Mulkay, 1981:167). The literal use of these accounts as descriptors of action and thought thus demands that inconsistency and discrepancy between accounts are resolved in some way, for interpretative purposes. Contrary to this trend, Mulkay and colleagues (Mulkay, 1981:168; Mulkay and Gilbert, 1982a:312; Mulkay et al. 1983:196; Potter and Mulkay, 1985:250) argue that the inevitable variability of accounts should be analysed as a feature of the construction of scientific practices, rather being resolved or eliminated.

Going back to Mulkay et al.’s critical appraisal of Collins and Pinch’s article, it is interesting to notice that, related to the two categories of “parapsychologist” and “orthodox scientist” mentioned above, Mulkay et al. (1983) pick up on an admission of a shortcoming by the authors at the beginning of their 1979 article – the fact that the assignment of participants to each category presents “an embarrassing number” (Collins and Pinch, 1979:238) of exceptions. Collins and Pinch thus point up, in this confessional tone, the problem created by the variability of the participants’ constructions of membership to one or the other of these categories. This is a fundamental problem for their analysis. The authors rely heavily on the possibility of a clear distinction between these two groups for their analysis of “tactics” used by each to either gain or deny scientific recognition (Mulkay et al., 1983:185). But when there are conflicting accounts about a given participant x, which one corresponds to the real participant x? Variations or discrepancies in these categorizations seem to force the analysts to choose the best version of the real participant, discounting other versions. However, variability in the way participants construct membership to various categories has to necessarily be an analytically significant feature. At the very least, these discrepancies point up the potential for the flexible use of membership. More importantly, perhaps, they suggest that this flexibility is constitutive of these groups, i.e. that these groups are discursive accomplishments and that participants use them in socially purposeful ways.

This brings us to the third and final critical point made by Mulkay and colleagues, which questions the way that these analyses seem to build up conclusions about the social generation of scientists’ statements about the physical world, yet, overlook the inherently interactional (and thus social) nature of these accounts (Mulkay et al., 1983:195). The construction of a particular account, Mulkay and

colleagues argue, is in itself an examinable social event, which should be the starting point for the exploration of scientific action and knowledge as a social phenomenon. If the analyst moves away from the idea of a one-to-one correspondence of scientists' accounts with an external unitary reality, these can be examined as ways of accomplishing the validity of an outcome, the correctness of a theory, the "received" nature of a conclusion or the undermining of an opposing idea as merely fabricated. In this sense, accounts are actions which necessarily have an interactional purpose and are, therefore, how science is made up. An example is available again in Mulkay and colleagues' critical assessment of Collins and Pinch's analysis. In addition to variability in the construction of categories of participants mentioned above, the authors considered how these categories might have been deployed by the participants for a purpose. They focus specifically on how the category of "orthodox scientist" comes about. In the construction of such a category, participants necessarily engage in a socially significant process, as the identification of a class of scientists as "orthodox", Mulkay and colleagues argue, is an inherently evaluative categorisation. For example, it is successful in providing a way of undermining and discounting the views of someone who is included in it as simply the product of narrow-mindedness. Mulkay et al. argue that the term adopted into Collins and Pinch's analysis from parapsychologists' own constructions is socially effective in providing a way of "making sense of their reception by the great mass of unconvinced or actively resistant scientists" (Mulkay et al., 1983:186). Therefore, the identification of a scientist as belonging to this category is an analysable social process, rather than a description.

Mulkay and colleagues therefore present a version of the analysis of the social construction of scientific knowledge which "focuses on describing how scientists' accounts are organized in ways which portray [their] actions and beliefs in a variety of specifiable and contextually appropriate ways" (Mulkay et al., 1983:197). Accompanying this methodological alternative, these authors also offer an alternative research question for the social study of science, which moves its focus from asking "what is going on in science?" to "how do scientists construct their versions of what is going on in science?" (Mulkay and Gilbert, 1982a:314). This alternative way of examining the social construction of scientific knowledge is not a "cop out" from dealing with real questions about the nature of science, nor is it a trivial terminological shift, or a compromise in making do with what available data there is. Mulkay and Gilbert (1982a:315) conclude that although the analysis of scientists' discourse neither replaces nor solves the epistemological problems of other qualitative analyses of scientific practice and thought, it does offer a methodological alternative, albeit one with radically different epistemological assumptions. They argue that previous questions "will continue to remain unanswered, and unanswerable, until we improve our understanding of how social actors construct the data which constitutes the raw material for our own interpretative efforts" (p. 315). As Mulkay and Gilbert (1982a:314) put it, this shift implies a radical change in the way analysts look at accounts and descriptions of scientific action and knowledge, which is significant in relation to the following three points.

Firstly, it allows the analyst to stick to the data, without the need to infer that accounts truly reflect previous actions and thoughts, which are supposed to be the really interesting reality. Rather, “scientists’ discourse, its organization and contextual production become the object of investigation” (Mulkay and Gilbert, 1982a:314). Secondly, this does not mean that these authors argue that the analysis of accounts provides a “realm of ‘pure data’ which does not require interpretations” (Collins, 1983b:102). On the contrary, it brings interpretation to the fore, by also explicitly taking the accounts of scientists as interpretative work, in themselves. The analysis of scientists’ discourse allows the analyst to “free [him/herself] from the direct dependence on participants’ interpretative work” (Mulkay and Gilbert, 1982a:314), because it is not concerned with the representation of what “actually happened from scientists’ own attempts to portray their own and their colleagues’ actions and beliefs” (Mulkay and Gilbert, 1982a:314), but with the critical exploration of how scientists build up versions of science which are effective constructions in different interpersonal contexts. Thirdly, and finally, the variability of scientists’ accounts, say, between two different social contexts (such as a research interview and a research paper) is not simply unproblematic but analytically desirable. Variability as a sought after feature of empirical data allows the analyst to have access to a (potentially never ending) set of versions of science that: a) are given equal epistemological standing (e.g. the informal account of an experiment in a research interview is as analytically significant or more constitutive of science as the account in the research paper); b) can be examined in terms of their local functional features (e.g. how rhetorically effective they are to each particular context); and c) can be analysed as fulfilling interactional tasks (Wooffitt, 1992:53). These analytical advantages free the analyst from the objective of producing one “best” or definitive version of scientific action. Furthermore, far from producing a disjointed version of analysis of the social construction of scientific thought and action, it provides a data-bound way of identifying the purpose of (discursive) social devices with which scientists constitute science into what it is, in context-specific ways.

The analysis of scientists’ discourse has been used in a considerable number of social studies of science. According to Mulkay and Gilbert (1982a:315), and Potter and Mulkay (1985:248), these studies have taken on previously examined themes in social studies of science, such as theory choice (Gilbert and Mulkay, 1982) or the construction of scientific consensus (Lynch, 1984). Discourse analytic studies have also taken on other themes which, as Mulkay and Gilbert (1982a) put it, appeared to be “easily amenable to discourse analysis” (p. 315), such as: accounts of error in science (Mulkay and Gilbert, 1982b; Gilbert and Mulkay, 1982); the structure of formal scientific texts (Yearley, 1981); the use of humour in scientific texts (Mulkay and Gilbert, 1982c); how scientists negotiate contradictory scientific models in conference discussions (McKinlay and Potter, 1987), and practices of social categorisation in psychology (Potter, 1988).

Among these, Gilbert and Mulkay’s book, *Opening Pandora’s Box* (1984) explores many of these themes, and has become a reference work in the proposition and development of discourse analysis as

a methodological alternative, not only in social studies of science, but also in other thematic domains, such as social psychology. This book consists of an investigation of scientists' discourse in an area of biochemical research, specifically a disagreement within bioenergetics over what is called "oxidative phosphorylation"⁸ (Gilbert and Mulkey, 1984:21). The authors collected data in interviews, research papers and "informal communications between the participants" (Wooffitt, 1992:52). In their initial chapter, they provide an account of their journey from what they set out to do (a sociological "best fit" account of this controversy) to what this analytical work turned out to be (the analysis of these scientists' discourse in their varying constructions of this controversy). As described above, Gilbert and Mulkey's shift was put down to the realisation of the variability between equally convincing and conceivable accounts of this debate (Wooffitt, 1992:52). They put forward that scientists' discourse is variable in "contextually appropriate ways" (Gilbert and Mulkey, 1984:14), that is, they consider that scientists use different versions of their actions and beliefs in relation to different social situations – in different settings or for different functions – and that these different versions are achieved by using different "interpretative methods" (p. 14). Discourse Analysis is thus defined here by these authors as "the attempt to identify and describe regularities in the methods used by participants (...) through which they establish the character of their actions and beliefs in the course of interaction" (Gilbert and Mulkey, 1984:14, italics added). These methods were named "interpretative (linguistic) repertoires" and constitute a central concept in their analysis (and for Discourse Analysis as a discipline). These are described as recurrently used systems of terms, stylistic and grammatical forms, metaphors and figures of speech which are used for depicting and evaluating actions, events, thoughts etc. (Potter and Wetherell, 1987:149) in contextually and functionally appropriate ways. Gilbert and Mulkey (1984:40) identified two sorts of repertoires – an empiricist and a contingent one. The empiricist repertoire is, according to the authors, the form that dominates and reproduces in formal contexts of scientific discourse (e.g. research papers in journals or conferences) a "received" version of science. Gilbert and Mulkey (1984:56) and Potter (1996:153) put forward three main linguistic and rhetorical features for these empiricist accounts: a) the data are given chronological and logical priority, i.e. data is the cause for all that is proposed in the paper; b) the method is described in a highly conventional manner, i.e. the actions that compose it are impersonal routines which have a clear-cut universal application; and c) the overall grammatical style is impersonal, i.e. overt references to the authors' actions and decisions are kept to a minimum. Therefore, the use of these linguistic and rhetorical features allows scientists to "construct texts in which the physical world seems to speak and sometimes act for itself" (Gilbert and Mulkey, 1984:56) by denying their character as interpretative products, and removing the scientist as their author. The contingent repertoire, on the other hand, does not feature in these formal accounts of scientific action and belief. However, the authors put forward that when they, in their data, compared these formal accounts of doing science with

⁸ This is concerned with the "organic processes that create, transport and store chemical and other kinds of energy" (Gilbert and Mulkey, 1984:18), within the cell.

descriptions in informal situations (such as in their interviews), scientists engaged in presenting their “actions and beliefs as heavily dependent on speculative insights, prior intellectual commitments, personal characteristics, indescribable skills, social ties and group memberships” (Gilbert and Mulkey, 1984:56). In this sense, they made use of a set of linguistic and rhetorical features which made relevant the notion that their thoughts, theoretical choices and experimentation practices could have been different if their social and personal circumstances had been different. In this sense, the physical world is no longer what determines knowledge and action, rather, the scientists are. In the situation of scientific controversy that was under examination, Gilbert and Mulkey put forward that scientists’ selective use of the empiricist and contingent repertoires in describing scientific actions and thought had the fundamental function of allowing them to provide asymmetrical accounts of scientific action when talking about correct and erroneous belief and practice. While the assessment of a theory as “correct” was rhetorically achieved through the deployment of features of the empirical repertoire (i.e. a correct version of science was determined by the world of physical facts), their accounts of error, i.e. the explanation of why a scientist adopted an incorrect theory or failed to accept a correct theory (Gilbert and Mulkey, 1984:67) were accomplished through the use of features of the contingent repertoire (i.e. an incorrect version of science was caused by the “interference” of the scientist). Gilbert and Mulkey with their analysis of these asymmetrical accounts, through the use of these two interpretative repertoires, provide us with a) a way of understanding why scientists produce different versions of science, and b) a demonstration of how these accounts are inherently functional and not descriptive, i.e. achieve different rhetorical and interactional purposes (Gilbert and Mulkey, 1984:73).

Despite the fundamental implications of Gilbert and Mulkey’s discursive project for the practice of sociology of scientific knowledge (and for sociology in general) – i.e. for the discipline’s uncritical reliance upon reports and descriptions, and its objective of producing realist accounts of the social world of science – it did not have a sustained impact on the discipline. The development of DA into a methodological alternative took place elsewhere, within social psychology. As mentioned above, quite a few authors initially took up Gilbert and Mulkey’s broad remit of analysis, and used DA in social studies of science. However, after a considerable amount of attention received during the late 1980’s, the use of DA in SSK was more or less abandoned, leaving no lasting impression (Wooffitt, 2005a:48). Its key proponents moved on to other areas and, in Jonathan Potter’s case, transported the idea of discourse as a topic of research to social psychology. As it will be possible to see in Chapter 3, DA is still very much in development within social psychology, and has been unfolded into many different (though heavily related) approaches, based on diverse intellectual traditions.

In summary, studies of controversies in science, and of parapsychology as an exemplar of scientific controversy, are crucial to understanding the social processes through which scientific knowledge is accomplished. What Gilbert and Mulkey showed is that such processes include the very accounts upon which social scientists depend for their analyses. Scientists’ discourse needs to be understood as

functional, rather than merely descriptive, and the discourse of scientists on matters of controversy is a fruitful area of analysis. As the studies referred to above have shown, parapsychology is an area of inherent controversy, as not only competing theories but also the scientific validity of the field itself is in question. Analysis of parapsychologists' discourse should therefore provide a particularly rich insight into how the scientificity of knowledge and practice is achieved. It is for this reason that the thesis examines the accounts that parapsychologists themselves give of their work.

1.2 The present thesis.

While informed by earlier research in SSK, this thesis sees itself as part of a self-reflexive tradition within parapsychology. This section will characterise the location of this thesis and its potential contribution towards the field of parapsychology (sub-section 1.2.1), and present a summary of the following chapters (sub-section 1.2.2)

1.2.1 The location of the thesis.

While this thesis may be seen as part of the DA project in social studies of science, this section will present it as part of a self-reflexive tradition within parapsychology itself. In doing so, it is hoped that it might contribute to the field with a discourse based form of analysis and self-reflection upon how parapsychologists constitute their scientific practices and knowledge.

This thesis sees itself as following the tradition of the analysis of scientists' discourse, as Gilbert and Mulkay first proposed. However, as will be described and discussed in Chapter 3, the present thesis' use of DA, when compared to Gilbert and Mulkay's form of analysis, has important methodological differences, given the evolution and development that DA has had within the field of social psychology during the last 15 years. While the initial issues and questions that interested the present investigation were akin to those that the above authors posed of their own data, the way in which the data was analysed was considerably different. It focused specifically on what is known within DA as the strand of research concerned with 'fact construction', that is (broadly speaking) interested in the way accounts or descriptions of reality "are constructed and made (to seem) objective and factual, or, conversely, the way (apparently) factual versions are undermined as partial, distorted or interested" (Potter, 2004:611). The importance of Gilbert and Mulkay's proposition for the social study of science, despite its lack of development and adherence within SSK, cannot be understated. However,

therefore, such an approach would benefit from being brought up to date with more recent and detailed ways of doing DA based research. This thesis thus sees itself as following up on the DA project within social studies of science, though locating this exploration of scientists' discourse within the field of social psychology.

Experimental parapsychology may be seen as a field with a special standing within science. Experimental parapsychologists share with psychologists, physiologists and physicists a similar perspective on human and animal psychological and physical functioning, a basic set of assumptions about the functioning of matter and energy, and a repertoire of core investigative methods. However, they differ significantly in terms of how their phenomena, claims and methods are viewed by a wider scientific community. Whilst the other disciplines are currently viewed as both robust and central to the way we build up ideas about ourselves and the world, parapsychology is perceived as a controversial and marginal field of research, in which both experimental expertise and basic propositions are actively disputed even within the field itself. This asymmetrical relationship makes parapsychology a particularly interesting area in which to explore scientists' discourse. This thesis is interested in how parapsychologists construct what they do as doing science, i.e. as representing something real in the world, and their field as a warranted area of research into this aspect of the real world. Parapsychologists are in the business of constructing a contested part of the real world, through the construction of what they do as scientific practice and their outcomes as scientific knowledge, as science is consensually the exemplary way of representing reality. The analysis of their factual discourse is crucial to our understanding of how parapsychologists are able to achieve this goal, or, conversely, how their descriptions can be undermined as mistaken or as an invention. Furthermore, the fundamental controversies that go on within experimental parapsychology, regarding, for instance, the reality of its object or the accuracy of its research practices (these issues will be explored in Chapter 2), mean that there are a multitude of available versions of what *psi* is, and disagreements over whether it exists, what constitutes good research or a successful outcome or an appropriate methodology. Although controversies exist in all areas of science, experimental parapsychologists seem to be particularly aware of this context of many available versions of the facts that make up their field, research object and research practices. This, as it will be possible to address closely in Chapter 3, makes the analysis of their fact making practices particularly interesting.

This thesis seeks to contribute to the field of parapsychology by using a DA based form of analysis of how parapsychologists constitute their field, concepts, research practices, outcomes and research object. Parapsychology has always enjoyed the contributions of various research traditions, from physics, to philosophy and psychology. This thesis proposes that this form and focus of DA, as it is developed and used in social psychology, can be a useful thread of research in parapsychology, offering the possibility of: a) a reflexive, functional and situated analysis of the competing versions of reality to which parapsychologists regularly orient, and with which they deal in their research lives;

and b) revitalizing the practice of what was introduced and named by Marcello Truzzi (1978:2) as a “Zetetic⁹ perspective” within parapsychology. The aim of this movement was to create, within the field, a habit of “continuous dialogue between the proponents and critics of claims of the paranormal, concerned mainly with enhancing communication, (...) interested not only in the adjudication of the claims, but with the sociology and psychology of the disputes themselves” (Truzzi, 1978:2). This movement was centred on the journal *The Zetetic Scholar* (edited by Truzzi himself), which ran from 1978 to 1987, overlapping, to some extent, with SSK’s interest in parapsychology. It enjoyed the contributions of influential scholars from areas such as parapsychology, SSK, psychology and philosophy of science (including such authors such as Stanley Krippner, John Beloff, Ray Hyman, Robert Rosenthal, John McClenon, Andy Pickering, Harry Collins, Trevor Pinch and Paul Feyerabend). The journal thus provided a motivated and focused forum for a self-reflexive examination of the controversies within parapsychology, one that was more interested in the issues that fired up the disputes within the field than with the final outcomes of these disputes. Although many of these authors would not describe themselves as parapsychologists (indeed, some would have been quite opposed to such a label), they contributed considerably to the widening of parapsychology’s horizons, to include questions and concerns that focused not on the search for experimental proof and understanding of the physical and psychological process of *psi*, but rather on wider social, cultural and psychological implications of the study of anomalous phenomena. Despite a continuation of some of these dialogues, the examination of such issues has become less focused and incisive, and has been diluted among various journals, research groupings and incidental discussions in internet based forums.

This thesis recognises the importance of this kind of examination and self-reflection upon important epistemological and social issues within parapsychology, and argues (for the reasons stated above) that the analysis of the discourse of experimental parapsychologists can offer a useful tool for the examination of how parapsychologists constitute and organize their descriptions of their field, concepts, research practices, outcomes and their research object. It is hoped that this will offer insights into how experimental parapsychologists realise and achieve the properties of their field, research work and object. The use of the formal and systematic analysis of discourse within the field of parapsychology, although a new proposition in the sense that was outlined above, is not an entirely new practice within parapsychology. Indeed, the analysis of the organization of language related to paranormal phenomena has been the main focus of Robin Wooffitt’s contribution to parapsychology, specifically in his analysis of sequences of conversational actions on the reporting of anomalous experiences (Wooffitt, 1992), interactions between psychics and sitters (Wooffitt, 2001), and

⁹ Zetetic literally means “proceeding by inquiry or investigation”, and the denomination was taken from the Greek philosophical tradition followed by the Pyrrhonians, characterised by a “continual enquiry after the truth” (Oxford English Dictionary, online version).

interactions between experimenter and subjects in laboratory based parapsychological experiments (Wooffitt, 2003; Wooffitt, 2005b). Wooffitt proposes that the analysis of language in parapsychology (in his case, using conversation analysis) can constitute an important part of the parapsychological project itself. The rigorous examination of the way that individuals describe anomalous experiences or the way in which experimenters and subjects collaboratively construct an experience in the laboratory, Wooffitt contends, can and should have a fundamental role within the analysis of a broader set of psychological, social and cultural implications of reports of anomalous phenomena (Wooffitt, 2005b, 325), irrespective of the ultimate ontological status of ostensible parapsychological phenomena (Wooffitt, 2005b:333). The present thesis, although using a different discourse analytic methodology (to be described in Chapter 3), aims to contribute to this recent and growing discursive thread of research within parapsychology.

1.2.2 Chapter summary.

This sub-section provides a summary of how the thesis is organized.

In Chapter 2, the thesis will explore specific aspects of this asymmetrical relationship between parapsychology and science. For example, formal research practices (such as general experimental methodologies and particular protocol designs) are viewed as problematic not only by critics of the field but also by experimental parapsychologists themselves. As it shall be possible to see, methodological expertise in experimental parapsychology seems to be undercut by the problematic construction of *psi* as a research object. The divergent versions of the experimental definition of *psi*, of what constitutes proper practice and, therefore, what amounts to proper proof and understanding of the phenomena, create a scenario of conflicting versions in the field's literature, for which there is no one consensual answer (Collins, 1981b:34). That versions of what constitutes *psi* (or experimental confirmation of its existence) are varied is neither a surprising nor an exclusive characteristic of parapsychological research. There are, however, two particular features of such variation. Firstly, the various constructions of *psi* are commonly accompanied by a statement regarding pre-existing belief in or scepticism about the very existence of the phenomenon. Indeed, experimental results are often construed as dependent upon the will of the experimenter or participant. Secondly, there are, among many, two possible answers to research questions in parapsychology: a) that *psi* does not exist, and b) that *psi* may exist, but is not amenable to experimental investigation. Such possible answers directly challenge the very existence of the field, and therefore are fiercely disputed "realities". This thesis will later argue in its analytical section that parapsychologists have constituted a paradoxical position in relation to standard accounts of scientific action: while they actively appeal to the primacy and rigour

of experimental methodology, they have constructed a set of characteristics for their research object that compromises the rhetorical power of empiricism.

An account of the methodological approach of this thesis will be provided in Chapter 3. The unique position of parapsychology requires parapsychologists to be particularly proficient in building up what constitutes something as scientific practice or scientific knowledge. Hence, the need to examine accounts of parapsychologists themselves, and the choice of discourse analysis as the most appropriate methodological approach. As the discussion above described, such an approach means that analysis of participants' accounts in this thesis are: a) independent of the interpretative practices of the participants in this study; b) focused on the accounts themselves; c) interested in variations in these accounts; and d) concerned with making sense of this variability, and taking these accounts as accomplishments through which participants make these versions into factual version of reality. It will focus analytically on how parapsychologists constitute factual versions of their field, research and outcomes that attended to normative versions of what counts as science, and will explore what discursive resources are used in their accounts of doing parapsychology to achieve accounts of doing science.

The analysis itself will start in Chapter 4, by exploring the various constructions of parapsychology as a research discipline. It will focus on how participants built up versions of the community, the evidence and the epistemological standing of parapsychology. In doing so, it will show how they oriented to traditional demarcation criteria, yet constituted parapsychology as a field of research that does not clearly satisfy their own constructed criteria. The analysis will argue that their accounts of scientificity and demarcation allow two general discursive actions. On the one hand, these demarcation issues are used to construct parapsychological research and *psi* as, respectively, a particularly troublesome field of empirical research and a reluctant empirical object. On the other hand, the construction of this misfit between the desirable characteristics of scientific quality and the "state of affairs" in parapsychology allows these parapsychologists to constitute their scientific actions as almost heroic achievements, as an ability to perform research within a less-than-perfect scientific field.

Chapter 5 will focus on research practices, and on how participants constructed their own research within parapsychology. It is concerned with what they do as researchers, and with their self-presentation as individuals doing safe science. Although there is nothing surprising or contentious about experimental researchers describing their work in a way that makes relevant its quality (and certainly not during an interview situation), the way in which parapsychologists constructed their own research, it is argued, is a way of asserting that their research constitutes safe science within a problematic field (as constructed above). By asserting that their claims and the quality of their methods are in line with common notions of what science is, they are able to constitute themselves as

robustly scientific researchers, even if the field in itself is essentially problematic. The chapter will focus on four particular discursive processes used by participants to: a) build up their research practices in parapsychology as doing ordinary science; b) constitute their research activities as a particularly demanding experimental science; c) make available the scientific quality of their work by contrasting it with the problematic research of a vague category of “others” in parapsychology; d) negotiate and minimise their membership to the category of parapsychologist or qualify their being in parapsychology.

Chapter 6 will examine a particular aspect of participants’ accounts of their research practices. It will explore how they constituted their research as strict empirical practice with no assumptions, boundaries of expectation, theoretical underpinnings or pre set objectives. Related to the discursive actions characterized in the previous chapter, this chapter focuses on one of the most interesting rhetorical achievements of these parapsychologists’ accounts – their stress upon facts and method, rather than theory. In doing so, they seem to be appealing to the ability of scientific practices to access neutrally the world out-there (Mulkay, 1979:20). As it shall be examined, however, it is the extremity of these descriptions that makes them analytically rich. In this sense, it will focus on how these parapsychologists construct: a) the causal and logical primacy of facts; b) the unproblematic dispensability of theoretical frameworks or assumptions of any kind; and b) their own impartiality (neutrality) towards their scientific objects and claims.

Chapter 7 is concerned with how participants built up categories of *psi* and anomaly, and explores these in relation to themes raised in previous chapters. The analysis in this chapter asks: what constitutes an anomalous outcome a) in the context of research into phenomena operationalized as experimental anomalies; b) when *psi* itself has been constructed as an ambiguous, elusive and reluctant scientific object, and c) when the experimenter has described their research as a process involving no expectations? The chapter will focus on how participants achieved four categories of *psi* and anomaly: a) the straightforward identification between anomaly and *psi*; b) the problematisation of the identification between anomaly and *psi* (indeed, of the interviewer’s request for definitions at all); c) the constitution of anomaly into an impossible category in parapsychology (orienting to accounts of extreme empiricism and neutrality explored in Chapter 6); and d) the distinction between *psi* and anomalous *psi*, i.e. outcomes supporting the reality of *psi* but which go beyond some boundary of expectation.

The final chapter (Chapter 8) will present a summary of the thesis and conclusions, and will discuss both the relevance of this analysis for the field of parapsychology and the limitations of this thesis.

Chapter 2

Experimental Parapsychology – a field of internal controversy.

Introduction.

This chapter provides a possible view of parapsychology as a field of experimental research. It focuses on its basic terminology, claims, methods, and explanatory models, and explores possible meanings of *psi* and anomalies in parapsychology. In doing so, it characterizes parapsychology as a field of internal controversy in several respects. First, section 2.1, will examine the controversy over terminology, looking at the various ways in which the phenomena that constitute the experimental objects of parapsychology have been defined, and focus on the difficulty in constructing a consensual operationalization of *psi*. Second, 2.2, will explore the construction of parapsychology as an experimental field, focusing on the potential tension between the use of the experimental method and *psi* as its object of research. Third, 2.3, will look into the construction of experimental evidence for *psi*, particularly appeals to replication in relation to results from the *Ganzfeld* procedure, and the role of the experimenter as a suggested explanation for the variability of *psi* results. Fourth, 2.4, will examine how parapsychologists have built up the scientifically “ideal” situation for parapsychology, one that would lead to both the resolution of the fundamental controversy about the reality of *psi*, and the legitimization of parapsychological research. Finally, 2.5, will link this characterization of parapsychology as a context of controversy (about the central validity and properties of the phenomena under study) to participants’ accounts of categories of *psi* and anomaly. In particular, I will examine a) the identification of anomaly with *psi*, and b) accounts of anomalous *psi*.

2.1 Defining terms – controversy over terminology.

Experimental parapsychology is commonly defined by its researchers, and by recognized organizations within the field, as the experimental exploration of “ostensibly paranormal phenomena, that is *psi*” (Parapsychological Association on-line Glossary of *psi* Terms, 2003; Parapsychological Foundation, Inc. online list of Basic Terms in Parapsychology, 2003). Parapsychology as an experimental science is thus articulated as the study of phenomena which are in their appearance paranormal, caused by, or associated, with the operation of paranormal factors, i.e. factors beyond or exceeding the limits of currently used psychological or physical scientific frameworks (Irwin, 1999:1). The term *psi* (literally, the Greek letter ψ) is ubiquitous in parapsychology, used as either a noun or an adjective to identify paranormal processes and paranormal causation (Parapsychological Association on-line Glossary of *psi* Terms, 2003; Parapsychological Foundation, Inc. on-line list of Basic Terms in Parapsychology, 2003). Although over the years there have been various classification systems and labels for these phenomena, the one currently in use in experimental parapsychological research identifies two major descriptive categories: Extra-Sensory Perception (henceforth ESP) and Psychokinesis (henceforth PK). The first descriptive category, ESP, refers to the acquisition of information about an external event, object or influence (mental or physical; past, present or future), by means other than the known sensory channels (Thalbourne, 2003:40). Basically, ESP can be seen as a process of information acquisition that appears to be analogous to conventional sensory processes of sight, sound, taste, touch and hearing. It includes three more specific sets of phenomena identified in both the Parapsychological Association and Parapsychological Foundation’s Glossary of terms (Parapsychological Association on-line Glossary of *psi* Terms, 2003; Parapsychological Foundation, Inc. on-line list of Basic Terms in Parapsychology, 2003) as: telepathy, i.e., the apparent acquisition of information concerning thoughts, feelings or activity of another conscious being; clairvoyance, i.e., the apparent acquisition of information concerning a concealed object or contemporary physical event; and precognition, i.e., the apparent foreknowledge of an event as yet undetermined, and not reliably predictable in terms of present knowledge available through conventional sensory channels (Koestler Parapsychology Unit Glossary of Parapsychological Terms 2003). The second descriptive category, PK, refers to the direct influence of the mind on a physical (living or not) system that cannot be entirely accounted for by the mediation of any known physical process (Thalbourne, 2003:98). Usually a distinction is made between extremely small effects (micro-PK) and larger effects (macro-PK). Related to this category, but not normally included in it, is the direct mental interaction between living systems (henceforth DMILS) referring to the interaction (which might include influence) between two biological systems (such as an individuals' physiology).

Despite the caution with which the term paranormal is applied in the articulation of parapsychology's main aim and definition of *psi* above, the careful location of these phenomena or their cause beyond or

exceeding the limits of current physical and psychological systems is both disputed within the field and fundamentally problematic. As Braude (1979: 242-273), Mabbet (1982:337, 350), McClenon (1984:68), Kurtz (1985:505) and Hyman (1987), among others, point out, it is difficult to understand what is exactly meant by paranormal in its many uses and references within parapsychology. At times the use of the term paranormal suggests the operation of factors that are ultimately inexplicable by science. At other times, however, its use seems to denote a set of phenomena that are temporarily inexplicable in terms of currently used scientific concepts and theories, or that cannot be explained scientifically without major revisions to the existing framework (Braude, 1979:260). The latter use would probably be closer to the term anomaly (McClenon, 1984:68; Kurtz, 1985:505). Interestingly, the two glossaries of terms cited above seem to contribute to the almost interchangeable use of the terms anomalous and paranormal in parapsychology. The aim of experimental parapsychology seems to be articulated here (as elsewhere) as the exploration of phenomena that are anomalies to currently known perceptual, cognitive and physical human capacities. As such, these phenomena are hypothesized as being new or challenging to currently used descriptive and explanatory systems of human perception, cognition and physical capacities used in psychological or physical research.

This seemingly unproblematic and interchangeable use of the adjectives paranormal and anomalous (Thalbourne, 2003:4), and the implied paranormal nature of *psi*, point up a fundamental difficulty in the generation and use of basic terminology in parapsychology. Indeed, there have been various instances of disagreement over the use of these terms by different researchers in the field (e.g. Hövelmann, 1983; Krippner, 1983; Zingrone and Alvarado, 1987). Palmer (1986a) provides a particularly well articulated criticism of the various meanings of the term *psi*. He points out that the term *psi* is “applied both to our phenomena and to the paranormal principle that we propose to account for them” (p. 138), i.e. *psi* is used as both a descriptive and an explanatory construct. However, because this distinction is not made clearly in practice, “the terms *psi* ends up serving neither function adequately” (p.138), as it is not strictly descriptive because of its connotation with paranormal causality and not properly explanatory because of its lack of explicit content (Palmer, 1986a:138). The consequence of this lack of clarity in the use of the term *psi* is the confusion of the “explanatory” principle with the subject matter of parapsychology, as if the paranormality of the phenomena that concern parapsychology was accepted *a priori* (Palmer, 1986a:140). Palmer (1983a) goes further in his comments when he asserts that “the present terminology also serves to retard the development of genuine theory building in parapsychology by giving us this subtle illusion of understanding” (p. 164). He puts forward the suggestion that *psi* should be stripped of its explanatory (i.e. paranormal) implications and only be used as a purely descriptive term for anomalous outcomes, i.e. as “a correspondence between the cognitive or physiological activity of an organism and events in its external environment that are anomalous with respect to generally accepted basic limiting principles of nature, such as those articulated by C.D. Broad” (Palmer, 1986a:139). These limiting principles, Broad describes, are the fundamental set of assumptions that “we unhesitantly take for granted as the

framework within which all our practical activities scientific theories are confined” (Broad, 1953, cited in Rao and Palmer, 1987:539), such as “the assumption that it is impossible for a person to perceive a physical event or a material thing except by means of sensations which that event or thing produces in his mind” (Broad, 1953, cited in Rao and Palmer, 1987:539). Palmer’s definition of *psi* implies that for an event to be described as *psi*, it must be anomalous, but not necessarily paranormal. The same point is made clear by Bem and Honorton (1994:1) when they state that the purely descriptive term *psi* neither implies that these anomalous phenomena are paranormal nor expresses anything about their underlying mechanisms. Similarly, in 1994, a group of senior parapsychologists (Lanz et al. and May et al.) declared their adoption of the terms anomalous mental phenomena instead of *psi*, and their use of the terms anomalous cognition and anomalous perturbation as substitutes for, respectively, ESP and PK, putting forward that “these terms are more naturally descriptive of the observables and are neutral in that they do not imply mechanisms” (Lanz et al., 1994:286,; May et al. 1994:304). Finally, there seems to be relative agreement in the parapsychological literature related to the point that *psi* should only describe an anomalous probability of an experimental event (e.g. Palmer, 1983b:54; Kurtz, 1985:513), i.e. that *psi* is an inherently probabilistic construct. As Palmer puts it:

“*psi* is a statistically significant departure of results from those expected by chance under circumstances that mimic exchanges of information between living organisms and their environment, *provided that a) proper statistical models and methods are used to evaluate the significance and b) reasonable precautions have been taken to eliminate sensory cues and other experimental artifacts*” (Palmer, 1983b:54, italics in the original).

It is precisely this that some “critics” of the field have found to be so problematic, asking “how is it that a (spurious) deviation from chance can be distinguished from *psi*?” (Kurtz, 1985:513; Ayer, 1965, cited in Collins and Pinch 1979: 247). Nevertheless, within this context of dispute over the definition and operationalization of *psi*, it is possible to see the experimental and probabilistic orientation of this definition (and of parapsychological research generally) as a focal point in the construction of parapsychology as a scientific research field. This will be explored in the next section.

2.2 The tension between method and object.

Experimental parapsychology, as it exists today in various academic and research institutions in Europe and the United States, has its roots in J.B. Rhine's 1934 research in the Psychology Department at Duke University, North Carolina (Allison, 1979:273; McClennon, 1984:9). With the

establishment of the Duke University Parapsychological Laboratory in 1935, Rhine and his collaborators proposed and maintained a set of standardized research procedures to investigate these anomalous cognitive and physical phenomena, which included: experimental protocols that attempted to eliminate fraud and the influence of known sensory processes; recording and interpretation systems that sought to establish the independence of the outcomes from the experimenter; and the use of adequate statistical models. Although some elements of these procedures were already in use in previous psychical research (McVaugh and Maurskopf, 1976, cited in Allison, 1979:274), Rhine is credited with the construction of a code of practice for experimental research in parapsychology, including validity and quality criteria. Similarly, Rhine is commonly associated with the creation of a nomenclature for phenomena in parapsychology (including some of the labels identified above), by using the experimental operationalization of events, rather than their ostensible phenomenology, as the basis for a terminology system (Zingrone and Alvarado, 1987:51). An important example, which will be explored in greater depth in the last section of this chapter, is Rhine's laboratory production of the experimental construct of *psi*-hitting versus *psi*-missing, based on the idea that ESP correspondences can meaningfully be both "right" (hitting) and "wrong" (missing) (Carpenter, 1977:205). He is also credited with the organization of experimental parapsychology as a social and professional scientific community and specialism, including the establishment of recruitment and training criteria for its members and the creation of communication vehicles such as the *Journal of Parapsychology* in 1937 (Allison, 1979:273; McClennon, 1984:9). According to John Beloff (1993:127), a senior figure in contemporary parapsychology, "the Rhine revolution" towards the development of experimental parapsychology involved three main objectives. The first objective was to introduce a coherent, progressive and valid programme of experimental research into the study of the paranormal or anomalous. This should be based on adequately robust methodology that could yield the construction of a coherent, consensually accepted and research-generating body of methods, outcomes, claims and problems. The second objective, following from the first, was to gain scientific legitimacy and the inclusion of parapsychology in the sphere of academic knowledge and activity. The third, and perhaps the most significant, was to establish these phenomena as universal properties of the functioning of the human mind (Beloff, 1993:127), just like any other psychological characteristic.

However, experimental parapsychology has since developed into an area of research in a uniquely paradoxical position in science, "in that it forcefully defends the primacy of the experimental method, but has so far been unable to fully develop a research programme in the manner of the other experimental sciences" (Maurskopf and McVaugh, 1980, p. xiv). According to Beloff (1993:127-128), parapsychology appears to be characterized by a lack of clarity or consensus in relation to: a) the validity and reliability of its methods and the adequacy of the labels referred to above, b) the validity, replicability and, ultimately, the scientific "reality" of its outcomes and claims, and c) its legitimacy as a scientific and academic specialism. As Bem, Palmer and Broughton, prominent researchers in the field, clearly pointed out in 2001, "the question of whether *psi* actually exists continues to be

controversial” (p. 207). In the view of Truzzi (2001), “most evidence for *psi* functions as suggestive rather than convincing evidence” (p. 321). These considerations point up a particularity of parapsychology. As Bauer (1984) puts it, controversies are common in science, but what is particular about the situation of current parapsychological inquiry is that the central controversy that it attempts to resolve is not related to the existence of competing explanatory models for *psi*, or competing ways of interpreting phenomena, “but refers to the very existence of the phenomena under investigation” (p. 142). Beloff (1973) makes the same point: “Rhine succeeded in giving parapsychology everything it needed to become an accredited experimental science except the one essential: the know-how to produce positive results when and where required” (p. 291).

This central controversy may also be related to some of the adjectives applied to *psi*, or hypothesized as its experimental properties. One frequently finds *psi* to be described as: “self-obscuring” (Braud, 1983:160); “not just elusive, but actively evasive” (Beloff, 1994:7); as being close to the archetype of a “trickster” (Hansen, 2001); and as “capricious” or experimentally “unsustainable” (Kennedy, 2003:54). All of these adjectives, derived from various analyses of the lack of repeatability of experimental and experiential evidence for the phenomena, confer properties upon *psi* that make it, at the very least, into a scientific object that is difficult to manage. However, what is salient about these adjectives is the basic uncertainty that they translate about the possibility of verifying and characterizing *psi* experimentally and, ultimately, of being able to argue the reality of *psi* in scientific and experimental discourses. This is a fundamental point in the examination of parapsychology as a context of scientific research. At the same time that parapsychologists put forward this elaboration of *psi* as a reluctant experimental object, they commonly appeal to the quality and strict rigour of their experimental methods and using, in Collins and Pinch's (1979) words, the “symbolic and technical hardware of science” (p.242). Parapsychologists have in this way constructed in their literature a fundamental tension between object and method of research. In using the experimental method, *psi* has been constructed as an object of research elusive to investigation by that very method. Furthermore, as we shall see below, *psi* has also been put forward as a phenomenon experimentally vulnerable to the influence of researchers’ personal factors, such as their “belief” in *psi*. During the analysis of parapsychologists’ accounts, this thesis will show that parapsychologists have constituted a paradoxical position in relation to standard accounts of scientific action: while they actively appeal to the primacy and rigor of experimental methodology, they have constructed a set of characteristics for their research object that compromise their appeals to empiricism.

To date, parapsychologists, as an identifiable community of professional experimental researchers, remain divided on what constitutes a) a coherent descriptive system for the phenomena, b) convincing experimental evidence for the reality of *psi*, and c) satisfactory explanatory and predictive models for its nature and process (Schmeidler, 1968, cited in Allison, 1979:282; Edge, 1986:313). Furthermore, even if parapsychologists share most of their methodologies and assumptions with psychology (for

example, in relation to their models of the mind and its functioning), they have failed to have parapsychological questions and phenomena included within psychology as a body of knowledge. The next section will explore the development of experimental methodologies in parapsychology, and their relationship to those within experimental psychology.

2.3 The construction of experimental evidence for *psi* – the noise reduction model and the *Ganzfeld* method.

Parapsychology as an experimental discipline developed a repertoire of experimental methods that share with psychology basic assumptions regarding design and measurement. An experimental system in parapsychology will typically involve the conceptualization of an interaction between a subject and its environment (e.g. a subject attempts to gather information about a remote target) or between subjects (e.g. one individual attempts to change the physiology of another). The measurements range from cognitive and behavioural responses or self-report measures to physiological indicators, such as electrodermal and electroencephalic activity. The construction of evidence, as in other areas of psychology, is heavily reliant on probabilistic and inferential models of the distribution of effect among the population. Furthermore, as in cognitive psychology, it is possible to identify a contemporary emphasis on the use of physiological measures in parapsychology. These studies explore anomalous cognitive and physical phenomena, and seek to associate them with brain functional states or with the activity of particular brain areas. Thus, the tangibility of the brain as an object that can physically cause or correlate with these phenomena offers parapsychologists, as well as psychologists, the possibility of material identification of the phenomena and, consequently, the potential to establish more convincingly their scientific “reality”.

In addition to these basic similarities with psychology, however, experimenters in parapsychology have developed specific protocols, along with methodological and measurement assumptions, in order to accommodate the anomalous nature of the phenomena they study. For example, one influential protocol that models how information transfer might happen in a situation where there is no normal sensory contact is known as the “*Ganzfeld* procedure” (Bem et al., 2001). It translates literally as “entire field” and is an experimental protocol, introduced by Honorton and Harpe in 1974, in which an homogenous and unpatterned sensory environment is created. It was devised as a method to test a model of *psi*-functioning called the noise reduction model (Bem, 1993:102). This proposes that the relaxation and relative perceptual isolation of the subject, by reducing external and internal sensory and somatic “noise”, creates an internal attention state (not dissimilar to dream or some meditative states) associated with successful performance on ESP tasks (Honorton, 1985:51). It thus

hypothesizes that: a) *psi* and *psi*-mediated information is a weak signal; b) if the ordinary external sensory input and internal somatic noise are reduced, the possibility of conscious access to “other” (i.e. *psi* mediated) types of information is enhanced, and; c) it is possible to measure this enhancement in relation to the probability of accessing the information by chance or guessing. Thus, the *Ganzfeld* procedure is based upon a “working model of *psi* in which *psi* mediated information is conceptualized as a weak signal that is normally masked by internal somatic and external sensory noise” (Bem, 1993:102). The procedure is assumed to create a “*psi* conducive” state in which the “signal to noise ratio” is enhanced, increasing the potential for detection and recognition of the *psi* signal (Honorton, 1977:466; Bem and Honorton, 1994:15).

As such, *Ganzfeld* is an experimental protocol, particular to parapsychology, which puts into practice a model (with assumptions and expectations) of how this anomalous process of communication might work. It is not the only model, but it has become the main source of both optimism and controversy in the community of experimental parapsychologists, as they seek to present replicable evidence across experimenters for a genuine anomalous process (Milton, 1999:309-310). However, while it is a procedure devised to test the noise-reduction model of *psi*, “the *Ganzfeld*” has been reified by parapsychologists into a self-sufficient practice in itself, one that seems to have taken primacy over the model it is designed to test. Since its introduction, the *Ganzfeld* procedure has been used with sufficient regularity to create a cumulative database, and this has been the target of various meta-analyses and resulting debates between proponents and critics of the field. The controversy over the status of the outcomes of these meta-analyses as “evidence for *psi*”, points up the difficulty that parapsychologists express in gathering consensus about their assessment of research outcomes and claims. Of course, controversies are common in any scientific field, but as noted above, what is particular about these controversies is that they are concerned with the very existence of the phenomenon being studied, and thus with the justification for the field itself. These controversies also illustrate how experimental outcomes are negotiated into being either “proof of the existence of *psi*” or “not proof of the existence of *psi*”, how they are accepted or rejected, to use Gilbert and Mulkey's (1984) terms, through empirical (e.g. the adequacy of the application of a method) or contingent arguments (e.g. the researcher's belief about the phenomenon). These negotiation processes will later be discussed in relation to the analysis of the data resulting from this project. For the moment, however, it is worth examining the last instance of debate in 1999, as an example of the difficulty in establishing a consensus about the reality of *psi*. It is an illustration of how the debate over the reality of a *psi* effect is translated into a debate over what counts as a successful replication of the *psi* effect and what counts as a “well done” *Ganzfeld* (Collins, 1981b:34; Milton, 1999:321).

2.3.1. The *Ganzfeld* debate – when replication fails to create a real effect.

The two most recent meta-analyses - Bem and Honorton (1994), and Milton and Wiseman (1999) -, were both published in an influential psychology journal – the *Psychological Bulletin*. Their access to what (in parapsychology) is called “a mainstream publication” is remarkable in three ways: a) it allows for the communication of parapsychological outcomes and claims to the community of psychologists, not just parapsychologists (Milton, 1999:313, 321); b) it is a step towards the aforementioned scientific legitimization to which Rhine and Beloff referred; and c) it is a rare and highly valued event within the community of parapsychologists. These two meta-analyses were transformed into two academic papers with the respective titles “Does *psi* exist? Replicable evidence for an anomalous process of information transfer” and “Does *psi* exist? Lack of replication of an anomalous process of information transfer”. Clearly, their titles construct the debate to be about the “existence” of *psi*, and the upshot from the results of each meta-analysis as, respectively, “evidence for *psi*” and “no evidence for *psi*”. Although there are many readings of this debate, the Journal of Parapsychology in 1999 dedicated a full issue to it, entitled “The ESP – *Ganzfeld* controversy” (vol. 63, 4). It consisted of a discussion paper by Milton (one of the authors of the second meta-analysis) and a community wide e-mail contributions and debate about this controversy, edited by Schmeidler and Edge (1999).

We can begin by looking at how the scenario of this controversy was laid out. The first meta-analysis, produced in 1994 by Bem and Honorton, was put forward as the culmination of years of experiments using the *Ganzfeld* procedure. It was presented to a wider psychological community as a careful, critical and stringent process of analysis that, in the end, asserted the replicability of a *psi* effect across experimenters under methodologically stringent conditions (Bem and Honorton, 1994:4; Milton, 1999:309; Bem et al., 2001:207), implying that *psi* was an experimentally real effect. Since then, as Milton (1999) argues, it has been built up as the best example of “the case for *psi*” (p. 321). Conversely, the second meta-analysis, produced in 1999 by Milton and Wiseman, came to be constructed as a challenge to the former. It presented an updated assessment of all *Ganzfeld* studies in the intervening years, but one that did not support the conclusions of Bem and Honorton (1994). Milton (1999) concluded that “the failure of the recent studies to replicate the success of the earlier work therefore presents a challenge in the same mainstream scientific forum to parapsychology's claims for a genuine, replicable effect” (p. 321). This statement points up the gravity of this “failure to replicate”, a failure that questions the validity of any “real” effect in parapsychology (acted out before a wide community of psychologists), and thus raises fundamental questions about the viability of parapsychology.

Replication is often presented as a fundamental demand in the social sciences, and specifically in psychology (Hendrick, 1991:41; Rosenthal, 1991: 2; Amir and Sharon, 1991:51). The establishment of an empirical outcome as the basis for the development of a theory in psychology demands, according to Amir and Shanon (1991:51), its reproducibility (the potential for an outcome to be found repeatedly under similar conditions) and generalizability (the potential for an outcome to be found under different conditions, populations, etc.). These authors present replication as the way to establish these validating properties. Reproducibility is thus built up as a way of distinguishing between a valid, genuine outcome and an artifact of unwilling or willing experimental manipulation (Rosenthal, 1991:2). In the particular context of parapsychology, as Milton (1999: 310) states, “unless the experiment's effects could be replicated across experimenters, there would always remain fraud, error or sensory leakage as strong alternative explanations to the *psi* hypothesis”. In this sense, the “replicability” of an outcome is identified with its genuineness (Milton, 1999:324). The term “replication” is thus being used by Milton in an existential sense, “raising reality questions” and seeking to “answer whether *psi* is genuine or spurious” (Rao, 1981a:311). This meaning of replicability has been disputed in parapsychology and elsewhere as an inappropriate and, ultimately, impossible criterion for the establishment of the reality of a great number of psychological phenomena (e.g. Blackmore, 1983:184). Nevertheless, the second meta-analysis was built up as a challenge to the replicability of *Ganzfeld* outcomes, and to the genuineness of an experimentally consistent *psi* effect that had been established beforehand (Bem et al., 2001:208). Had the two meta-analyses been published in the reverse order, there can be little doubt that the controversy would have been played out quite differently. As the last Chapter explored, Collins and colleagues (e.g. Collins 1985) showed that what amounts to a replication of an experiment and, hence, of a phenomenon, can be seen as part of a process of negotiation, rather than an absolute arbiter of the reality of phenomena.

The negotiation over the validity of this challenging second meta-analysis, borrowing Gilbert and Mulkay's terms, used both empirical and contingent talk. The debates that made use of empiricist talk concerned issues related to: the use of meta-analysis as a device for establishing the “reality of *psi*” (Schmeidler and Edge, 1999: 337-339); the choice of the statistical measure (effect size or probabilistic significance of the *psi* effect) (Schmeidler and Edge, 1999: 339-349); the selection or omission of particular studies from the analyzed pool (Schmeidler and Edge, 1999:349-359); the consequent construction of what constitutes a “proper” *Ganzfeld*; and, therefore, the justification of what studies should (or should not) be included in the pool (Schmeidler and Edge, 1999:360-372). These issues were explored at length both in the aforementioned issue of *The Journal of Parapsychology* (1999, vol. 63, 4) and in later published journal articles (critiques and responses) about the meta-analysis and *Ganzfeld* database (Bem et al., 2001; Storm and Ertel, 2001; Milton and Wiseman, 2001). The debates that made use of contingent talk appeared in more informal contexts (e.g. e-mail discussion forums dedicated to parapsychological research, or correspondence exchanges published in *The Journal of Parapsychology*). These contingent arguments were generally concerned

with the internal state of disbelief in *psi* of the authors of the second meta-analysis. Three extracts from this exchange follow as brief examples of this debate. The first extract is a critical comment by a prominent member of the field; the second is the reply by one of the authors of the meta-analysis; the third extract is the response by the critic:

“Controversies have erupted when there is obvious self-interest in the methodology set up to test a scientific question or in the interpretation of research results (Ben, Palmer and Broughton, 2001; Milton and Wiseman, 1999, 2001; Storm and Ertel, 2001)” (Zingrone, 2002a:8);

“There are many different reasons why the people engaged in parapsychological research disagree with one another. They may hold very different theoretical positions, base their judgments on different types of evidence (...). However, any statement suggesting that a particular strand of research is driven by “obvious self-interest” is a serious accusation (...)” (Wiseman, 2002: 211);

“The only reason I can propose to explain why Milton and Wiseman steadfastly deflect criticism of their own work, presenting it as unproblematic and unflawed, is that they are unshakably convinced of their conclusion and may well have been convinced of it long before they even began their work.” (Zingrone, 2002b:215).

This sequence illustrates the kind of contingent argumentation that was involved in this controversy. The accusation of “self-interest” (what would probably be called a “skeptical agenda”) is used here as an overriding argument against the empirical validity of the meta-analysis. In the first extract, the accusation of stake or interest is an effective way of discounting the facticity of its outcomes (Potter, 1996a:123). Zingrone uses here a statement suggesting the authors have an “axe to grind” (Potter, 1996a:124) in relation to the replicability of the effect, therefore, are not disinterested scientists engaged in the neutral enquiry of nature. The defence by the author of the meta-analysis in the second extract builds up the forcefulness of this accusation. Wiseman works up the seriousness of the suggestion of self interest by clearly distinguishing it from the normal situation of having an opinion on an issue. In doing so, he is also making rhetorically relevant his reasonableness as a researcher who realizes that if he were to make a claim to his (or any) scientists’ complete neutrality, it would not be convincing. In the third extract, it is possible to see the escalation of the “self-interest” suggestion and the further undermining of the second meta-analysis’ outcomes: it is about the confirmation of the authors’ inner convictions about the reality of the *psi* effect, not about the discovery of a valid scientific outcome. It is interesting to see that Zingrone constructs the authors’ defence of their meta-analysis as a series of actions that are excessive – they “steadfastly deflect criticism”, they present their work as “unproblematic and unflawed”, they are “unshakably convinced”. In constructing this extreme case of defence (Pomerantz, 1986), Zingrone persuasively constructs Wiseman’s defence as not only unreasonable, but also conspicuous in its unreasonableness. She makes available the idea that, even if experimenters are generally “allowed” to hold opinions and beliefs (as Wiseman pointed

out in his previous response), the two authors of the meta-analysis acted beyond the threshold of what is acceptable. Through this contrast, Zingrone constructs a norm of acceptable opinions and beliefs, against which she rhetorically “instructs” the reader to view Milton and Wiseman’s position as extreme and out of the ordinary. In this sense, Zingrone “cuts out” (Smith, 1979:39) their position from the realm of reasonable scientific views.

This exchange is a good example of the use of contingent argumentation in a dispute over the status of a scientific outcome. Appeals to experimenters’ motivation or belief are relatively common in parapsychology, particularly when addressing issues such as the variability (or lack of replication) of *psi* outcomes between different experimenters. The next section explores this categorization of researchers according to their position in relation to the *psi* claim. As we shall see, experimental parapsychology seems to have embraced experimenter belief or motivation as an area of empirical interest, one that might offer a reliable explanation for the variability of *psi* outcomes (Palmer, 1983b:49; 1997:110).

2.3.2 Belief in *psi* and its explanatory role of the variability of *psi* results.

In the *Ganzfeld* debate above, Zingrone’s accusations of “self interest” (2002a:8) and of being “convinced [of a result]” (2002b:215), touch upon a central issue in parapsychology, that of the demarcation between skeptics (i.e. opponents of, or disbelievers in, the *psi* claim), and believers (i.e. proponents of the *psi* claim). These labels are frequently used in dialogues and publications by parapsychologists as a short hand for describing researchers’ position on the question of the existence of *psi*. According to Truzzi (1987b:5), this constructed divide has had the consequence of building up parapsychology as a dysfunctional polarized scientific setting, as the value of empirical claims (in the form of p-values or effect sizes) is undermined by rhetoric about the “kind” of researcher (i.e. skeptic or believer) that produces or assesses such claims. This polarization divides researchers within parapsychology, and rhetorically creates a chasm between them that is difficult to cross by using arguments based upon “experimental evidence”. Furthermore, in common constructions of experimental research, the experimenter is removed from the production and interpretation of outcomes (Potter, 1996a:153). These can then be put forward as the untouched product of an experimental system, and thus, factual and universal. In parapsychology, however, the production and interpretation of *psi* outcomes are regularly put through the “explanatory” filter of the experimenter’s belief in *psi*, and offered as a possible explanation for the variability of experimental outcomes.

One particularly interesting articulation of this has been referred to as the experimenter effect (Palmer, 1989a:1). The class of phenomena designated as experimenter effects (such as observation, error,

expectation or style of interaction effects) has been investigated at length within experimental social psychology (e.g. Rosenthal 1969, 1976). Its relevance in parapsychology, however, has added another possible mediating variable to the class of effects attributable to the experimenter, that of *psi*. Directly related to the researchers' state of belief or motivation, the novelty that the *psi* experimenter effect hypothesis introduces is that of a new route of communication or influence of these beliefs or expectations. It suggests that, beyond the sensory social influence of the experimenter upon subject or outcomes, there might be a psychic influence of the experimenter, not necessarily on the subject, but on various elements of the experimental system, such as the selection of targets or the randomization sequence (Blackmore, 1985:429; Palmer, 1986b:214). The term *psi* experimenter effect was first introduced by Kennedy and Taddonio (1976) to refer to "unintentional *psi* which affects an experimental outcome in ways which are directly related to the experimenter's needs, wishes, expectancies, etc." (p.5), and it is considered by Palmer (1997:110) as one of the most reliable effects in parapsychology. Although in this initial definition the term "belief" did not feature, in later developments of the *psi* experimenter hypothesis it was identified as the main factor related to the variability of outcomes. According to Palmer (1986b):

"one thing we know about most, and probably, all established *psi*-conducive experimenters (PCEs) is that they are believers in *psi*. This is hardly surprising, considering that even if they started out as goats they would soon be converted by the success of their experiments, but it is likely that many of them started out as sheep as well" (p. 215).

This excerpt illustrates the particular and problematic nature of the experimenter *psi* hypothesis to parapsychology. One can see in it the use of categories of belief like "goats" and "sheep", i.e. disbelievers and believers in the *psi* claim. This categorization system was first introduced by Schmeidler and McConnell (1958, cited in Akers, 1984) as a correlate of subjects' performance in an ESP test. However, as it is possible to see in Palmer's contribution above, it has become reified into almost natural categories, traits or states that researchers have, and which cause certain experimental outcomes. Furthermore, the author asserts that there is a link between belief in *psi* and the qualities of a *psi*-conducive experimenter. In doing so, he constructs categories based upon the experimenters' conduciveness of *psi* (i.e. *psi*-conducive and *psi*-inhibitory experimenters). The reification of the experimenter's conduciveness into a factor within the experimental system is then used to account for the variability of results among experimenters, and the difficulty of broadly based replicability (Palmer, 1989a:1). The problem arises precisely here, where the replicability of results in parapsychology is related to the "unconscious effect of the experimenter", or *psi* mediated experimenter characteristics. The central claim of empiricism (of independence between outcomes and experimenter) is thus compromised. Linking success in obtaining results that support the *psi* hypothesis to the experimenter's belief (or expectations about) the effect is particularly problematic. If the reality of the *psi* claim is, in some way, bound up with the relatively unknown ability of the

experimenter to “draw it out” or to “conduct” it into measurable outcomes, a main feature related to the construction of the validity of experimental outcomes is seriously put at risk – its out-there-ness (Woolgar, 1980). Linking *psi* as an experimental outcome to the experimenter thus undermines the scientific validity of the outcome. Indeed, the use of the *psi* experimenter effect as a possible account for the variability of results adds to the controversy of the *psi* claim. It removes not only the empirically desirable passivity of the experimenter, but also the independent role of the data in scientific statements (Gilbert and Mulkay, 1984:42; Potter, 1996a:153). The *psi* experimenter effect interestingly points out the potential role of the experimenter within the experimental system, and provides a possible way of understanding variability of results. However, the danger of considering such a hypothesis, regardless of its reality, is not only that it is difficult to test (Alcock, 1981:124; Blackmore, 1983: 191; 1985:429; Palmer, 1989b:1), but also that “there seems to be a slippery slope from using perfectly reasonable arguments about possible experimenter effects to making the whole notion vacuous and untestable” (Blackmore, 1985:429). The *psi* experimenter effect hypothesis is therefore not particularly helpful in explaining the variability of the data, as it invokes variables and processes that, in themselves, need explanation (Truzzi, 1981:303), and thus seems to create more controversy around the *psi* claim than it resolves.

2.4 The construction of anomaly and *psi* in parapsychology.

Parapsychology has so far been characterized as an experimental field of research dedicated to the investigation of phenomena described as anomalies to currently known perceptual, cognitive and physical human capacities. On the one hand, parapsychology appeals to regular methodologies, concepts and contexts of scientific practice that allow for its construction as a “regular” field of scientific action (Collins and Pinch, 1979:241; McClenon, 1984:10). On the other hand, it focuses its regular investigative practices on claims that lie low on a continuum of “known” or “explained” phenomena, and remain effects without a consensual explanatory context. They are thus constructed as anomalous processes of communication or influence in themselves. Furthermore, the characterization of parapsychology as a field of controversy within itself, points up the difficulty of the consensual assessment of outcomes and claims within it. Within this context of research into anomalous phenomena, and uncertainty about them (including their very existence), this section will also ask the following questions: “how is it that parapsychologists construct what anomalous outcomes are?” and “how is it that these are made sense of in relation to *psi*?”.

Parapsychology is a particularly complex and challenging case in which to investigate the construction of anomalous outcomes. It therefore requires close attention to the possible meanings

that the word 'anomaly' might have for a parapsychologist. One available meaning of anomaly in the parapsychological literature is that of results that are actively sought in an experimental system. In Palmer's (1983b) definition of *psi* cited above, the author talks about "a statistically significant departure of results from those expected by chance under circumstances that mimic exchanges of information between living organisms and their environment" (p. 54). *Psi* is thus characterized as a statistical deviance from the outcomes that would be expected if no anomalous process was in action. In an ESP test, for example, the number of instances of correct identification (henceforth, hits) of a remote and unseen (in the "normal" sense) target is different from the number of hits that would be expected through chance alone. Such an outcome might be seen as one meaning of anomaly for parapsychologists, in which *psi* is identified with it, and vice versa.

However, the same definition of *psi*, as a statistical deviance, is problematic in its ambiguity. After all, as parapsychologists work with experimental models, such as the *Ganzfeld* procedure, it might well be expected that through their repeated use, boundaries of expected outcomes would be built up. This brings us to a second meaning that anomaly might have in parapsychology, that of anomalous *psi*. The repetition of experimental processes, and the use of models like the *Ganzfeld*, provide parapsychologists with boundaries either in level (the magnitude of the effect) or direction (the quality of the effect) with which to analyze, frame and make sense of experimental outcomes. Level boundaries seem to have been created with the repetition of procedures like the *Ganzfeld*. Through such repetition, outcomes have been accumulated and analyzed, and despite controversy and the variability of results, patterns have been constructed that, in turn, generate expectations for parapsychologists (even if these are temporary, working expectations, neither consensual nor robust). With such expectations about level of outcomes, how might an experimenter make sense of an outcome whose magnitude is outwith the boundaries of expected magnitude? Watt (2002) provides an example of how an unexpectedly large effect was made sense of, when addressing this issue in a journal article about a study of experimenter effects (in a remote facilitation of attention procedure¹⁰):

"(...) it is interesting to note that we made substantial efforts to identify whether the *psi* results in Study 1 were artifactual was because we felt the apparent *psi* results were unusually strong, and were therefore *unexpected*. If the remote helping effect had been of a similar magnitude to the previous two remote helping studies, we might have reacted quite differently. In that case, we might have regarded our results as successfully replicating the effect size of the previous two studies, and we might not have made any further effort to check whether the results were valid" (69-70, italics in the original).

¹⁰ This type of experimental protocol hypothesises that a person can influence another's capacity of focusing their attention, at a distance. Therefore, two people are put in separate rooms. While one (helper) tries or not in random epochs to help the other focus on an object, the other's (helpee) task is to signal behaviourally (with a button press) every time her/his attention diverges from the object. Thus, according to the hypothesis, in the epochs in which the helper "helped" there should be *fewer* button presses, when compared to the epochs where the helper did "not help".

What is interesting about this example is the suspiciousness with which a very strong result was received. Instead of being put forward as “good news” for the field, and as compelling evidence for *psi*, Watt's outcome was suspected of being an experimental artifact, and therefore not valid as evidence. Indeed, the same point was made by Braud (1983), commenting on parapsychologists' distrust in relation to “exceedingly significant findings” (p. 161). Watt's report is an illustration of the construction of the category of unexpected *psi*, what might be seen as a second meaning of anomaly in parapsychology, here associated with the category of artifact.

In addition to these level expectations, there are other boundary making aspects to experimental outcomes in parapsychology, conceptual boundaries brought forward by experimental models. The *Ganzfeld* procedure, for example, is based upon what was introduced above as the noise-reduction model, as Bem (1993) put it, a “working model of *psi* in which *psi* mediated information is conceptualized as a weak signal that is normally masked by internal somatic and external sensory noise” (p. 102). Thus, according to this model, the previously unspecified statistical deviance is hypothesized to be a particular deviance from chance expectation. The expectation that the model creates is that, in a *Ganzfeld* state, the probability of a subject identifying remote targets is greater than what would be expected by chance or guessing. The deviance, therefore, “should” be in the direction of an increase or enhancement of the probability. There are, therefore, direction boundaries or expectations, which are closely related to the quality of the effect (i.e. what it is thought to be like). Yet even when a subject is consistently wrong in the identification of the targets (i.e. when s/he does worse in a sequence of target identification than s/he would have done by chance), this “deviation” counts as evidence for *psi*. This is known in experimental parapsychology as *psi*-missing, which describes an experimental event in which “ESP correspondences are consistently and meaningfully wrong” (Carpenter, 1977:205). The concept represents an elaboration of the dependent variable in ESP experiments from *psi* vs. chance, into *psi*-hitting or *psi*-missing vs. chance. According to Carpenter (1977), the concept of *psi*-missing was probably, at first, a way of “making do with an apparently bad situation” (p. 206), as an effect but not the desired one. However, *psi*-missing has since moved on to be absorbed into the primary dependent-variable construct for parapsychologists. The *psi*-hitting/*psi*-missing construct is currently viewed as a dimension along which the subjects' performance may shift, and which is contingent to potentially specifiable and experimentally controllable factors (Carpenter, 1977:206). For those who venture beyond the experimental operationalization, *psi*-missing suggests “some kind of unconscious censoring of the *psi* information” (Beloff, 1977:19). This is one way of making sense of this type of experimental outcome. However, short of the possible causal attributions to the subject's behaviour towards *psi*, to the aforementioned determining role of belief, or to the “trickster” nature of *psi* (Hansen, 2001), *psi*-missing identifies events that are taken as significant within the experimental system and model assumptions. Although given a special kind of meaning, *psi*-missing is nonetheless described in the parapsychological literature as a *psi* effect. These *psi* effects violate the inherently probabilistic operationalization of *psi*

within the *Ganzfeld* model, and could thus be understood as anomalous *psi*. A *psi*-missing outcome, as a strict probability value, is anomalous since it violates the *Ganzfeld* model's assumption of facilitation of accuracy in the identification of targets. However, rather than being expressed as anomalous outcomes, they are described as one of the possibilities of the phenomena that co-exist under the label of “experimental *psi* effect” (Carpenter, 1977: 206), or, following theoretical elaboration, as a translation of the subjects’ avoidance or suppression of *psi* (Milton, 1988:31).

The unexpectedly strong outcome, and *psi*-missing, illustrate outcomes in parapsychology that could potentially be constructed as anomalous, since they challenge either a pattern of level of results, or an expected or modelled property of the phenomenon. However, the former was located within the category of artifact and thus dismissed, while the latter was given a meaning and eventually built into the operational definition of “experimental *psi*”. In this process of dealing with anomalous outcomes, parapsychology is no different from any other field of experimental psychology, even if the level and conceptual boundaries are not consensual. The interesting questions, however, are: how is it that these distinctions are made?; and why is it that one is dismissed as an artifact and the other included?

2.5 Constructing scientifically ideal parapsychology.

As it was possible to examine in Chapter 1, the description of parapsychology as a field of controversy is by no means new (e.g. Collins, 1983), and the controversy over the variability of outcomes, and specifically over the *Ganzfeld* meta-analyses, is not yet resolved. This ongoing characterization of parapsychology as a research context begs the question of what, if anything, might lead to a resolution of the controversy about the reality of *psi* and the legitimization of the field. The answers to these questions have been many and varied, and have come from both parapsychologists and critics of the field. They have presented steps towards, crucial experiment and strategies or recommendations about what scientific parapsychology “should be like” (e.g. Beloff, 1980; Hyman, 1980; Rao, 1981b; Hövelman, 1983; Krippner, 1983; Bunge, 1987:576; Pinch, 1987; Stanford 2003). Following a general reasoning based on inductive logic, i.e. with the aim of defining *psi* and associated theories as being highly probable based on the available evidence (Lakatos, 1978:3), they have listed criteria other than that of a repeatable outcome that would establish *psi* as a valid effect and parapsychology as a viable field of science. These include: a) a standardized purely descriptive terminology of the phenomena; b) one or many consensually accepted explanatory systems; c) the generation of patterns of outcomes, predictions, hypotheses; d) the devising of a crucial (also repeatable) experiment or demonstration of the effect; e) the formation of a coherent research programme; f) which would, ultimately, result in models that would allow the application of the knowledge about these effects.

These suggestions point towards an idealized model of “scientificity”, almost a wish-list that would demonstrate the robustness of the phenomena under study and legitimize parapsychology as a field of research, in relation to normative ideas of science. Such epistemic invariants put forward a view of scientific inquiry as it were inherently distinguishable from what is not scientific inquiry (Gieryn, 1983:781), and list what it would take for parapsychology to be part of the former category.

Again, as it was possible to see in the last chapter, discussions of these legitimizing criteria have enjoyed particularly interesting contributions from the field of social studies of science. Other contributions by critics of the field are also valuable. For instance, the expression “extraordinary claims require extraordinary proof”, proposed, among others, by Truzzi (1978:11; 1981:298; 1987a:614), expresses the Humean idea that any claim that seems to its audience to go beyond what is common or ordinary, demands, in order to be accepted, evidence that goes beyond (in quantity or in quality) what is required of common claims. This consideration points up two important issues in relation to the construction of parapsychological claims: a) that the extraordinariness of a claim or an effect is relative to an existing framework of common, explained claims or events of a scientific community; and b) that, therefore, the amount and quality of evidence demanded for the acceptance of extraordinary claims will significantly vary depending on different starting points for each scientific community (Truzzi, 1978:14; 1981:298). The significance of this assertion is in its implication that the use of evidence to establish *psi* as a “real” effect is necessarily bound up with what seems to be plausible and reasonable (Truzzi, 1981:299; Pickering, 1980:62) to a particular community, which is consistent with Pickering’s (1981) analysis of the controversy over the magnetic monopole, described in the last chapter.

Within the parapsychological community and literature, one frequently finds the claim that the methodological and statistical rigour of experimental work in parapsychology (e.g. the focus on the control of fraud and [known] sensory leakage) fulfils and exceeds the requirements placed upon most other work in the directly comparable field of experimental psychology (e.g. Truzzi, 1978:11; McClenon, 1984:10; Broughton, 1987:575; Hansel, 1987:590; Rao and Palmer, 1987:541; Zingrone, 2002:23). Related to this idea, it is also frequent to find the proposition that the assessment of the validity of claims in parapsychology (even if anomalous and theoretically uncontextualised) should be made on the sole basis of the assessment of how strict its experimental and statistical procedures are. In this sense, doing science is, to some extent, translated into doing methodology (Proctor and Capaldi, 2001:760). This view was clearly articulated by Morris (1987:247), a central figure in the field, as he proposed that the issue regarding the scientific status of parapsychology should be principally dependent upon an assessment of methodological symmetry, i.e. the comparison between parapsychological methodological practices and those employed in other fields, consensually considered to count as science. He argued that the symmetry of status between fields should be articulated in terms of the symmetry in their practices: as long as the research practices are equivalent,

so should be their evaluation in terms of the scientific quality of their claims. Overall, the demand for parapsychologists to transform an admittedly uncertain system into an all-predicting, all-controlling experimental operationalization is deemed, by some in the field, to be unreasonable at least, and impossible at best (Rao and Palmer, 1987:542). On the other hand, Truzzi (1978:12) considers that, far from being “unreasonable”, the demand of increasingly tighter experimental rigour to which parapsychologists often respond (including the founder, J. B Rhine) is a direct consequence of the controversial nature of the claims and the experimental context in which they are being made.

Again, returning to issues dealt with in the last chapter, sociologists of science have shown that the factuality of *psi* and the legitimization of parapsychology is a process of construction and negotiation of status. They dispute the very idea that there are clear, real demarcation criteria (like “scientific rigour” or “an x-cut off point of statistical evidence”) that separate a real effect from an artifact, or distinguish between what constitutes science and what does not. These assessments and demarcations are thus considered to be situated outcomes of processes of negotiation and agreement between participating scientists. In parapsychology, the decision about the reality of *psi* is not merely the result of a “crucial experiment”, the “repeatability” of the effect or the “empirical verification” of a model. As the *Ganzfeld* debate above illustrates, the decision about the reality of *psi* will be in the construction, acceptance and agreement among the main players in the field of the crucial experiment as being crucial, of the replication as being a replication, or of the empirical verification of a model as being a verification. These issues and many others concerned the “Zetetic perspective” (Truzzi, 1978:2) within parapsychology, developed during the 80’s. As it was already addressed in Chapter 1, the aim of this movement was to create, within the field, a habit of “continuous dialogue between the proponents and critics of claims of the paranormal, concerned mainly with enhancing communication, (...) interested not only in the adjudication of the claims, but with the *sociology and psychology of the disputes themselves*” (Truzzi, 1978:2; italics added). It enjoyed the contributions of influential scholars from the areas of parapsychology, SSK, psychology and philosophy of science, and encouraged a motivated and focused examination of the controversies in themselves, interested specifically in the issues that fired up the heated disputes within the field, than with the final outcomes of these disputes. Its journal, *The Zetetic Scholar*, was thus the forum of discussions that sought to understand how parapsychology was or failed to be constructed as a scientific field, its object as a real phenomenon or its practices as accurate practices. The issues raised above and that concern this thesis, are very much in line with the Zetetic interest in exploring these controversies.

Summary.

In summary, parapsychology has been characterized as a field of internal controversy. Parapsychologists deal with anomalous phenomena a) with uncertain properties and using terminology that is not consensual, b) characterized as “reluctant” scientific objects, c) by using disputed criteria of evidence for their existence, and d) which are not consensually “real”. The distinction between what is *psi* and what is not, and the further distinction between *psi* and an anomaly, are complex issues in the parapsychological literature with particularly diverse outcomes. Experimental parapsychology is thus a context within which the boundaries of what counts as good practice, what is the role of ‘belief’ in experimentation, what constitutes an outcome supporting the reality of *psi* or an anomaly are, currently, actively disputed constructions, and therefore, a privileged context in which to examine and reflect on how these properties are discursively achieved.

The study's methodology.

Introduction.

This chapter sets out the methods used in the thesis. It begins (section 3.1) by describing the main aims of the thesis and methodological issues relating to the type of discourse analysis to be used, and the form of data collection. It then describes in detail how data was collected (section 3.2), i.e. the development of the interview schedule, the criteria for recruitment of participants, and the interview contexts. Finally, it explains the specific details of transcription, coding and analysis of data (section 3.3).

3.1 Aims and methodological issues.

3.1.1 Research aims.

This thesis is an examination of accounts of research in experimental parapsychology, given by parapsychologists themselves. As Chapter 1 argued, the unique position of parapsychology means that parapsychologists are particularly experienced in building up accounts of the properties of their properties of their field as a field of warranted scientific practice on a (disputed) real scientific object. Consequently, their accounts of their field, research practices and object would be expected to involve particularly insightful discursive processes designed to robustly construct what they do as doing science. Given this context, the aims of this study were the following: a) to explore these parapsychologists' accounts of their field, research practices and outcomes; b) to analyse how these accounts attended to normative versions of what counts as science; c) to analyse the discursive resources used in these accounts to achieve their factuality; and d) to analyse how these accounts were

used to constitute the properties of their field, research practices and object as being in line with normative versions of what counts as science.

3.1.2 Methodological context and of the form of DA used in the thesis.

This section provides a possible overview of DA as an approach in psychology, and describes and justifies the particular DA approach used in this study.

3.1.2.1 Characterisation of DA as a theoretical and methodological context.

As Chapter 1 explained, as part of the “linguistic turn” in social studies of science, the analysis of scientists’ discourse was introduced by Mulkay and Gilbert as a radical methodological and conceptual shift within the Sociology of Scientific Knowledge that: a) abandoned the aim of providing realist accounts of scientists’ actions and beliefs, i.e. the way things are; b) proposed that language is used variably, purposively and is contextually bound; and c) gave absolute priority to the analysis of how scientists talk and write up their actions and beliefs. Gilbert and Mulkay’s seminal work “*Opening Pandora’s Box*” (1984) is the exemplar of this proposed shift. It begins with the realisation and acknowledgement of a fundamental problem in previous SSK work, namely: a) the use of scientists’ accounts of their thoughts and actions as literal descriptions of reality; b) the variability in scientists’ accounts as a problem that needs to be eliminated; c) the failure to take notice of the way such variability challenges the representational validity of accounts, as it reflects the functional specificity of particular accounts in particular contexts; and d) the failure to recognise accounts in themselves as social practices the analysis of which is fundamental to social studies of science. The proposed methodological shift provided not only a methodological solution, but also the inspiration for a new way of doing social sciences (Gilbert and Mulkay, 1984:191), which focused on how people produce descriptions of who they are, what they do, what they think, feel, etc that are variable and functional, and which understood such descriptions as social actions (rather than representations). Gilbert and Mulkay’s definition of scientists’ discourse includes all sorts of talk and texts in all kinds of contexts, from journal articles to conference presentations, from discussions at the laboratory bench to those around a coffee table (Gilbert and Mulkay, 1994:14). They put forward that scientists’ discourse is variable in contextually appropriate ways, and that different versions are achieved by using different “interpretative methods”. DA is thus defined as “the attempt to identify and describe regularities in the methods used by participants (...) through which they establish the character of their actions and beliefs in the course of interaction” (p. 14). These methods were termed “interpretative repertoires”, i.e. recurrently used systems of terms, stylistic and grammatical forms,

metaphors and figures of speech which are used for depicting and evaluating actions, events, and thoughts in contextually and functionally appropriate ways (Potter and Wetherell, 1987:149). Their analysis of scientists' accounts identified two types of repertoires, the 'empiricist' and the 'contingent', which provide us with a) a way of understanding how, and with what purpose, scientists produce different versions of science, and b) a demonstration of how these accounts are inherently functional rather than descriptive, i.e. they achieve different rhetorical and interactional purposes (Gilbert and Mulkay, 1984:73).

Conceptually, DA as a methodological and epistemological shift was an influential development in SSK taken up by various authors (as outlined in Chapter 1). Despite sociology being where DA was first introduced, it was not taken up more widely across the discipline. Conversely, in social psychology, DA found a group of receptive researchers, namely Edwards, Middleton, Potter and Wetherell (Wooffitt, 2005a:48). DA's original critical orientation has been maintained throughout its development up to this day, and was taken up by psychologists as a central part of a strong critique of existing theoretical and methodological assumptions of experimental and social psychology (Wooffitt, 2005a:56). DA was thus taken into psychology as a new way of studying topics that have long been the focus of the discipline's attention, which proposed a change in the ontological status of social and cognitive phenomena (such as attitudes, attributions and memories) as socially organised products through everyday discursive practices (Hammersley, 2003:757). This new way of analysis put forward language as being: a) constitutive of these previously defined social and cognitive psychological entities; b) functional in itself; and therefore c) central to our understanding of our social psychological life.

However, as, among others, Potter & Wetherell (1987:6), Wooffitt (1992:51; 2005a:39) and Potter (2004:607) have pointed out, even within psychology the term DA corresponds to a variety of perspectives concerned with the study of linguistic practices in talk and texts, the boundaries of which are rather hard to define. These include a variety of research traditions, from approaches concerned with the study of 'discourse' as the study of the structure of sentences (which find their niche in linguistics), to research focusing broadly on 'discourse' as sets of statements available to groups or institutions which constitute subjects, objects and social practices (inspired by the work of the post-structuralist historian Michel Foucault). DA has evolved as a methodological and conceptual alternative within psychology to include a significant diversity of discourse analytic research, in terms of both the treatment of data and the range of topics covered (Wooffitt, 2005a:78). Potter (2004) sums it up when he writes that DA "can be seen as a contested disciplinary terrain where a range of different theoretical notions and analytic practices compete" (p. 608). Despite this "contested" nature, DA remains consensually in the literature as a powerful proposition, engaging at a theoretical, methodological and conceptual level with mainstream psychology (Potter, 2003:784). It carries with it (to those who accept it as a useful and insightful form of analysis) the consequence of forcing one to

rethink the way that psychology has so far treated discourse – paradoxically both ubiquitous yet overwhelmingly invisible to research (Potter, 2004:607). Whilst the discursive project in psychology has been overtly critical of traditional approaches within the discipline, it has not been without its own critics. For instance, in a debate between Hammersley and Potter (2003), Hammersley pointed to the ambiguous status of DA as method or paradigm, arguing that in neither case does DA meet the requirements of theoretical coherence and methodological sustainability. Nevertheless, DA as an empirical project in psychology has offered more than a critical appraisal of traditional methodologies. It has also tackled many of the traditional topics of cognitive or social-cognitive psychology, and has introduced the study of accounts as a fundamental object of research in psychology.

DA has been primarily concerned with the investigation of the ways in which accounts in talk or text are: a) action-oriented, b) situated, and c) constructive (Potter 2004:609-610). Firstly, by ‘accounts’, discourse analysts broadly mean any section of talk or text in which versions of events are formulated, or opinions or thoughts are described (Wooffitt, 2005a:79). Secondly, by ‘action-oriented’, as Potter (2004:609) explains, DA authors mean that they take discourse to be put together to perform actions (as part of broader practices). The central concern of this analysis, therefore, is to explore and make visible what discourse is doing. This concern, as both Potter (2004:609) and Wooffitt (2005a:78) have pointed out, originates in the separate empirical tradition of Conversation Analysis (henceforth CA), which Wooffitt (2005a) describes as the study of “the social organisation of activities conducted through talk (...) [seeking] to discover sequential patterns of interaction, and to explicate the web of normative expectations and assumptions which perform and underpin the production of those sequences” (p. 79). Thirdly, by ‘situated’ authors mean that accounts need to be examined as: a) being embedded in particular sequences of interaction, b) making relevant particular institutional settings and identities, and c) being rhetorically put together, i.e. put together in ways that render them effective accounts for specific purposes. While the first and second of these concerns have their origin in CA, the third, Potter (2004:610) argues, is particular to DA. Finally, by “constructed”, discourse analysts mean that discourse is both constructed by words, expressions or rhetorical devices, and constructive of versions of the world, objects, individuals, psychological entities or groups (Potter, 2004:610). These concerns are central to many of the forms of analysis into which DA has evolved within psychology.

Central to the account of the development of DA in psychology is Potter and Wetherell’s early work “*Discourse and Social Psychology*” (1987). These authors articulated an introductory and critical examination of traditional methods and constructs in social psychology (such as attitudes, attributions and social identities). In their examination of white New Zealanders’ racist discourse, Potter and Wetherell articulated how DA might be practically deployed in the study of how racism can be discursively obscured and legitimated, and thus, how this form of social relations is constructed and perpetuated. This study was therefore a clear example of how DA can make visible the constructed

and discursive nature of previously defined cognitive, enduring, internal characteristics of individuals or groups. The authors' approach shared with Gilbert and Mulkay an inclusive definition of what was meant by 'discourse', the use of the notion of interpretative repertoires, and an analytical approach that focussed upon mapping variability of discourse across contexts. However, they added two important features to DA as a form of analysis. First, thematically, the authors placed great analytical emphasis on the examination of how ideologies are embodied in, and reproduced as, available ways of constituting individuals, societies, institutions, etc. in everyday discourse (Wooffitt, 2005a:51), and how they are perpetuated in social relations (e.g. asymmetry and inequality between social groups). Second, methodologically, Potter and Wetherell's analysis drew heavily upon conversation analytic work, and placed much of their emphasis on examining the way accounts and descriptions display an action orientation (Wooffitt, 2005a:78), i.e. on the explication and description of how discourse does things. These two innovations are echoed in later developments in DA. The development of DA in psychology is more easily described in relation to the thematic focus of the analysis. Both Potter (2004) and Wooffitt (2005a) map out a range of themes that discourse analysts have covered since the late 80's: a) fact construction; b) psychology in practice; and c) ideology and political and social practices (though both authors also present a disclaimer to this sort of typology, as most discourse analytic research touches upon and draws from different strands and themes). These themes will now be addressed in turn.

The first strand of research – fact construction – is one of the major themes in discourse work and is concerned with the way accounts or descriptions of reality (individuals, objects, events) “are constructed and made (to seem) objective and factual, or, conversely, the way (apparently) factual versions are undermined as partial, distorted or interested” (Potter, 2004:611). The analysis of fact construction includes a number of themes, such as accounts of troubles in relationships (Edwards, 1995), accounts of the paranormal nature of unusual experiences (Wooffitt, 1992), and accounts of a political controversy by journalists and politicians (Potter and Edwards, 1990). This strand of research has favoured using as its data talk and texts in which competing versions of events are available, or the controversial status of the topic of talk is made relevant. The analysis of fact construction is particularly influenced and closely related to another field of empirical research, rhetorical psychology, specifically associated with the work of Michael Billig (the reference work “*Arguing and Thinking*” was also published in 1987). While the rhetorical orientation of accounts has been one of the main concerns of DA, rhetorical psychology has focused less on the action orientation of discourse than on its argumentative and persuasive nature (Wooffitt, 2005a:96), and on how rhetoric should be examined as a pervasive feature of the way people interact and arrive at an understanding, i.e. think (Potter, 1996a:106). The theme of fact construction will be discussed again later when characterising the approach that was taken in this thesis.

The second strand of research – psychology in practice – has taken an increasingly important place in discourse analytic research (Potter 2004:611; Edwards and Potter, 2005). It is concerned with the “respecification of psychological notions in terms of their role in talk and texts” (Potter 2004:611). This is presented as a new form of psychology, which treats the discipline itself as a discursive enterprise, and its products, instruments and methods as discursively constructed (Harré and Stearns, 1995:3). Discursive Psychology (henceforth DP) takes the current central construct of the discipline, i.e. cognition, and reworks it as discursive action (Edwards and Potter, 1995:88). Reports of cognition (e.g. memories, attributions, thoughts) or of mental states are thus treated not as internal features or processes accessible through language, but as actions oriented to interactional and inferential concerns, such as the construction of fact, the inference of interest or the management of accountability (Edwards and Potter, 1995:89). DP therefore constitutes, within the discourse analytic project, a coherently defined area of study that seeks to impact upon how psychology itself has so far treated discourse (Edwards and Potter, 2005:243). Edwards and Potter (2005:241-242), the main proponents of the version of DP focused upon here [although at least three other forms of DP exist, drawing from different intellectual traditions (Wooffitt, 2005b:331)], outline three areas of research in DP: a) the respecification of standard psychological topics as discursive practices (such as memory, script or causal attribution), and the critique of the cognitive (realist and experimental) approach to reports of psychological states [i.e. of cognitivist assumptions, instruments and methods directed at the characterisation of a central, internal processing mechanism (Harré and Stearns, 1995:1)]; b) the exploration of the situated, occasioned and rhetorical uses of commonsense psychological terms (such as knowing, feeling or consciousness); and c) the management of psychological implications, i.e. how psychological states are made available and the implications they afford managed through descriptions of events, persons, etc. Methodologically, Wooffitt (2005:136) and Edwards and Potter (2005:242) note, this formulation of DP overlaps considerably with CA, as DP authors commonly use the conversational analytic examination of the sequential organisation of talk as a resource in their work.

The third strand of discourse analytic development – ideology and political and social practices – was already addressed in Potter and Wetherell’s original study of white New Zealander’s race discourse. This strand of discourse analysis is directed and engaged in a critical view of current and enduring social and political practices. Wooffitt (2005:137) presents two different traditions of DA that fulfil this remit – Critical Discourse Analysis (henceforth CDA) and Foucauldian Discourse Analysis (FDA). Both these approaches focus on how social and political inequalities, types of oppression and power are formed, legitimised and perpetuated through discourse (Potter, 2004:611). Despite sharing an analytical interest, they depart in their assumptions and aims. Whilst CDA seeks to link the analysis of linguistic features to wider social and political structures (Wooffitt, 2005a:138), FDA draws on the work of Foucault and understands ‘discourse’ as a particular concept. FDA takes a discourse to be a “a set of statements that formulate objects and subjects” (Potter, 1996a) and, broadly put, examines

how these systems of meanings reflect real power relations which are the consequence and, in turn, perpetuate, the social and economic organisation of institutions and groups (Wooffitt, 2005a:146).

3.1.2.2 Form of DA used in the thesis.

Even the general outline provided above illustrates the diversity of approaches within DA, and how, unlike other methodologies in the social sciences, this forces each individual study to specify what form of analysis was used. The main analytical concern of this thesis is that of fact construction, i.e. the analysis of the organisation of the interviewed parapsychologists' factual language. The main aim is the examination of how these parapsychologists presented their field of research, what they do and their research objects, using ostensibly factual accounts of the way things are or of what they do, and how they managed issues such as accountability, interest or bias in these descriptions. The starting point for the analytical concerns in this thesis, and its main reference, was therefore the work of Jonathan Potter's "*Representing Reality*" (1996a). This takes the point of view that when we present accounts of the world, people, events, our actions or ourselves, such accounts are aimed at presenting these things or issues as the way things 'really' are, or of what 'really' went on. In our accounts, i.e. in constructing versions of reality, we are ostensibly representing reality. In doing so, we construct reality. As was mentioned above, one of the main propositions of DA is that discourse is both constructed and constructive. Potter (2004:610) calls this a sort of "discursive constructionism", and defends the idea that it is crucial for us to understand the ways in which facts are discursively constructed to be facts, and the ways versions of reality are accepted as truthful and authoritative, while others are discounted as erroneous or merely invention (Potter, 1996a:1). As Potter (1996a:1) notes, the concern with constructing reality necessarily permeates our lives, from mundane conversations to professional activity. In particular, the practice and representation of science and its products, as Potter (1996a:17) puts it, epitomises the world of facts. There is, therefore, a clear link between the study of fact construction and the study of scientific discourse. Next, the choice of this form of DA for this thesis will be justified and located, relating it to a) SSK and Gilbert and Mulkey's discursive approach, and b) the examination of parapsychologists' discourse.

As Chapter 1 pointed out, SSK and the discursive project within it, were the points of departure for this thesis. SSK's concerns were centred on an understanding of science as "interestingly and constitutively social, all the way to its technical core: scientific knowledge itself [has] to be understood as a social product" (Pickering, 1992:1). Its empirical focus on instances of scientific controversy aimed at displaying the "production of consensual knowledge as the outcome of 'negotiations' between social actors" (Pickering, 1992:1). Its authors propose that these negotiations and their terms, whilst kept out of traditional and formal accounts of scientific action and belief, are similar to those found in any context of social interaction. Chapter 1 also described how the turn to

language in social studies of science offered a possible specification of how these negotiations might be explored and how one might “precisely treat scientific culture as a social product” (Shapin, 1979, cited in Woolgar, 1981:366). Gilbert and Mulkay’s discursive proposition within SSK was defined as “the attempt to identify and describe regularities in the methods used by participants (...) through which they establish the character of their actions and beliefs in the course of interaction” (Gilbert and Mulkay, 1984:14). This thesis takes from SSK and Gilbert and Mulkay the need to examine scientists’ discourse, and draws upon later developments in DA for its methodological orientation. Adding to the form of analysis used by Gilbert and Mulkay, this thesis set out to explore, as Potter (1996a) puts it, “the empirical orientation and the action orientation” of these parapsychologists’ accounts. Potter (1996a:1; 108) thus separates two (related and interwoven) sets of questions that can be asked of factual accounts. On the one hand, the analyst can ask questions that have to do with the epistemological concerns of a description: how is it that descriptions are made so that they are treated as factual? How is it that these are made to appear solid, neutral, independent of the speaker, and merely representing the way things are, or “telling it like it is” (Potter, 1996a:108)? Conversely, how can a factual account be undermined as biased or concocted, and how can an account be made to be immune from undermining? (Potter, 1996:1). On the other hand, the analyst can ask questions that relate to the action orientation of accounts: what kinds of actions are these factual accounts accomplishing? Why are descriptions used? How are they put together so that they can perform activities? These sorts of questions guided the analysis of the parapsychologists’ interviewed in this thesis. Analytical issues such as the construction of the ‘out-there-ness’ of a description (i.e. its independence of the speaker), and the management of stake (i.e. personal interest) in a description, are examples relating to the former set of questions. Issues such as the way descriptions allow speakers to enact categorisations and normalisation of contentious claims, are examples relating to the latter set of questions. As mentioned above, regarding the characterisation of DA as an analytical approach, some of these concerns and issues find their origin in CA. This thesis will therefore use references to empirical work developed in DA in relation to the study of fact construction, and use references from CA for technical and empirical support of claims.

Finally, Chapter 2 characterised experimental parapsychology as an area of controversial science, where a) its object’s reality is contested, b) the reasonableness of the existence of experimental parapsychology as a scientific practice is regularly thrown into question, and c) the factuality of any outcomes – namely those supporting the reality of psi – is a matter for heated discussion both within and outwith the field. Given this context, the thesis’ interest in how parapsychologists construct what they do as doing science, i.e. as representing something real in the world, and their field as a warranted area of research into this aspect of the real world, is particularly relevant to an analytical approach concerned with factual discourse. Parapsychologists, it is thus argued, are in the business of representing a contested part of the real world, through the construction of what they do as scientific practice and their outcomes as scientific knowledge. The analysis of their factual discourse is

therefore crucial to any understanding of how parapsychologists are able to achieve this goal or, conversely, how their descriptions can be undermined. Overall, the discursive orientation of this thesis' analysis of parapsychology can also be related to what has been called by Wooffitt (2005b) the discursive/conversational project within parapsychology (as it was addressed in Chapter 1).

3.1.3 Methodological issues concerning the use of one-to-one interviews.

Interviews have been used extensively in social research, including social psychology, largely as a method of obtaining people's views or reports of past situations (Speer, 2002:512). They have also been used extensively in DA where, rather than being seen as neutral media through which data about events external to the interview situation can be accessed, they have been seen as moments of social interaction in which issues or experiences are negotiated, worked up, undermined or avoided (Potter 1996b:134). In this sense, respondents are treated, "not as passive containers of knowledge, but as participants within the research process who construct, rather than report on reality" (Speer, 2002:511). Interviews in DA are therefore not a standardised resource for getting at ontologically independent data, as in other research paradigms in social psychology (Speer, 2002:511). Rather, the data resulting from an interview are seen as the product of a specific interaction between interviewer and interviewee. This notion of interviewing allows a) the analysis of the collaborative work through which the participants and interviewer negotiate and achieve objects, descriptions or justifications, and b) the equally significant analysis of the commonly available constructions and rhetorical strategies that permit objects to be accomplished (e.g. the formulation of a "successful outcome") and accounting strategies to be effective (e.g. the use of "extreme empiricism" in the construction of scientific competence). It was from this perspective that this study used individual interviews with experimental parapsychologists as the method for obtaining data.

While recognising interviews as interactional situations, their use as methods for collecting data nevertheless raises numerous questions. After all, there remains a crucial difference between interaction in the context of a research interview and a conversation (or text) that occurs independently of the researchers' intention to generate it. The social situation that the research interview creates is the product of the interviewer's actions, interests, categories, and ideas of what is going to be talked about, and in what sequence (Potter 1996b:135). In this sense, even in semi-structured or open interview schedules, there is a pre-existing strategy that serves a purpose beyond the actual interaction in the interview (Mazeland and ten Have, 1998:1). Interview data, in the eyes of many authors within conversation analysis and discursive psychology, are thus located in a category of contrived or researcher-provoked data, and contrasted with naturally occurring or naturalistic data (which is, in turn, deemed analytically distinct and preferable to the former) (Speer, 2002:513).

Although such a distinction is not clear cut, the artificiality of the interview situation, and the data it generates, is nevertheless often presented as a shortcoming of interviews as a method, and a reason for preferring the spontaneous properties of “naturally occurring data” (e.g. Potter, 1996b:135; Hutchby and Wooffitt, 1998:14; Silverman, 2001:159; ten Have, 2002:529).

Ten Have (2002) distinguishes between two possible ways of interpreting the argument for a “general preference” (p. 529) for the “naturalistic” properties of the latter type of data: a) as a methodological preference, because it focuses analytical interests upon what people do in specific contexts of their everyday lives (Potter, 1996b:153; Potter, 2002:540); and b) as an ontological preference, because the very ordinariness and naturalness of such data endorses the validity and widespread significance of the analysis (Ten Have, 2002:529). Speer (2002) takes issue with this latter point, as it suggests some sort of empiricist concern with the relationship between the data and a reality of ordinary activities beyond the data. Speer’s challenge is directed at a potential incoherence between the conviction that activities and objects are constructed locally in any interaction, and a concern over the respective ‘purity’ that different recordings (naturally occurring talk versus interviews) may possess. She argues that the interference of the researcher in interviews is being presented as a source of artificiality or “bias”, which seems similar to “assumptions about data and the role of the researcher that discourse and conversation analysts elsewhere seem keen to refute” (Speer, 2002:518). Although the discussion of these matters is complex, and has been extensively worked through by influential authors within these fields, an immediate upshot of Speer’s challenge, and the ensuing dialogue with Ten Have (2002) and Potter (2002), is the need to discuss openly how the interviews in this project provided a particular data set.

In this study interviews were used as the only source of data. The reasons for this choice were both practical and empirical. In practical terms, the amount of data that these interviews yielded was vast, and this was deemed enough for a meaningful analysis and thesis. It is relatively common for DA research to include various sources of data, i.e. use different contexts of discourse as instances that can be put in parallel or compared. For instance, Gilbert and Mulkay’s (1984) investigation included interviews and private letters from the participating scientists, and published research papers, i.e. informal and formal contexts of scientific discourse, respectively. The analysis of the participants’ different accounts of their methods, outcomes, theories and beliefs (within the context of a scientific controversy) between these different contexts, allowed Gilbert and Mulkay to answer their main research questions and provide us the readers with a series of insightful analytical conclusions: the systematic use of different “interpretative repertoires” in formal and informal contexts of scientific discourse. However, Gilbert and Mulkay’s methodological choice of combining sources of data was empirically consistent with the research question that guided their investigation – to discover how scientists systematically organise their accounts of their scientific beliefs and actions in ways that are contextually appropriate (Gilbert and Mulkay, 1984:14). In the present study, there was no initial

empirical interest in the variability of accounts of scientific belief and action across different contexts. Although this would undoubtedly have led to an interesting analytical process and allowed potentially wider conclusions, the initial research focus did not require this sort of combination of data. Rather, the interviews were carried out with a particular set of themes in mind, based upon a) my engagement with the group of experimental parapsychologists at the Koestler Parapsychology Unit (henceforth KPU), University of Edinburgh, and b) a critical reading of both current and seminal works within parapsychological literature. The selection of the topics and themes that the interview questions included was therefore my own, but overlapped considerably with a context of meanings and accounting practices present in other talk and texts in the community and literature of experimental parapsychology. Thus, in empirical terms, the interviews were seen as appropriate situations in which these available meanings, accounting and rhetorical practices were used by the participants. Potter (2002) has argued that the use of interviews puts participants “in the position of disinterested experts on their own and others’ practices and thoughts (...) encouraging them to provide normatively appropriated descriptions” (p. 540). While this is a potentially significant problem in the attempt to access how people talk in other contexts, it is precisely the management of these “normatively appropriate descriptions” that is so interesting for this thesis. As discussed in Chapters 1 and 2, what counts as a “normatively appropriate description” of research, outcomes or the reality of *psi* is in fierce negotiation. In this sense, these interviews allowed the opportunity for participants and myself to engage in instances of such negotiation, and provide data for the analysis of how accounts of doing science are produced in interaction.

3.2 Data collection.

The next two sections will describe the specific research process (Willig, 2001:8) of data collection and analysis. This section describes the process of data collection i.e. the development of the interview schedule, the criteria for recruitment of participants, and the contexts in which participants were recruited and interviewed.

3.2.1. The development of the interview schedule.

The data collection used a purpose-built semi-structured interview schedule (see Appendix 1). The use of a semi-structured schedule allowed for variations in the wording and sequencing of the questions and probes, in order to adapt as much as possible to the flow of conversation with each participant

(Fielding, 1993:136). The development of the interview schedule was informed by three complementary sources: a) daily interactions with the researchers and postgraduate students at the KPU, University of Edinburgh and participation in the Unit's research meetings¹¹; b) reading of current and seminal works in the experimental parapsychology literature, and c) formal interviewing of members of the KPU. The daily interaction with members of the KPU provided an ideal situation to learn about different issues or themes that were topical, fundamental or problematic for the group. Together with the reading of the field's literature, it allowed me to develop familiarity with relevant terms and jargon, which later formed a useful "vocabulary" for the interviews. Reading of the literature also provided background information about how problematic issues (later oriented to during the interviews) were treated and argued in published form. Finally, previous interviews with senior and postgraduate members of this group¹² provided an opportunity to explore relevant themes, which later became part of the final interview schedule for this study.

After a period of gathering input from these sources, the first version of the interview schedule was produced and piloted in two interviews with two senior researchers in the field. This initial draft of the schedule was subsequently altered and cut down in length, and after four subsequent revisions, the final format was piloted in one interview with a senior researcher in the field. The questions were all open-ended, and some were followed by additional probes to be used when necessary. The questions in the final version of the schedule covered three broad topics: a) conceptual models and experimental practices used by participants; b) categorisations of experimental outcomes (i.e. successful, unsuccessful and unexpected or anomalous outcomes); c) conceptual and experimental responses to unexpected or anomalous outcomes. The final interview schedule (see Appendix 1) was used flexibly in the interviews with the participants. The next section provides relevant information on these participants, and describes the process of their recruitment.

3.2.2. Participants and recruitment.

The aim was to recruit a group of experimental parapsychologists who: a) had a research background in psychology; b) came from a variety of research groups, academic or research institutions¹³; and c) varied in terms of how long they had been in experimental parapsychology. The reason for the first selection criterion was a pragmatic concern with the ability of the interviewer to understand as much as possible of conceptual and methodological issues that participants might make reference to. Sharing

¹¹ I have been a postgraduate student at the KPU since October 2000.

¹² These interviews formed part of the data set of the study that I developed for my MSc. dissertation (Coelho, 2001). For that purpose, I carried out 4 semi structured interviews with senior and postgraduate experimental parapsychologists.

¹³ Excluding members of the KPU.

a common research background in experimental psychology with the participants was assumed to help in this respect. The reason for the second selection criterion was to obtain as much diversity in accounts as possible. There is an identifiable community of experimental parapsychologists that, although not large (around 40 individuals worldwide, according to the Parapsychological Association Member Index, 2004), is geographically dispersed, with substantial groupings located in the United States and United Kingdom, and smaller groups in Germany, the Netherlands, Sweden, Austria, France and Spain, as well as in more distant locations such as Japan, Australia and Fiji. These groupings differ substantially not only in their location, but also in terms of: a) organisation (e.g. some groups have numerous senior researchers working in collaboration, others consist of one isolated senior researcher and one or two postgraduate students); b) affiliation (e.g. some groups are embedded within academic institutions, others are independent research institutes); c) thematic and methodological focus (e.g. some groups are known for a particular focus of research, such as telepathy, which sometimes overlaps with the choice of a particular experimental method, such as the *Ganzfeld*); and, d) crucial differences in either overall standing in relation to attributed paradigmatic or methodological expertise (e.g. some groups are renowned for their expertise in psychophysiological experimental methods and techniques, and upheld as arbiters of the quality of work in this area), or in relation to their position regarding the question of the reality of *psi* (e.g. some groups are generally seen as “sceptical” in relation to the *psi* hypothesis, others as “believers”, while others still are actively construed as “empirically neutral”). The international community of experimental parapsychologists, although comparatively small in relation to other communities of researchers, are therefore a highly diverse group of individuals and institutions. One of the aims of recruitment was to incorporate as much of this diversity, so far as it was practically and financially possible. Finally, the third selection criterion, regarding the period of experience in the field, was also related to the general issue of diversity.

These criteria helped constitute the sampling frame for this study, and recruitment was carried out in two different contexts, the first during two international parapsychology conferences, and the second via direct contact of individual parapsychologists. In both these contexts, the recruitment and sampling strategy was deliberate, targeting members of this pre-specified group of experimental parapsychologists, therefore, following a process of focused or judgemental sampling (Silverman, 2001:250). According to Arber (1993:73), this is a common process of selection of participants in small studies which use, for instance, repeated interviews in order to increase understanding of social processes. However, it has no probabilistic properties on which to base precise inferences about the larger category of experimental parapsychologists as a whole. There was, however, a deliberate attempt to target individuals recognised as active voices within the parapsychological community, based on their contributions to journals, conferences and email discussion lists. In the end, this study involved 20 active experimental parapsychologists, all of whom have a background in experimental psychology, providing a manageable but sufficiently rich and diverse data set. Given the size of the

field overall, this group of interviewed experimental parapsychologists includes a large part of today's active experimental parapsychologists with a psychology background, and therefore constitutes a representative group in relation to the field as a whole (Parapsychological Association Member Index, 2004).

In terms of recruitment settings, most participants were recruited at the 2002 annual international conferences of the Parapsychology Association (henceforth PA) and the Society for Psychical Research (henceforth SPR). These two conferences constitute the two main annual events of the international parapsychology calendar. Although the SPR conference is largely a British initiative, and therefore held in mainland Britain every year, the annual PA conference has always been an explicitly international event, with its location regularly alternating between Europe and North America. These conferences therefore provided access, in one location, to many experimental parapsychologists from several different countries. They also gave me the opportunity to attend a number of plenary sessions, which were elucidating in terms of the selection of topics for discussion in this thesis. The second context of recruitment – direct contact of individual researchers – therefore had the objective of completing the selection of participants, targeting UK researchers that were not present at any of these events.

The first of these conferences was the 45th Annual Convention of the Parapsychological Association (Paris, August 5-9th, 2002). The PA conferences provide an annual forum for the communication and discussion of experimental results. They are therefore geared towards experimental parapsychologists, i.e. those with a recognised affiliation (to an academic institution or independent laboratory) and research credentials, and are built up by experimental parapsychologists as the key event of the year. The participants were recruited (with the collaboration of the conference's programme chair and organising committee) in two ways: a) through an open letter to all registered delegates, which was included in the delegates' conference folders, stating i) the aims of the study, ii) the limiting condition of a background in psychological research, iii) the invitation to a one-to-one interview, and iv) confidentiality assurances (see Appendix 2); b) through a brief presentation to the initial plenary session of the conference, which included much of the same information as the letter; and c) through interaction with delegates in which the invitation was reinforced. The second conference was the 26th International Conference of the Society for Psychical Research (Manchester, 30th August-1st September 2002). These conferences are aimed at a wider section of the parapsychological and psychical researchers' communities, and include delegates who neither are experimental researchers nor have academic or institutional affiliations. Nevertheless, experimental parapsychologists also attend this conference. Once again, the recruitment of participants (with the collaboration of the organising committee) used an open letter (see Appendix 3), which was enclosed in the conference material folders and distributed to all delegates, and the invitation was reinforced through interaction with delegates.

The second context of recruitment, direct contact of individual parapsychologists, took place after the conferences described above. Four experimental parapsychologists in two research groupings (none of whom had attended the above conferences) were recruited via e-mail. Their selection was based upon their active participation in the field (e.g. in the form of journal articles and contributions to email discussion lists), and their location within the UK (for practical reasons). The text of the email (see Appendix 4) was similar to the open letters used at conferences. The response from all of them was positive, and they were interviewed in the academic institutions in which they work. Table 1 presents the list of the 20 participants (in their code designation) in relation to the country in which they practice their research and an approximation of the years of experience in the field.

Table 1 – Code designation and characterisation of the experimental parapsychologists recruited for the main study.

Participant code designation	Country of research practice	Approximate period of active research in the field (in years)
PA 1	Germany	> 10
PA 2	Germany	> 5
PA 3	UK	> 10
PA 4	USA	> 10
PA 5	UK	> 10
PA 6	Germany	> 10
PA 7	USA	> 5
PA 8	UK	> 5
PA 9	USA	> 5
PA 10	USA	> 10
PA 11	UK	> 5
PA 12	USA	< 5
PA 13	UK	< 5
PA 14	UK	< 5
PA 15	UK	< 5
PA 16	UK	> 5
PA 17	UK	> 10
PA 18	UK	> 5
PA 19	UK	< 5
PA 20	UK	> 10

Note: The three time intervals in the third column – >10, >5 and <5 years of research experience – were constructed as approximate indicators of the duration of the participants' research experience.

3.2.2 The context in which interviews took place.

Following the recruitment of participants, I conducted the interviews in various locations, corresponding to the different contexts described above. All interviews were recorded on audio cassettes. During both the PA and SPR conferences, once a delegate agreed to take part, the interview would take place either in an available separate room, or in a quiet secluded corner in the conference venue. The latter spots were selected in order to guarantee the confidentiality of the interview, making sure that the participants could not be overheard by delegates in the vicinity, and also trying to prevent interruptions and keeping the level of surrounding noise to a minimum. In the case of the researchers that were contacted individually, the interviews took place in their offices. Prior to each interview, I gave the participant a consent form to read and sign (see Appendix 5), which explained the anonymity of the content of their interview, the restricted use of its content and the secure and coded storage of the recorded audio tapes. They were again provided with a verbal account of the aims of the study, and particularly of the interview, and a summary of the main topics that would be discussed. Finally, just before starting the interview, I asked the participants if they wanted to have any issue clarified. The interview followed using the semi-structured protocol that was described above. A total of 20 interviews were carried out, and their duration varied from a minimum of 50 minutes to a maximum of over one hour and a half¹⁴.

3.3 Data analysis.

The final section describes the specific details of transcription, coding and analysis used in the study.

3.3.1 Transcription.

The transcription process is a reductive process, through which the data set is made to fit into what is deemed interesting and necessary for the analysis. It is, therefore, a part of the analysis itself, rather than a representation of the participants' talk (Hutchby and Wooffitt, 1999:73). The transcription of the recorded tapes containing the interviews was done after data collection was completed. All of the interviews were transcribed by myself, and the transcriptions checked against recordings for any

¹⁴ All of these intervals are approximations.

mistakes or omissions. In the transcripts participants were given a code designation – PA followed by a digit (e.g. PA 4) and the interviewer was referred to as C. The transcription format that was adopted was a very abbreviated version of the Jefferson system of notation conventions (Atkinson and Heritage, 1984:ix-xvi). The selection of transcribed features of participants' interview talk was tailored to the aims of the analysis (Taylor, 2001a:38). The analysis was concerned, on the one hand, with broad accounting practices of the participants and, on the other, with the functional properties of these accounts. The transcription therefore included a limited selection of talk events, including: a) the content of the interaction between participant and interviewer; b) the dynamics of turn taking, i.e. details of the beginnings and endings of turns, with features such as overlap and pauses or gaps; and c) characteristics of speech delivery, i.e. features such as emphasis, loudness and intonation (Hutchby and Wooffitt, 1999:76). The aspects included in the transcripts are listed and described in Appendix 6.

3.3.2 Coding and selection of themes and extracts for analysis.

The twenty interview transcripts were coded separately. The process initially amounted to a careful reading of the transcripts, one by one, and the construction of a set of “boxes” that accommodated the content of all the interviews. At first, this coding was guided by the broad topics covered in the interview protocol, which corresponded to the research questions in mind. These were: a) conceptual models and experimental practices; b) categorisations of experimental outcomes (i.e. successful, unsuccessful, unexpected or anomalous outcomes); and c) conceptual and experimental responses to unexpected or anomalous outcomes. This largely content-based system of boxes progressively turned into a more action- and function-based set of coding units. This meant that, as the coding system changed in the light of a new reading of the protocols, so did the coding of the protocols, which in turn influenced the system itself. It was thus a recursive process during which four extensive coding systems were produced. The final version of the coding system consisted of three themes: a) versions of parapsychology as a community of people and practice; b) accounts of personal research practices in parapsychology; and c) categorizations of outcomes (as successful, unsuccessful, anomalous and *psi*).

The coding of this set of data was led by two contradictory concerns: on the one hand, to reduce the information to a helpful number of categories that would provide patterns, either in function or content, across participants' talk; on the other hand, to do so in a way that did not affect the variability in the data, which is one of the cornerstones of DA (e.g. Gilbert and Mulkay, 1982a:314; Potter and Wetherell, 1987:33). The coding of the data in this project was eventually drawn to a close as decisions were gradually made on what analytical issues to include in this thesis. Coding was therefore an ongoing process of broad and sometimes preparatory detailed analysis that allowed for

the selection of both topics of interest and relevant extracts for subsequent more detailed and formally written up analysis (the distinction between broad and detailed analysis was inevitably subjective).

As was mentioned above in relation to the construction of the interview schedule, the selection of themes for analysis was dictated by the data set itself. Nevertheless, it was necessarily informed by: a) my daily engagement with the group of experimental parapsychologists at the KPU; and b) a critical reading of both current and seminal works within parapsychological literature. The selection of the topics and themes for formal analysis was, therefore, data driven, but informed by a context of topics, meanings and accounting practices present in other talk and texts in the community and literature of experimental parapsychology. In this study, my own understanding and participation in the field of parapsychology and the knowledge of the field's literature played an important part in this phase of the analytical process. Indeed, Taylor (2001a) points out that DA is never a form of neutral technical processing and always involves a conceptual background. However, this knowledge of what could be termed the wider context to this study was only important to the extent that it reflected the participants' own orientations. For instance, the themes addressed in Chapter 4 – the participants' building up of a dichotomy of skeptics and believers or the construction of *psi* as an inherently elusive scientific object – or in Chapter 6 – the construction of an extreme form of empiricism and the construction of neutrality – are themes that resonate across the parapsychological literature and feature frequently in parapsychologists' interactions (some of these themes were also already addressed in Chapter 2). The themes explored in the following analytical chapters were therefore selected because of their presence in the data set, their analytical interest (i.e. the potential for insightful observations about parapsychologists' accounting practices, and their reflection of topics that frequently find their way into the literature and interactions of the community).

Necessarily related to the selection of themes, the selection of extracts included: a) collections of exemplary extracts, identifying available resources and accounting practices that were repeatedly and consistently used by different participants in the data set (Hutchby and Wooffitt, 1999:93); and b) isolated extracts including specific significant features that were deemed important enough to receive analytical attention, even if occurring only once or twice in the data set. As an example of the former, the aforementioned construction and use of a dichotomy of skeptics and believers (Chapter 4) was found in almost all interviews (there were two exceptions). However, from this collection of instances of the construction/use of this dichotomy, two extracts were selected. The two that feature in the analysis were taken from interviews with two different participants and chosen because of: a) their clarity in constructing the dichotomy; b) the richness and consistency of their discursive use of the dichotomy and of each category; and c) the way that the two participants positioned themselves in each of the opposed categories (skeptic/believer). In this sense, these two extracts stand as representative of a wider discursive practice found both among the participants in this study and in other contexts of parapsychological scientific discourse. An example of the latter sort of selected

extracts (i.e. the isolated cases) is the construction of doing parapsychological research as doing ordinary research (Chapter 5). This analytical theme is based on a single extract and features in this thesis primarily because of the significance of a participant's construction of his parapsychological research as one which is not immediately identifiable or identified with parapsychological research methods and objects, but rather with methods and objects usually attributed to other areas of scientific expertise – i.e. where the particularity or fringe-ness of research in parapsychology is attended to as a problematic self-presentation issue. Additional reasons for the inclusion of this single-instance analytical theme are its consistency with other themes and discursive actions observed in the thesis, and its similarity to themes and discursive actions present in the field's literature and observed in interactions with parapsychologists elsewhere.

The empirical justification for the inclusion of the first sort of extracts (i.e. collections) is that of repetition and consistency, and the potential for suggesting that such discursive practices are available to the participants in the data set and, arguably, beyond it. The justification for the inclusion of single case extracts is one of empirical interest, i.e. it is argued that these single cases afford important insights into the way parapsychologists construct their community, research and outcomes. These sorts of claims to the generalizability and validity of observations from this data are by no means unproblematic. This is a particularly thorny methodological issue in the practice of DA, addressed by various authors critical of this form of research. Hammersley (2003:764), for instance, points out that the possibility of this process of generalization or confirmation of the findings' validity is difficult to judge, as DA does not provide consistent inferential or validation "instructions" as do other methodologies in psychology and other social sciences. Authors within DA [among them, for instance, Potter and Wetherell (1987:162) and Taylor (2001b:319)], put forward the view that such issues of generalizability and validity are not central to the evaluation of the products of this methodology, and provide alternative criteria for this evaluation. Nevertheless, when one is engaged in the process of justifying thematic choices and analytical arguments, these questions remain challenging ones.

3.3.3 Analysis.

As the general methodological approach has already been discussed above, this section aims to present a possible step-wise account of the process of analysis. Having established that fact construction was the central focus of the analysis of this data, it proceeded, using Potter's (1996) terms, by asking questions concerning both the empirical orientation and the action orientation of accounts. This section aims at characterizing other overarching analytical concerns in this process,

namely, both the variability and consistency between accounts, and the concern with the action orientation of discourse.

There is a trend in the discourse analytic literature according to which authors start their descriptions of the process of doing DA with the declaration that this is an almost ineffable process (Potter and Wetherell, 1987:168; Gill, 1996:143). Discourse Analysis is a context where authors shy away from providing an undisclaimed version of its method and, therefore, from constructing an authoritative reference version of what it is to do DA. As Wooffitt (2005a) puts it, “even discourse analysts acknowledge that it is hard to capture in a formal guide what is essentially a series of interpretative engagements with data from which emerges a sense that the functional orientation of a section has been captured” (p. 43), likening it to a craft based on a sort of tacit expertise (Wooffitt, 2005a:43). However, in the wider context of empirical social research in psychology, the construction of a sequence of actions as a “procedure” is crucial in securing the perception of this analytical process as a valid empirical methodology, yielding valid outcomes, as Gilbert and Mulkey (1984) so effectively articulated in their description of the empiricist repertoire. In spite of the difficulty and disclaimers in describing the procedure of DA, authors have nevertheless attempted to identify specific processes involved. In doing so, they have described both a general procedure, i.e. a sequence of steps concerned with noticing and organising features of texts, and specific terms and turns of phrase.

Among other authors, Potter and Wetherell (1987:169) stressed that central to how DA proceeds in practice is the identification of variability in discourse. The transcripts of the participants’ interviews were analysed in relation to the research aims – their accounts of parapsychology as a field of research, of their research practices and of categorisations of outcomes. The initial focus of the analysis thus attempted to identify the various ways in which these parapsychologists addressed these themes. The outcomes of this analysis therefore reflect this variability. For example sometimes parapsychologists described their methods as similar to other scientific disciplines, at other times they made relevant the specificity of their practices in relation to their own field. The analysis sought to identify such variation as meaningful instances of how parapsychologists describe their research. This in turn relates to the next key aspect of DA as a procedure.

A second central aspect of the procedure is the attempt to understand the function of these different accounts – i.e. the action orientation of accounts. That individuals talk about things in different ways in different contexts is in itself not particularly insightful. However, by asking why a particular version is used in a particular context allows the analyst to consider the social purpose of particular versions, i.e. what it is that they achieve in a given interaction. Therefore, the focus on the variability in how parapsychologists talked about the themes above made possible the consideration of the function of different versions. For example, in describing their research practices as similar to those of uncontroversial scientific disciplines, such as psychology or physics, the version of parapsychology

that they were presenting was therefore that of an ordinary scientific field. On the other hand, in describing their practices as specific to their field, they are building up their expertise in dealing adequately with fringe phenomena. In the first case the function is to present themselves as normal scientists, and in the second as particularly as competent scientists.

In identifying variability and considering its function, the analysis also sought patterns of consistency within it (Potter and Wetherell, 1987:168). Whilst looking for variability in discourse, the third key feature of DA is that it also seeks patterns of consistency within the variability, that is, the identification of available ways of talking about things and achieving social purposes. For example, in relation to the two constructions of doing research above, whilst they presented their practices in different ways, both can be seen as fulfilling a more general function. The presentation of the normality of their methods and the building up of their expertise, can be seen as serving the same purpose, that of solving a problem regarding the common perception of parapsychological research as being placed outwith a norm of scientific validity. Both normality and expertise inoculate against (Potter, 1996a:125) the potential attribution of lack of scientificity.

Finally, most of this analytical work was done in reference to other empirical studies, both in DA (mainly studies of fact-construction) and in CA. As it was already mentioned above, the point of such references is to seek support for the analytical claims in the present thesis.

Introduction.

The aim of this first analytical chapter is to set out the types of constructions that participants made of their context of research. It seeks to complement the view of the field that was outlined and explored in Chapter 2, by focusing on the ways parapsychologists themselves constituted their field as a context of research. It will also provide a backdrop for the examination of the participants' descriptions of their research practices and their constructions of anomaly in later chapters. The task of building up an account of these participants' descriptions is necessarily a selective process among the multiplicity of characterizations that were on offer. Following participants' own orientations, the chapter will examine in the next three sections how the field was constructed as: a community of people (section 4.1); a body of results (section 4.2); and a field with an identifiable position and role within science (section 4.3).

4.1 Constructing a community of people.

This first section explores the construction of parapsychology as a community of people. This theme was addressed by all participants, and, interestingly, the construction of what this community is like involved in all of these participants' descriptions distinctions between experimenters based upon their positions in relation to the claim that *psi* is a real and demonstrable phenomenon. This theme was explored in Chapter 2 and its peculiarity to the context of parapsychology has been pointed out by Collins and Pinch (1979; 1982), as it was described in Chapter 1. This same practice also features in David Hess's wide cultural analysis of parapsychologists and skeptics, as two identifiable communities within the 'world of the paranormal'. Hess (1993:43) remarks that the way in which both communities (in their respective literature) define themselves and their goals by reference to each other (i.e. through what they are not). This, the author argues, means that each community is central to

the other's existence. Similarly, the categories of "believers" and "skeptics" demarcate, respectively, between proponents and opponents/doubters of the claim that *psi* is real, and each category seems to be fundamental to the other's definition. These seem to have become part of the "taken for granted linguistic furniture" (Potter, 1988:29) of parapsychologists, and are frequently used both in parapsychologists' informal dialogues and formal publications, often as a genuine and occasionally essential division of two coherent groups of people, distinguishable by their views (or internal states of belief) about *psi*. Thus, the use of these categories makes available a set of discursive resources that: a) allow the attribution of internal states of belief to be reified into how researchers are really like (i.e. believers and skeptics) and, in turn, offer a warrantable basis for deductions about a whole range of behaviours and thoughts (Potter, 1988:20); b) provide a set of groups with identifiable characteristics and membership criteria, (i.e. the motivation to obtain experimental results that do or do not support the *psi* hypothesis); and c) offer a way of making sense of the variability of experimental outcomes supporting the reality of *psi*, namely, that they are directly linked to the belief or scepticism of those that produce or interpret such outcomes. It is therefore a central way of accounting for differences, controversy or dissent in relation to the fundamental experimental question in parapsychology – is *psi* a real and demonstrable experimental effect?

This last use of the categories of "skeptics" and "believers", their explanatory value, seems to be of particular importance to parapsychologists and of central interest to the analysis, as explored in Chapter 1. Gilbert and Mulkay's (1984) study of scientists' discourse proposed a way of making sense of the discursive resources available to scientists when accounting for their research practices and products, by identifying two repertoires, an empiricist and a contingent one. These repertoires provide a way of understanding how scientists constitute what counts as science and what does not. On the one hand, the empirical repertoire can be understood as a set of "recurrent stylistic, grammatical and lexical features" (Gilbert and Mulkay, 1984:55) that allow the production of versions of science as a set of warranted and prescriptive practices, which produce valid, neutral, representations of a reality that exists separately from them (Woolgar, 1988:30). At the heart of this formal way of articulating "science" is the question of researcher agency. The job of the scientist should be that of facilitating or channelling representations of the world or facts, which exist independently from his/her actions, ready to be discovered by anybody (Woolgar, 1988:101). This version of the scientific agent's actions allows scientists to constitute their procedures and outcomes as being prescribed unequivocally and solely by the facts themselves. On the other hand, the contingent repertoire was described by Gilbert and Mulkay (1984) as another set of linguistic features used in constituting versions of scientific action that involve personal agency, in the form of beliefs, motivations or allegiances (Gilbert and Mulkay, 1984:57). The inclusion of "the agent" in the way science works naturally introduces a problematic feature in the constitution of warranted science.

The use of categories of skeptic and believer in parapsychology, as will be seen in Extracts 1 and 2, are instances of the use of the contingent repertoire in action. Accounts of scepticism or belief construct a preferred view about *psi*, and seem to be the sorts of issues that Gilbert and Mulkey (1984) examined as the personal and social circumstances scientists made use of in warranting or discounting a particular set of practices or theoretical position. However, in these parapsychologists' accounts, it was possible to see another use of these contingent rhetorical features, as they were constituted as variables in themselves. The personal and social circumstances of the experimenter (e.g. their belief, their capacity to get outcomes supportive of the *psi* hypothesis) were transposed from a category of contingent issues in relation to their work, into a category of empirical variables. The idea of the *psi* experimenter effect (discussed in Chapter 2) is an example of the way in which parapsychologists integrate agency into versions of scientific action without damaging the scientific value of their outcomes and practices. As will be examined in extract 3, the job of the experimenter in parapsychology can include the manipulation of his/her role in making *psi* work, or hindering its experimental manifestation, as an independent variable. Thus, these parapsychologists' constructions of scientific action transpose features of agency into the realm of available ways of constituting empirically robust science.

In summary, this first section will examine how participants built up a dichotomy of people in parapsychology: skeptics and believers. This characterisation of the community in terms of internal states of belief is problematic in the sense that it undercuts the normative view that scientists are neutral media through which nature shows itself. As the following analysis will show, participants orient to this problem by: a) constructing these characterisations into essential properties of this community of scientists; b) setting up a contrast between extremists and moderates; and c) transforming these (contingent) characterisations into (empirical) variables. This section will make use of the contributions of three parapsychologists, PA 20, PA 19 and PA 10. The first two stand as clear and sophisticated exemplars (as it was mentioned in the beginning of this section) of a discursive practice used by all participants (i.e. the establishment of a clear division between skeptics and believers and). Therefore, in extracts 1 and 2, the analysis will show how both PA 20 and PA 19 individually construct categories of skeptics and believers, and locate themselves in the former and latter, respectively. In extract 3, the analysis will show how PA 10 transposed belief from the contingent realm to the empirical realm. Although not a representative of a wider practice (this is single case analysis), this transposition, it is argued, is important enough to warrant its inclusion here, as it undermines the normative scientific divide between the experimenter and the experimental system.

Extract 1 – PA 20, lines 284-341

284. PA¹⁵ I mean the thin- the thing about the “world of the paranormal” ((caricature of a
285. mysterious voice)) is that it’s kind of full of such such a wide range of different
286. kind of participants.
287. C Hmhm.
288. PA Ehm, ranging from kind of, you know, **if you like** moderate parapsychologists
289. with whom I’ve got a great deal in common, I’ll quite happily collaborate, get on
290. with very well, [you know, have a lot of respect for. To the kind of extremists
291. C [Hmhm.
292. PA = at either end, where, you know, you’ve either got on the skeptic side the kind
293. of extreme debunkers who seem to worry (.) **eh, in my case kind of slightly**
294. **unnecessarily**, being £ overwhelmed by this tide of irrationalism £.
- [Suppressed lines 295-304]
305. PA Ehm, (1) but I think people also, you know, there’s a danger **if we just** - (.)
306. particularly in respect to media presentations **if we just let** eh self-proclaimed
307. psychics just [go and make these claims and nobody actually stands up =
308. C [Hmhm
309. PA = and [says “well hang on maybe not”=
310. C [yeah, yeah.
311. C Yeah.
312. PA = and presents a kind of skeptical alternative for people to consider. Well I think
313. again in just in terms of public education that’s not a good thing, so, so there’s
314. that side to it as well, I think you need informed skeptics around.
315. [And really the **informed skeptics ought to be**, ahm you know making =
316. C [Hmhm.
317. PA = parapsychology kind of better and more rigorous, and (I’d say) if you look at
318. the history of parapsychology that’s what that’s what’s happened you know. I’d
319. say that it’s actually benefited from the likes of people like [Name] and
320. [and and [Name] and various other people being critical and saying =
321. C [Hmhm, hmhm, yeah, yeah
322. PA = “no you’ve not done that right, not done this right” and eventually-. (.) Is very
323. interesting to (.) to consider the current state ((sniggering voice)) of
324. parapsychology because you talk to some people and they really don’t think it’s
325. got much of a future at [all it’s on its last legs. A:::h and you get =
326. C [Yeah, yeah, yeah.

¹⁵ PA, interviewed parapsychologist; C, interviewer.

327. PA = you talk to others who kind of think that it's (.) just sheer bigotry on the part of
 328. traditional scientists they won't accept this overwhelming proof, and I must admit
 329. I kind of (.) identify myself - I'M STILL I'VE I'VE MOVED from being a
 330. believer, to being an extreme skeptic to being I would like to think a kind of
 331. **moderate skeptic. I'm still on this side of s- of scepticism certainly**, but I think
 332. there are **certain claims** within parapsychology that should be **taken seriously** =
 333. C Right.
 334. PA = by the wider scientific community, **even if it's just a matter of explaining**
 335. why the kind of paranormal explanation is not the right one. [But eh but at least=
 336. C [Yeah
 337. PA = you know, they are this interesting =
 338. C = hmhm.
 339. PA = ehm eh an- eh **interesting anomalies** are there [that need to be explained =
 340. C [Yeah.
 341. PA = and you know that's therefore of eh you know **something worth looking at**.

Extract 2 – PA 19, lines 407-545

407. C How do you think [that your views of what's a success and what's a failure =
 408. PA [Yeah
 409. PA Yeah.
 410. C =are reflected or are shared [within the field of parapsychology?
 411. PA [((snigger)) a:::h
 412. PA (1) Ok (2) from what I've seen through e::hm-. Again I would like sort of split
 413. into social context and literature context =
 414. C = Go for it, yeah =
 415. PA = Yeah?
 416. C It's very useful.
 417. PA Ehm (2) in terms of social context **people seem to have different agendas**.
 418. C Hm.
 419. PA Some are in it - (.). Tell me if I'm getting that right or (wait) I might answer a
 420. different question (you want) later anyway.
 421. C Hmhm.
 422. PA I think basically some people, **in all honesty** are-, don't care too much about
 423. para.
 424. C Hmhm.
 425. PA It's a small field, it's a trendy field. You get on the telly, [you can do your stuff,=

426. C ((laughs))

427. PA = yeah? I think there's an element of that, bec- is an easy it's an easy, easyish

428. ehmm field to become one of the best people in the world at. Because there are so

429. few people in it.

[Suppressed lines 430-455]

456. PA (1) Ehm I think other people- [1]. I think there are some people that (.) [2] they

457. think that [3] they think that **they just-** [4] (3) there're some people like me,

458. basically, who know **this happened**, real, who think they know **it's real**,=

459. C = hm =

460. PA = and **they're just playing around**.

461. C Yeah.

462. PA Which is (such as) how I see what I'm doing. And to have a **bit** of a formal

463. structure and **some people to chat to now and again**, it's **quite useful**. (1)

464. There's other people who don't believe, or are not sure if they believe, and they're

465. looking for proof, and they think they'll find proof in the numbers, =

466. C = Right.

467. PA = which I disagree with, but [(.) yeah. But there again if someone starts scoring =

468. C [Hmhm

469. PA = really high, there's always the sort of stuff that-. There seems to be an

470. assumption that if someone can come up with a protocol where people start

471. scoring really high, then that'll provide proof, and it just won't, because people

472. will come up with all sorts of stupid reasons, how people can cheat, =

473. C Hmhm.

474. PA = and that's not changing, you know, we got this computer system which is

475. pretty secure, but with all things are getting smaller you can have an earpiece, =

476. C Hm.

477. PA = sender receiver can communicate that way without anyone knowing.

478. C Hmhm.

479. PA Ehm, even shielded rooms don't tend to be that shielded, [etcetera etcetera.

480. C [Hmhm.

481. C Yeah.

482. PA So there's that aspect to things. (I had) another one which was actually the most

483. interesting.

[Suppressed lines 484-515]

515. PA [OH NOW I KNOW what my final category of person was.

516. C Go on.

517. PA **I seriously think** there're some people (2), I was gonna say [Name]¹⁶ but I keep
 518. on getting confused about who's who, who think that they're s- ah saving society
 519. from madmen.
520. C Aha.=
521. PA Yeah? And they view parapsychol- **and this isn't to put people down like I I**
 522. **can see where they're coming from.**
523. C Yeah.
524. PA (1) Like say like [Name]¹⁷, he's trying to do this, but I don't think he knows
 525. anything at all about para, yeah? But they've got their model and they say
 526. like this is what applies, this is what's real, =
527. C = Hmhm.
528. PA = we need to defend society from this bunch of lunatics who say that people can
 529. be telepathic and people can see into the future bla-di-bla.
530. C Yeah, [yeah
531. PA [I think some people come in AND PROBABLY, from their point of view,
 532. really good personal reasons. [They're they're trying to do the right thing.
533. C [Hmhm.
534. C Hmhm.=
535. PA = **But they're on a crusade.**
536. C Yeah, yeah.
537. PA So that's my final category of persons.
538. C Ok, ok.
539. PA Interesting people, crusaders. And then you **probably got** the some crusaders the
 540. other way as well.
541. C Right, the ones that hm...=
542. PA = totally believe, =
543. C Hmhm.
544. PA = and need to prove (.) that is right. (3) Well I'm not too sure if I've got an
 545. example of that ().

¹⁶ Name of a researcher who is a self-declared *skeptic*, and consensually categorised as one within parapsychology.

¹⁷ Name of a researcher who is a self-declared *skeptic*, and consensually categorised as one within parapsychology.

4.1.1 Building up a dichotomy of skeptics and believers.

In Extracts 1 and 2, PA 20 and PA 19 respectively propose a series of distinctions between people belonging to the “world of the paranormal” (Extract 1, line 284) or the “social context” of parapsychology (Extract 2, line 413). In doing so, they construct, using Potter’s (1988) metaphor, “layered cake accounts” (p. 22) of different possible positions in parapsychology. These accounts are directly concerned with identifiable or concrete groups of individuals, and not with abstract positions such as “scepticism”. Moreover, these categorisations stand out to the extent that they are concerned with inner motivations or beliefs of participants about the reality of *psi*. During these accounts they accomplish a sequence of evaluative actions regarding different positions and categories of participants in the context of parapsychology. In doing so, they distinguish between participants who belong to moderate or balanced stances, and extremists or “crusaders”, to use PA 19’s term (Extract 2, line 539). In Extract 1, these contrasts between moderate and extreme positions allow PA 20 to locate himself within the “moderates”, but on the skeptic side. In Extract 2, PA 19 constructs a set of beliefs or “agenda” (line 417) bound categories, which allow him also to locate himself within the “moderates”, but on the believer side. Finally, in positioning themselves clearly, they manage to construct these accounts of categories of participants in parapsychology as balanced observations, from the vantage point of moderate insiders.

4.1.2 Setting up a contrast between extremists and moderates.

Extract 1¹⁸ begins with a key contrast being constructed (in lines 288 to 294) between moderation and extremism. PA 20 begins his description of the kinds of participants in the “world of the paranormal” (line 284) by setting up an account of the range of the diversity of people it contains (line 288). The initial category of people that is built up is that of “moderate parapsychologists” (line 288). In doing so, he makes available that this is not a straightforward or normative category by signalling it with “if you like” (line 288, in bold). This category is thus constructed as his own evaluation of this group of people. In this sense, “the relevant thing” about them (Edwards, 1998:18) is what he feels and thinks about this set of parapsychologists (lines 289 and 290). Using this reference, PA 20 constructs a set of “moderate” people about whom he feels and thinks positively, providing a three part list description of his feelings and thoughts, in which he builds up the factuality of a “positive relationship” between him

¹⁸ This extract is part of PA 20’s answer to a question that asked him to provide an assessment of the strengths and weaknesses of his particular approach to research of *psi* phenomena (question in lines 149 to 157). Previous to this extract, PA 20’s answer included a lengthy construction and warranting of his personal position in parapsychology. For the purposes of this analysis, these previous 127 lines were suppressed.

and the moderate parapsychologists (Jefferson, 1990, cited in Potter, 1996a:196). The construction of this closeness to them, despite his exclusion from this category, makes available the implication that PA 20 is also a “moderate” something, if not a “moderate parapsychologist”.

This implication is made clearer when looking at the contrast to this construction of moderation, as PA 20 introduces the “opposites” in parapsychology, the “extremists at either end” (lines 290-292). In tracing a continuum with extremists at either end, PA 20 locates the moderate parapsychologists in a middle-ground position, though where he is locating himself remains unclear. In line 292, the first of these two extremes is constructed as the “skeptic side”, the “extremity” of this side being constructed by PA 20, as Edwards (2000) puts it, by “hearably, commonsensically, going to extremes” (p. 350). PA 20’s introduction of this category of people in parapsychology – the “extreme debunkers” (line 293) – characterizes their internal disposition as that of being worried about “being overwhelmed by this tide of irrationalism” (line 294). This invoked image is oriented to as a humorous description – note the smiley intonation (Antaki, Billig, Edwards, Potter, 2003) in line 294. In using this extreme formulation of skeptics’ take on parapsychology, PA 20 seems to be signalling that this is non-literal description of skeptics’ thought, but one that can be taken as “essentially so” (Edwards, 2000:365), i.e. essentially accurate. The exaggerated description of skeptics allows PA 20 to make available his distance from the category of “debunkers”. It achieves, along with the third person formulation (line 293) of their worries, his exclusion from this group. It is interesting to see that PA 20 makes a further clear movement away from this category of people by including an incidental remark in lines 293 to 294 (in bold) in which he further deepens the extreme – moderate divide, and accountably distances himself from the extremists in the process.

The other side of the coin is introduced later in lines 305 to 309. PA 20 seems to orient to the possible consequences of his sarcastic construction of extreme skeptics. Specifically, he seems to orient to the available implication that he endorses psychic claims being made without critical examination. The construction of the unsupervised actions of “self-proclaimed psychics” (lines 306-307) as potentially dangerous to the public, allows him not only to introduce another extreme category of participants into the “world of the paranormal” (line 284), but also to construct his position as not a member (or even a supporter) of this category. In doing so, he constructs a role in parapsychology which is involved in the control of such individuals’ actions – “there’s a danger if we just (...) if we just let (...)” (lines 305 and 306, in bold) – yet exerts this control moderately, not through sanction but by presenting an alternative view on these claims, a sceptical one “for people to consider” (line 312). In this way, PA 20 further constructs the contrast between moderate scepticism and the extreme kind examined above. The analysis will return to Extract 1 to look more closely at the way PA 20 explicitly constructs his position as a moderate skeptic. First, it will focus on Extract 2, and PA 19’s construction of another kind of moderate – extremist divide.

4.1.2.1 Constructing the “extremists”.

In Extract 2, PA 19 constructs a similar contrast to the one above, by setting out an account of people in parapsychology that distinguishes those with “an agenda” (line 417, in bold) from those “on a crusade” (line 535, in bold). PA 19 thus provides an account of the “social context” (line 413) of parapsychology that focuses on the inner motives of its participants. The use of the word “agendas” suggests that this account will focus motivations, i.e. on what people are in parapsychology for. The construction includes the following categories: a) the notoriety seekers (lines 422 to 429), who look for what parapsychology can provide them with, namely, fame and expertise status; b) the warranted believers (lines 456 to 463), who know from experience that *psi* is real, and thus use parapsychology as a context for its demonstration, where PA 19 includes himself; and c) the doubters (lines 463 to 469), who rely on experimental empirical evidence to confirm or disconfirm the reality of *psi*. This “layered cake structure” (Potter, 1988:21) of participants in parapsychology seems to be constructed as stemming from inner qualities of its participants (i.e. what is made relevant are primary dispositions of people in parapsychology). The essentialist rhetoric thus builds up the factuality of the account.

Like PA 20, PA 19 also provides his own evaluations of these categories as he lists them, which will be looked at later. First, however, PA 19, similarly to PA 20’s divide between moderates and extremists, constructs a fundamental divide between these “agendas” (line 417, in bold) and “crusades” (line 535, in bold). Crusades are constructed as extreme cases of agendas, and, like PA 20, this extremity is built up in a series of descriptions that hearably “go to extremes” (Edwards, 2000:350). PA 19 introduces this category as his “final category of people” (line 515). The preface (line 517, in bold) establishes the account that is about to be produced as a serious representation of his thoughts on the matter, which in turn builds up its factuality. This is followed by a description of extreme skeptics in parapsychology – marked by his reference to a “known” skeptic (line 517) – which is similar to the one provided by PA 20. In constructing this category of extreme sceptics, PA 19 makes similar use of a characterization of their “agenda” as “saving society from madmen” (lines 518-519), which seems to be an exaggerated or sarcastic description of skeptics’ thoughts. Indeed, this formulation seems similar to PA 20’s humorous image of extreme skeptics as being worried about an “overwhelming tide of irrationalism” (line 294). In contrast, however, PA 19 seems to orient to this description as one that could potentially be accountable as a literal critical description of skeptics’ agenda, as in lines 521 and 522 (in bold), when he introduces a softening gloss to his account of extreme skeptics. In doing so, PA 19 constructs a disclaimer for the possible implications of providing this extreme version of scepticism – e.g. being a closed minded or dismissive account – and inserts a claim to his own open-mindedness and empathy with extreme skeptics’ constructed position, when he says “like I can see where they’re coming from”. It is interesting to note that despite this disclaimer, the account is not softened at all, and in lines 525 to 529, PA 19 constructs an uptake of this description with a more extreme account of these skeptics’ agenda – “we need to defend society from

this bunch of lunatics who say that people can be telepathic (...)" (lines 528-529). Again, this uptake is oriented to as a description that might imply his siding with an opposing "faction", as it is counterbalanced by another disclaimer regarding the fundamental "good intentions" that these skeptics might have in such extreme thoughts and measures. In doing so, PA 19 once again constructs his own moderation and open-mindedness. This disclaimer also provides the moderation context for the later uptake of this category as "crusaders" (line 535, in bold).

The construction of this extreme category of participants in parapsychology is later concluded in lines 539 to 545, in a similar manner to PA 20's construction of extremism. Here, PA 19 indicates the possibility of crusaders on the "other side" (line 540), which is then constructed as the "believer" side. The imbalance between the "probability" (line 539, in bold) of crusaders on the believer side and the detailed, extreme accounts used in the above characterization of the skeptics is noticeable. The probabilistic formulation of the former category contrasts with the definitiveness of the latter. Moreover, in the construction of the crusader skeptics category, PA 19 made use of two readily available examples of its participants, but in the construction of the believer crusaders no examples came to hand (544 to 545). It is thus made available that this category is a less obvious group of people. In PA 19's construction of the non-crusader categories, he included himself in the warranted believer category. The next segment will focus on the self-categorization work done by both PA 19 and 20 by examining a) how PA 19 manages his membership to the category of warranted believer, rather than the "believer crusader" category constructed above, and b) how PA 20 constructed a position of "moderate skeptic", also initiated above.

4.1.2.2 Achieving the "moderate believer".

PA 19 constructed a sequence of categories of participants in parapsychology, during which he distinguished between participants with "agendas" and those "on a crusade". The description of "agendas" included his own positioning as what he termed a warranted believer. This category follows his construction of the notoriety seekers (lines 422-429), those who "don't care much about para" (lines 422-423), and precedes the construction of the doubters (lines 463 to 469), who rely upon experimental empirical evidence to confirm or disconfirm the reality of *psi*. The category to which PA 19 constructed membership included those who "know this happened, real, who think they know it's real (...)" (lines 457-458). The formulation of this category, compared to the preceding and following ones, is repaired a few times, which suggests that PA 19 is orienting to this category as a potentially problematic one. This can be seen in lines 456 to 458, where he makes four attempts to describe what this category of people includes (numbers in square brackets, in bold). This sequence of repairs and its resolution, i.e. that the category consists both of people who know that *psi* is real and that he is part of this category (line 457-458), suggests the reason for this category being problematic:

that of claiming to have had personal and direct experience of *psi*. This sequence can therefore be seen as an instance of trouble shooting in respect to the membership inference that might be made available as a result: that of gullibility or lack of critical thinking. This can be further seen in the three second delay that follows this sequence of repairs (line 457), and suggests that PA 19 designs and orients to this category as a “dispreferred category” (Pomerantz, 1984a:64). In line 458, it is possible to see that the formulation of what this group of people is like presents another feature of PA 19’s construction, the use of “this happened” (line 458, in bold) and “it’s real” (line 458, in bold), in which there seems to be an “oblique reference” (Wooffitt, 1992:104) to *psi*. In this sense, PA 19 seems to be putting forward a general class of events, yet “not naming” them. This “not naming” action might suggest that there is something problematic in displaying knowledge about “this” and “it’s” (Wooffitt, 1992:105). As Wooffitt (1992) proposes, being able to name an event or state of affairs “also suggests a commitment to the in-principle existence of the object or state of affairs so named” (p. 105). Thus, an oblique reference to *psi* might display PA 19’s sensitivity to the personal commitment of constructing himself as a “believer”. Finally, there seems to be a significant repair in line 458, where PA 19 downgrades the initial formulation from “who know” to “who think they know”. The downgrading of this claim adds to the idea that PA 19 is making available something problematic about the claim as an inclusion criterion to this category.

The question remains, however, why is this claim problematic? A closer look at PA 19’s continuation of his account of this category provides a possible answer. In lines 460 and 462 to 463, it is possible to find the description of activities that are bound to this category of people in parapsychology. They are described as “just playing around” (line 460, in bold), having a “bit” (line 462, in bold) of structure, having “some people to chat to now and then” (line 463, in bold). The description of the activities of this category’s members, including himself, are thus downplayed as casual, informal or even trivial activities like “playing” or “chatting”. Through these versions of “doing research in parapsychology”, he constructs a non-committal informality that might compromise the inclusion of his activities within the category of professional scientist (Hutchby and Wooffitt, 1998:209). However, it makes available his entitlement to being in the “agenda” rather the “crusader” camp, a significant achievement if one takes into account his later construction of the “believer crusader”. His own version of the believer position contrasts with the crusader position as a nonchalant or casual believer, one who is not particularly bothered with the “need to prove” that *psi* exists (line 544), but rather who “plays around” and “chats”, in his version of doing research. His membership of the moderate set of categories is thus achieved, even if this might be at the expense of being seen as a serious scientist. Given the general importance of this latter concern, the importance of being seen as a moderate believer in this particular context seems clear.

4.1.2.3 Achieving the “moderate skeptic”.

The analysis has already noted how PA 20 constructed a contrast between the extreme scepticism and the moderate kind. It will now focus on the way PA 20 explicitly constructs his position as a moderate skeptic. This follows the construction of the benefits of the presence of “informed skeptics” (line 314) in parapsychology, as he puts forward an account of their role within the field. It is interesting to note that this account sets out a normative role for this category of people (line 315, in bold), assigning to them an almost clear cut job description within parapsychology. In this way, “informed skeptics” category bound activities (Sacks, 1979:13) are constructed as the straightforward control and regulation of research quality in parapsychology (lines 315 to 317), or as being “critical” (lines 320 and 322). This regulatory role is constructed in the next segment, where PA 20 appeals to the authority of history to warrant the presence of this type of skeptic as an asset to parapsychology. The appeal to “the history of parapsychology” (line 318) seems to be used here to construct more of a fact than a personal opinion, i.e. the role of these skeptics is built up as an “out-there” consensus (Potter, 1996a:158) which corroborates his own view. The use of this externalising resource could arguably also be used as a way of “inoculating” (Potter, 1996a:125) this account from a potentially undermining invoked interest. Thus, PA 20 seems to orient to the stake that he, as a “skeptic”, would have in producing this normative account of the important regulatory role of skeptics. The historical warranting seems both to stave off the “then again you’d say that, wouldn’t you” (Potter, 1996a:130) effectively, and construct the factuality of the “positive influence” that such skeptics have on the field.

In line 323, there is a shift from the regulatory role of skeptics, to a reprise of the description of the extremes explored above, drawing up a “layered cake account” of these categories (Potter, 1988:22), which in turn is used to position himself as a “moderate skeptic” (line 331, in bold). In this segment, it is possible to see that he used the “current state of parapsychology” (line 323-324) as a way of setting the context for the account that follows (lines 324-328). In doing so, he lays out two camps of parapsychologists, who can be identified through their thoughts either about parapsychology’s survival or about the evidence for *psi*. Similar to Potter’s (1988) argument in his examination of psychologists’ social categorisations, this account seems to go beyond an account of conceptual positions, to suggest that these “have an identifiable membership” (Potter, 1988:20). The construction of these two opposing poles is achieved through two the following descriptions – those who “(...) don’t think that parapsychology has got much of a future at all, it’s on its last legs” (lines 324-325) and those who think “(...) it’s just sheer bigotry (...) they won’t accept this overwhelming proof” (lines 327-328). They are (as before in line 294) oriented to as humorous, exaggerated or sarcastic descriptions, made available as non-literal descriptions of parapsychologists’ positions (Edwards, 2000:367). They also function as a way of constructing two easily identifiable poles of people in parapsychology. The construction of these two poles, it is argued, enables PA 20 to (in lines 328-331) to use them as coordinates for the location for his own position. Thus, the “moderate skeptic” (line

331, in bold) is achieved through the identification of the poles with the labels “believer” and “extreme skeptic” (line 330). It is interesting to note that this account of his positioning is now deeply personal, as he draws his lifeline in parapsychology, prefacing it with a marker of confession or admission (line 328, in bold), then tracing his move from one pole to the other, before settling for a middle position.

What follows is the characterization of what it is to be a “moderate skeptic”. He presents an account that seems to orient to the possible problematic implications of being too supportive of parapsychology’s investigation of the *psi* claim. In line 331 (in bold), there is a preface to this account which clearly marks how it should be read, i.e. in a similar way to a disclaimer. In lines 331 to 341, PA 20 develops an account of what it means to be a “moderate skeptic”, through the negotiation of his proximity to positive assessments of parapsychological research and claims. The use of systematic vagueness (line 332, in bold), and the downgrading of the positive assessment of parapsychological research (lines 334-335, 339 and 341, in bold) to its bare essentials, makes it innocuous to the “skeptical”, even a “moderate” one. In doing “not being too positive about these things”, PA 20 achieves a position of open-minded moderate scepticism, which allows and warrants his layered account as a balanced observation of the different participants in the “world of the paranormal” (line 284).

4.1.3 The transformation of belief into an empirical variable.

So far, the analysis of these two accounts (PA 19’s and PA 20’s) has shown that they produced categorizations of parapsychologists’ into groups with identifiable membership and designations, which refer to the participants’ state of belief about the reality of *psi*. While the construction of social categories within research communities is hardly unique – for instance, the same issue was analysed by Potter (1988) regarding the social categorizations of psychologists, into “humanists” or “mechanists” in psychology¹⁹ – the categories of skeptic and believer appear to differ from these social categorisations in two important ways. First, the issue at the heart of these distinctions is the belief or scepticism about the reality of *psi*. As the extracts above described, when pushed to their limits, these positions are constructed as almost two opposing movements in parapsychology, one involved in its maintenance and the other in its destruction. Therefore, more than a distinction between cognitive positions which influence the selection of theories or empirical models, these distinctions seem to construct the researchers’ role in parapsychology in relation to the survival or demise of the field itself. Second, they seem to make available a ready-made operationalisation of

¹⁹ Examples of categorisations explored by Potter (1988).

belief that, as was mentioned in Chapter 2, is one of the aspects that transforms the experimenter into a variable with experimental significance – the “experimenter effect”. In Extract 3 it will be possible to explore a construction of the “experimenter” as an identifiable experimental variable, and as a way of transposing the experimenter’s personal characteristics from, in Gilbert and Mulkey’s (1984) terms, a contingent realm into the empirical repertoire of parapsychologists.

Extract 3 – PA 10, 608-638

608. PA Then of course there’s the experimenter effect, which I’m becoming very
609. interested in, =
610. C Right
611. PA = and I think there’s a lot of that going on. (1) And I think one of the main
612. complications is the source of *psi*, where it eh who is (.) [who’s doing the *psi* =
613. C [exactly, yeah, yeah.
614. PA = and eh (.) that is a real mess.
615. C Hmhm.
616. PA (1) E:h you get someone like Charles Honorton and I’m gonna do () I don’t
617. think that guy has ever done a study that he was first author on where he didn’t
618. get exactly what he wanted.
619. C ((sniggers)) yeah, yeah.=
620. PA = And why is that?
621. C Exactly.
622. PA That’s that’s odd, and this comes across, it’s not just a particular kind of
623. (experiments) it it comes across in all kinds of stuff he’s done.
624. C Hm, hmhm.
625. PA A::h and there’re other people who were e:h, I don’t think QUITE as extreme as
626. him in that regard, but, you know I mean, **I think the BEST predictor o::f**
627. **results in this field is (not necessarily the experi-) who the principal**
628. **investigator is, =**
629. C Right.
630. PA = **Is the best [single predictor =**
631. C [right, right, hmhm
632. PA = by far.
633. C Hmhm.
634. PA A:nd that’s an important data point =
635. C Yeah =
636. PA = **and it it it it and implies a kind of lawfulness.**

637. C Yeah.
638. PA **It's probably the most lawful thing we've got.**

PA 10's elaboration of the "experimenter effect" (line 608, in bold) is used here as an exemplar of the construction of "the experimenter" into the realm of experimentally significant variables. The term "experimenter effect" (line 608, in bold) is constructed at the beginning of this extract as an already available class of effects, that is, as an existing topic of research in parapsychology (as addressed in Chapter 2). The elaboration that follows describes this as a specific class of effects, generated by the experimenter's *psi* influence on the experimental system (lines 611-612). In line 626, PA 10 presents an absolute formulation of the importance of this effect in the results of the field. His assessment of the effect of the principal investigator in the experimental system, constitutes it as the "best predictor" (lines 626, in bold) – later upgraded to "the best single predictor" (line 630, in bold) – of results in parapsychology (i.e. of whether positive results for *psi* are obtained). The extremity of PA 10's formulation of the influence of this effect is once more upgraded in line 638 (in bold), where he describes it as "probably the most lawful thing we've got". This construction of the investigator's (or experimenter's) effect is analytically interesting in the sense that it is described using terms and jargon that are commonly used to produce accounts of empirical science – "predictor" (lines 626 and 630, in bold), "data point" (line 634, in bold), "lawful" (line 338, in bold). This not only removes the influence of the experimenter from the context of contingent elements (which are not part of formal accounts of science), but also allows this to be transposed into the realm of experimentally significant variables. In this way, PA 10's formulation of the experimenter/investigator's effect in the experimental system transforms it from being a compromising element to the scientificity of the system, into being one that consistently contributes to it.

In summary, this first section examined how parapsychologists gave an account of parapsychology as a community of people that consisted of sceptics and believers. While this is a common theme in the parapsychological literature, the analysis of the participants' accounts showed how these social categorisations were managed in this context. The categorisation of scientists according to their belief is not one that is compatible with normative views of science, and therefore is a problem that needs to be attended to in an account of scientific practice. By constructing a further dichotomy of extremists and moderates, and by locating themselves within the latter category (whether skeptic or believer), participants were able to a) present an account of parapsychology as a balanced community, and b) construct their beliefs as more compatible with doing science. The importance of the constructed dichotomy between skeptic and believer can be seen in the way that participants orient to it both as an essentialist distinction and one that has empirical value. This transposition of belief from the contingent to the empiricist realm allowed belief to be put forward as a "predictor" of (either positive

or negative) results. The lack of consistent positive results is a ubiquitous concern within parapsychology, and one that will also be discussed in the next section.

4.2 Constructing parapsychology as a body of evidence.

This section examines how participants constructed their field as an identifiable body of evidence. These accounts offered characterizations of a cumulative body of experimental results in parapsychology, one that invariably attended to its status as constituting evidence for the claim regarding the existence of *psi*. These accounts, then, orient to what was put forward in Chapter 2 as the central controversy within parapsychology, the controversy over the reality of *psi*. The importance of this issue cannot be overstated, as most arguments that dispute the legitimacy of parapsychology as a scientific field stress that its central scientific object remains elusive to experimental demonstration (Collins and Pinch, 1979:237). Apart from its rhetorical use, the issue of evidence in parapsychology appears to be constituted as both controversial and accountable, not only in the parapsychological literature, but also by the participants in this study. Most of them seemed to attend to the interactional need to account for the status of evidence in parapsychology that such a class of phenomena exists (Collins and Pinch 1979:237). These participants' accounts appealed either to essentialist characterisations of *psi* (i.e. constituting *psi* as having identifiable properties as a reluctant or elusive research object), or to essentialist characterizations of evidence in parapsychology (i.e. constructing inherent characteristics of experimental outcomes in parapsychology, such as their ambiguity and unrepeatability). Participants engaged in significant negotiations of what counts as evidence, and what would be enough evidence in relation to different "audiences". In these descriptions, despite the difficulty of definition and description, *psi* was nevertheless constituted into an existing object.

This subsection will focus on the analysis of three main constructions of evidence in parapsychology. The first, in extracts 4 and 5, is the construction of ambiguity as an inherent characteristic of evidence in parapsychology, i.e. as something intrinsic to the nature of evidence in parapsychology. The second, in extracts 6, 7 and 8, is the construction of the elusiveness of evidence for *psi*, i.e. the lack of repeatability of *psi* effects as a trend in parapsychological research, and the construction of the consequences of this. The third, in extracts 9, 10 and 11, is the construction of demarcating criteria for what would count as "ideal" evidence in relation to the *psi* hypothesis.

Extract 4 – PA 11, lines 426-463

426. PA Ehm the little anecdote I always give is, any time I visit the house of somebody
427. who's interested in parapsychology you look in the bookshelves, many of them
428. seem to have the same kinds of books.
429. C Yeah.
430. PA They all seem to be interested in areas like quantum physics, eh cosmology,
431. origin of the universe, eh evolutionary theory, nature of consciousness.
432. C Hmhm.
433. PA And what those different areas have in common for me is a notion that this
434. they're likely to be so overwhelmingly complicated you're never gonna get a a
435. **discrete answer.**
436. C Hmhm, hmhm.
437. PA And there's a sense in which people who work in a field like this are highly
438. ambiguity tolerant.
439. C Yeah.
440. PA They don't expect- as as, I mean, I I've got friends who work in areas like
441. biochemistry, and they're so kind of rigid, things work out this way, =
442. C Hmhm.
443. PA = this is gonna be the outcome, you know, and it's gonna be a fact.
444. C Yeah.
445. PA You know, they're **still** in the business of facts. The people I know in this field
446. are very happy with the idea "we wanna work with the bi::g questions,
447. [but we're not gonna make much headway towards the big answers", =
448. C [Hmhm
449. PA = [you know? And it's a bit of a trade-off.
450. C [((laughs))
451. C Yeah, [yeah.
452. PA [Ehm (1) and I think is probably the same I'd I I accept wholly that I'm
453. looking at such complex systems all the time, =
454. C Yeah.
455. PA = and, in fact, genetically I'm better at dealing with them than I ever am gonna
456. be as a scientist, you know, £ because I I'm still alive. £
457. [you know so so far so good. Ehm but it's not a worry for [me because
458. C [((laughs)) [yeah, yeah.
459. PA = I'm not after definitive bottom line answers, it's more now and again we're
460. gonna get something of a glimpse or a snapshot, something =
461. C hmhm, hmhm

462. PA = and that's gonna be sufficient [really, I think.
463. C [Right, right

Extract 5 – PA 5, lines 918-942

918. PA There've been minds far greater than mine working on this for a long time, =
919. C Hmhm, hmhm.
920. PA = a:hm, you know, if it's gonna happen overnight it'll be, you know, I don't
921. think it's gonna be that (.) that easy.
922. C Yeah, [yeah, yeah .
923. PA [If it was, (we'd have) gotten it.
924. C Hmhm.
925. PA And so, given that we are looking at a hugely complex area where we do not
926. understand the moderating variables yet, and ambiguous outcomes are to be
927. expected.
928. C Hmhm, hmhm.
929. PA From time to time, if not, you know, more often than not. I don't know. A:hm,
930. **hopefully** not too much more often than not ((laughs)). **Hopefully** rarely,
931. **hopefully** you you get a better grasp [on things as time goes on, and eh =
932. C [Yeah
933. PA the outcomes. But then again, if if you're asking good process oriented questions,
934. half the time you're gonna get a null result.
935. C Yeah, yeah, [yeah
936. PA [It's just a part of what you're looking at, and and asking about. So
937. no, I mean, it would be, it would be wonderful if everything was clear cut and
938. there was no ambiguity.
939. C Hmhm.
940. PA But **certainly** that's, there's not much in *psi* [which works that way.
941. C [Yeah.
942. PA I don't know if there's much in psychology that works that way, (frankly).

4.2.1 Constructing inherent ambiguity.

In extracts 4 and 5, respectively, PA 11 and PA 5 engage in the construction of evidence in parapsychology as inherently ambiguous, by building up an account of this ambiguity as being empirically warranted and consensually agreed upon. Both participants make use of rhetorical features in their descriptions which transform them into factual accounts of what evidence in parapsychology is really like. In Extract 4²⁰, PA 11 constructs a normative account of the essential ambiguity of evidence in parapsychology, building up a “rule” in which a link is made between the inherent complexity of parapsychology as an area of inquiry and the inherent ambiguity of the evidence, i.e. the lack of definitive answers (line 435, in bold). In constructing parapsychology as an essentially complex field of research, the ambiguity of its experimental outcomes and claims is not only to be expected, but is even desirable, since it grants it the possibility of membership to the category of scientific fields where they deal with “big questions” (line 446) and, in doing so, forfeit the possibility of “big answers” (line 447). In Extract 5²¹, PA 5 upgrades the ambiguity of parapsychological evidence from an empirically warranted observation to an inherent characteristic of parapsychology. The construction of parapsychology as a difficult (line 921), “hugely complex” (line 925), largely unknown area (line 925 to 926), where clear cut answers are hard to come by, warrants (as in Extract 4) the construction of ambiguous outcomes as expected outcomes. This construction of expectation is then upgraded into an account of ambiguity as “part” (line 936) of parapsychological inquiry, i.e. into an essentialist account of the way “things work” in relation to *psi* (line 940).

The overall rhetorical achievement is the transformation of ambiguity as a result of not knowing enough, into ambiguity as a result of the way *psi* is. Both participants seem to attend to the discounting implications of constructing ambiguity of evidence within an empirical context. Thus, by a) establishing ambiguity as an inherent characteristic of the questions or phenomena that concern parapsychology, b) by accounting for the scientific desirability of ambiguity, and c) by establishing

²⁰ This extract is part of PA 11’s answer to a probe regarding a construct in parapsychological literature (“fear of *psi*”, attributed to Charles Tart, by the participant) as a possible explanation for the variability of results in parapsychology, which PA 11 mentioned during the interview. The interviewer (lines 371 to 383) asks the participant to assess the usefulness of this explanation, proying that it is “complicated” (line 375), in contrast with simpler explanations for this variability. Previous to this extract, PA 11 makes a case for the potential inadequacy of a simpler explanation, given that the system is complex in itself.

²¹ This extract is part of PA 5’s response to a question about the sorts of issues that the ambiguity in outcomes raises for her, as an experimenter, in relation to the validity of her work (lines 905-911). PA 5 had previously constructed ambiguity to be a characteristic of results in parapsychology, generally. Previously in her reply she stated that the ambiguity did not raise any doubts about the validity of her work. The extract included here includes the warranting of this claim.

that this is a characteristic of a consensually constructed “field of science” (i.e. psychology), these participants manage to present ambiguity as an empirical feature, rather than an empirical liability.

4.2.1.1 There are no facts in parapsychology, and that's a fact.

The analysis will examine how this account of ambiguous evidence in parapsychology was achieved in extract 4. At the beginning of this extract, PA 11 sets out the account with an anecdote regarding the varied, but always complex, interests of parapsychologists (lines 426–435). In doing so, he portrays the use of this anecdote (line 426, in bold) as something that routinely happens, is talked or thought about. During this anecdote, PA 11 lists a set of disciplines in which parapsychologists are commonly interested (lines 430–431). The function of this listing, as the participant points out in lines 433 to 435, is the construction of a general class of disciplines that examine phenomena which (like parapsychology) are so “overwhelmingly complicated” (line 434) that (like parapsychology), they will “never” have access to “a discrete answer” (lines 434–435). The use of the extreme case formulation in line 434 (in bold) seems to work as a way of building up the rhetorical load of the norm-like link between very complex questions and not getting “discrete” answers. This extreme case formulation suggests that PA 11 is engaging in the business of persuading that this maximal lack of “answers” is a consequence of the complexity of such areas of scientific inquiry, which include parapsychology.

This introduction allows PA 11 to move on to a normative description of the way parapsychologists view this ambiguity as part of the process of dealing with their research questions (lines 437–438). He constructs this state of affairs not only as inevitable, but also as desirable. This is achieved through a contrast between parapsychologists’ and biochemists’ expectations and scientific practices (lines 440 to 449). Having established the link between the complexity of questions and the impossibility of “discrete answers” (line 435), PA 11 constructs this as the consensus among parapsychologists. In a sense, the account of this “norm” authorises a version (Smith, 1978:34) of parapsychologists’ activity as a complex form of science making, one that is focused upon questions rather than answers. The use of biochemists as a contrast to this complexity allows research into “discrete answers” to be constructed as lower status, or less desirable, than parapsychological research. In lines 440 to 445, there is a sequential upgrading of this “lower status” as he makes relevant their rigidity, their prescriptive practices, their prescribed outcomes, and ultimately their focus on obtaining “facts” (line 440). The construction of an empirical inquiry that is “in the business of facts” would seem, at face value, an unproblematic description. However, the construction of this contrasting backdrop provides the “instructions” (Smith, 1978:39) to see fact-making science as an “inferior” version of scientific inquiry to the complexity that parapsychologists deal with. In describing parapsychologists as “being happy” with the idea that dealing with “big questions” sacrifices the ambition of getting “big answers” (lines 446–447), PA 11 makes relevant not only the inherent ambiguity of evidence in parapsychology,

but also the willingness of parapsychologists to deal with this ambiguity. The same claim is reinforced in lines 459 to 462, this time transposing the norm to his own case. It is interesting to note that the construction of the superfluousness of “bottom line answers” (line 459), and the sufficiency of “a glimpse or a snapshot” (line 459) also suggests that PA 11 orients to the description of this inherent ambiguity as an accountable issue. PA 11 seems to build up the practice of experimental parapsychology as an almost heroic pursuit, in which researchers take on a lot (big questions), but obtain very little. This construction of parapsychology as a “heroic pursuit” will be developed later in the analysis of extract 7.

Focusing now on extract 5, PA 5 participant also engages in the construction of this inherent ambiguity of parapsychological evidence. As was mentioned above, during this extract, PA 5 upgrades the ambiguity of parapsychological evidence from an empirically warranted observation to an inherent characteristic of parapsychology as an area of inquiry. In doing so, PA 5 starts (as did PA 11 in Extract 4) by setting out the status quo of parapsychology as a difficult (line 921) and “hugely complex” (line 925) area of inquiry. The use of the construction of “if this was that easy, we’d have already gotten it” (lines 921-923), and the reference to the longstanding expertise in the field (line 918), builds up parapsychology as a context of research where, despite the efforts of skilled dedicated researchers, *psi* phenomena remain largely unknown (lines 925-927). This rhetorical construction of a context of intense difficulty warrants the expectation of ambiguous outcomes. As PA 11 did in the last extract, PA 5 seems to be drawing up a causal link between difficulty, complexity and ambiguity of results. Ambiguity is thus not such an unexpected or problematic feature of parapsychological research. It is not an indicator of incompetence, but a characteristic that naturally follows from the complexity of the system.

The expectation of ambiguity is then upgraded into an account of ambiguity as an essential “part” (line 936) of parapsychological inquiry, despite the quality of questions that are asked (lines 933-934). In lines 933 to 936, PA 5 constructs an account of an essential ambiguity that is part of what a researcher in parapsychology “is looking at, and and asking about” (line 936). This account of the difficulty of maintaining clear cut or predictable outcomes, irrespective of the quality of research questions (line 933 to 934), allows the transformation of ambiguity as a result of having incomplete or unsatisfactory knowledge of the phenomena, into ambiguity as a result of the way *psi* is, i.e. as a known characteristic of the way *psi* works (line 940). From the repeated use of markers for the construction of uncertainty about the consistency of outcomes – “hopefully” (lines 930-931, in bold) – to the marker of certainty about this ambiguity – “certainly” (line 940, in bold) – PA 5 manages to transform the ambiguity into something that is known within the field, i.e. an empirically warranted outcome. As PA 11 did, PA 5 also seems to attend to the ambiguity of results as a potentially compromising issue in science by making available that this is a characteristic of a consensually accepted “field of science”, namely, psychology. The use of this comparison allows the construction

of a) ambiguity as an empirical feature, rather than a troublesome necessity, and b) by proxy membership to the category to which psychology belongs.

4.2.2 Constructing *psi* as an elusive research object.

In addition to these accounts of the inherent ambiguity of evidence in parapsychology, participants constructed *psi* as elusive in itself. Their accounts of the intense difficulty in obtaining coherent outcomes were constructed not only as empirically warranted and consensually agreed upon, but also as the result of what *psi* is like as an experimental object. In this sense, the ambiguity of experimental evidence is made sense of by making available a theoretical characteristic of *psi* that would account for this ambiguity. The analysis here will focus specifically on how the construction of this elusiveness was performed and used. Analysis of extract 6 will focus on how a process of research without coherent outcomes is framed successively as admirable, persistent and potentially pointless. Analysis of extract 7 will focus on the construction of the intense difficulty in parapsychology of replicating outcomes. It will also examine how the participant constructs a direct upshot from his account of lack of repeatability of outcomes to be the unsustainability of the existence of *psi*, thus orienting to a construction of replication as a neutral judge of the disputed claim (Potter, 1996a:28). Along with the analysis of extract 8, it will also be possible to explore the construction of doing research in parapsychology as an almost heroic pursuit.

Extract 6 – PA 9, lines 942-956

942. C Do you think this this your your views about this this idea of what is, you know,
943. wha- what's .h successful what's not that successful, do you think that they are
944. reflected in what you see within parapsychology, other folk in the field?
945. PA Oh sure. I'm I'm totally impressed by the dedication here²², by the creativity, by
946. the eh the number of eh people in the field who are doing these experiments very
947. often in university settings, and who **come year after year** reporting null results
948. and then go back and do more experiments £ and then again with null results £
949. and, and yet they do these whole series of experiments and they
950. have these very, you know, logical and and even creative hypotheses and .h and
951. they just keep plugging away at it =
952. C = Yeah.

²² The 45th Annual Convention of the Parapsychological Association, during which the interview took place.

953. PA = and eh it's it's marvellous the dedication you [know, the perseverance =
 954. C [Hmhm, hmhm.
 955. PA = ((laughs)) and bit like knocking your head on a brick wall, but... ((laughs))
 956. C [Yeah, it's true, yeah, yeah,

In Extract 6, PA 9 presents an account of the routine process of experimentation in parapsychology without positive outcomes. The analysis will examine how PA 9 performs successive constructions of this process as admirable, persistent and, finally, potentially pointless. It is interesting to note that this account is clearly constructed as a spectator's commentary of what she sees, i.e. she uses the "footing" (Goffman, 1979) of an independent observer. This seems to have two important functions here: a) it constructs the neutrality of the description, warranting it as a factual account of the way things happen in parapsychological research; b) it allows her to construct such research as "a bit like knocking your head on a brick wall" (line 955) in a way that undermines the validity of such practice in general, without offering the implication that this refers to her own practice. In lines 947 to 948, PA 9 offers an account of research work in parapsychology, one of a cycle of practices in which experiments yield null results, then these null results encourage more experiments with more null results. The description of a) the routine – "year after year" (line 947, in bold) –, b) the compromising action of coming to a conference to report "null results" (line 947), and c) the extra detail of this barren repetition taking place within "university settings" (line 947), makes up a version of the experimental routine that is potentially compromising. The construction of this version of research seems to be designed in a way that it is hearably extreme, i.e. it seems to orient to this description as a demonstration (Edwards, 2000:348) of the ordeal that parapsychologist go through every year. In doing so, PA 9 seems to offer three ways in which to assess this routine, by appealing to three different frames (Goffman, 1974) through which 'what is going on' might be understood: a) as a manifestation of "dedication and creativity" (line 945); b) as an action of persistence or steady work – "the just keep plugging away at it" (line 951); and c) as a fruitless or pointless endeavour – "and a bit like knocking you head on a brick wall" (line 955). The availability of these three alternative ways of making sense of the constructed routine allow PA 9 to move from talking about it as an admirable effort to a pointless process, with the latter frame being made rhetorically safe by the former. In warranting the construction of this routine in the first frame, yet undermining it in the last, PA 9 seems to orient to the questionable validity of an empirical process that is constructed as routinely failing to yield evidence for *psi*. This, in turn, can be seen as orienting to the problematic question of how parapsychology can continue researching *psi* as a warranted and viable pursuit when, as Collins and Pinch (1982) put it, it requires a consensually warranted "natural substance to be manipulated in experimental circumstances" (p. 131). In the next extract, we will examine how PA 13 orients to this question as he constructs the lack of replicability of *psi* as a compromising issue, and employs the first of the frames used by PA 9, that of parapsychological research as an admirable, goal oriented pursuit.

Extract 7 – PA 13, lines 682-747

682. PA I think again researchers and I know there's some controversy cause ehm within
683. the the community, about whether we really we should put so much emphasis on
684. replication. I th- I think we are under obligation to to replicate whenever we can.
685. (.) [Ehm, the danger o' course is (.) ehm and one of the disheartening things in =
686. C [Hmhm
687. PA = parapsychology is that you get an effect, you try and replicate it and you can't
688. replicate it, and eh that's one of the great disappointments in parapsychology. So
689. my, I mean I've looked at eh most of the data on on the main replication, and it's
690. looking like it's not (.) it's (.) well it's certainly not gonna come out significant,
691. maybe a slight effect but it's it's not gonna be be replicated on [the replication.
692. C [Hmhm
693. PA [Which is (1) ehm (.) is disappointing, because it indicates that there wasn't =
694. C [Hmhm
695. PA = really an effect, then maybe eh it was just a fluke, ahm and we were being
696. mislead [mislead there's not a real effect there.
697. C [Hmhm
698. C Hmhm.
699. PA Cause it obviously would be nicer if if it we would get some evidence that
700. **there's something real here, in terms of something that is (.) is lawful.**
701. [For we can then we can then go on to the next step of research in terms of =
702. C [Hm.
703. PA = trying to understand it. **We can assume that there is something happening**
704. **here** so let's try and do the next step. We're always being- it's snakes and ladders,
705. isn't it. [We we kind of climb climb the ladder and then suddenly we go back to =
706. C [Yeah
707. PA = stage one again cause it was a it was a false lead. And that's the really how
708. parapsychology operates a lot of the time. At it is it it can be quite a disheartening
709. thing, I I know a lot of parapsych-, well I don't know a lot of parapsychologists,
710. but I (.) I sense that a lot of parapsychologists get disillusioned with the the whole
711. discipline particularly within experimental parapsychology,=
712. C = Hmhm =
713. PA = because the the the more experiments they, it's the law law of large numbers
714. again, the more experiments they do they may start off doing well but then
715. [(1) it all it all kind of eh dissolves and dissipates after a while and and =
716. C [Hmhm
717. PA = you're left wi- with nothing. [It's it's disheartening I mean (scientists) =

718. C [Hmhm

719. PA = researchers want to be finding out real things. They want to be contributing to
720. knowledge and if they spend **all this time** doing **all this research** and at the end
721. of the day there is **nothing going on**,=

722. C Hmhm

723. PA = then (.) ehm it it's (.) it's valid science, in as much as we proved that there's
724. nothing going on, but it's on a personal level it's quite disheartening I [think.

725. C [Yes. And I

726. suppose that that would sort of raise questions (.) to yourself as (.), you know,
727. [in terms of not only the validity of the work that's been done=

728. PA [Yeah.

729. PA Hm.

730. C = but also in terms of, you know, is this worth it. =

731. PA = Is it worth it? [Yeah on a personal level, at the end of the day we're we're =

732. C [(laughs))

733. PA = people doing doing research and and we do research because we think it's
734. important to do it. If you **really thought** that you're gonna spend **50 years** doing
735. parapsychological research, and **at the end of the day** the **sum total** of your
736. research will **mean nothing**, that you **haven't really found anything**, then really
737. we should be doing something else.

738. C = [(sniggers))

739. PA [(laughs)) So I gue- I suppose it depends upon your faith in (.) in in the poss-
740. in the reality of the thing that you're you're hunting. =

741. C = Hmhm

742. PA Yeah? So it's £ hunting of the snark £ =

743. C = ((laughs))

744. PA Is there a snark there that we're ehm that we're really hunting, [or are we =

745. C [Hm

746. PA = hunting [shadows?

747. C [Hm. Yeah, yeah.

This extract is a long account of how results supporting the *psi* hypothesis fail to be replicated. As Collins and colleagues explored and pointed out (e.g. Collins, 1985), replicability is often constituted in scientific disputes as the touchstone of empirical research, as it implies the trustworthiness of an outcome. Repetition is often constructed as an assurance that the representational practices of science are repeatedly accessing the unchanging object of research. Hence, it can be appealed to as an “out-there” neutral judge of the validity of products of scientific actions. In this extract, PA 13 attends to

this idea of replicability as a neutral criterion for distinguishing between a valid, genuine outcome and an artifact of unwilling or willing experimental manipulation (Rosenthal, 1991:2), i.e. replication is built up as the marker for the “reality” of *psi*. In the latter part of this extract (lines 725 to 746), PA 13 manages the relationship between the logical implication of the lack of replicability of *psi* effects (i.e. its non-existence) and the warranting of the continued search for *psi*. As in the extract above, the question that PA 13 seems to orient to here is the one Collins and Pinch (1982:131) put forward – how is it that empirical questions about *psi* can continue to be posed and answered, if, ostensibly, there are no natural phenomena to be manipulated and examined?

4.2.2.1 Constructing a normative account of the lack of replicability of *psi* outcomes.

In the extract above, PA 13 introduces the issue of “the emphasis put on replication”²³ as a controversial issue in parapsychology (line 682-684). In lines 685 to 688, PA 13 puts forward the central claim of the extract – that *psi* effects do not replicate – as a normative account of the state of affairs in parapsychology. The use of the formulations “one of the disheartening things in parapsychology” (lines 685-687) and “one of the great disappointments in parapsychology” constructs this failure to replicate as an inevitable characteristic of doing work in the field. The use of this initial normative account seems to have three rhetorical functions. First, the construction of this norm makes available a sort of coherence between events, in the sense that the ongoing lack of replication can be seen itself as a form of replication. Second, the inference that the inability to replicate findings is widespread contributes to its constitution as a fact, but without committing PA 13 to the implication that it happens every time, or with everyone (Potter, 1996a:162). Finally, the construction of this norm attributes the ability to “dishearten” (line 685), to “disappoint” (line 688) or to “disillusion” (line 710) to the effects themselves (and away from the actions of the experimenters). As in the characteristic of ambiguity explored above, effects in parapsychology seem to have these properties and, thus, cause these feelings. After all, it is not reasonable to think an experimenter would choose to disappoint or dishearten himself. In lines 688 to 691, PA 13 then introduces his own inability to replicate a successful effect as an example of this norm. Indeed, this example seems to have been constituted as almost a consequence of this norm – notice the signalling of consequence “So,(...)” (line 688, in bold). The previous construction of the norm provides a safe context for talking about his own experience of not replicating, thus forestalling the discounting implication of being an incompetent researcher. In this context, not being able to replicate an effect is made not to count as a failed experiment, but rather one that follows a trend.

²³ This extract is part of PA 13’s reply to a question about his empirical response in the case of obtaining an anomalous outcome, following on from his own construction of what an anomalous outcome would be in his research.

4.2.2.2 Constructing the empirical consequences of the lack of replicability.

Having made this normative account of the non-replicability of positive *psi* effects, PA 13 also constructs the empirical consequences of this trend i.e. that a positive result might be a fluke (lines 695-696). He further links replication or lawfulness with reality (line 700, in bold), and replication with the possibility that “there is something there” (lines 703-704, in bold). Given the norm of “non-replicability” that was drawn up before, these links offer the logical implication that if *psi* does not replicate, then it does not exist. However, this upshot is not made. Instead the participant constructs the failure to replicate as a game of “snakes and ladders” (lines 704-707), making available the idea of a pursuit with an end point, albeit an elusive one. Rather like PA 9’s final frame – “a bit like knocking your head against a brick wall” (Extract 6, line 955) – this construction of perseverance in the search for a replicable effect is built up as a “1 step forward, 2 steps back” situation. However, rather than being a potentially pointless activity, PA 13 constructs it as a search, admittedly difficult but with a goal in mind. Despite orienting to replication as a marker of reality, PA 13 is finally able to put forward an account of the difficulty of replication as a factual characteristic of *psi* research (line 703-704, in bold), rather than the alternative available account that *psi* is not a real experimental effect.

This normative account of the lack of replicability of *psi* outcomes as the way things are in parapsychology is pursued further, and upgraded as PA 13 makes a reference to the “law of large numbers” (lines 713-717). The idea that PA 13 puts forward is that as parapsychologists repeat trials, the less likely they are to find a positive outcome, as these “dissolve and dissipate after a while and you’re left with nothing” (lines 716-718), i.e. that there is something lawful about this dissipation of results. This is not normally what is meant by the concept of the “laws of large numbers”, these are mathematical theorems that guarantee that very large samples will be highly representative of the population from which they are drawn (Tversky and Kahneman, 1971). Although the accuracy of the participant’s assertions is not of analytical interest, the dissonance between PA 13’s definition and that available in textbooks points up the rhetorical value of making a reference to a law, regardless of the correspondence between its rhetorical use and textbook definitions. The fading away of results in parapsychology is made much stronger and factual as a manifestation of a mathematical law, than as a personal observation or opinion. It also makes available the implication that *psi* belongs to the universe of events that are subject to this law.

In lines 717 to 725, the participant provides a gist of the trouble with lack of replication in parapsychology, setting it against another normative account of scientists’ motivations and desires as “finding out real things” (line 719) or “to be contributing to knowledge” (lines 719-720). In doing so, he achieves the inclusion of parapsychologists within the category of scientists, but at the expense of admitting that lack of replicability is a problem that might eventually frustrate the search for the

reality of *psi*, or might lead to the conclusion that “there’s nothing going on” (line 721). This is in direct contrast to his earlier comparison of parapsychological research as a game of “snakes and ladders” (line 704), in which such a conclusion is avoided. Instead, he uses a series of extreme case formulations (lines 720 to 721, in bold) to downplay the possibility that “all this research” might be pointless. Nevertheless, the possibility is attended to immediately afterwards, as he concludes that “it’s still valid science in as much as we proved that there is nothing going on” (lines 723-724). At the end of the day, the empirical consequences of lack of replicability are that *psi* might not exist after all, but the scientific worth of the quest is nevertheless achieved.

4.2.2.3 Managing the implication of pointlessness – the construction of a “heroic” stance.

The last segment of analysis of extract 7 is concerned with the compromising issue of potential pointlessness. This is developed in the last few lines of the extract (lines 725-747), where PA 13 takes up a comment by the interviewer (lines 725-730) regarding the “worth” (line 730) of developing research work that might result in “nothing being there”. The comment is immediately taken up as a personal question (line 731), and the worth of research justified on the grounds that they (researchers in parapsychology) think it is “important to do it” (line 734). This is oriented to as an insufficient formulation of this justification, as PA 13 goes on to construct and rhetorically use the possibility of parapsychology being a worthless enterprise (lines 734 to 737). It is argued that this construction of worthlessness is used as something against which to contrast the justification “we think it’s important to do it” (lines 733-734) that was offered before. In doing so, PA 13 uses rather extreme terms (in bold), working up the absurdity of continuing to do research if no outcomes were to be forthcoming. The use of this contrast allows PA 13 to reinforce the idea that he does not believe that this to be the case. This is then spelled out in lines 739-740 i.e. his “faith in the poss- in the reality of the thing you’re hunting”. This construction is interesting in two ways. First, in contrast with the common construction of valid research as the neutral representation of facts, independent of factors such as belief (Mulkay, 1979:20), it uses personal belief to warrant an empirical quest. As in the case of the experimenter effect (section 4.1.3), the distinction between personal (contingent) variables and empirical ones is being blurred. The second interesting feature of this formulation is that it constructs the experimenter as a resilient hunter with only his faith to keep him going. A similar point is made by PA 16 in Extract 8, examined below

Extract 8 – PA 16, lines 628-637

628. PA [It's kind of saying is there something here which really is meaningful, =
629. C [Yeah
630. PA = that beyond kind of people's a- anecdotes of unusual things? Is there
631. something here we can do science with? (.) And maybe even then develop and
632. and take you know apply it that [kind of ().
633. C [Hmhm, hmhm.
634. PA And I think, you know, **people still sort of almost dream, I think it's what**
635. **strives many of the research in the [field saying, saying thinking, =**
636. C [Yeah, yes.
637. PA = **"yeah that it's just round the corner".**

This quest for the presently unattainable, for something that is almost dreamt of, is reminiscent of PA 11 (extract 4) and PA 13 (extract 7), in which formulations of the difficulty in obtaining positive results is used to present experimental parapsychology as an almost heroic pursuit. As in extract 7, where PA 13 used the metaphor of the "hunting of the snark" (lines 742-746), PA 16 justifies the continuing parapsychological research in terms of a quest which is based on a "dream" (line 634) or hope that evidence might be produced in the future. The articulation of this quest for *psi* in terms of something the "is just around the corner", allows him (like PA 13 above) to construct a point to doing parapsychological research, i.e. the exploration of unknown territory, forestalling in this way implications of pointlessness and constructing the activities of parapsychological researchers as a near heroic search for *psi*. It is interesting to note that Hess (1993:) points out similar themes in his cultural analysis of parapsychological literature. Although his analysis is substantively and methodologically different from the present thesis, Hess (1993) identified in parapsychological literature (namely, in the writings of J. B. Rhine) two similar metaphors – that of "explorers of the New World" (p. 54), and that of "heroes" (p. 85) – as organising devices for the justification of both parapsychologists' practices and their position in relation to science. Thus, like the explorers of the New World, parapsychologists can afford to be ignorant or uncertain about what lies ahead (i.e. what is there to find out), and like heroes, parapsychologists "battle the entrenched forces of science in order to help bring about a new knowledge" (Hess, 1993:85).

In summary, the construction of elusiveness as an inherent characteristic of *psi* provides a) an explanation for the ambiguity of evidence in parapsychology; b) an explanation that does not compromise the scientific competence of parapsychologists (on the contrary, it allows them to construct themselves as being involved in an heroic exploration); and c) an ostensible experimental finding, one that potentially explains problems of lack of replication (indeed, might be seen as

providing replicated findings). However, as the extracts above show, participants nevertheless oriented to lack of replication as a persistent problem with empirical consequences if one wants to be seen as doing science. The next section describes how participants oriented to this problem and others in their accounts of what is missing in parapsychology i.e. what would make it scientifically ideal.

4.2.3 Constructing an ideal version of parapsychological evidence.

The various constructions explored above seem to have in common the idea that evidence in parapsychology is troublesome. The constitution of such evidence as ambiguous, or as elusive in character, raises the problem that the phenomena under investigation, either, may not be amenable to the practices prescribed by consensual accounts of empirical science, or may not exist at all. Accounts of evidence for *psi*, if they are to succeed rhetorically, would arguably need to fit into accounts of warranted scientific knowledge. Many participants offered accounts of “it would be good if we got...”, orienting to standard accounts of demarcation of what counts as warranted knowledge (Woolgar 1988:16). The extract above showed how replication was appealed to as an essential entity that could transform the claim that *psi* exists into reality. The analysis of the following extracts will explore how these exercises in demarcation were constructed, and to what features of “science” they appealed.

Extract 9 – PA 4, lines 226-289

226. C Hmhm, hmhm yeah, ok. And (following along) from that and I suppose talking
227. about, ahm, your experience and, you know, your experience in working er with
228. an experimental model, =
229. PA Hmhm.
230. C = what would to you constitute a successful outcome?
231. PA (1) What, (.) what type of outcome?
232. C Yeah, what type of outcome would you say ok this is a success, this this this
233. experiment =
234. PA = Well, I don't think there's anything in parapsychology to [drink of water], that
235. to me provides an ideal outcome, because there's nothing in parapsychology that
236. is easily repeatable. Now, (.) for something to have an ideal outcome in
237. parapsychology I would impose 3 conditions, anyone of which would be
238. satisfactory. First of all would be (.) **a repeatable experiment.**
239. C Hmhm.

240. PA **Now, I don't see anything in parapsychology that's repeatable. The irony is**
241. there are many (1) accepted principles in psychology based on experiments
242. which are not repeatable either.

243. C Hmhm.

244. PA In other words, repeatable results in psychology in general are very difficult
245. because human beings are so complicated. So, (.) ahm, (.) **but the difference is**
246. that in mainstream psychology, we know enough about the laws of learning, about
247. perception, about cognition, about the brain, to give an explanation as to
248. mechanism, for these results. So even if not, even if the (2) results of the
249. experiments themselves are not always repeatable, if they're generally repeatable,
250. and if we have an explanation as to the mechanism, it becomes part of what goes
251. on in psychology texts and classrooms.

252. C Hmhm, hmhm

253. PA S- and there are many, many theoretical mechanisms in parapsychology, like the
254. observation effect, which is the most sophisticate-, mental radio which is least
255. sophisticated, and of course there could be experiments that eh take each of these,
256. propose mechanisms and test it out.

257. C Hmhm.

258. PA **Well, this requires several years, this requires a great deal of money,**
259. **parapsychology simply has not had the resources to do that.** Now the third
260. possibility, (.) would be that some practical use be eh found.

261. C Hmhmh.

262. PA In if there were practical use, one wouldn't worry about mechanism, one
263. wouldn't worry so much about replication. (.) **But what practical use can you**
264. **find with telepathic dreaming?**

265. C Hmhm.

266. PA In recent years, as you know, the US government and other governments too
267. spent a great deal of money trying to ah (.) use (.) er clairvoyance, remote
268. viewing for espionage purposes. And after all that money was poured into it,
269. there's still this controversy today whether that was satisfactory or not.

270. C Hmhm

271. PA One group of people says yes, that was a good use of the taxpayers money, the
272. other group says no, nothing really was found out about that, that was a waste of
273. time.

274. C Hmhm.

275. PA So I **get back to my point**, I simply do not see a dependable practical use of
276. parapsychology today. Certainly not on psychic healing, sometimes it seems to
277. work, sometimes it doesn't work.

278. C Hmhm.
279. PA When I'm sick I don't depend upon getting well by psychic healing alone. Of
280. course I call upon healers I know, but I also call upon doctors I know.
281. C Yeah, yeah.
282. PA So, (.) you know, the good news is that there has been a lot of progress made in
283. parapsychology. The BAD news is, number 1, we don't have a repeatable
284. experiment, number 2, we do not have (.) a:: easily understood mechanism, and
285. number three we do not have a dependable application.
286. C Hmhm, hmhm.
287. PA So, that's the conundrum [that parapsychology is in, and this is why =
288. C [((sniggers))
289. PA = parapsychology is not a part of mainstream psychology textbooks.

This extract shows the construction of the ideal version of parapsychological evidence in a particularly clear structure, indeed, in a way that resembles a recipe. This “recipe account”, which lists the ingredients for making “ideal” (line 235) parapsychological evidence, is presented from the vantage point of a neutral expert. It is constructed by PA 4, therefore, as an “avowedly disinterested story” (Potter, 1988:20), similar to accounts of demarcation in the philosophy or history of science. The criteria are also used for the construction of a series of contrasts which “supply the instructions” (Smith, 1978:39) with which to read parapsychological evidence as dissatisfactory or troublesome. Overall, this account of ideal evidence sets out the ways in which parapsychology could achieve the resolution of both the controversy over the reality of *psi* and the controversy regarding its own position within the category of field of science.

The extract starts out with a question regarding the participant's assessment of what would be a “successful outcome” (line 230) within the context of his research. PA 4's reply is slightly delayed, but, as he takes up the question, he first transforms the category “successful outcome” into “ideal outcome” (line 235), and then he contests the existence of “anything in parapsychology” (line 234) that would fit this bill. This transformation from successful to ideal is significant in that a) it avoids answering a question about what would be a successful outcome; b) it redirects the question towards outcomes that are, by definition, virtually impossible to obtain; and c) focuses upon an extreme and, therefore, warranted abstract category of outcomes, rather than actual ones. Thus, in explaining what “ideal” means, PA 4 is able to explain why nothing in parapsychology matches it (lines 235-236). In doing so, he appeals (as others did above) to “replication” as a guarantor of the validity of evidence. This is provided immediately, and in isolation, suggesting this is to be the most readily available of the conditions that follow. From this point onwards (lines 236 to 238), PA 4 sets out the “recipe account” for ideal evidence in parapsychology, introducing the list of attributes that are presented as

using Widdicombe's (1998:58) term, criterial for a scientifically ideal outcome. In line 237, PA 4 constructs these as "conditions" that he would "impose" in order for him to consider an outcome as ideal. Thus, this account seems to set out demarcation criteria for ideal outcomes, constructed as factual and self-evident, and PA 4's footing (Goffman, 1979) as a disinterested agent of application of these criteria. Each of these criteria will be examined in sequence.

The first criterion to be established is that of replicability of evidence (line 238, in bold). This is different from the subsequent two criteria in that a) it is initially presented as sufficient by itself (line 240); and b) the significance of its absence is qualified by a comparison with psychology. In respect to the latter, PA 4 offers, and "shows" to be offering (Antaki and Wetherell, 1999:9), a concession in relation to the robustness of this criterion as a sufficient guarantee of the validity of evidence – "the irony is (...)" (line 240, in bold). On the one hand, the comparison downplays the relationship between replication and scientificity. On the other hand, however, it equates results in parapsychology with results in psychology, an area of accepted scientific enquiry. The implication, then, is that while replication might be an available criterion, it is not a necessary one. The upshot is, therefore, that the lack of replication should not be sufficient to bar parapsychological evidence from the category of scientific knowledge. The insertion "the irony is (...)" (line 240, in bold) is therefore readily readable (Antaki and Wetherell, 1999:10) as a way of pointing up the injustice of lack of replicability excluding parapsychological claims from this category (lines 241-242). In line 245 (in bold), PA 4 qualifies the rhetorical parallel with psychology, and rehabilitates replication as a valid ingredient for the construction of ideal evidence. This is done through citing another criterion, an explanatory mechanism (lines 246-250), the lack of which in parapsychology warrants the exclusion of the latter from "what goes on in psychology texts and classrooms" (line 251). In the next segment PA 4 explores this second demand for the construction of ideal evidence – the presence of a "mechanism".

The second criterion constructed by PA 4 as a "condition" (line 237) for "ideal" evidence in parapsychology is the existence of a "mechanism" (lines 253-259). This criterion is also used to set up a contrast that supplies the necessary context for reading parapsychological evidence as dissatisfactory or troublesome. In lines 253 to 259, PA 4 provides an account of the current status of development of explanatory models. This account is interesting as it begins by making available the multiplicity of explanatory options in the field (line 253, in bold), then immediately constructs the logistic impossibility of testing and empirically warranting a mechanism in parapsychology (lines 258-259, in bold). The formulation of these demanding conditions – "this requires several years, this requires a great deal on money" (lines 258-259) – justifies the lack of a mechanism by appealing to structural reasons, rather than matters related to the validity of the quest, or indeed the competence of experimenters.

The third and last ingredient (in lines 259 to 260) is the possibility of a “practical use” for *psi* phenomena. It is interesting to note that this “practical use” is constructed as a more reliable and convincing representation of *psi* than replicated experimental outcomes, or the experimental verification of an explanatory mechanism for *psi* (lines 262-263). PA 4 seems to attend to the ultimate fact- or reality-making value of an application of the phenomena, i.e. the transformation of parapsychological knowledge into parapsychological technology allows the implication that parapsychological facts act, and leave an impression, upon the world out-there. He orients to what Woolgar (1988:34) called “the appeal to an hierarchy of representation” as a solution to the problem of representation, i.e. the construction of “a practical use” as offering a more dependable correspondence between the object (*psi*) and its representation (its recordable actions upon the world). In the following extracts (10 and 11), PA 19 and PA 20, respectively, attend to same “hierarchy of representation”.

Extract 10 – PA 19, lines 874-892

874. PA **Now, if someone could eh come up with a machine that showed you**
875. **telepathy waves, which might sound stupid, but I don't think it probably is that**
876. **stupid, then that probably would convince people.** [In that way we could do =
877. C [Right.
878. PA = brain scans and can see different bits of activity.
879. C Hmhm.
880. PA If you had some sort of video camera where you had two people, and when they
881. were trying to be telepathic you saw this blob come out of someone's head and
882. the blob come out of the other and they [met, blink.
883. C [meet, yeah, hmh
884. PA Then that people believe in (1), yeah?
885. C Right
886. PA I'd say.
887. C Hmhm.
888. PA **Because then you can actually see it happening.**
889. C Yeah.
890. PA **If you haven't got that then I don't think it will,=**
891. C Right
892. PA = just because there's too many arguments and counterarguments.

Extract 11 – PA 20, lines 877-885

877. PA I mean I think that **the only way that-**. I mean what would really be very
878. convincing, would be some kind of technical, (.) technological application.
879. [Let's say if you could-. You know, if this, (.) I mean that would just kind of =
880. C [Right.
881. PA = would I think in in in one (.) fell swoop just silence the skeptics and the
882. critics, you know, you've got this little device here and this
883. [relies on £ paranormal power to work £ and there's no other way =
884. C [((sniggers))
885. PA = of explaining what going on, well that's it, you know, case closed.

As PA 4 constructed “an application” as the most convincing representation of *psi*, both PA 19 (lines 874 to 876, in bold) and PA 20 (line 877, in bold) construct the status of a technological representation as evidence for *psi*. In the same way that the correspondence between the readings of a voltmeter and voltage (Woolgar, 1988:34) are constructed as more reliable than, say, a person's answer to a questionnaire about a feeling or thought, PA 19 and PA 20 orient to the physical representation of *psi* as the ideal confirmation of its reality. Similar to the current emphasis in psychology upon material technological representations of thought, the machine that PA 19 and PA 20 talk about would provide the rhetorical power that an EEG trace or an fMRI image of the brain allow psychological claims to enjoy. These inscription and imaging devices thus “embody the capability of making an apparently direct (or unmediated) connection” (Woolgar, 1988:103) between the shape of a trace of electrical impulses, or images of blood flow in the cortex, and the character of memories and thoughts in themselves. Based on this simple idea of “seeing it happening” (Extract 10, line 888, in bold), this physical testimony for *psi* – the imaging of a telepathic blob (Extract 10, line 881) or the *psi* operated device (Extract 11, line 883) – is constructed as the answer to the controversy over its reality (line 890, in bold).

Returning to PA 4 (extract 9), the participant constructs the “practical use” of *psi* as a way of curbing the representation problem, i.e. the development of *psi* technology would make evident its agency in causing actions or events in the world. However, in lines 263-264 (in bold), PA 4 sets up the third and final contrast between the ideal condition and current state of affairs. He then raises the example of significant funding towards a potential application for *psi* in espionage (lines 266 to 273), the result of which he presents as being disputed. This is interesting for two reasons: first, the lack of funding was raised moments earlier as a reason for a lack of a mechanism for *psi* being found; second, despite such funding, no agreement was reached upon the worth of any application. These are therefore interactional matters requiring attention, as they suggest contradictory accounts of both the necessity

of funding and the status of an application as a self-evident illustration that *psi* is real. These are attended to by describing the dispute as between “others” in or outwith parapsychology, employing the footing of a neutral outside observer of the controversy, before “getting back to my point” (line 275) that no practical application is available. Thus, the transformation of “successful outcome” into “ideal outcome” that began this segment allows him to avoid the problems that lack of success poses (e.g. implications of incompetence, or of being engaged in a pointless endeavour), whilst discussing ideals (replication, mechanism or application) that are unattainable. The “conundrum that parapsychology is in” (line 287) can thus be explained by the lack of replicability, of an “easily understood mechanism” (line 284) and of a dependable use, providing a justification for parapsychological claims being outside the category of those accepted in “mainstream psychology textbooks” (line 289). The exclusion from normal science is therefore explained, but the reasons are kept distant from the practical day-to-day business of doing parapsychology.

In summary, this section examined the construction of parapsychology as an identifiable body of evidence. The problem of lack of consistent results was constructed as being the result of evidence in parapsychology being inherently ambiguous. The problem was attended to further by constructing *psi* itself as an inherently elusive research object. Both these essentialist constructions locate the problem of finding evidence for *psi* within realm of evidence rather in the practices of the experimenters. The problem of lack of replication was further constituted as a form of replication itself, but one with empirical consequences, i.e. that the experimental quest for *psi* might be pointless. This, however, was constructed as an “heroic” quest (i.e. one that prevails in the face of difficulty), thus managing the implications both of incompetence and of pointlessness. The ongoing problem of lack of consistent evidence was attended to in the construction of ideal evidence for *psi*, rather than existing evidence for *psi*. In doing so, however, these ideals of replication, mechanism and application were undermined as absolute arbiters of scientificity. Hence, the “conundrum that parapsychology is in” that even ideal practise would not obtain ideal evidence. The problematic position of parapsychology as a field of scientific research is the theme of the final section.

4.3 Working up a position and a role for *psi* within science.

This section will examine participants’ constructions of the role of parapsychological evidence and knowledge in the context of science. Collins and Pinch (1982) have stated “a belief in the existence of *psi* phenomena and the incompatibility of *psi* phenomena with some part of science leads to the conclusion that science must be changed to undergo a revolution” (Collins and Pinch, 1982:49). Hess (1993:79) also points out the availability of this revolutionary metaphor for the role of *psi* in relation

to science. In this study, parapsychologists themselves oriented to this revolutionary position as one which affords the implication that parapsychology has a dramatic contribution to make to scientific knowledge. However, as we shall see, this position was built up as being conditional upon the (ever pending) central question of the reality of *psi*.

Extract 12 – PA 13, lines 510-530

510. PA Yeah, I'm not sure wha- what my train of th-. One one point I was making was
511. the (1) ehm, well **obviously** a lot of the scientific community (1) are gonna take a
512. lot of convincing **simply because** if if ES- if *psi* is real then (.) it **can't be**
513. **incorporated** within th- the general scientific understanding of the world.
514. C Hmhm.
515. PA So basically it it it (1) eh it invalidates their their whole th- theory of the universe
516. [really, so it's it's heresy basically. So parapsychology is is heretical for [for =
517. C [Hmhm [Right
518. PA = many scientists. So many scientists will will simply dismiss it. Because it can't
519. be true because it violates the laws of nature.
520. C Hmhm
521. PA Ehm, (.) I think most open minded scientists, s- if if we could present them with
522. (1) eh unchallengeable data, we could put the cards on the table and say "look we
523. got all this data, you can't ignore it", =
524. C Hmhm.
525. PA = we we need to try and find some way of incorporating that within science, then
526. I think a lot of scientists would would say "yeah, this is amazing, there is all this
527. data, we ne- we need to revise our our models of the universe".
528. C Hmhm.
529. PA And the problem is that we're we're we're not yet in a position to be able to
530. present the case strongly enough, I don't think.

In this extract, PA 13 provides an account of the role and position that parapsychological claims have in relation to "the scientific community" (line 511). This account appears to have three analytically interesting features. First, it constructs *psi* as being inherently peripheral to accepted scientific knowledge, i.e. in a position outside an established realm of "science" (lines 511-512). Second, it constructs a role for *psi* that is inherently revolutionary, i.e. if accepted within this context of science, it will clash with fundamental ideas that make up science's accepted knowledge about nature (lines 512-519, 527). Third, it constructs the imperative of "unchallengeable evidence" (line 522) in order

for *psi*'s revolutionary role to be fulfilled. In doing so, the participant orients here to what was explored in the previous extract as the unsatisfactory or troublesome status of reality making evidence for *psi*. The analysis will explore each of these points sequentially.

The first construction to be examined here is PA 13's location of *psi* outside the realm of issues that concern "the scientific community" (line 511). He does this by orienting to an existing boundary that separates *psi* from accepted, known things. What is interesting here is that Collins and Pinch (1979:254) have identified the appeal to this boundary by critics of parapsychology, when warranting their views about *psi* being an illegitimate scientific object. What has been seen as a rhetorical strategy of critics, therefore, can also be seen as a problem to which parapsychologists themselves attend. In lines 511 to 512, PA 13 builds up an obvious clash between the potential reality of *psi* and the maintenance of the "general scientific understanding of the world" (line 513). This is then upgraded to an incompatibility imperative – *psi* "can't be incorporated" (line 512-513, in bold). Thus, parapsychologists themselves are located separate from "a lot of the scientific community" (line 511), with the task of convincing others in that community that *psi* is real. Their position and the difficulty of this task are worked up as normal and expected, using words such as "obviously" and "simply" (lines 511 and 512, in bold). PA 13 therefore performs an exercise in demarcation in which the exotic nature of *psi* (rather than the practices or competence of parapsychologists) determines the exclusion of parapsychologists from the scientific community.

The second consequence of the incompatibility imperative is the construction of a revolutionary role for *psi* and parapsychology, one that would invalidate much of what science holds to be known. The extreme case formulation of "their whole theory of the universe" (line 515), invokes a maximal potential of *psi* as an undermining discovery (Hutchby and Wooffitt, 1998:211). The upshot of this extreme version of *psi*'s revolutionary power is the construction of parapsychology itself as "heretical to many scientists" (lines 516-518). Such a construction could work not only as a persuasive feature, but also as a characterisation of "many scientists" as unreasonable, even dogmatic. The latter characterization is made available when PA 13 adds that the "dismissal" of *psi* is their preferred resolution to the clash between *psi* and the "laws of nature" (line 519). As the next part of the analysis will show, however, this revolutionary role is downgraded from being an essential property of *psi* to one that is conditional upon there being sufficient evidence to warrant its reality.

In lines 521 to 527, PA 13 constructs the imperative of "unchallengeable data" (line 522) for *psi*'s revolutionary role to be fulfilled. The construction of this imperative appeals to what was examined in extract 7 as a demarcation between sufficient and insufficient evidence for a phenomenon. With the analogy of "putting the cards on the table" (line 522), PA 13 orients to the relationship between parapsychology and scientists as adversarial, during which the former seek to convince the latter. The use of this metaphor constructs the negotiation of what comes to count as a scientific "fact" (as in

poker) as being dependent upon the presentation of a straightforward quantity and quality of convincingly supporting data (as in a winning hand). In line 522 to 523, PA 13 uses the reported speech of an imaginary community of parapsychologists appealing to the reasonableness of scientists to see the potential consequences of this robust data. This way of describing the community of parapsychologists plead to be heard in face of unchallengeable data is designed to be heard “*as if* they were said at the time” (Wooffitt, 1992:161). This kind of design Wooffitt calls the use of “active voices”, and, as he puts it, is related to the construction of facticity, sustainability or objectivity of the accounts. Here, both in lines 522-523 and lines 526-527, it is argued that PA 13 used the active voices of the participants of a possible, imagined dialog in the future (respectively parapsychologists and a general class of scientists) to construct its sustainability or its potential for turning into a actual, real situation. It is interesting to note that the active voicing of the response of this general class of “scientists” (line 526) refers to an “amazing” (lines 526) amount of data which is sufficient to force a revision of their “models of the universe” (line 527). This hearably rather extreme account of the scientists response allows the revolutionary role of *psi* to be maintained as potentially believable, i.e. as something that might just happen, despite of the lack of “unchallengeable data”. Finally, in lines 529 to 530, PA 13 constructs the contrast between the ideal and present reality by orienting here (as several other participants did) to the unsatisfactory or troublesome status of evidence.

Summary.

This chapter has examined how parapsychologists constructed their field as a community, as a body of evidence and the relationship between these and science as a whole. The first section examined how participants constructed the field as one made up of sceptics and believers, and how the role of belief was both managed and built up into an empirical variable. The second section examined how participants constructed the field as a body of evidence, and how the lack of replicability was managed by essentialist formulations of the evidence as ambiguous, and of *psi* as elusive. In doing so, the implications of incompetence or pointlessness were attended to, and their research practice constructed as a particularly difficult (i.e. heroic) quest. The last section examined participants’ accounts of the role of *psi* and parapsychology, and how both were located outside the realm of normal science, with inherently revolutionary implications for science. Such implications, however, are nevertheless constructed as dependent upon “unchallengeable data”. Throughout the chapter, then, the analysis has shown how participants attended to the problem of a lack of consistent evidence by building up the inherent difficulty of doing science in a problematic, yet potentially significant

field. The next chapter will examine accounts of participants' own scientific practice within this constructed less-than-perfect-field of scientific research.

Constructing doing safe science in parapsychology.

Introduction.

The last chapter examined how participants constructed parapsychology as a field of science. This chapter, however, is concerned with how participants constructed their own research within parapsychology. The chapter therefore focuses on accounts of what they do as researchers, and is concerned with the participants' self-presentation as doing safe science within the context of a research field that they themselves constructed as problematic. There is nothing surprising or contentious about experimental researchers describing their work in a way that makes relevant and asserts its scientificity, and certainly not during an interview in which he or she is being specifically asked about his or her scientific work. This can function as a resource to build up, for instance: their credibility as scientists, or the accuracy of their empirical claims about reality. However, the way in which parapsychologists constructed their own research has to be understood in the context of their construction and assessment of parapsychology as a field of research, examined in the last chapter. Chapter 4 explored how, while participants located parapsychology within the category of empirical science, they constructed their methods, outcomes and claims as empirically problematic, i.e. less-than-perfect. This chapter will examine how these participants' constructions of their own research within parapsychology can be seen as a way of solving the self-presentation problem of asserting that their research is "safe" science, whilst they are themselves performing research within a less-than-perfect field. This chapter will thus make use of the term "safe" as a way of expressing constructions of scientific action or of a position/role within the field that guard against the implications that doing science in parapsychology may make available (e.g. lack of empirical competence, lack of a warranted research object).

The chapter will explore, in turn, four analytical themes relating to how some participants constructed their own research within parapsychology as, what will be called from now on, 'doing safe science'. Section 5.1, will consider how these participants built up their research in parapsychology as ordinary science. Section 5.2, will explore how these participants constructed their research as a particularly demanding empirical science. Section 5.3, will explore how they established their own research as

good science by contrasting it with the research of others in parapsychology (consequently constructed as less scientific). Finally, section 5.4, will consider how these participants negotiated their membership and position in relation to what was constructed as the community of parapsychologists, or the field of parapsychology. Although the discursive practices analysed here were not widespread, it is argued that the participants whose extracts were selected here displayed analytically interesting ways of construction their own practice of parapsychology. Therefore, it is argued that the analytical claims that follow are meaningful claims, that are found in other discursive contexts (e.g. the field's literature), albeit restricted in their application to the parapsychologists that feature in this chapter.

5.1 Making doing research in parapsychology ordinary.

The first analytical theme to be examined is how some participants constructed their own research in parapsychology as doing ordinary science. This process, it is argued, is similar to what Sacks (1984:414) called doing "being ordinary". Sacks' analysis focused on ordinariness as an accomplishment of description, as characterisations of people or events that actively engage in the business of constituting them as ordinary, or as Sacks puts it, in "announcing the event's ordinariness, its usualness" (Sacks, 1984:414). It is argued here that these parapsychologists engaged in similar sorts of actions when describing their research in parapsychology, as they included in their descriptions and accounted for how their methods and phenomena can be made into usual and ordinary methods and phenomena, within the context of empirical science. The issue of the ordinariness of their research was prominent in these participants' descriptions, orienting to the implication of parapsychology having strange, fringe-like, or, using Sack's term (Sacks, 1984:419), "storyable" aspects. In this section, participants' versions of their research engaged in mitigating such fringe-like characteristics, constructing their own parapsychological research as ordinary, normal or usual and, thus, normatively warranted scientific work. Collins and Pinch (1979) identified a similar process in their sociological examination of parapsychology as a field of science within a wider academic context. The authors point up a series of "tactics"²⁴ used by parapsychologists "in their efforts to gain scientific acceptance for their discipline and their findings" (p.237). Amongst these, the authors identify that parapsychologists make relevant that there is nothing strange or peculiar in parapsychological research, rather, that their research is ordinary, and, hence, legitimate, science. In this section, however, the construction of doing ordinary science will be explored through the analysis of an extract of a participant's account in which the ordinariness or the usualness of methods and

²⁴ Collins and Pinch (1978) explain their use of the word "tactics" as a way of "making sense of individual actions" (p.242).

phenomena are announced. In the description of his research work, doing ordinary science is thus built up as a characteristic that needs to be accounted for.

Extract 1 – PA 1, lines 137-215

137. PA Er as I mentioned, I did lot of (1) brain work, I mean brain electrical data work,
138. so: for me this was just a challenge to transfer my expertise in my field of
139. knowledge to this area, to look what is the correlation between .h (.) er so called
140. states of consciousness, (.) which I was not that much familiar with, and
141. functional states of brain which I was pretty familiar with. In terms of, you
142. know, **spectral analysis of EEG data or more advanced analysis and so on.** .h
[suppressed lines 143-183]

184. PA =all all this u::rm converges in what (1) I'm trying to do now. .h So back to .hh
185. our activities now in [department's location]. (2) Er so the prime (2) areas of
186. interest were (2) description or quantitative characterization of brain functional
187. states.

188. C U-hum.

189. PA In **some special** states of consciousness like (1) dream or dream like experience.

190. C Right

191. PA For example so called hypnagogic states. You know transition between
192. wakefulness and sleep. .hh On one hand, on the other hand (.) those at that time
193. **very for me very new and unknown** and existing *Ganzfeld*²⁵ phenomena. .h (.)
194. The very (.) potentiality of *Ganzfeld* to induce dreamlike or, in fact pseudo-
195. hallucinatory experience, ya? **It was completely new to me**, and you know there
196. was the .h long tradition of referring to *Ganzfeld* as inducing hypnagogic states,
197. in fact. Which we showed in our first large study was a completely wrong
198. hypothesis. .hh So this is how we started. (.)

[suppressed lines 199-201]

202. PA .hh So these were the beginnings. (1) And we still keep some part of this original
203. emphasis on on altered states of consciousness and brain functional states, that is
204. we continue some *Ganzfeld* work, (2) ur:m looking now more specifically on the
205. (.) .h brain electrical correlates of those short episodes of hallucinatory
206. experience. (1) Oh it's interesting. I think now disregarding parapsychology, .h

²⁵ As it was already described in Chapter 2, *Ganzfeld* translates as “total field” and names an experimental protocol first introduced in parapsychology by Honorton and Harpe (1974) to test the noise reduction model of *psi*. The model hypothesises that, if the ordinary external sensory input and the internal somatic noise is reduced, the possibility of conscious access to *psi* information is enhanced.

207. *Ganzfeld* could serve as very safe and very interesting model situation for
 208. something like experimental psychosis you know.
 209. C Yes.
 210. PA What people did in 60s and 70s with quite dangerous substances and so on, it. (.)
 211. *Ganzfeld* (1) delivers, so to speak yeah, hallucinations for free, in most subjects
 212. we tried you know. .h Er so, urm I hope that sooner or later our research could
 213. show more relevance even for y'know, not only for parapsychology, but .hh
 214. ur:m general to understanding much more about human nature and human
 215. psychology and so on.

In this extract, PA 1 constructs a version of his research work within parapsychology in which the ordinariness or the usualness of his methods and investigated phenomena is announced (Sacks, 1984:414). The participant carries out three main discursive actions through which doing ordinary science in parapsychology is achieved: a) by making relevant that his methodological research expertise originates from the scientifically robust, and ordinary, field of psychophysiology; b) by downplaying the particularity of parapsychological phenomena, through his construction of scientifically ordinary research objects in parapsychology; and c) by emphasising the relevance of his constructed version of ordinary parapsychology to a wider and ordinary scientific context. As we shall see, the first discursive action is accomplished through making a show of the scientific hardness of the research methods that he uses, i.e. by making relevant the physicality of his research questions and methods. The second action is achieved through: a) the translation of not-so-common phenomena of parapsychology into their scientifically ordinary parts; and b) the management of the proximity between the “fringe-ness” of parapsychological phenomena and his research, using vagueness or the claim to ignorance. The third action, almost as a consequence of the first two, is achieved through making relevant the central place that these methods and phenomena have in an ordinary scientific context. Throughout this extract, the particularity or fringe-ness of research in parapsychology is attended to as a problematic self-presentation issue. However, PA 1 achieves a version of his research work that is safe from the problematic implications of doing this kind of research, by making accessible the scientific ordinariness of his own research, even if it is within parapsychology. For analytic convenience, the actions outlined above will be focused on sequentially.

5.1.1 Constructing the robustness and ordinariness of research as an entitlement of scientific expertise.

The first discursive action to be explored is how the participant made relevant the origin of his methodological expertise (from a scientifically ordinary field of science), accomplishing the scientific robustness and hardness of his methodology and research questions as an entitlement of this origin. In making relevant that his work deals with the electroencephalographic characterization of brain functional states in “some special states of consciousness” (line 189), the link to parapsychological questions and methods is obscured. Instead, it brings to the fore the connection between his research in parapsychology and research in other “ordinary” fields of science (namely, psychophysiology). In the initial segment of this extract (lines 137-187), it is possible to see that PA 1’s account of his research is done mainly through the account of his research methodology – “brain electrical data work” (line 137). The construction of electroencephalic research as his area of expertise allows him to: a) locate the origin of his research expertise in an area of science other than parapsychology (lines 138-139); and b) position the origin of his expertise within an area of research concerned with the physical (brain) dimensions of “states of consciousness” (line 139-140), it entitles his research to scientific robustness and ordinariness. He offers an account of his activities, making a show of his skills in this area. The repetition and detailed account of the various components of his expertise seem to be there in order to be heard as an account of the scientificity of his work. In lines 141-142, the participant builds up a list of items that compose his research skills (line 142, in bold) and, in doing so, constructs a general class of research “know-how” that is specialized and to be heard as hard, safe science.

Furthermore, in lines 139-141, PA 1 sets up a contrast of “familiarity” between “so called states of consciousness” (lines 139-140) and “brain functional states” (line 141). The achievement of this contrast seems to be not only the construction of normal scientific expertise, but also the construction of a category of phenomena that (in being contrasted with known phenomena) are cued as being scientifically obscure or fringe-like (Smith, 1978:39). The expression “so called” (line 139) seems to function as a way of PA 1 footing in relation to his use of “states of consciousness” as a narrator, i.e. a user of someone else’s category, (Goffman, 1979:18), thus committing himself only to the use of these words to describe this set of phenomena, and avoiding accountability for this choice (Edwards and Potter, 1995:91).

5.1.2 Constructing ordinary research phenomena and methods in parapsychology.

The second discursive action is the construction of scientifically ordinary research objects in parapsychology. PA 1 constructs this ordinariness by: a) asserting the “mainstream-ness” of his research questions and phenomena by managing the proximity of his own research to parapsychology through the use of vagueness, disclaiming knowledge and by constructing an oppositional position in relation to them; and b) transforming parapsychological research phenomena into ordinary research objects like “special states of consciousness” (line 188) or “pseudo-hallucinatory experience” (lines 194-195). As mentioned above, PA 1 contrasted familiar functional brain states and the unfamiliar “so called states of consciousness” (lines 139-140). By stating his unfamiliarity with these phenomena, their proximity to his research seems to be oriented to as an accountable issue, cueing their construction as undesirable or obscure scientific objects. In mitigating in advance this involvement, PA 1 solves the self-presentation issue of a researcher doing ordinary science, yet dealing with such phenomena. The use of this unfamiliarity disclaimer is used two more times in this extract, both in relation to the use of *Ganzfeld* experimental protocol and the state of consciousness it induces (lines 192, 195, in bold). In both cases, these disclaimers are done in extreme case formulations, working up the factuality of this unfamiliarity in a way that “asserts the strongest case in anticipation of non-sympathetic hearings” (Pomerantz, 1996:227). These extreme formulations of ignorance and unfamiliarity about potentially scientifically controversial phenomena assert his minimum involvement with one of the central protocols in parapsychological research, even though he uses it himself (lines 203-204). This distancing was also achieved through the use of vagueness in referring to “some special” (line 189, in bold) states of consciousness that his research deals with. The use of vagueness is conspicuous in this extract when compared to the detail given in relation to the types of analysis that he performs on the electroencephalographic data (line 142). In using vagueness here, PA 1 seems to be downplaying the less-than-ordinary states of consciousness that his research is nevertheless constructed as focusing on. In addition to the use of vagueness, PA 1 also managed the proximity to parapsychological research phenomena by building up a track record of opposition to what was constructed as parapsychology’s knowledge about the *Ganzfeld* phenomena (line 196-198). Later, in section 5.3 of this chapter, we shall see how he built up a contrast between his use of the *Ganzfeld* and that of “traditional” parapsychology.

The participant also engaged in the transformation of parapsychological research objects into ordinary research objects, thus downplaying their particularity. In lines 194-195, PA 1 describes *Ganzfeld* as a “dreamlike” state, or, as “pseudo-hallucinatory” experience. In doing so, he not only provides two scientifically ordinary translations of the *Ganzfeld* state, but he constructs the factuality of the latter explanation. In using the expression “in fact” (line 194) followed by the report of the empirical support for this translation, he offers it not only as a normal way of talking about *Ganzfeld*, but as a

scientifically robust explanation for what the *Ganzfeld* state really is. This ordinary way of talking about the *Ganzfeld* is thus constructed as not simply a personal terminological preference, but as an empirical fact. In the second case (lines 204-206), PA 1 links this translation of *Ganzfeld* phenomena to the initial description of his research methodology, i.e. the electroencephalic characterization of brain functional states. The discursive result is the construction of his *Ganzfeld* as scientifically ordinary, and his own research and phenomena as “mainstream”.

5.1.3 Constructing the wider scientific relevance of the ordinary parapsychological phenomena and methods.

The third discursive action to be explored in relation to Extract 1 is concerned with how the participant makes relevant that parapsychological questions have significance for a wider scientific context. This seems to be an upshot of the ordinariness of *Ganzfeld*, the construction of which is interrupted (line 206) in order to introduce the potential wider role of this line of research “disregarding parapsychology” (line 206), with the performative insertion “Oh it’s interesting” (line 206). In exploration of this next segment, and particularly in the consideration of the shift in context that PA 1 suggests above, it seems important to consider the central role that *Ganzfeld* has within experimental parapsychology (explored in Chapter 2). This is put forward as almost the signature method of parapsychology, which is commonly described as the most convincing representative of the “case for *psi*” (Milton, 1999:321). Taking this background, it appears that this suggestion to exclude parapsychology from the context of discussion, could function as a performative piece in PA 1’s construction of his scientifically ordinary version of *Ganzfeld*. Looking at the way PA 1 constructs the potential new role for *Ganzfeld* research, the terms “a very safe (...) model situation for experimental psychosis” (lines 207-208) or “*Ganzfeld* delivers (...) hallucinations for free” (line 211), construct a version of *Ganzfeld* that is far removed from its original concept, and would provide answers to ordinary research questions such as: what is an hallucination like?; or what is psychosis like? With this performative construction of the ordinary scientific use of the *Ganzfeld*, PA 1 seems to make available his investment (Edwards, 2000:363) in making it scientifically ordinary, thus, safe.

The exploration of this extract focused on the different ways in which the participant constructed a version of his research work within parapsychology in which the ordinariness or the usualness of his methods and investigated phenomena are displayed, thus, constructing a version of his research orienting to displaying its ordinary character. This was accomplished through a) building up the robustness and ordinariness of his research as an entitlement of the origin of his expertise in another field of science, b) translating research phenomena and methods in parapsychology into ordinary ones, and c) constructing the wider scientific relevance of the parapsychological phenomena and methods,

fitting them in a place within usual science. The next section will be concerned with what at first glance would seem to be the opposite, i.e. with how participants constructed the particularity of their research in parapsychology. However, the construction of particularity that will be explored is related to the establishment of the special rigour and quality of parapsychological research, and thus a desirable kind of particularity.

5.2 Making doing research in parapsychology particularly rigorous.

This section is concerned with how participants constructed their own research in parapsychology as being particularly rigorous. In making use of this process, participants seem to be orienting to the problem exemplified by Truzzi's (1978:11; 1981:298) phrase "extraordinary claims require extraordinary proof". As Chapter 2 explained, this translates the Humean idea that any claim that seems to its audience to go beyond what is common or ordinary, in order to be accepted, demands evidence that goes beyond the common requirement of quantity or in quality for that audience. In this sense, the scientific legitimization of controversial claims would demand extra-robust evidence, extra-rigorous research. In the parapsychological literature, one frequently finds the assertion that the methodological rigour and competence of experimental work not only fulfils but exceeds the requirements placed upon most other work in directly comparable fields of research (Truzzi, 1978:11; Collins and Pinch, 1979:243; McClenon, 1984:10; Zingrone, 2002:23). This claim is often constructed as a result of the critical scrutiny that parapsychological claims and methods are subjected to by other researchers, related to the difficult or controversial state of parapsychological research questions and claims. This section will explore how a participant constructed his concern with doing particularly rigorous research, and his self presentation as doing safe science in parapsychology, by building up a description of parapsychology where doing particularly good quality research is constructed as the imperative for a researcher's success.

Extract 2 – PA 8, lines 184-215

184. C What would you consider a successful experimental outcome?

185. PA (1) Let me see. A succ-

[suppressed lines while PA and C adjust the microphone]

186. PA Successful experimental outcome. I guess someone (.) who: achieves what they set

187. up, **someone whose hypotheses are supported**, I guess initially, that might not

188. necessarily be in terms of performance better than chance. It might be the opposite,

189. if y-, you know, if y-, or or null results even, [if you don't think there's anything to it.
190. C [Hmhm.
191. PA Ah, but also in terms of the idea of replication. In terms of **the person being able to**
192. consistently, a.h, repeat their findings, and also, I think **to have a reputation** for
193. well designed and executed studies. [I think that's important.
194. C [Hmhm
195. C [Yeah.
196. PA [Because I feel, you know, it's pt-, y' know, the the standards seem to be a lot higher
197. and **people** are a lot more critical of parapsychology than other areas of psychology,
198. say. And so, it's, you know, it (.) if your research isn't particularly well designed and
199. executed, you just open yourself up to more criticism than [normal, and it's just, =
200. C [Yeah.
201. PA = perpetuates the idea that there's nothing to it, and parapsychologists do sloppy=
202. C Yeah
203. PA =research. I think, it's not, that there're two elements (really), one is the the kind of
204. outcome and the other think is the quality [of the research, you know, =
205. C [of the () yeah.
206. PA = within the field it doesn't matter so much whether you get null results. You know,
207. we can publish anything, but, you know, the important thing it's good quality
208. research. So you can have a good quality piece of research but null results, =
209. C Hmhm
210. PA = it's still accepted. Whereas in other fields, it maybe that, if you get the results,
211. that's the key thing, and y' know, the research mi- n- might not be as good.
212. C Hmhm
213. PA But I think that, you know, the quality of research is very important, =
214. C Right.
215. PA = in this field.

In this extract, PA 8 presents an account of doing successful research in parapsychology, in which he constructs his concern with doing particularly rigorous research. His self presentation as doing good science in parapsychology is built up by the description of the field in which doing good quality methodology is an imperative particular to parapsychology. The particularity of this emphasis on methodology in parapsychology is constructed in two ways: first, as a consequence of the particularly critical scrutiny that parapsychology is subjected to, thus it has to be high quality research in order to survive such scrutiny; second, in comparison with other fields of research (in which results are emphasised rather than methods), and thus parapsychology as a field in which experimental results are less of a concern than methodological rigour (as it was already explored in Chapter 4, Extract 3). In

this way, the importance of claims is upstaged by a particular emphasis on methodological quality, itself being put forward by the participant as a result of the way things are in parapsychology.

In PA 8's answer to the initial question (line 184) about what he would consider a successful outcome to be, he sets out a list of three criteria: a) the experimenter meeting his/her hypotheses, of either a positive or null *psi* effect (lines 186-189); b) the experimenter being able to produce replicable outcomes (lines 191-192); and c) the experimenter having "a reputation for well designed and executed studies" (lines 192-193). All of these criteria for success are talked about here concern the actions or characteristics of the experimenter, rather than characteristic of the outcome itself (lines 187, 191, 192, in bold). Thus, PA 8's next turn (Wooffitt, 1992:48) displays his treatment of the initial question as a question about what makes a researcher in parapsychology a successful one. The last criterion that PA 8 puts forward orients to the social context of, or audience for, his work, as he makes relevant that having a "reputation" (line 192, in bold) as a rigorous researcher is an important aspect of this constructed success. The rest of this extract is concerned with the warranting of this initial assertion about the imperative of good methodology in parapsychological research, i.e. as the consequence of critical scrutiny of parapsychological claims. The analysis will look at these two actions sequentially.

5.2.1 Constructing high quality research as a consequence of the critical scrutiny of parapsychology.

In lines 186-203, PA 8 constructs parapsychology as a field of research where the demand for rigour is particularly high. The particularity of this demand is constructed as the result of a general ("people", line 197, in bold) critical view of the field, compared to the scrutiny of "other areas of psychology" (line 197). In doing so, he seems to be making relevant the existence of an audience or community that is actively critical towards parapsychology (lines 199-204). It is noticeable that this is located within the realm of his "feelings" (line 196), displaying that this is a personal opinion, rather than an accountable claim to the way things really are. He makes relevant, when referring to the critical audience's construction of parapsychological claims and methods as hollow and defective, that these "ideas" (line 202) exist out there and are commonly known. At this point, the imperative of doing particularly good research in parapsychology is achieved, as PA 8 sets out that failure to do so would "perpetuate the idea that there's nothing to it" (line 201). Thus, the participant builds up the need to be extra-rigorous and competent as a way of forestalling the negative attributions of a critical audience. This extreme rigour is thus accounted for as coming with the territory, i.e. as a direct upshot, of doing parapsychological research.

5.2.2 Contrasting the emphasis on methodology with the emphasis on results.

The participant also contrasted parapsychology's emphasis on methodology with the emphasis on results in "other fields" of research. In building up this contrast the participant makes available the implication that the quality of research in parapsychology is higher than in these unspecified "other fields". In the last segment of this extract (lines 206-215), PA 8 builds up a description of parapsychology as a field where the importance of the quality of research overrides the importance of obtaining results that support the *psi* hypothesis, through the example of publication selection criteria (lines 207-210). The construction of methodological quality, not merely as an alternative to results, but as the preferred selection criterion for publication, accomplishes: a) the mitigation of the ongoing problem of a lack of consistent results in parapsychology; and b) the warranting of these null results as an expression of high methodological quality. Having constructed the importance of "quality of research" in parapsychology, the opposition between this and "other fields" (lines 210-211) implies that consistent results might actually be the product of less rigorous methodology. PA 8's construction of this direct relationship between lesser methodology and the production of positive results, offers the upshot that the lack of positive results can be construed as a guarantee of good methodology.

In summary, this section examined how PA 8 constructed his concern with doing particularly rigorous research, by building up a description of parapsychology as a field in which doing good quality research is the imperative for a researcher's success. He did this by constructing not only the reality of critical demands for methodological quality, but also the reality of his own research as responding to the challenges created by these demands. In doing so, he seemed to orient to the constructed difficulties and controversies of parapsychological methodology and claims as guarantees of the scientificity of his research, in the sense that they keep parapsychologists "on their toes". The implication is that in order to work in parapsychology, a researcher has to be particularly rigorous. He also did this by contrasting parapsychology's emphasis on methodological rigour and "other fields" emphasis on obtaining results. In doing so, he not only manages the problematic implications of the lack of consistent results in parapsychology (e.g. experimental incompetence), but also implies that a lack of positive results actually reflects rigorous methodology. In this sense, the upshot of this construction of parapsychology's particular emphasis on methodological rigor, is that the lack of positive results is a form of desirable assurance that such standards are kept up. The next section will focus on how participants used contrasts as ways to discursively accomplish the characterization of their own research as safe science.

5.3 Contrasting good research and not-so-good research in parapsychology.

The third section examines the participants' use of contrasts between constructed categories of research in parapsychology. These contrasts will be explored here as a discursive resource which allows us, as Dickerson (2000:386) points up, to construct for ourselves a favourable position. It is argued that these participants use contrasts in a similar way, i.e. as a way of carving out for themselves a favourable stance in relation to the community of parapsychologists and its practices of research. The analysis will consider how participants established their own research as good science by contrasting it with the research of others in parapsychology, which they presented as less scientific. In making use of these contrasts, participants attended to the constructions of parapsychology as a less than perfect field of science explored in Chapter 4. They use these constructions as a normative description of a research background against which to contrast their own research. Thus, the use of contrasts here allows these participants to un-problematise their research practices. McKinaly and Dunnet (1998), in their analysis of how individuals take care of the sensitive interpersonal business of constructing their identities as gun-owners, observed how speakers oriented to the negative implications of being implicated in this category of "gun owner". The authors noticed how the speakers made use of "contrastive pairs" (McKinaly and Dunnet, 1998:40) between constructed factions of this category in order to achieve an identity of a gun owner that, in contrast to criminals and vigilantes, is acceptable, average and unexceptional. Similarly, here, these participants make use of contrasts between their research practices and those of others. This allows them to create a separation of two factions of parapsychology researchers, and locate the empirically dubious activities in this "other" faction. In doing so, it is argued, they manage (by allocating them to the "other faction") the implications of belonging to a category of people who do work in parapsychology (e.g. lack of agreed scientific quality).

The section will first explore, in extract 1, how the participant constructs "traditional" parapsychology as a less than perfect category of empirical research, and presents his own research in contrast with this category, identifying it instead with the scientifically "safe" physics. It will then explore, in extract 2, how the participant makes relevant the contrast between his scientific competence and the competence of other researchers in experimental parapsychology, and uses this competence as a guarantee of doing good scientific research. Finally, in extract 3, it will examine how the participant manages the proximity between his and others' research in parapsychology (even if using the same experimental protocol), by making relevant the contrasts, rather than the similarities, between them.

Extract 3 – PA 1, lines 212-246:

212. PA Ok er, (.) the other line is of course (.) what people call
213. (.) **ESP extrasensory perception**, or mo- in more modern wordings er (.)
214. **anomalous cognition**. .h Which is also of (.) very intense interest (.) for me, for
215. us. .hh Hu:m (1) in this area we did also some not that spectacular what, (.) I
216. would say, some, .h some not so uninteresting studies. E:::r first transforming
217. some experimental paradigms .h to something (.) making more sense to us. .h Er:
218. for example (1) instead of card guessing which was the traditional .h traditional
219. parapsychology ur:m task. Er original task of experimental parapsychology for
220. years. .h Er we transmuted this into (1) search for a certain symbol from a an
221. array of options, which is no way new. In fact if you look at at proceedings of er
222. the first, (.) I don't know, the first or second congress of psychical research held
223. in Copenhagen in late 20's, I think, there were experiment reported
224. C U-hum.
225. PA Using more or less the same paradigm.
226. C Right.
227. PA Only this was not er exactly ESP in the sense clairvoyance, it was telepathy, .h
228. what was e:::r what s- should have been going on. (1) Er: but, you see, w-we are
229. trying to to stay with more or less well defined experimental situations.(.) What
230. people call forced choice and .h considered to be obsolete again, you know,
231. outdated by by free response like *Ganzfeld*. .h (2) But we prefer to stay with
232. ur:m forced choice for some very good reasons. **Firstly** it enables us to construct
233. much more specific (1) probabilistic models of what's going on.
234. C U-hum
235. PA I know y-you were not attending my talk today
236. [no no. So I I I was insisting on that this should be an =
237. C [I was (outside ())
238. PA = integral part of our research work in, be it parapsychology, or be it in
239. mainstream psychology and so on. You know not to collect data and and ur:m
240. look for (proper) statistics or model post-hoc. We should act proactively. And
241. construct models. And construct experimental designs fitting our models. That is,
242. we should know what we are doing, you know, in advance, .hh which is the
243. standard in (1) sciences like physics for about 300 years, which is still not the
244. standard in psychology or=
245. C =na=
246. PA =.h not speaking parapsychology.

This extract includes a segment of PA 1's description of his research work within parapsychology. The description can be seen as the construction of his research as good scientific research, in contrast with a less scientific "traditional" parapsychology. This is achieved via four discursive actions by: a) establishing a contrast and distance between his research and "traditional" research in parapsychology (including labels and methods); b) constructing a category of good scientific research; c) including his research within the category of good scientific research; and d) excluding "traditional" research in parapsychology from this category of good scientific research.

5.3.1 Building up a contrast between personal research and "traditional" parapsychology.

The analysis will start by focusing on how the participant constructed a contrast and distance between his research within parapsychology and "traditional" parapsychology. The contrast makes available the idea that the proximity between them is, in some way, problematic. In the beginning of his description (lines 212-214), he identifies the type of phenomena that his research has focused on, offering various labels for the same idea: ESP, extrasensory perception and anomalous cognition (in bold). In offering different ways of referring to the same phenomena, the participant makes a show of how uneasy he is in using them, or how problematic they are. By prefacing these labels with "what people call" (line 212), he is able to distance himself from them, or from the process of attribution of these labels to events. In this sense, his footing (Goffman, 1979) in relation to these labels is that of a user, rather than an author, and he is able to avoid accountability for them, and the implications they carry.

He goes on to state that ESP or anomalous cognition are areas of "very intense interest for me, for us" (213-214). This strong statement of interest in these topics of research seems to try to secure a particular position, i.e. he seems to be making relevant that, although critical about such labels, he nevertheless invests strongly in the phenomena to which they refer. He is therefore able to construct his entitlement to knowing and, more importantly, to knowingly criticize the way these phenomena are labelled. As he goes on to describe his work in this area (lines 215-216), the first noticeable aspect of the description is that he is putting forward an evaluation of his work. For this purpose, he uses two formulations, "not that spectacular" (line 215) and "not so uninteresting" (line 216). This evaluation appears to be in itself a process of negotiation of both the presentation of his work and his self presentation as a researcher. As the first assessment term is put forward, the second seems to come as a repair or as the management of the former. The first formulation of his work as "not that spectacular", on its own, is a relatively weak evaluation of the results of his research, while the second formulation ("what I would say, some .h some not so uninteresting studies) seems to be a repair of

this. By attending to the relational problem of the first formulation undermining both his research and his competence, he seems to accomplish a description of his work that establishes it as “good enough” scientific work. This first introduction and evaluation of his work sets up a self-presentation of an experimenter with a strong interest in, and knowledge of, parapsychology, yet one who is critical of, and distant from, traditional labels in the field. This sort of positioning in relation to parapsychology, what might be called ‘participation in, without being part of’, will be discussed more fully in section 5.4.

In the following segment of this extract (lines 216-223), the participant goes into greater detail in his description of what his research work is about. This is his first contrasting instance between his and “traditional” parapsychology’s experimental methodologies, as he makes relevant that his research involved his own transformation of a “traditional parapsychological task” (line 218-219). By using the example of a central methodology in parapsychology, he points up the significance of the difference between his research and that of others. By making it into an accountable issue, however, it needs to be warranted with the use of an “historical” argument. He locates his methodological choice within a set of past parapsychological experiments, thus warranting his choice, and emphasizing the normality and scientificity of the procedure. As Gilbert and Mulkey (1984:137) point out, making use of past corroboration in a scientific context helps the construction of a forceful and coherent characterization of scientific research work.

5.3.2 Constructing a category of good scientific research.

From this moment onwards, the warranting of this particular methodological option is made central to PA 1’s description of his research work. Not only are the contrasts intensified, but also a category of good scientific research is constructed in contrast with traditional parapsychology. In building up this category, he locates his research within it, and later offers the implication of excluding “traditional parapsychology” from it. As the participant builds up the warranting of his choice of research methodology (lines 228-233), he accomplishes two related actions: first, a contrast between the rigour of his methodological choice and others’ lack of rigor (which allows him both to make the exclusion argument, and to manage his accountability for a controversial argument within the field); second, he sets up a “straw man” criticism of his choice in order to reinforce the scientific adequacy of his own methodology, and thus warrant his choice. This process starts with a straight claim to rigorous experimentation by virtue of his methodological option, and introduces the “straw man” criticism, “voiced” by a vague group (“people”, line 230). The agency of the criticism is built up in a somewhat obscuring way, in which the verb “considered” (line 230) carries the implication of someone doing the “considering”. The ambiguity and obscuring of the agent is sufficient to allow him to avoid endorsing

a particular story about responsibility (Potter, 1996:182), but is clear enough to build up an opposition to his work within parapsychology. Another aspect made available in this anticipated “straw man” criticism is that scientific research evolves, that methods and ideas have a shelf life, and that new methodologies are generally regarded as superior to older ones. In this sense, his methodological choice is out of kilter with progress in science, and perhaps needs to be warranted further, as an advantage rather than simply breaking the rule.

This warranting process continues, in what seems at first to be a list of reasons (line 231-233). As it is introduced (line 232, in bold), it is framed as a list of “very good reasons” for his choice, thus constructing the factuality and regularity of the advantages. The first is put forward (lines 232-233) as an empirical reason, which is particularly significant in a field where probabilistic arguments are considered to be central yet problematic (Collins and Pinch, 1979:247). In starting with “Firstly” (line 232), PA 1 creates the expectation of at least a second item. However, what follows is a clear shift, an interruption, as he switches focus and pace. The warranting process is no longer just about his particular methodological option, it is about what all scientific research should be like. In doing so, he builds up a category of good science, and makes accessible the inclusion of his research work within it, and the exclusion of “traditional parapsychology” from it.

5.3.3 Including personal research in – but excluding parapsychology from – good scientific research.

PA 1 interrupts his listing to make a reference to his own reported speech at a talk given earlier in the day to the plenary of the Parapsychological Association Annual Meeting (lines 235-237)²⁶. The introduction of this reported talk works as a way of signalling both his status within the field (that of an active researcher who is listened to by his peers) and the importance of the claim he is about to make enough to be “insisted” (line 236, in bold) upon. The claim that follows is put forward in a strong statement in which he says that “the construction of more specific probabilistic models of what’s going on” (line 233) is a requisite of scientific research not only in parapsychology, but also in “mainstream psychology and so on” (line 239). Thus, he is no longer warranting the advantages of his own research methodology, but stating that this is what all good science should do. By using a list of fields to which this demand should be central, he builds up the universal importance of specific probabilistic models (Jefferson, 1990, cited in Potter, 1996:195; McKinlay and Dunnett, 1998:38).

²⁶ As explained in Chapter 3, this interview took place during the 2002 Parapsychology Association Annual Meeting.

Another contrast is then introduced (lines 239-240) as what not to do in science. The last part of this extract (lines 214-246) seems to be the central claim that PA 1 works towards. As noted above, the direct warranting of his research in parapsychology was switched for an implied warranting of his research as safe or proper. In talking about what good science should be like, the implication of his own research as good science is thus made available, even though it is in parapsychology. His other contrast (lines 239-240) spells out what scientific research should and should not be. In both halves of this contrast there are lists of characteristics, making them factual and regular. However, in the second half (lines 240-242), there is a noticeable quickening of pace in his description of what scientific research should include, reminiscent of a “manifesto”, i.e. small sentences, closing each point separately, spelling out and summing up the common feature of this list of research requirements, “that is, we should know what we are doing, you know, in advance” (line 242). At the beginning of this warranting process was a justification of his particular methodological choice. He moved on from warranting his own research to making a strong claim about what scientific research should be, regardless of the discipline, building this almost universal category of good scientific research. The inclusion of his experimental option within this category is left unsaid, but the implication is clear.

The very final segment of this extract (lines 242-246) seems to function as a clinching argument for contrast between good scientific research and research in parapsychology. The participant again uses a list, this time in order to draw a continuum of good-not so good science, upon which physics, psychology and parapsychology may be located. He makes accessible that there are two clear poles in this continuum, and pushes them apart – physics is an old proper scientific discipline, parapsychology is so far from it that it is almost an obvious case. The addition of parapsychology at the end – “not speaking parapsychology” (line 246), which works as “not to mention parapsychology” – constructs this as an obvious example of this lesser scientific quality. This addition is an unnecessary, yet available, final contribution to the claim that is being developed, that parapsychology is outwith the category of good scientific research. It is possible to identify three discursive achievements in drawing up this continuum between fields. First, it warrants the claim that parapsychology is distant from the ideal of scientific research. In presenting a rank order of decreasing scientific quality, it demonstrates the extent of the difference. Second, the comparison makes available a referential category (Potter, 196:187) as he builds up fields of scientific research that may vary in quality, but are scientific nevertheless. Parapsychology may be distant from physics, but one can still do science within it. Third, he can compare his own research with physics, the ideal standard of scientificity, bypassing psychology in the process. Having described what research should be like, and made available the link between that “ideal” and his own research, he now links this ideal with physics. Hence, the unsaid yet available inclusion of his own research within the field of physics.

Going back to the beginning of this analysis segment, I argued that the main claim was that the participant moved towards a description of his work as a researcher that does good scientific research

within parapsychology, through: a) drawing up a contrast between his research work and traditional parapsychological research; b) constructing a category of good science; making accessible the inclusion of his research work in parapsychology within it, and the exclusion of others' traditional parapsychology from this category. These actions allowed him to describe his research as good science, even if he was doing research in parapsychology.

Such contrasts between good and not-so-good research in parapsychology, can also be seen in extract 4, when PA 6 contrasts his own knowledge and methodological competence with others in the field. In doing so, he works up both his own scientific credentials and makes them available as a virtual guarantee of the scientific quality of his research. In this sense, doing good research in parapsychology is constructed as an entitlement of being more knowledgeable about methodological and statistical issues than others, who do less than good science.

Extract 4 – PA 6, lines 627-648

627. C What is it that's going on?
628. PA I don't know. **These people** they ignore the problems we have. We just heard a
629. talk of someone carrying out experiment with 40 subjects. And he was saying
630. there' re so many people have carried out experiments with small sample sizes.
631. This is taking into account ehm power analysis, which is, you know, you cannot
632. publish in a medical journal if you can-, if you do not carry out a power analysis.
633. C Hmhm, hmhm.
634. PA I mean you can question the whole thing, but then it must be in the paper.
635. C Yeah, yeah, [yeah.
636. PA [Yeah you can question that, of course, but until, you know, you do
637. not question that, you better do a power calculation.
638. C Yeah.
639. PA And people don't do that.
640. C Hmhm.
641. PA They simply, (.) it's doesn't they do not tell where this sample size comes from.
642. C Hmhm.
643. PA According to good medical practice, which is the standard in medical research, it
644. must be done.
645. C Hmhm, hmhm.
646. PA And people who do do not do that, they're simply ignoring the the state of the art
647. experimentation.
648. C Right.

The significant analytic features of this extract include: first, the construction of a category of researchers in parapsychology doing less than good science – “these people” (line 628, in bold); and second, the construction of competence and knowledge above that of others in parapsychology. These will be looked at sequentially, with a focus on how the construction of contrasts about scientific competence and knowledge allow him to make a claim to doing good science in parapsychology.

5.3.4 Constructing a category of researchers doing less-than-good science in parapsychology.

At the start of this extract (line 628), the participant constructs a category of people in parapsychology who ignore the “problems” that parapsychology has as a field of science. An interesting aspect of this opening claim is that he positions himself within the field (“we have” in line 628). This positioning will change slightly later on. The way in which the category of “these people” (line 628) is built draws a clear contrast between them and him. The implication of this category is that, while “these people” ignore the problems that parapsychology has, he himself does not. Thus, he shows himself to be critically aware of them, building up the entitlement to knowledge, competence and a position to criticize the work of other researchers. This initial category making is then followed with a concrete example as a demonstration of this claim (lines 628-630). His use of an example from the Parapsychological Association Annual Meeting (during which this interview also took place), builds up the factuality of this category by making relevant that this “ignoring of problems” goes on even in the most significant event in the experimental parapsychology calendar. In addition to the construction of the factuality of the claim, the use of this readily available example from a conference presentation seems to accomplish a further discursive purpose, that of constructing the prominence of this category in the field. By making available that those researchers who do less than good science are present at this meeting, he seems to be building up the widespread character of the lack of awareness of problems in parapsychology.

5.3.5 Constructing better scientific expertise.

The second significant feature of this extract is the construction of PA 6’s scientific competence and knowledge above that of “those” others in parapsychology (lines 631-680). This contrast is achieved through a specific example – the use of statistical power²⁷ analysis to warrant the choice of sample

²⁷ Power, as a statistical concept, refers to “the probability of correctly finding a hypothesized relationship or effect when it

size in parapsychological experimentation. The contrast is thus between the competence of those who do not use power analysis and those who do (lines 631-632). By establishing his critical and distant position in relation to those who do not (lines 639-640), he offers the implication that, unlike them, he is aware of the importance of power analysis, and therefore doing better science than them (line 646). In lines 636-637, he brings a sort of reasonableness to the demand for power analysis as a marker of good research, by making available that it is still possible to do good science without it, thus forestalling a possible dispute that it is a determinant of good science. The warning that follows (“you better (...)”, line 637) towards those who neither use power analysis nor warrant the omission, seems to be left unresolved. The resolution of this warning follows later, however, when the consequence is drawn, i.e. by ignoring these procedures, those researchers are ignoring the “state of the art in experimentation” (lines 646-647). This resolution again provides a marked contrast between them and him and the implication that he is aware of the “state of the art in experimentation”. Thus, PA 6 achieves, as PA 1 did in the previous extract, the inclusion of his research in parapsychology in the category of good science, using rhetorically effective contrast with not-so-good science found elsewhere in parapsychology.

In this extract, there is another type of contrast, between the scientific rigor demanded in medical science²⁸ and that practiced in parapsychology (lines 631-632 and 643-647). In using this contrast, the participant builds up two claims: a) that power analysis is a fundamental feature of publication within medical science; b) that medical science is an “ideal” of scientific practice. In a similar way to PA 1’s construction of physics in the previous extract, this participant makes use of the reference to medical science to construct it not only as a standard of scientificity (lines 643-644), but also as a category to which he might claim entitlement. His membership to parapsychology, that was constructed in line 628, now changes slightly, being achieved in two ways: first, by displaying his knowledge about power analysis as a fundamental feature of medical research; second, by contrasting medical practice as a scientific “ideal” with those who do not use power analysis (lines 643-647). By drawing together these two scientificity markers, the participant seems to be working on his membership to the category of people who do medical science, and, thus good and safe science.

Analysis of extracts 3 and 4 explored how participants PA 1 and PA 6 built up their research in

exists” (Hair, Anderson, Tatham and Black, 1998:3). It is a function of, both, the size of the sample and the size of the effect under study. The sample size should, thus, be determined according to the size of the effect and the necessary power in order to statistically find it. So, in the case of a very small effect, the choice of a large sample size would mean that the power of the statistical analysis would be increased, increasing the probability of correctly finding the effect. In parapsychological research, in most experimental paradigms, *psi* is statistically a very small effect, therefore, the smaller the sample, the smaller the probability of accurately finding a *psi* outcome.

²⁸ This researcher’s work within parapsychology is related to issues of health and well-being.

parapsychology as good research, thus making it safe in relation to the constructions of parapsychology as a less than perfect field of research. This was achieved through contrasting their research with that of others in parapsychology. What seems to be interesting about this last extract is that PA 6 does not talk about his research directly. Rather, he offers the implication of his research as good and competent research through the construction of his awareness and knowledge about methodological and statistical issues in parapsychology. In the next extract (5), a more literal use of contrasts will be considered. As he resumes the description of his research work, PA 1 accounts for his own use of a methodology that is central to parapsychological research, the *Ganzfeld*.

Extract 5 – PA 1, lines 253-289.

253. PA This is one line. The other line is of course, **sooner or later we would like to**
 254. **integrate our *Ganzfeld* work**, which has been done until now without any
 255. reference to *psi*, you know. There is no *psi*, no telepathy, .h nothing in our
 256. *Ganzfeld* studies we=
 257. C =just the [state] itself =
 258. PA [we]
 259. PA =Yes yes. We were studying **just** the just the:: ur::m (.). **Simply** the question was
 260. what does *Ganzfeld* do to the brain, [to the mind] Ok?
 261. C [Ok Ok].
 262. PA What does it produce?
 263. C Yeah.
 264. PA What are the brain electrical correlates? But this is not a (self) purpose. Of
 265. course this is int- very interesting for itself, but **we would like we hope to to be**
 266. **able to integrate it sooner or later** .h with the: well known parapsychology's
 267. *Ganzfeld*.
 268. C U-hum.
 269. PA The whole setting you know, sender receiver and so on. .h I'm a little bit nervous
 270. using those words as [you know. You know it's]=
 271. C [((sniggers)) Yes yes]
 272. PA = it's it's it's .h er it's in fact a (really) obsolete model of you know sending
 273. something from A to B, ya? But but anyway we had to start we with something.
 274. .h **Er so we hope to be able to integrate our (.) methodology, (.) our**
 275. approaches to *Ganzfeld*, you know EEG e:::r people trained in in giving their
 276. reports as as briefly but concisely as as possible.
 277. C Right [right
 278. PA [Ya? e:r =

279. C =You mean the mentations?[The actual yeah yeah.
 280. PA [Yes the mentation yeah. You know, they cannot,
 281. ur:::m they shouldn't verbalize continuously, which is unfortunately the standard
 282. in in parapsychology work. Er: you cannot have any any usable EEG records
 283. with continuously verbalizations, so they have to be (re)-trained to a somewhat
 284. different protocol, Ok? (.) .h **But sooner or later you want to to combine this**
 285. **with (.) the *psi Ganzfeld*.** .hh Er to this end we joined for a while, for 2 years, er
 286. our efforts with people in [location], [name of parapsychologist] and his
 287. collaborators. (.) So now we have the urrm digital *Ganzfeld* system, (.)
 288. developed by them. This still doesn't mean that we we'll use it exactly in the
 289. same way or exactly in the same shape, .h but again we have (a form to do it).

In this extract, the participant accounts for his use of an experimental protocol that is central to parapsychology, the *Ganzfeld* protocol (examined in Chapter 2). In talking about the use of the same experimental protocol, he manages his proximity to what was constructed as traditional parapsychology, through contrasts between his and others' research work. I will explore how traditional parapsychology's use of the *Ganzfeld* is undermined and how PA 1's own use of the methodology is constructed as a scientifically valid use of the same protocol.

The last chapter examined how parapsychology was described as a problematic or less-than-perfect field of research. In extract 3 PA 1 constructs his research work in parapsychology as good science, in contrast with others' work in parapsychology. The analysis of this extract is related to the instances of contrast that were examined before. Specifically, it will examine how PA 1 manages the common use of the *Ganzfeld* protocol by him and the not-so-good traditional parapsychologists. It will be possible to focus on four instances in which the same move towards proximity to (or integration with) others' use of the *Ganzfeld* protocol is followed by a distancing action, qualifying and differentiating his use of the *Ganzfeld* from that of others in parapsychology. Thus, PA 1 manages the methodological commonality with others in parapsychology by mitigating the similarities and emphasising the differences between the two. Looking at these four instances, I will examine how the contrasts are made in four different ways: first, by distinguishing in terms of the purpose of his and others' use of *Ganzfeld*; second, by distancing himself from the conceptualization of the anomalous process that the *Ganzfeld* procedure was designed to test; third, by making relevant the incompatibility of parapsychology's *Ganzfeld* method with the use of an electro-encephalographic measure; fourth, by a clear and isolated claim to his prerogative of using this protocol differently.

5.3.6 Contrasting the purpose of *Ganzfeld*.

The first of these management instances (lines 253-264) starts with a discursive move that achieves an intention of integration of his with others' *Ganzfeld* studies (253-254, in bold). It is, however, preceded by the qualification "sooner or later" (line 253 in bold), constructing either a potential or a hope of integration, but not one that has actually happened or is going to happen at any specific time in the future. Throughout the extract the participant repeats both the use of "sooner or later" (lines 266 and 284, in bold), and the same claim to integration (lines 253-254, 265-266, 274, 284-285, in bold) building up further a reserved intention to integrate. The discursive move that follows is in the opposite direction (lines 254-256), when he puts forward a contrast between his and others' use of the *Ganzfeld* protocol. He makes relevant a fundamental difference, as his work has been done "until now without any reference to *psi*" (lines 254-255). The context of this claim is worth considering. As was examined in Chapter 2 (section 3.3.3), the *Ganzfeld* procedure is a core parapsychological experimental protocol, devised as a method to test a model of *psi*-functioning called the noise reduction model (Bem, 1993:102), increasing the potential for detection and recognition of the *psi*-mediated signal (Honorton, 1977:466; Bem and Honorton, 1994:15). It was therefore designed to create a "*psi* conducive" state. PA 1 is therefore establishing a fundamental difference in purpose between his research and that of parapsychology, i.e. the investigation of physical and psychological processes, not the investigation of *psi*. The content of the description in this segment could easily be included in the area of psychophysiology. In this contrast, then, PA1 seems to be "announcing the ordinariness or usualness" (Sacks, 1984:414) of the purpose of his research (as discussed in section 5.1 above).

In the structure of this contrast, it is possible to see how the reference to *psi* or telepathy (line 255) is only a passing one, used to establish a marker of parapsychological research, and making available the distance between this and his own research. However, the reference to his own research questions is vivid and systematic, constructed as normal and empirically verifiable. The words "just" and "simply" (line 259, in bold) seem to work here as a normalizing clause, characterizing his research questions as ordinary scientific research questions. Thus, he inoculates against possible contamination or undermining of the safety of his science by its proximity to the search for *psi* (Potter, 1996:125).

5.3.7 Disputing parapsychology's model of the *Ganzfeld*.

The second instance of the participant's management of his proximity to the use of parapsychological methods follows the same structure as the previous one (lines 264-273). The same discursive move towards the integration of his, and parapsychology's, use of the *Ganzfeld* protocol (lines 265-266, in bold) is followed by a distancing one, this time related to the conceptualization of the model of *psi* that it is designed to test (i.e. the noise reduction model). This contrast therefore focuses on another fundamental dimension of the use of the *Ganzfeld* protocol – its theoretical assumptions.

Not only is the same discursive move (towards an intention of integration between his and others' *Ganzfeld* studies) used as before, it is also qualified by a "sooner or later" (line 266, in bold), again constructing it as a potential rather than an actual integration. It is interesting to see here the clear construction of "the well known parapsychology's *Ganzfeld*" (lines 266-267) as the categorization of an experimental procedure. From this point on, "parapsychology's *Ganzfeld*" is made into a thing with specific qualities (Potter, 1996:111). In doing so, PA 1 makes available three of these qualities: a) it is a consensually known protocol in parapsychology; b) it uses a specific setting; and c) it hypothesizes a communication process between a sender and a receiver. The last of these qualities is then used as the focus of the contrast, making relevant the difference between his and parapsychology's *Ganzfeld* model of *psi* communication (lines 272-273). Here he provides, not a contrast, but his reservations about the model. This move allows him to distance his research from this model, yet make available that he too, despite his criticisms, makes use of the model. In this sense, this last segment seems to be a resolution of a potential relational problem, i.e. the warranting of his use of a model of which he is critical. There are, however, two discursive actions here that seem to be other ways of solving the problem: first, by constructing from the start a narrator footing (Goffman, 1979:18) in relation to this operationalization of *psi* communication (lines 269-270); and second, by constructing the adoption of this model as almost an inevitability (line 273), thus avoiding direct accountability for choosing to use it.

5.3.8 Disputing parapsychology's *Ganzfeld* procedure.

The third negotiation instance (lines 274-284) once again begins with the claim of intent to integrate his and parapsychology's *Ganzfeld* (line 274). The contrast that follows focuses, however, on the practical application of the *Ganzfeld* process itself. As in the case of research purpose and theoretical model, this third instance also focuses on a fundamental dimension that defines what the *Ganzfeld* procedure is – the series of actions that go on during a session, i.e the procedure itself. In establishing

this contrast, the participant makes relevant that the specific measurement needs of his use of the methodology clash with the specifics of the procedural actions of *Ganzfeld*. In constructing this clash (lines, 280-283), the agency of the clash is located within the actions of participants in such “standard parapsychology work” (lines 280-281) as the *Ganzfeld* process, using such formulations as: “they cannot (...) they shouldn’t verbalize continuously” (lines 280-281) and “they have to be (re)-trained (...)” (line 283). The clash between the procedure and his measurement needs seem to be constructed ultimately as a consequence of the inadequacy of parapsychology's *Ganzfeld* to accommodate his measurement, rather than, for example, the inadequacy of his measurement in relation to the *Ganzfeld* procedure. In constructing the procedures’ clash in this way, parapsychology’s *Ganzfeld* seems to be undermined as a flawed procedure, as it cannot sustain the use of electro-encephalographic measures.

5.3.9 Constructing the right to use *Ganzfeld* differently.

The fourth and final instance of negotiation of proximity (lines 284-290) differs from the previous three in terms of the claims of an intention to integrate his and others’ *Ganzfeld* procedures, and the contrast that qualifies this intention. In the previous three, the development of the contrasts outweigh the claim to integration (in the sense that the claim to integration was normally formulated as an isolated claim, while the contrasts were developed and warranted). On this occasion, while the contrast is simply an isolated claim to the prerogative of using the *Ganzfeld* methodology differently (lines 288-289), the claim of an intention to integrate is developed into one involving actions (lines 284-288), i.e. the prolonged joining of efforts with other researchers in the field (lines 285-286) and the adoption of their digital *Ganzfeld* system (lines 287-288). However, the qualification that follows – his right to use this experimental system differently (in lines 288-289) – manages this proximity with a distancing move. In this last instance, PA 1 asserts quite simply that his use of the same protocol is “in principle” different from that of others.

In summary, this section examined how participants used contrasts that allowed them to make claims that their own research is good, and therefore safe, science within a controversial scientific field. By contrasting “traditional” parapsychology with their own versions of parapsychology, and by constructing a category of good science, they were able to include their own research within this category, and exclude traditional parapsychology from it. They also constructed a category of researchers who do less than good research in parapsychology, and built up their own competence and knowledge as superior to members this category. Contrasts were also used to warrant their use of the *Ganzfeld* procedure, which is central to “traditional” parapsychology, without compromising the above distinctions. The contrasts related to fundamental aspects of the *Ganzfeld* – its purpose, the theoretical assumptions upon which it is based, and the procedure itself – warranting the declaration of

their right to do *Ganzfeld* differently. This use of contrasts allowed participants to construct their own research within parapsychology as good, safe science, albeit in a problematic scientific field. The next section will examine how participants negotiated their relationship to this problematic field by negotiating a qualified membership of it.

5.4 Negotiating a qualified membership of parapsychology.

In this final section, I will consider how some participants negotiated a qualified membership of parapsychology (as a community and field of research). The qualified memberships that were accomplished by these participants (and the ways in which these are achieved) include: a) stressing their membership of another (safe) scientific field; b) presenting it as being detached, as in a “consultancy” position; c) presenting their membership of parapsychology as part-time, temporary or conditional upon (future) events in the field; and d) constructing non-membership to parapsychology. Similarly to what Widdicombe (1998:53) observed in relation to data from interviews with members of youth subcultures, the negotiations observed here point up that the appropriateness and relevance of the identity of parapsychologist or of being in parapsychology is, to different extents and in a variety of ways, resisted. In making use of these negotiations, participants seem to be once again attending to constructions of parapsychology as a less than perfect field of science (as discussed in Chapter 4). In this sense, similarly to what Widdicombe (1998:59) analysed in her paper in relation to her participants’ construction of resistance to the affiliation with the category of “punks”, these parapsychologists’ qualification of their membership to the category of parapsychologist seems to be a way of resisting inferential consequences that might follow from accepting a full membership of parapsychology, namely, that of doing problematic research. In this sense, as it will be analysed in the next sub-sections, being a parapsychologist or in parapsychology is thus problematised, negotiated and resolved.

The analysis of extract 6 will examine how the participant constructed his membership of parapsychology as a purposeful, conditional and temporary “stay” within the field. Analysis of extract 7 will examine how, in building up a special membership to parapsychology, the participant makes use of a professional contractual metaphor, in which he makes relevant his independent and conditional position in parapsychology, as provider of expertise. In both extracts 7 and 8, the focus will be on how participants explicitly work up their membership to other scientific disciplines, and downplay their involvement with parapsychology to a minimum. Finally, in the analysis of extract 9 it will be possible to consider how the non-membership of parapsychology was achieved.

Extract 6 – PA 6, lines 350-378

350. PA E:h, you know, always the the idea of clear, eh of a clear result.
351. C Yeah.
352. PA This is an assumption. That su- such a thing exists. I think in an- in empirical
353. (access) there is no clear result.
354. C Hmhm.
355. PA There's-. Unless you have a theory. [Eh, because we don't have a theory,=
356. C [Hmhm
357. C Yeah.
358. PA =we're walking around in the in the dark room,=
359. C Hmhm.
360. PA =you know, looking for s- for something to find
361. [and we never know, you know, even, we might even=
362. C [Yeah, yeah
363. PA =grab it and eh the next time we try and we recap-, we we remember it was there
364. and it's gone.
365. C Yeah.
366. PA If there is such a thing. And I think I'll give it 10 years, and that's enough.
367. C Ok.
368. PA If I cannot find it after 10 years, I simply say I don't believe en- that there is
369. such a thing.
370. C Hmhm.
371. PA But this does not mean, **of course**, that there is not such a thing,
372. [it just means with to- today's technique we are not able to prove to- =
373. C [hmhm.
374. PA = Or I am not able to prove it to myself.
375. C Ok.
376. PA I do research because I wanna convince myself.
377. C Right.
378. PA And of course this is where I set the standards and I have to fulfil my standards.

5.4.1 Setting the context and constructing a temporary membership of parapsychology.

This extract presents an interesting construction of a particular membership of parapsychology, one that is temporary and conditional. There are two significant discursive actions being performed here. With the first, PA 6 constructs a clear causal link between the presentation of parapsychology as a less-than-perfect field of science and the temporariness of his participation or stay within it. With the second, the participant constructs the temporariness of his stay within parapsychology as subject to a self-imposed time limit relating to his ability to convince himself of the reality of *psi*. The analysis of this extract will be divided into two segments: lines 350-365, which can be seen as providing a context for the second segment; and lines 366-380, in which the central claim to a temporary and conditional membership of parapsychology is worked on.

In the first segment, we can see how the description of *psi* as a reluctant scientific object (that was explored in Chapter 4) warrants the claim to temporary membership of parapsychology. In the description that PA 6 provides, he uses an image (line 358 and line 363-364) of research in parapsychology as a theory-less pursuit (line 355), “looking for something to find” (line 360), which is impossible to replicate (line 363-364), thus making relevant the seriousness of the empirical problems of the field. This description is an extreme formulation of parapsychology as an almost “lawless” endeavour, thus justifying the claim that follows. As Pomerantz (1986) pointed out, the construction of an extreme case is compelling in “portraying the precipitating circumstance as necessitating the action” (p.228). The context of parapsychology constructed here both precipitates and justifies the setting of a time limit of his stay. In linking the description of the circumstances of the field to the statement of his temporary membership – “**And I think I’ll give it 10 years...**” (line 366) –, PA 6 thus warrants his setting of a limit for his stay. It is argued that, in this way, he makes his own position in parapsychology safe from the implication of engaging in a pointless pursuit of an imagined effect, through both a) displaying his awareness of the problems of doing research on *psi* with no theory or assurance that it is real, and b) setting a limit to how much of an investment he will make on being able to empirically establish its existence and its properties.

In the second segment of this extract, PA 6 makes his claim to temporary membership of parapsychology. This establishment of a time limit (lines 366-369) is rather like a “shelf-life” for both his participation in the field, and for his assumptions about the existence of *psi*. As before, the extremity of the statement seems to accomplish: a) the seriousness of the shortcomings of *psi* and parapsychology stated above; b) his determination in setting this cut-off point; and c) his independence from the field and, consequently, from its limitations. In line 366 (in bold), PA sets out the time limit as an ultimatum, thus making his claim to a temporary stay compelling, and in lines

368-369 (in bold), describes the consequences of not finding *psi* within that time limit (leaving the field, and assuming *psi* does not exist) as almost pre-determined, natural consequences of his departure. The definitive quality of the statement – “**And I think I’ll give it 10 years...**” (line 366) – is significant in signalling his commitment to a time limit and to complying with its consequences. This might be seen as a display of a position, indexing a stance or an attitude in relation to the field of parapsychology, rather than as an “accountably accurate description” (p. 369) of his future actions or thoughts.

The following segment (lines 371-376), however, seems to qualify the consequence of not finding *psi* in the time limit, i.e. his assumption that *psi* does not exist (lines 368-369). In offering the “explanation of what he meant”, PA 6 signals that this could be heard as an accountable description of a future event. In offering a qualification for the extremity of his claim, he is orienting to this description as a literal one of his future state of belief in *psi*. Looking at the sequence of qualifications that are offered (lines 371-378) it is possible to see that the accountability does not orient to the accuracy of the description of his belief, but to the implication that he might come to such a conclusion as a result of his research work. The upshot, after all, is that he is setting out the conditions for an empirical claim to *psi* not existing (line 371). It is the forestalling of this implication that makes the stated consequence into an accountable issue, functioning like a softener (Edwards, 2000:360). The use of the expression “of course” (line 371, in bold) constructs the obviousness of this implication being inaccurate, thus anticipating and mitigating against possible hearings. From this moment on (lines 372-380), PA 6 introduces a series of qualifiers with “it just means that (...)” that limit the consequence a) to current methodological knowledge (lines 374-376), and b) to a personal and internal dimension of his ability to convince himself through his research (lines 376). In restricting this consequence to a personal, internal feeling of “being convinced” (line 376), PA 6 seems to accomplish two actions: a) orienting to a construction of scientific research as personal inquiry, in which the criterion with which outcomes are assessed is the individual cognitive measure of “being convinced” (line 378); b) locating the claim within the context of private feelings and thoughts, where any challenge to their reality would be inconsequential. The description of this consequence is, thus, moved away from the universal implication that was mentioned above, a normative claim about the experimental reality of *psi*, to his own personal feelings about it. In this way, there is no accountable implication of his decision and consequence but those to his own feelings or thoughts about *psi*.

In this extract, PA 6 constructed and managed a membership of parapsychology that was both temporary and conditional. In the next extract, we will examine how another participant worked up a qualified membership of parapsychology, in this case by constructing and using an independent and professional contractual position in relation to the field of parapsychology.

Extract 7 – PA 1, lines 778-842

778. PA Secondly as I mentioned to you urm some parapsychologists simply don't er like
779. what we do with *Ganzfeld*. That is .h that, you know, .h er it took some time for
780. me to realize what parapsychologists did expect from **us physiologists** or
781. **psychophysicologists**, er **was something like** (hire) work, hired work, you know.
782. .h (Rea-) "hey guys, (.) come on, (.) do your work along our lines of thinking,
783. **bringing in your expertise**, you know, how how to **how to measure things** and
784. **how to plot things** and **what to compute** and so on, but for our lines of
785. thinking, =

786. C Yeah.

787. PA =Ya? And help us to prove that there is something", you know (1) =

788. C ((snigger))

789. PA = *psi* or whatever, you know. .h Er unfortunately we are not doing, we are not
790. that co-operative. We do simply the things as .h along that lines we find
791. **interesting, productive, viable** .h and urm instead of reproducing the whole
792. complex building, rebuilding again the whole complex building of a
793. parapsychology *Ganzfeld* experiment, we started with **single building blocs**, you
794. know, that is **studying one subject in simple *Ganzfeld*** without any sender any
795. **any any sending any- anything, no BBC** (claps hands).

[Suppressed lines 795-803]

804. PA .h So you see n::ot everything (.) what we do (.) is urm in (2) full (.) agreement
805. with what the, so to speak, mainstream of parapsychology is. **Ha, frankly, I do**
806. **not** consider me as a parapsychologist. Disregarding the fact that **I am member**
807. **even a full member of the parapsychology association**, and that I urm **come**
808. **regularly to those conventions and give papers and so on.** (1) Er the **crucial**
809. **point** is we find some areas of shared interest=

810. C Right.

811. PA = with parapsychology.

812. C Yeah [yeah

813. PA [Ya? And so I find it useful and productive to, you know, to exchange
814. with them and communicate with them. (.) **Frankly**, were they not interested in
815. what we do, say today or tomorrow, .h Ok, guys, forget it, y' know it was nice to
816. to learn you, to know, ya? (.) We continue **of course, ya?** (.) **In fact, we don't**
817. **need it, we don't need**, you know, er what was, what was the funny theory of of
818. of er .h er Christian Hardy all all those semantic fields and all those constructs
819. which have no bearing on reality, are just. (.) .h On the other hand, fortunately
820. there are people in this field e:r who are quite sensible, and who I think do

821. understand.
- [Suppressed lines 821-827]
828. PA e:::r but I would say frankly from my point of view it's a minority.
829. C Right =
830. PA = ya? The other looks (they) don't know. .h h (.) I think we are still a little bit
831. suspect, **what do they want?** .h I mean, ya, they [[the parapsychologists]] think
832. about us, **what do they want?** Do they corroborate our assumptions, hypothesis,
833. expectations? Or do they want to destroy it sooner or latter?
834. C U-hum.
835. PA I would say [may n:a ((sniggers)) none of above, =
836. C [N-
837. PA = or maybe both(h), I don't know (((laughs))
838. C (((laughs))
839. PA [You understand? You understand ya? ((laughs))
840. C [that's reassuring, yes, yes ((laughs)), oh yeah ((laughs))
841. PA [Unfor- unfortunately we have not enough man power to ((laughs)) to realize the
842. second. The strong program so to speak ((laughs))

This extract presents an example of how participants constructed and managed a claim to qualified membership of parapsychology. In his description of his research work, PA 1 builds up a claim to being an independent, specialist, necessary and conditional “consultant” to parapsychology, rather than a parapsychologist. For practical purposes, examination of this extract will be divided into two parts.

The first part is concerned with how the participant constructed the story of his relationship with parapsychology using a professional contractual metaphor. Much like an outside “consultant” in a corporation, PA 1 constructs a version of his relationship to parapsychology as independent and temporary, providing necessary specialist expertise to parapsychology. In using this metaphor, the participant makes relevant that: a) he possesses and provides specialist scientific expertise that parapsychology needs; b) he is independent from parapsychology; c) he has the power in this contractual arrangement to set its conditions and purpose, and terminate without any loss to himself. This metaphor makes available readymade images (such as employer/employee or contractual conditions) as a set of available pieces for the construction of a version of his relationship to parapsychology that stresses his independence from the field, and downplays his intellectual involvement with its claims. The second part is concerned more directly with how PA 1 constructs his membership of psychophysiology, and how his membership to the parapsychological community is further mitigated both by reinforcing his position of independence, and by contrasting his appearance

of being an active parapsychologist and the reality of what he feels and thinks.

5.4.2 Constructing a professional and contractual metaphor.

In the first segment of this extract (lines 780-794), the story is being told from the point of view of what he “eventually realized” (line 780) was his image and role within the field. This “reported” view, making use of the reported speech of a vague group of parapsychologists (lines 782-789) builds up the factuality of the version that is going to be put forward (Potter, 1996:161), sets up a “them” in relation to “me”, and allows him to set up a later contrast between what parapsychologists think he is and what he really is (lines 781-805). In providing this “report”, PA 1 uses a metaphor that constructs his relationship to parapsychology as that of a professional contract. Thus, he provides the view that parapsychologists have of him (and psychophysicists like him), i.e. an expert professional, brought in to work towards the aims of parapsychology in the production of experimental proof of *psi* (lines 780-788). Although the participant signals (line 781, in bold) that this metaphor should be heard as a performance rather than a literal description (Potter, 1996:181), this version of his relationship to parapsychology claims: first, that he is not a parapsychologist, rather he works in parapsychology; second, that he has expertise that is indispensable to parapsychologists. In lines 783-784 (in bold), PA 1 lists the expertise that he has been “asked” to bring into parapsychology. This listing of expertise could be used here to “summarize a general class of things” (Potter, 1996:196), rather like “basic research skills”. In making relevant that this is the kind of expertise that he is bringing into the field, he achieves: a) his self-presentation as a skilled and competent scientist; b) a construction of parapsychology as a field in which these very skills are lacking and in demand; and c) his indispensability to parapsychology.

He then contrasts this version of how parapsychologists perceive him and his work with what he really is and does (lines 781-805). The key contrast is that while they see him as a taken-for-granted collaborator, he disputes this. In saying “unfortunately (...) we are not that co-operative” (lines 789-790), this could function as a way of signalling the obviousness of his un-cooperativeness. The straightforwardness of his “non-compliant” position is also made relevant in lines 790-791, when he describes what he really does as “simply” doing the work that he finds valid. Again, he makes use of a list to summarize a general class of independent and scientific criteria (line 783, in bold) that guide his work in parapsychology. The contrast between these criteria and the aim of parapsychology (the experimental search for *psi*) is made clear when, by using the word “instead” (line 791), parapsychology’s “whole complex building” (lines 791-792) of the *Ganzfeld* is put in direct opposition to his straightforward and scientifically simple choice of aims and methods, even if using the same general *Ganzfeld* methodology. As discussed in section 5.3, contrasting what one does with

what others in parapsychology do, constructs a “safe” type of parapsychological research. Here, it also seems to function as an available display of his own work as different, independent and more scientifically competent. In the description (lines 793-795), the construction of his version of the *Ganzfeld* is made in a way that makes relevant the scientific superiority of his way of working in parapsychology, including the lack of interest in *psi*. The extreme case formulations (lines 793-795, in bold) display a position (Edwards, 2000:363), one that is adversarial to parapsychology. Indeed, the sarcastic reference to “no telepathy” (i.e. transmission) as “no BBC” in line 795 (in bold) undermines the aim of setting up an experiment to investigate the possibility of telepathy by making it into a comical issue. In lines 804-805, PA 1 summarizes his relationship to the “mainstream of parapsychology” (line 805) as one that is not in full agreement, but only after having established both his work in parapsychology as independent from the aims of parapsychology, and the scientific superiority of his approach over that of other parapsychologists.

5.4.3 Upgrading the metaphor – constructing power in his relationship to parapsychology.

In this part of the analysis, I will focus on how this construction of a professional contractual relationship with parapsychology allows PA 1 to talk about contractual conditions and balance of power. In lines 814-819, PA 1 seems to make use of the contractual metaphor to present a version of his relationship to parapsychology as one in which he set conditions and decides upon potential termination without risking any personal loss. In line 814, the participant introduces an upgrade with the expression “Frankly” (in bold), prefacing the next utterance as a moment of sharing a personal and honest opinion. The claim to independence that follows is thus constructed as one which is relatively private, suitable for a moment of “honesty”. The hypothetical situation presented makes available the condition that he demands in this relationship – parapsychologists must be interested in what he does (lines 814-815). The consequence of lack of compliance with this condition is the termination of the contract, with no loss to his research activity. In line 816, the words “we continue of course, ya?” (in bold), seem to build up the right and the obviousness of his immunity to damage or loss. In using “in fact, we don’t need it” (in bold), the participant performs a further upgrade to his claim to one of independence. This is not only repeated, but the emphasis given to the “don’t” also signals his investment in this constructed immunity. Indeed, PA 1 makes available that it is parapsychology that would suffer from the withdrawal of his expertise, and that this is a logical implication of the need of such expertise (that was built up earlier). The warranting of this immunity again orients to parapsychology as a less than perfect field, with little to offer him (lines 817-819).

In the final segment of this extract (lines 830-842), the claim to independence and power in his

relationship to parapsychology seems to be upgraded again. PA 1 uses, this time, the reported thoughts of parapsychologists (lines 831-833), regarding what they hypothetically make of his intentions towards parapsychology – “**what do they want? (...) what do they want?** Do they corroborate our assumptions, hypothesis, expectations? Or do they want to destroy it sooner or latter?” (lines 831-833) In doing so, he constructs an interesting display of both his power and independence from the field, as he shows himself to be a) deliberately ambiguous or mysterious about his role within the field; and b) holding the “better hand”, i.e. holding the desired and necessary expertise. In the repetition of the question “what do they want?” (line 831-832, in bold), PA 1 emphasises the undisclosed or inaccessible (to some parapsychologists) nature of his activities and the suspicion that it causes amongst parapsychologists. This emphasis performs a deliberately suspicious positioning among parapsychologists.

A clarification is then offered in lines 832-833, where he contrasts two possible intentions for his actions within parapsychology is made, either to corroborate or to destroy the field. In asking these questions in such strong terms, it is argued, that he seems to point up that these questions are performative and non-literal. The two alternative intentions being offered exaggerate the claim and function as a way of joking about this relationship. The sequence of joking turns that follow (lines 835-842) seem to be ways of continuing to upgrade this claim to independence, without the potential implications of being seen as overcritical or unreasonable. In lines 841-842, PA 1 presents the punch-line for the joke, that “unfortunately” (line 841) there are not enough people like him to “realise the second” (line 841-842). The use of this punch-line allows him to build up this sequence into a joke, “disarming” the talk about the destruction of parapsychology from the interpersonal consequences of being heard as someone who literally wants to end the field of parapsychology. The use of “strong program” (line 842) can be seen as a metaphor for intense criticism and levelling down of parapsychological theories, beliefs and methods, rather than being a literal reference to the research orientation within the sociology of scientific knowledge.

So far, the analysis of extract 7 has considered how PA 1’s construction of his relationship with parapsychology as that of an independent and temporary collaborator who provides necessary specialist expertise. During this construction, he emphasised the contractual nature of the relationship and the powerful position that he (as a provider of a necessary service) holds within parapsychology, while maintaining his independence from the field. He thus builds up a self-presentation as one who works in parapsychology, but is not a parapsychologist. Part two of the analysis of this extract is concerned with how PA 1 constructs his membership of psychophysiology, and how his membership of the parapsychological community is further mitigated both by reinforcing his position of independence, and by contrasting his appearance of being an active parapsychologist and the reality of what he feels and thinks.

5.4.4 Constructing membership to one category and mitigating membership to another.

The construction of the participant's membership of psychophysiology is established at the beginning of this extract. With it PA 1 achieves not only independence and differentiation from parapsychologists, but also the entitlements that come with membership of that particular group of scientists (physiologists or psychophysiology), i.e. the robust scientific status of their work and the legitimacy of the claim to hold scientific credentials and specialized knowledge. In this way, by becoming a particular category of scientist, especially a commonly constructed category of consensually accepted scientific expertise like physiology or psychophysiology, the factuality or persuasiveness of his claims to scientific knowledge are "guaranteed" as an entitlement of that category (Potter, 1996:114). It is interesting to note how the membership of physiology or psychophysiology is almost spelled out. When he refers to what parapsychologists, as a distinct category of researchers from himself, expect from "us" (line 780, in bold), this is followed by the explanation of who "us" are (line 780-781, in bold). The explanation identifies two possible memberships, of psychophysiology or physiology, i.e. not a specific field, but a general class of scientific fields of which he is part, with robust scientific status, and which, of course, is distinct from parapsychology. This working up of membership of a field of science that is distinct from parapsychology, seems to be linked to what Collins and Pinch (1979) put forward as a "tactic" of legitimization of parapsychology as a field of science. The authors called it "metamorphosis" (p. 253), and explored it as parapsychologists' "transformation into ordinary quiet members of the scientific community" (p. 241). Collins and Pinch sociological analysis focused specifically on the integration of parapsychologists within social communities of consensually recognized scientificity, like academic settings, or their participation in social practices like the publication in scientific mainstream journals. From a discourse analytic point of view, one is able to appreciate not only the diversity of ways in which different "metamorphoses" are accomplished and what these achieve, issues which Collins and Pinch gloss over with the notion of legitimisation.

Moving now into the second aspect of membership construction, we find that PA 1 actively mitigates his membership of the parapsychological community (lines 805-814). He achieves a position in which the overlap between his work and mainstream parapsychology is reduced to minimum contact, that of communication and exchange of interests (lines 813-814). In lines 805-806 we can see a direct claim to not being a parapsychologist. This claim is direct and emphasised (note the emphasis on "I do not" in lines 805-806) and is introduced with "Ha, frankly" (line 805, in bold). This interjection features here again, and, as before, prepares the claim that follows as one which should be heard as a moment of sharing private and sincere thoughts. It is followed by the confession of a list of items that could undermine the initial claim to not being a parapsychologist (lines 806-808). From the items on

this list (in bold), PA 1 includes two aspects of his participation in parapsychology that are inherently public, i.e. regular attendance and presentations at the PA annual meetings (lines 807-808). This builds up the opposition between what he appears to be on the surface – an active member of the parapsychological community – and what he actually is – the private “considering” (line 806) of not being a member of parapsychology. The reality of his non-membership to parapsychology (constructed through the sharing of his private and sincere thoughts in lines 805-806) overrides the appearance of his participation in parapsychology. This contrast between the reality of his thoughts and the appearance of his membership to a parapsychological organisation and regular participation in conferences, makes use of what Edwards (1991:532) calls the “appearance-reality device”. In invoking a distinction between the superficial appearance and the true version of himself, PA 1 acknowledges the inference that these external signs of being a parapsychologist allow, but, crucially, subverts this appearance with a confessed and sincere version of the way he really feels or thinks (Edwards, 1991:532). The logical development of this reality (as opposed to the appearance) is the mitigation of his participation in parapsychology down to the “crucial point” (line 808, in bold) of simply having some shared interests with the field. By reducing his activities to activities of minimum contact, such as “exchanging” and “communicating” with parapsychologists, his practical and intellectual involvement with parapsychology is successfully downplayed.

In summary, the analysis of this extract examined how the participant constructed a position of an independent, specialist, powerful and conditional “consultant” to parapsychology, rather than that of a parapsychologist. It explored how he made direct claims both to membership of another “normal” field, and to non-membership of parapsychology. Thus, he negotiated a qualified membership of parapsychology, and constructed his own position within the field as a “safe” position. The analysis of the next extract will explore how another participant constructs his membership to a “normal” scientific field, and qualifies his participation in parapsychology as a “part-time” activity.

Extract 8 – PA 8, lines 150-159

150. PA But think also it depends upon (.), if you like, where you’re coming in terms of
 151. where you work. For example **I’m a lecturer in kind’a University []** and
 152. **I teach** (.) part of the psychology department [and the psychology course.=
 153. C [Yeah.
 154. PA = **So obviously, most** of my work has to do with normal psychology, with
 155. broader (psychologies), and **any** parapsychology I do is **almost on the side**.
 156. **[So, it’s part-, if you like, it’s part of where I work** (and what) my job, =
 157. C [Right
 158. C Right.

159. PA = **I have** to kinda integrate it.

The analysis of this extract will focus on how PA 8 constructs his membership of psychology as the main thread of his research life, and his membership of parapsychology as a “part-time” activity. His membership of psychology is warranted as a natural consequence (lines 154 and 156, in bold) of working in the environment of a psychology department. It is thus bound to: a) his status as a lecturer (line 151, in bold); b) the activity of teaching within the context of a psychology department (line 152, in bold); and c) the context itself. It is almost as though being part of that context is being used as a guarantee for the factuality of his categorisation as a psychologist. In line 159, the psychological characterization of his work is built up as an imposition, hence an entitlement, of his context. This construction of context bound membership and the entitlements that it brings with it (i.e. participation in an academically legitimate field), can also be seen as discursive accomplishment, perhaps related what Collins and Pinch (1979) meant by the “metamorphosis” of parapsychologists into “ordinary” scientists (p. 241, 253).

Parapsychology is then constructed as a part-time activity. In lines 154-155, PA 8 builds up a version of his work in parapsychology as “almost on the side” (line 155). He first constructs the category of “normal psychology”, the implication being that parapsychology is not included within this “normal” category. A second contrast is then set up, as “normal psychology” is the context of “most” (line 154, line) of his work, “any parapsychology” (line 155) he does is almost done “on the side” (line 155, in bold). The way in which his activities in parapsychology are constructed makes relevant that a) parapsychological work is not a regular activity (note the use of “any” as a construction of rarity or lack of regularity), and b) it is literally built as being done “on the side”. The mitigated location of parapsychology on the fringe of his research activities, as almost as a part time job (or even a hobby), makes, not only, a show of the contrast between his professional involvement in parapsychology and in psychology, but also, makes particularly relevant his minimal contact with parapsychological activities.

This sub-section has examined how participants negotiated their qualified membership of parapsychology using the construction of their membership to another scientific discipline and community. The next section will examine how a participant put forward a clear refusal of the appropriateness of the category of parapsychologist as an accurate description of himself.

5.4.5 Constructing non-membership to the category of parapsychologist.

This last section is concerned with the analysis of extract 9, where PA 20 orients clearly to the issue of affiliation to an implied (though available) category of “parapsychologist”. This is presented as a preface to his response to a question regarding his work in parapsychology. This preface includes, first, the refusal of his membership to the implied category of parapsychologist, and, second, the proposal of an alternative membership.

Extract 9 – PA 20, lines 1-9

1. C Right so ehm (.), just to start with, eh, the first question is very very general
2. which is, you know, what has your work within parapsychology mainly focused
3. £ on £. (.) £ V(h)ery, very- it’s a huge question £ ...
4. PA Sure. (.) **Ahm [1], w- [2] again [3]** first of all I mean I wouldn’t mainly describe
5. myself as a parapsychologist. =
6. C = Hmhm, hmhm
7. PA Ahm, and so:: I think of myself as being a psychologist with an interest in
8. psychology of ostensibly paranormal experiences.
9. C Yep.

Overall PA 20 uses this preface to resist the category of parapsychologist. Prior to further analysis, it is important to point out that this category, although not explicit in C’s question, is nevertheless available as an appropriate description of PA 20, given the invitation (and his acceptance) to participate in this interview as a parapsychologist, and the background of his regular participation in parapsychological conferences and discussion forums. This prefacing of a response regarding the type of research work that PA 20 does is initiated in line 4 with three elements (in bold, numbers in square brackets) that could be seen as delaying the answer. The statement of refusal of the category of parapsychologist that follows seems in this way to be oriented to as one which is not the preferred response to the question, and thus builds up this statement as a preface. PA 20’s resistance to the appropriateness of the description of “parapsychologist” is presented in a very straightforward way (lines 4-5). The appearance of being a parapsychologist is thus taken up directly as an issue that needs to be corrected or clarified, almost as a misunderstanding, before continuing our interaction. According to Widdicombe’s (1988) study of the accomplishment of membership and non-membership of youth subcultures, “respondents take account of the way that their appearance may be used to infer their category affiliation, and that this in turn can be used by others to ascribe motives to them which

are largely negative” (p. 61). Although appearance here needs to be understood in a somewhat different way, it could be argued that PA 20’s refusal of the description of parapsychologist makes available that a) this category is inference rich, and b) that these inferences are not necessarily desirable.

PA 20’s presentation of an alternative description of himself as a (lines 7-8), points up that the category of “psychologist” is, by contrast, a desirable one (note the repetition of “psychologist” who is interested in the “psychology” of paranormal experiences). This alternative category, and its clear presentation, seems to be almost ready-made, as if prepared earlier. It is also arguably indicative of where the troublesome inferences of the category of “parapsychologist” lie: not in the content of parapsychological study (ostensibly paranormal experiences), but in the affiliation to the field of parapsychology. In this way, PA 20 constructs a version of who he is that can be articulated as “I do parapsychology, but I am not a parapsychologist”.

Summary.

This chapter focussed on how participants’ versions of what they do as researchers in parapsychology are concerned with the presentation of their work as doing safe science (i.e. investigating claims and using methods that are in line with common ideas of what science is), in a less than perfect scientific field. This was examined through four analytical themes. The first section showed how PA 1 constructed doing ordinary science in parapsychology by: building up the scientificity of his research as an entitlement of his expertise in psychophysiology; using ordinary scientific, rather than parapsychological, terminology; and appealing to the mainstream relevance of his work. The second section showed how PA 8 constructed his research as particularly rigorous as a result of external criticism and internal difficulties within the field, and by contrasting the emphasis upon methodological rigour in parapsychology with the emphasis upon obtaining results in “other fields”. Thus, the problems and controversies in parapsychology were built up as a guarantee of the scientificity of his research, and the lack of consistent results in parapsychology is built up as evidence of rigorous methodology. The third section showed how participants contrasted “traditional” parapsychology with their own parapsychological research, overlapping with categories of good science (and scientists) and not-so-good science (and scientists), even in relation to use of the *Ganzfeld* procedure, which is central to “traditional” parapsychology. The fourth section showed how participants negotiated a qualified membership or even non-membership to parapsychology and constructed in diverse ways relationships with parapsychology that were independent of it. All four of

these analytical themes were explored and argued for as processes through which participants constructed their own research in parapsychology as doing safe science in a less-than-perfect-field. The next chapter will focus on the related issues of how parapsychologists constructed doing research as doing strict and rigorous empiricism.

Constructing doing empiricism.

Introduction.

This chapter examines a particular aspect of the participants' constructions of doing research in parapsychology, as strictly empirical, bottom-up and neutral. Related to the discursive actions explored in the previous chapter, it focuses on parapsychologists' constructions of their research practices as being firmly based in evidence, as experimentation with no assumptions, boundaries of expectation or theoretical underpinnings. In doing so, these parapsychologists achieve versions of their science that are strictly limited to the "virtues" of neutral experimental practices and concerned primarily with doing methodology.

As with parapsychologists' accounts explored in the last chapter, there is nothing contentious or surprising about experimental researchers describing their work in a way that makes relevant and asserts the independent, "received" (Woolgar, 1996:13) and therefore valid nature of their outcomes. These parapsychologists' accounts are therefore not exceptional amongst scientists in putting forward approaches to the world that put facts logically and causally prior to theory, what Sheffler (1967, cited in Mulkay, 1979:19) called "the standard view of science". This version of science builds it as an "intellectual enterprise concerned with providing an accurate account of objects, processes and relationships occurring in the world of natural phenomena" (Mulkay, 1979:20), one that is considered to be real and objective, and not determined by "the preferences or intentions of its observers" (Mulkay, 1979:19). According to this view, the validity of scientific statements and practices is determined by their ability to neutrally represent the "true character of this world" (Mulkay, 1979:20). This view also recognizes another type of scientific statements that are more speculative, which go beyond observations and used to develop observed regularities into coherent intellectual frameworks (Mulkay, 1979:20). This "standard view of science" therefore implies a crucial distinction between observed facts and theoretical statements, the latter being revisable and dispensable, but the former being primary and based upon the very "fabric of the natural world" (Mulkay, 1979:21). Such a view logically posits the possibility that facts are expressed as representations of the observed world, and therefore theoretically neutral. This common view of what science is has, of course, been seriously

challenged through the examination of the logical and practical difficulties posed by assumptions such as: the stability or regularity of the world out-there; the unproblematic neutral access to this world through observations; the unequivocal distinction between fact and theory; the causal and logical primacy of factual accounts in what counts as valid knowledge; and the possible dismissal of theoretical statements as secondary aspects in scientific work. The “standard view of science” remains, however, rhetorically powerful, and can be found, with the softening of some of the ideas above, in textbooks, journal articles or informal accounts (Woolgar, 1996:13).

Yet, while there is nothing exceptional about researchers putting forward accounts of doing science that make relevant some these ideas. What is particular about some of these parapsychologists’ accounts, and therefore what will concern this chapter, is the extremity of their descriptions of a) the primacy of facts, b) their neutrality or impartiality towards their scientific objects, and c) the problematic nature or dispensability of theoretical frameworks. The extremity that is referred to here concerns the discursive form that these accounts take. The chapter will focus on the way these accounts of scientific practice are accounted for at length, achieving versions of science that are strictly caused by, and logically based upon, facts. Section 6.1 examines how participants constructed extreme versions of the primacy of data and the dispensability of theory, orienting to the unproblematic independence of facts from theory. Section 6.2 focuses on how participants’ constructions of their strictly factual and theory-free research are reified in the identity of bottom-up researcher. Finally, section 6.3 explores how the participants made relevant their neutrality in relation to their outcomes, orienting to the construction of valid research as the neutral representation of facts, independent of factors such as personal investment in results or belief in *psi*. All of these issues and discursive achievements are necessarily related, and interwoven in all of the descriptions of research practices presented in the following extracts. The following sections are used as a way of organizing and pointing up what these parapsychologists accomplished in the accounts of their research. Although these sections were organized using particularly clear extracts, these stand as exemplars of widespread discursive practices within this group of participants and, for that matter, in the field’s literature. It is therefore argued that the analytical claims that these extracts afford, could potentially apply or be relevant to other practitioners in the field.

6.1 Constructing extreme empiricism – the primacy of data and the absence of theory.

In this section, I will focus on how some participants put forward versions of extreme empiricism, i.e. versions in which the primacy of data and experimentation was maximised, and in which assumptions, models or theories were constructed as being dispensable or removed altogether. With this purpose in mind, I will explore (in extracts 1 and 2) how two participants put forward and warranted versions of their research as exclusively producing data from experimentation, orienting to the unproblematic independence of facts from theoretical frameworks.

The characterisation of “extremity” of these accounts of empiricism, as was alluded to above, refers to the discursive form that these accounts take. This discursive form is necessarily related to the conversational structure of extreme case formulations (henceforth ECF). This was first suggested by Pomerantz (1986) as a set of conversational resources present in our practices of description, and which we use for the purpose of turning a claim into a legitimate one, or, as Pomerantz (1986) puts it, “to portray a set of affairs as believable, obvious, compelling, unreasonable, illogical etc (p. 219). Pomerantz’s 1986 paper examined the way extreme case formulations work in complaining, accusing, justifying and defending. Formally, ECF refers to the use of terms in description that invoke maximal or minimal properties of events, objects, actions, etc. (Hutchby and Wooffitt, 1998:209), for example, with the use of adjectives (best, least), nouns (everything, nothing) or phrases (as good as it gets) (Edwards, 2000:349). In a later paper Edwards (2000) extended Pomerantz’s work by observing that ECFs, in addition to their legitimising features, “can be oriented to in conversational interactions as rhetorically weak, indicative of various kinds of speaker investment and nonliterally meant” (Edwards, 2000:348). In his paper, among other points related to the empirical and conceptual features of ECFs, the author proposes that, although ECFs should indeed be understood and identified as statements which are conceptually semantically extreme, the important thing about these conversational practices is that they allow us ways of “hearably, commonsensically, going to extremes” (Edwards, 2000:350). Inspired by Edwards’ exploration, this chapter will make use of enlarged and looser notions of extremity, power or potency in descriptions as hearable discursive practices, and will explore these participants’ descriptions of what they do as doing a powerful or, at times, extreme form of empiricism.

Extract 1 – PA 6, lines 14-85

14. C Hmhm, hmhm. Ok, and, through all that, what would you say that are your main
15. assumptions, your main **theoretical** models that you work with, your main
16. **theoretical** assumptions?
17. PA **It's a very empirical, ehm, model?**
18. C Hmhm.
19. PA **I just wanna know if there's anything in the data.**
20. [You know, if I give a talk on it=
21. C [Right
22. PA = (If I)- I don't care, or I actually say I give a shit on it,=
23. C Hmhm, hmhm.
24. PA = **a:nd I'll see what the data tells me, and I'm gonna learn from the data. I've**
25. **no theoretical model, no theoretical parapsychological model.**
26. C Hmhm.
27. PA So my, the model of course, or the model on the other side is very empirical.
28. C Yeah.
29. PA **I'm data driven empirist [empiricist].**
30. C Ok, ok. What would you say, in your assessment, are the strengths and the
31. weaknesses of working from that particular kind of perspective, of using that
32. particular kind of assumption or model.
33. PA Pfu:: ((exhalation)), this is a difficult one ((laughs))
34. C ((laughs)), ooh, sorry ((laughs)).
35. PA [What are the
36. strengths? Ehm, **I could say why I do what I do**, but what is the strength? The
37. strength of the model is that I don't -, (1) eh, (.) that it (.)...
38. C In the sense, ahm, that you think that it provides you a good way, or a useful way,
39. or a valid way [of understanding human experience, of...
40. PA [**Of course.**
41. PA **For me** the emper-, the empirical access, access, the empirical method is the (.)
42. method that I (.) which **I'm interested in.**
43. C Hmhm, hmhm.
44. PA **I believe in** this kind of means and data,=
45. C Hmhm.
46. PA =if it's done in an appropriate way, and this is where **of course** the hook,
47. [the hook is?
48. C [Yeah, yeah
49. PA Or this is where the problem [arises.

50. C [Right, right.
51. PA It's not like many people believe, you can tell, you know, you can prove anything
52. with statistics. **Of course you can't** .
53. C Hmhm.
54. PA But once you're trained and once you've you're experienced, it's very difficult to,
55. ehm, to mani- manipulate you. You know, once you know how a design has to
56. look like in this particular field,=
57. C Hmhm
58. PA = it gets better and better. **And I love this, I very much like this empirical**
59. **access and I very much like this data.**
- [suppressed lines 59-77]
77. PA And, **of course**, th- the **big weakness** is if you have no theory, if you have no
78. theoretical funding, like, you know, a huge theory, how this effects work and no-
79. and not, you can **never prove or dif- disprove ev- anything empirically.**
80. C Hmhm.
81. PA **I know**, you know, all (.) all the (access) that I have is like, ehm, walking
82. **through a (.) a dark room and looking for the answer.**
83. C Yeah.
84. PA **It's very difficult.**
85. C Right.

The analysis of this extract will focus on the extremity with which PA 6's research work was characterized, what might be called "doing extreme empiricism". It argues that PA 6's description of his research seems to function both as a "persuading" description and assessment of his activities as thoroughly empirical, and as a way to "*index his stance or attitude*" (Edwards, 2000:363, italics in the original) in line with the standard view of scientific action. The analysis focuses on PA 6's: a) simplification of the process of research into a straightforward and construction of data driven empiricism; b) construction of a sequentially incremented personal warranting of his choice of approach; and c) construction of this research option as both difficult, but inevitable, form of enquiry – i.e. constructing "extreme science".

6.1.1 Constructing strict empiricism.

This first section looks at how PA 6 achieved the simplification of research into a process of “looking at” or “hearing out” the data. The initial segment of this extract (lines 14-29) includes his characterization of his research model, following a request to talk about theoretical assumptions and models. It is worth noting that the interviewer revises the question three times, repeating the word “theoretical” each time (lines 15-16, in bold). This unequivocally builds up the question to be one about theory, thus orienting to theory as a feature that is universally present in doing science. In the first segment of PA 6’s reply, however, instead of talking about theoretical elements, he characterizes his approach as “very empirical” (line 17, in bold), challenging the existence of theoretical aspects in his research from the start. In doing so, it is argued that PA 6’s straightforward construction of his empiricism seems to function here as an efficient response to the implication that doing research necessarily includes holding assumptions and theories about phenomena.

The following characterization of what he means by a “very empirical model” includes the construction of the primacy of data and the absence of theory, particularly in lines 19 and 22 to 25. The achievement of the clear formulation of the lack of a theoretical model (line 25) in his research practise is boosted by the delimitation of his role within it, that of seeing or listening to what the data tells him, or learning what the data has to teach (line 24, in bold). Thus, PA 6 downplays his personal agency and constructs a position in relation to his data as a naïve observer concerned with “just” (line 19, in bold) looking, hearing or learning from “the data”. Furthermore, in “just” wanting to know if there is “anything” (line 19, in bold) in the data, the participant constructs a version of doing research which is not guided by a particular effect, rather it takes anything that the data shows him to be meaningful. The absolute neutrality constructed in this sentence, and the simplicity of this position (note the use of “just”), allows PA 6 to achieve an account of his science that “denies its character as an interpretative product” (Gilbert and Mulkay, 1984:56). Instead, his “actions and beliefs [are made into] a neutral medium through which empirical phenomena make themselves evident” (Gilbert and Mulkay, 1984:56) and the world of phenomena (through “the data”) seems to speak and act for itself. This “data” is thus “given chronological as well as logical priority” (Gilbert and Mulkay, 1984:56) in PA 6’s account of his practice. The participant’s research is thus constructed as a rhetorically robust process of receiving knowledge from the world, which by itself warrants claims and allows the further construction of his scientific conclusions as unproblematic representations of what the phenomena “are really like” (Gilbert and Mulkay, 1982:400). Having established the strict priority of data, the upshot about the absence of any “theoretical model” (line 25, in bold) is a straightforward step in his description. However, it is also interesting to note that PA 6’s outright exclusion of a “theoretical model” is repaired and limited to “parapsychological model” (line 25, in bold). This repair could function here as a softener of the absolute removal of theory from his data, reinforcing the

reasonableness of this assessment. Furthermore, in narrowing the absence of theory to a parapsychological context, PA 6 could be making available that “other” theoretical models do exist in his research. In terms of his construction of a qualified membership to parapsychology (examined in the last chapter, Extract 6), this bracketing of parapsychological models leaves open the potential relevance of “other” theoretical models (such as those from areas of higher scientific status, such as physics or physiology).

The first segment of this extract closes with the participant’s description of his position as a “data driven empiricist” (line 29, in bold). This identity making statement at the end of the description of his research practices seems to work here as both an upshot of this description and as a form of “take-home-message”. In constituting himself as a “data driven empiricist”, PA 6 appeals to the “understood” entitlements of this category. By assigning himself to this category, PA 6 ensures that the conventional knowledge about the practices of “empiricists” can be used to interpret or explain his actions (Hutchby and Wooffitt, 1998:214). The interactional resource of appealing to an identity will also be explored later in this chapter (in the analysis of extracts 3 and 4). With this construction of a definite position in relation to data and theory, PA 6 establishes a closing point in the description of his research practices.

6.1.2 Warranting empiricism as an internal disposition.

This sub-section is concerned with PA 6’s warranting of his research practices as doing extreme empiricism in terms of an internal disposition. The analysis starts with the negotiation of the second question that was asked (lines 30 and 39). The question asks PA 6 to talk about his assessment of the strengths and weaknesses of the research practices he has just described. The participant’s initial response begins with a marked exhalation (line 33), which could be seen as a preface to his response “this is a difficult one” (line 33), signalling its design as a dispreferred response (Levinson, 1983:334-5, cited in Heritage, 1984:266-7). The construction of the difficulty of the question is centred on the word “strengths”, and the participant then provides a preferred alternative to it – “I could say why I do what I do” (line 36, in bold). This alternative hearing moves the question from a request for a general assessment of the strengths and weaknesses of his research into a personal account of this choice of research model. However, from the interviewer’s pursuit of a response (Pomerantz, 1984b:152), it is possible that the participant’s proposed alternative hearing is oriented to by the interviewer not as a suggestion, but as a misunderstanding. Thus, in the next turn C responds to PA 6’s proposed alternative question by clarifying an understanding problem about the word “strengths” (lines 38-39) that she originally used in line 31. The interviewer’s clarification is heard by PA 6 as an assessment, rather than a question, with which he emphatically agrees (line 40, in bold). From this point onwards,

PA 6 moves on to the assessment of his research practices, making relevant his personal interest in them, orienting to the alternative question, i.e. “I could say why I do what I do” (line 36, in bold). The negotiation of what the question is about is thus closed by PA 6. He moves on to warrant his use of empirical “access” or “method” (line 41) as a personal choice, driven by an internal disposition towards such practice, marked by internal dispositions such as “interest” (line 42, in bold) or “belief” (line 44, in bold), and feelings such as liking and loving (lines 58 and 59, respectively, in bold). The participant’s assessment seems to use formulations that emphasize the strengths of doing empiricism, yet seem to be, as Edwards (2000) puts it, “simultaneously available to be treated as signalling the speaker’s investment in that point” (p. 364).

In lines 42 and 44 (in bold), PA 6 begins this warranting in terms of personal interest and belief. Such an express evaluation of his interest and belief in empiricism, could be argued, might potentially be heard as undermining his self-presentation as a naïve empiricist, with the implication of a clear stake in doing research in this way (i.e. clashing logically with the claim of unmediated access to nature, where the experimenter’s interest and belief just do not need to feature, or even to be expressed, for that matter). In this sense, PA 6 offers here a confession of interest (Potter, 1996a:130). However, in providing this assessment of his personal feelings and thoughts about empirical research, the participant displays that he is not providing an abstract and objective assessment of the “strengths” of this model, but, as he proposed in line 36 (in bold), an explanation of “why he does what he does”. It can thus be treated as a way of establishing the question that he is answering, as it constructs his choice of empiricism as one that is full of interest. Nevertheless, in the following sequence (lines 46 to 52), it is treated by PA 6 as excessive, as he twice softens his interest and belief in empirical research. The first softening restricts his interest and belief in empiricism to the occasions when it is correctly used (lines 46 and 49), a restriction he constructs as an obvious one – “of course” (line 46, in bold). The same constructed obviousness is used to introduce the second softening, this time concerned with the restriction of the empirical insight that statistics provides (line 52, in bold). In doing so, he introduces two opposing categories of researchers: those who think that statistics and data can prove anything (lines 51-52); and those who, like him, do not. This is used to construct a further dichotomy between those who (because of their “blind” belief in statistics) are fooled by bad data analysis, and those who, like him, use their knowledge and experience of empirical methods to safeguard against being “manipulated” (lines 52-54) by faulty research. The construction of these two opposing categories not only makes available his distance from the “wrong” use of empiricism, but also warrants his interest and belief in empiricism as being discerning, informed and experienced (rather than, say, naïve).

Following these two instances of softening, the assessment of his personal reasons for strict and thorough empiricism is upgraded with terms like “[I] very much like” and “love” this data (lines 58 and 59, in bold). This upgrade could be treated as a way of “hearably going to extremes” (Edwards,

2000:350), having just offered more “reasonable” versions of the same claim. In returning to an extreme version of his personal investment, PA 6 retains the extreme formulation’s performative use (Edwards, 2000:359). It therefore succeeds as a way of hearably constructing his research as robustly empirical.

6.1.3 Constructing “X-treme science”²⁹.

The last moment of this extract (lines 77 to 84) is concerned with the second part of the question in lines 30-32, i.e. the request for the participant’s assessment of the weaknesses of his model of empirical research. This time, rather than offering, say, a personal account of problems, he provides an abstract assessment of “the way things are”. Framing it as a factual evaluation of the model, he puts forward the “big weakness” of not having a theoretical model, the fact that “you cannot prove anything” (line 79, in bold). In line 77, PA 6 prefaces this assessment of weaknesses with an “of course” (in bold). This is effective in mitigating the potential damage of the assessment to the previous claim, by making it obvious (and therefore “old news”). Indeed, his acknowledgement of it signals his honesty and objectivity, as he makes available that, regardless of his personal dedication to empiricism, he is able to step back and see its flaws (Potter, 1996a:13). Much the same is achieved with the prefix “I know” (line 81, in bold), in constructing the same disarming awareness of the problem of proceeding with research in extreme conditions of uncertainty.

The formulation of “the big weakness” in line 77 (in bold) can be looked at as a way of indicating his reasonableness, bringing in theory as a necessary frame for the production of empirical proof. For instance, the use of “never” (line 79, in bold) emphasises the impossibility of meaningful empirical facts in the absence of a theory. PA 6’s assessment of this “big weakness” is used to construct his research as a form of “x-treme science”, i.e. necessarily difficult and challenging. In the segment between lines 81 and 82, PA 6 offers a metaphor for the extreme empirical research that he practices. This metaphor – “walking through a (.) a dark room and looking for the answer” (lines 81-82) – is a way of being hearably extreme, i.e. as Edwards (2000:369) puts it, “offered and received as something other than an accountably accurate proposal about the world” (p. 369). In using this metaphor, the participant puts forward a version of doing research in conditions of great uncertainty, in which he

²⁹ In analogy to the notion of “x-treme sport”. Extreme sports are defined in relation to “mainstream sports” as new and innovative activities and which are often technically difficult and challenging (e.g. snowboarding, sky diving or bungee jumping). These involve competition between athletes, but more frequently they are concerned with the athlete’s testing of his/her own limitations, i.e. going faster, higher, more danger etc. The description of doing science in parapsychology that PA 6 provides, as it will be possible to see later, involves some of these characteristics.

“looks for the answer” (line 82, in bold) despite it being “very difficult” (line 84, in bold). PA 6’s positioning as a researcher who operates under extreme conditions is similar to the construction of the “heroic stance” explored in Chapter 4. The achievement is also similar, i.e. the translation of limitations into difficulties, which not only safeguard but also build up the competence of the researcher.

In summary, the exploration of this extract focused on the different ways in which the participant constructed a version of his research work as doing extreme empiricism, that is, where the primacy of data and experimentation were maximised, and the presence of assumptions, models or theory was mitigated. The construction of this version of extreme and neutral empiricism was achieved through the use of a series of extreme case formulations, either in describing his research option, or in constructing a personal account for his investment in data driven research. With the next extract, the analysis will focus on another version of extreme empiricism, this time focusing on the ways in which, in talking about “models and theories”, these were constituted into intensely problematic topics.

6.1.4 Designing “experimental and theoretical models” as a problematic topic.

Extract 2 – PA 17, lines 21-61.

21. C Ehm, (.) thinking about the whole thing or sort of selecting one area if you want
22. to (s- select) remote staring, [if that’s kind of the biggest chunk.
23. PA [Yeah.
24. C What types of experimental and theoretical models would you say underlie what
25. you do?
26. PA (2) Ehm, [theoretical models...
27. C [or or assumptions. I mean I’m I’m using [models very loosely, ehm.
28. PA [Yeah, yeah.
29. PA Ehm, not quite understand what the question means. What do what do what do
30. you mean?
31. C [1] **Basically** wha- what do you start out from? [2] I mean what are the ideas that
32. you start out from? =
33. PA = Ok =
34. C = [3] Either experimentally or theoretically as well.
35. [4] [What’s that phenomena is?
36. PA [Right.
37. C [5] How to investigate it? So...

38. PA I guess (.) **I don't really have too many of them.** I mean th- th- you have this
39. **basic notion that two people can communicate when they shouldn't be able to**
40. **in the sense that you cut off all normal sources of of channels, =**
41. C hmhm.
42. PA = ehm, and **that's pretty much it. That's THAT'S IT.**
43. C Hmhm.
44. PA And then a lot of my stuff is, say with the the remote staring stuff, is about
45. replicating other people's. So, William Broad who came up pretty much
46. [with that that that that model, eh that sort of experimental =
47. C [Yeah.
48. PA = procedure, **no doubt he's got all sorts of ideas** about why he's doing what
49. he's [doing. **When it comes to me, I don't. I just** sort of take that and go =
50. C [Hmhm.
51. PA = "**ooh, fair enough, I'll try to replicate that, I'll try and have a go at that**".=
52. C = Right.
53. PA And we try and do it as carefully as possible in terms of following what he's he's
54. done broadly speaking, as it were ((smiley voice)). Ehm, but eh I wouldn't have
55. models that I'm (.) sort of trying to get at [it doesn't quite work like that.
56. C [Hmhm.
57. PA = I think that's partly coming from a sort of skeptical perspective, if you believe
58. **then you're gonna have ideas about how this stuff works, and you're gonna**
59. **try and build that into the experiment. If you don't the best you can do is**
60. **really just try and copy what somebody else has done, to see if it works for**
61. **you.**

Extract 2 presents PA 17's version of his research work in parapsychology, in which the straightforward simplicity of neutral experimentation is constructed. This extract's analysis will be particularly concerned with the way in which PA 17 achieved and warranted a version of his doing research as extreme empiricism using the following discursive actions: a) constituting "experimental and theoretical models" into an intensely problematic topic in the interview; b) limiting the influence of "ideas" in the account of his research practices; c) making a claim to naïve disinterested experimentation as the simple task of "replicating others' work"; and d) appealing to "scepticism" towards *psi* as a guarantee of this disinterestedness. These four actions will be explored sequentially. The following analysis will, for the first time in this thesis, include the exploration of both PA 17's and C's (the interviewer) conversational turns. This type of analysis is particularly relevant here as PA 17 constructs theories or models as problematic topics, delaying a response to a request to talk about such topics, and designing his response to this request so that the topic itself (experimental and

theoretical models) is transformed into an intrinsically problematic topic. Simultaneously, the analysis will also pay close attention to C, who, in turn, actively pursues a response from PA 17 in relation to such topics. The analysis of the organization of interaction during this segment of talk (namely the pairing of the delay, and the eventual negative response, and the pursuit of a response) is particularly important for understanding the discursive actions being performed here.

The analysis of this first sub-section focuses on how PA 17 designed talking about “experimental and theoretical models” as an intrinsically problematic topic, by both delaying his response and eventually proving what amounts to a negative reply (i.e. how the request to talk about models was responded to by saying “I don’t really have too many of them”, line 38) to C’s request to talk about such topics. The first sequence of the extract, begins with the initial question about experimental and theoretical models that underlie the participant’s research work (lines 24-25). The participant’s initial reply is delayed, and used an “ehm” as a preface to the repetition of part of C’s question. It is argued here that these are initial markers of PA 17 transformation of the topic in itself into one that is troublesome and not straightforward. The following overlapping response by the interviewer seem to orient to the trouble displayed in PA 17’s reply, and begins in overlap with the completion of the participant’s repetition of part of the initial question. In pursuing a response, the interviewer constitutes the word “models” to be the source of the difficulty, and reviews her question (Pomerantz, 1984b:159) by replacing the word “models” with “assumptions” (line 27), and by evaluating (Pomerantz, 1984b:153) her use of the word “models” as a “very loose” one (line 27). This evaluation seems to work as a way of discounting the value of her use of the word models and for the interactional problems that it created, i.e. C’s revision constructs her choice of the word “models” as a trivial choice. PA 17’s overlapped agreement with the interviewer’s revision (line 28) is followed immediately by his request for a clarification of the question. On the one hand, this questioning of the meaning of C’s request could be seen here as a possible account for the delay so far. On the other hand, this suggests that C’s review did not function as a remedy, as PA 17 displays that C’s assumption regarding the source of difficulty was not accurate. Indeed, the request for clarification that follows (line 29) suggests the problem is the question itself. While one would not expect a scientist (parapsychologist, with a background in experimental psychology) to have difficulty in understanding such a question, PA 17’s pursuit of further clarification seems to orient to the question as one which is accountable, perhaps a “loaded” question. The participant’s question is nevertheless heard by the interviewer as a clarification request, who builds up a five part list of “what she means” (lines 31-36, preceded by numbers in bold within square brackets). The preface “basically” (line 31, in bold) at the beginning of this list, it is argued, again displays that there is nothing hidden (or “agenda” like) in her question. The list of questions in itself could be understood as indicating the general class of issues she is interested in, thus, presenting its disinterested-ness.

In line 37, a response is finally given. It seems clear, then, that: a) the answer to the request to talk about “experimental and theoretical models” is constituted by this participant into a problematic request and the topic itself into a problematic one, delayed, and oriented to as one which is not self-evident, and which needs to be accounted for; and b) was given a negative reply with what arguably amounts to a “no” (“I don’t really have too many of them, line 38). The significance of this analytical point can be better understood if one considers both the context in which this action takes place – an interview about a parapsychologist’s research work – and the position that PA 17 constructed in this interview – that of a scientist developing empirical work. The construction of experimental and theoretical models as problematic and accountable topics when considered in relation to his work can be, it is argued, commonsensically analysed as a rather specific and noticeable construction of doing science. The delay, questioning of the question itself and the eventual negative reply, presents a version of doing science where experimental and theoretical models are not straightforwardly included, i.e. where these can be heard as elements with undesirable implications for the status of one’s research, namely, compromising one’s claim to neutrality.

6.1.5 Mitigating the presence and importance of “ideas”.

The second segment of analysis is concerned with the construction of the relationship between PA 17’s account of research and “theory”. From line 38 onwards, the participant builds up a response to the revised question about experimental and theoretical models. In doing so, he starts with an assessment of the presence of experimental or theoretical “ideas” in his research work (line 38). This assessment is prefaced with “I guess”, setting it out as a response that should be heard as tentative. The assessment constructs the limited presence of ideas in his research – “I don’t really have too many of them” (line 38, in bold) – thus minimizing without excluding their role. Furthermore, in referring to “ideas” by the general pronoun “them”, PA 17 makes an “oblique reference” (Wooffitt, 1992:104) to this general class of things, suggesting that there is something problematic about them. As Wooffitt (1992) has argued, “being able to name a state of affairs or an object implies having knowledge about them (...) it also suggests a commitment to the in-principle existence of the object or state of affairs so named” (p. 105). In not naming items such as ideas or experimental and theoretical models, PA 17 displays sensitivity to the personal commitment of using such labels, particularly when these topics have been constituted as dispreferred ones.

What follows is an explanation of this initial minimising of the presence and importance of ideas, as the participant puts forward one general “basic notion” (line 39, in bold) that is present in his research. This seems to function as a concession to his later version of doing research as a “naïve” experimenter. In conceding the presence of this general idea – note the use of the impersonal “you

have this basic notion (...)” (line 37, in bold) – its potential for undermining his statement about the lack of ideas is minimized, thus making this general assumption into “old news” (Potter, 1996a:130). Furthermore, in constructing the “basic” status of the notion that anomalous communication is possible, he reduces the assumptions present to a single one that is fundamental to parapsychological research (and therefore safe). With this concession, PA 17 closes the discussion of this topic with the emphatic repetition of “that’s it” (line 42, in bold). Given the context of the claim that follows, and the preceding delay in talking about such topics, it is argued that PA 17 “made a show” of this concession (Antaki and Wetherell, 1999:8) as a way of achieving a version of his research practices where “ideas” are emphatically, though reasonably, limited.

6.1.6 Constructing an account of doing simple research.

This segment of analysis focuses on PA 17’s account of his research practices as simple (i.e. straightforward, almost formulaic), thus limiting the role of “ideas” and emphasizing the disinterested nature of his research. The construction of his naivety in relation to the process of research is achieved and warranted in the next few lines. In doing so, the participant seems to orient to a notion of scientific practice as one which “is generated, and acquires its objective character, by processes of disinterested observation” (Shapin, 1979:139). This includes the reduction of his research practices to an extremely streamlined process – the straightforward, neutral and impartial replication of others’ work (lines 44-51). This simplification is achieved through a contrast between his and others’ practices. In lines 44 to 48 PA 17 puts forward an example of another researcher whose experiments he has replicated. He attributes to this researcher the authorship of that particular model and experimental procedure (lines 48-51). In line 48 (in bold), he moves on to attribute to this researcher “all sorts of ideas” about it. This formulation functions as a characterization of the other researcher’s practice as research that is “laden with ideas”, in contrast with the streamlined version of his research in constructing it as “just” (line 49, in bold) a matter of replicating, or having a “go at it” (line 51, in bold). The contiguity and contrast between his pure experimentation and other’s “ideas-laden” research further builds up his own work as thoroughly empirical.

PA 17 follows this contrast using reported speech of himself (line 50, in bold), which is effective in building up the factuality of this description of his activities (Wooffitt, 1992:161). In constructing his work as a straightforward replication of others’ work, he also builds up his position as “just” a user of others’ ideas, rather than an author. This “user” position is developed in lines 53-55, further distancing himself from this type of research and avoiding accountability for his research options. This positioning as a disinterested doer can be seen as an achievement of the construction of his research as just doing a simple practice. Like PA 6 in the previous extract, PA 17 presents himself as a

neutral experimenter in the business of following a formula in a disinterested way. In this sense, when he does science, he is doing methodology.

6.1.7 Orienting to “scepticism” as a guarantor of neutrality.

This segment of analysis is concerned with the last sequence of the extract, where the participant provides a warranting for the description of his research practices above. This warranting appeals to an internal state of scepticism towards *psi* (lines 57-61). As discussed in Chapters 2 and 4, this is a common feature in the construction of the community of parapsychologists. The labels of “skeptic” and “believer” seem to constitute a set of discursive resources that, on the one hand, reify and maintain the “factuality” of these attributions of internal states of belief, and, on the other, provide a rhetorical resource for making sense of the lack of consistency in experimental outcomes. The participant warrants his version of disinterested experimentation by linking belief in *psi* to “having ideas about how this stuff works” (line 58, in bold), and thus scepticism about *psi* to disinterestedness, i.e. being forced to “copy what somebody else has done, to see if it works for you” (lines 60-61, in bold). The formulation of this contrast achieves an assessment of what he does as a reasonable “default” position – “if you don’t the best you can do is really (...)” (line 58). This modest assessment of his research (which is in line with standard scientific ideas about the experimenter being as neutral as possible) makes available, not only the reasonableness of his position, but also its strict empirical quality.

In summary, the exploration of this extract pointed up four discursive actions that sequentially allowed PA 17 to build up a version of his research work as an extreme case of doing empiricism. It is interesting to note that, in contrast to PA 6’s extract 1, PA 17’s account achieved this extremity by presenting a streamlined version of experimental research as a matter of “just doing” research, i.e. without ideas or thoughts about it. The problematization and limiting of theoretical elements of his research work constructed a description of research that is as extreme as PA 6’s in its claim to neutral empiricism. The next section will be concerned with how some participants hearably constructed their researcher identity as “bottom-up” researchers. Necessarily related to constructions of extreme empiricism in the last two extracts, the following analysis will focus on how some participants included this identity work at the beginning of their accounts as a way of signalling how these accounts of research practices should be heard.

6.2 Constituting the researcher's identity as a "bottom-up kind of person".

This section focuses on how participants, in describing their research practices, constituted and positioned themselves within a category of "bottom-up kind of researcher". This will be analysed as an interactional resource (Wooffitt, 1992:71) through which participants actively signalled how their accounts should be heard, making use of a culturally available way of describing overall approaches to data and theory. The sketching of a division between "bottom-up" and "top-down" approaches in experimental psychology and parapsychology provides a short-hand to talk about views on the relationship between fact (i.e. observational or experimental evidence) and theory (McKinlay and Potter, 1987:445). It functions as a way of making sense of differences amongst psychologists' and parapsychologists' views on the validity of inferences that can be made between observational and theoretical statements (Valentine, 1992:90). The analysis will focus on how these participants use the identity of "bottom-up researcher" as a resource to persuasively account for their specific relationship with data and theory.

From the start of their account of their research practices, PA 11 and PA 5 make relevant their identity as a "bottom-up kinda person" (Extract 3, lines 62 and 63). In asserting this identity upfront, they are, borrowing Edwards' (1998) expression from his analysis of how social identity categories can be used in discourse, making being a bottom-up researcher "the relevant thing about [them]" (Edwards, 1998:15) in this description. The analysis thus argues that the participants orient to this category of bottom-up researchers as a rhetorical resource used to construct the factuality of the priority of experimental evidence and absence of theory in their research. As Edwards (1998) puts it identities can be selected (out of a range of possibilities) in order for "speakers to perform and manage various kinds of interactionally sensitive business, including their motives and reasons for doing and saying things" (Edwards, 1998:19). Here, it is argued, PA 11 and P5 5 use this category of bottom-up researcher to directly link the description of what they do to who they are. In doing so, these participants also seem to orient to a view of science (put forward in the introduction to this chapter), which assumes a fundamental distinction between observational facts (which are primary and causal) and theoretical statements (which are revisable and dispensable) in the production of scientific knowledge (Mulkay, 1979:21). What is remarkable about these accounts is the reification of this version of scientific action and thought into a scientific identity. The analysis will focus on extract 3 and will, where relevant, consider extract 4 in parallel.

Extract 3 – PA 11, lines 54-107.

54. C [Hmhm, hmhm. Linking to that I I suppose ehm, what would be the (.) you know
55. the types of models, theories and assumptions that in a way underlies the work
56. that you do, for example in those two areas [that you just mentioned.
57. PA [Right, hm. Tha- that's a good
58. question 'cause I saw that that was on the sheet
59. [and and I'm not sure if I'm much of a theorist. =
60. C [Yeah, yeah.
61. C = Haha.
62. PA I I think probably I'm a (.) **what you'd call an empiricist, y'know, a bottom up**
63. **kinda person.**=
64. C = Haha.
65. PA Ehm (1) **and so** I'm I'm more interested in engaging with people who have
66. experiences, =
67. C = hmhm
68. PA = and then using that as a start point.
69. C Right. =
70. PA = Ehm, I'm not sure if anything I've done is **particularly** informed by models.
71. W- we: quote eh Chuck Honorton stuff quite a lot, =
72. C = Yeah.
73. PA = in terms of noise reduction and saying well, maybe the *Ganzfeld's* gonna be
74. good for these reasons, =
75. C = hmhm.
76. PA = but still in a sense that's just a means to an end because we don't manipulate
77. [(.) the conditions to see whether that model holds,=
78. C [hmhm
79. C Yeah.
80. PA = we almost take that as a given,=
81. C hmhm.
82. PA = and then look at other stuff.
83. C Right.
84. PA Ehm, when I- when I was an undergrad I took some ehm Sociology and History
85. of Science courses, and they were **really very influential on me** and, you know,
86. reading papers by people like Ron Westrum.
87. C Yeah, [yeah.
88. PA [You know, and the whole notion of asteroids not existing
89. [until members of the academic elite discovered them.

90. C [Hmhm. Hmhm.
91. C Yeah, yeah.
92. PA **And and so I'm very resistant to the idea that you might still find even here, that**
 93. some phenomena are not possible because they don't fit in with a particular
 94. theoretical framework.
95. C Yeah, yeah.
96. PA **My bottom line is if the theory doesn't fit the data, then it's the theory that's**
 97. **gotta go and not the data.**
98. C Hmhm, hmhm.
99. PA And that's where science **should be** according to kind of a Popperian approach,
 100. but it's- it doesn't tend to be in practice ((smiley voice)).
101. C Yeah, yeah.=
102. PA = Is mu- is much more if the data don't fit, well we'll try and (.) shuffle out (to)
 103. the side somewhere.
104. C Hmhm, hmhm.=
105. PA = So, I- I try and avoid that by **starting with data** and then (.) kind of
 106. extrapolating from there.
107. C Hmhm, hmhm.

Extract 4 – PA 5, lines 137-146.

137. C Ahm, (.) based on (.) on what you just described,=
 138. PA Hm.
139. C = what are, and eh you already talked a bit about this, what are sort of the m-, the
 140. main assumptions, or the main models that you're working with?
141. PA [.hhh
 142. C [You already talked a wee bit, but...
143. PA I am (.) **very much a bottom (.), bottom up, not a top down person.** Ehm, I
 144. adopt *psi* as a **working hypothesis, if you want** to consider that as an assumption.
145. C Hmhm, hmhm
146. PA But I don't think we know much about what's going on yet.

6.2.1 Constituting a “bottom-up kinda person” identity.

As with the extracts in the previous section, these extracts start with a question relating to “models, theories and assumptions”. In extract 3, PA 11’s reply begins with an insertion (lines 57 and 58) in which he assesses the quality of the question. This positive assessment of the question seems to function as a preface to a negative response to the interviewer’s request. In the ensuing segment, making use of the comment about “not being much of a theorist” (line 59, in bold), the participant begins to reject the request that was made in the interviewer’s question. This initial reply is effective in managing the rejection in two ways. First, the initial positive assessment of the question could be a way of balancing the later refusal to answer it. Second, in making relevant the category of “theorist” and its entitlements – being able to talk about models, theories and assumptions – and his softened exclusion from it (line 59, in bold), PA 11 constructs a position that does not allow him to talk about such issues, even if he would like to. In this way, not talking about theory is constructed into a matter of a lack of ability, rather than a matter of refusing to do so.

In excluding himself from the category of “theorist”, PA 11 builds up a context for the account of his identity as a “bottom-up researcher”. Following the construction of “who is not” above, PA 11 puts forward in his next turn its corollary – who he is. He again invokes, in a softened way, the identity of “empiricist” (line 62, in bold) and of “bottom-up kinda person” (lines 62 and 63, in bold). The use of “I’m a (.) what you’d call (...)” (line, 62), makes relevant that these are two pre-existent possible descriptions of people, and that they can be used without an explanation of what they mean. In appealing to these identities, PA 11 makes use of an available short-hand not only to describe his view of the primacy of facts over theory, but also to imply his entitlement to the scientific benefits of being an “empiricist” or “bottom-up person” (lines 62-63). In putting these identities upfront, PA 11 thus ensures these can be used as markers of how to hear the account of his research practices (Hutchby and Wooffitt, 1998:214). Similarly, in extract 4, PA 5 makes available, in her reply to a similar question, both the kind of “person” she is and the kind she is not (line 143, in bold). Thus, she seems to orient to the same short-hand as she constructs her identity as a “bottom-up” researcher, and explicitly distances herself from the “top-down” kind (line 143, in bold). In doing so, she presents a clear version of who she is as a “bottom-up” person, which, both, functions as a way of building up the factuality of this self attributed identity, and suggests her investment in being clearly recognised as this kind of researcher.

Returning to Extract 3, PA 11 follows the construction of his identity as a bottom-up researcher with a causal link between this identity – using the continuity particle “and so” (line 65, in bold) – and the description of his research interests (line 65 and 66) and methods (line 68). The construction of his identity is thus made into what determines his interest in empirical questions rather than theoretical

ones. The construction of this link allows PA 11 to present his research choices as being essentially in accordance with an overall disposition in doing research, and one that is in line with common ideas of what are valid scientific practices. This is a persuasive way of PA 11 building up the “priority of facts” in his work as an enduring characteristic of who he is, rather than of what he does. Finally, in terms of the dispreferred response (referred to at the beginning of this sub-section), the construction of this identity seems to “close” the sensitive interactional business of “not answering” the initial question. In making relevant that he is a “bottom-up kind of person”, PA 11 is able to warrant (even if obliquely) why he declined to talk about theory, models and assumptions.

6.2.2 Managing the presence of assumptions – making concessions.

Participants in both extracts followed the upfront construction of their identity as “bottom-up” researchers with instances where they managed the presence of assumptions in their work. In extract 4 (lines 143 to 146), PA 5 puts forward the adoption of *psi* as a “working hypothesis” as a possible exception to her previously constructed bottom-up approach. However, what could be seen as a potentially undermining argument against her claim to be “very much a bottom-up person” (line 143, in bold), it is actually used to support the claim. The point that is put forward has the format of a conceded point (line 144, in bold), but can be seen as a hearable concession, i.e. a rhetorical device designed to reinforce the reasonableness and persuasiveness of the first rather extreme, and thus brittle, claim. Similar to what PA 17 did in extract 2 (who reduced the assumptions present to a single and indisputable assumption), PA 5 concedes to having one the fundamental theoretical assumption that *psi* is a possibility. Moreover, looking at the form of this concession – “if you want to consider (...)” (line 144, in bold) – PA 5 seems to orient to its “value” as a concession as being dependent upon the listener, rather than one that is universal. This can be seen as a way of making use of a concession format to display her willingness to examine the limitations of her claim.

Turning now to Extract 3, PA 11 also makes rhetorical use of a concession, though rather differently. PA 11’s use of the concession resembles what Antaki and Wetherell (1999) called “making a show of conceding” (p. 7). The authors argue that by using a particular design to the action of conceding a counter-argument or exception to a claim or position, it is possible to strengthen the initial position. The authors suggest that this is a common feature of concessions that take the form of: an initial proposition, the concession itself and a reprise of the initial proposition, and which functions as a way of performing a concession in a hearable way. The achievement of making a show of conceding is, they argue, to “fire-proof something in the speaker’s own position, making it less liable to challenge” (Antaki and Wetherell, 1999:11). In lines 70 and 82, it is possible to identify PA 11 using this device in his self-presentation as a “bottom-up” kind of researcher.

It is first important to note that, earlier in the interview, PA 11 had described his research work as being engaged with the *Ganzfeld* experimental procedure, which derives from a particular model of *psi* communication (called the “noise reduction model”) particularly associated with Charles Honorton. As was discussed in Chapter 2, this model and the *Ganzfeld* procedure are presently extremely prominent in the experimental exploration of *psi* communication. It is in this context that PA 11 can be seen as making a show of conceding. In line 70, he begins the three part sequence by putting forward the initial proposition, as he makes a softened claim (softening elements in bold) to the lack of theoretical underpinning in his work. This softening seems to suggest that the participant orients to the initial claim as one that could, if formulated in an absolute way, be challenged. The concession itself is made in lines 71 to 74, as he refers to using “Chuck Honorton’s stuff” (line 71) and goes on to specify it as the use of the “noise reduction” (line 73) model. Despite the lack of a “concessionary marker” (Antaki and Wetherell, 1999:9) like “well” or “ok”, this still seems to function as a concessionary rhetorical move, making explicitly relevant the existence and use of the aforementioned “stuff”, including a model. The reprise of the initial proposition is introduced in line 76. It is prefaced by a contrastive element – “but” (line 76, in bold) – and downplays the importance of the model to his research by limiting its use as “just a means to an end” (line 76), and in taking it almost “as a given” (line 80). This three part sequence of proposition, concession and reprise allow PA 11 to show himself to be making a concession, whilst reinforcing the persuasiveness of the initial claim that his work is not particularly informed by theories.

6.2.3 Accounting for the primacy of “data” – the warranting of this position continues.

In the next segment, PA 11 provides another warranting sequence of the first claim to doing bottom-up research, as he builds up his authority as an informed researcher, thus signalling that his position should be heard as the result of thought and knowledge about research issues. PA 11 presents his view that data is primary and theoretical statements are secondary. In lines 84 and 89, PA 11 narrates his qualifications in the “sociology and history of science” (line 84 to 86), appealing to “instances” of learning in these areas (“courses”, line 85), which were “really very influential” (line 85, in bold) on him. With them PA 11 constructs a set of credentials to which he later appeals as a cause of his position as a researcher. It is interesting to note his use of category of people whose work he has read (line 86) and the introduction of the detail in his account of a Ron Westrum (line 86) paper³⁰. This detail seems to work as way of building up the facticity of his account of expertise in these areas

³⁰ It is possible to assume that PA 11 refers here to Ron Westrum’s (1982) “Science and social intelligence about anomalies: the case of meteorites”.

(Potter, 1996a:117), which is later appealed to in relation to the perils posed by theoretical frameworks. In line 92 (in bold), PA 11 makes available this causal link quite clearly, as he constructs a position of resistance to the idea that phenomena may be dismissed because they do not fit into existing theoretical ideas (lines 92-94). The Parapsychological Association conference (during which this interview took place) is constructed as a context where one would not expect the use of these discriminatory practices. In saying “even here” (line 92, in bold), the participant builds up both the extension of this practice, and orients to this context as one where it would be most unexpected, i.e. where theoretical primacy would be less common, but still present. This is followed, in lines 96-97 (in bold), by a maxim that derives from the former “resistance”. In building up this imperative, he makes available the status of his position in relation to theory and data as one which is: a) essential to his way of practicing science (“my bottom line is (...), line 96); b) straightforward and thought through, as it is prone to be articulated in a simple slogan-like expression; and thus c) factual in representing what he really does. This seems to be a clear articulation of the primacy of fact and dispensability of theory that Mulkay (1979:21) proposed as a characteristic of a “standard view of science” (Sheffler, 1967, cited in Mulkay, 1979:19). The articulation of this rule is backed up by a second imperative, which opens up the application of this maxim to all science and warrants the validity of his approach – this is “where science should be” (line 99, in bold). In appealing to “a Popperian approach” he is again bringing into this claim the authority and credentials that “Popper” and “knowing about Popper” achieve, and warrant his claim as not just his opinion. The last segment of the extract contrasts his approach to “shuffling data out to the side” (lines 102-103), thus building up his way of doing science as the way science should be done.

In summary, the analysis in this section concentrated on how PA 11 and PA 5 constituted, and positioned themselves within, a category of “bottom-up people”, and used this identity work as an interactional resource (Wooffitt, 1992:71) to persuasively account for their specific relationship with data and theory. The analysis also focused on the warranting of this position, and explored a feature shared by the accounts in both extracts, i.e. the use of the rhetorical properties of concessions as ways of reinforcing the initial claim. While section 6.1 showed how participants constructed doing empiricism as a fundamental characteristic of their research practices, this section showed how it was constructed as an essential characteristic of who they are. The analysis has thus shown not only the importance to parapsychologists of establishing the empirical basis of their research, but also the variety of ways in which this can be done. In the final section the analysis will focus on the related issue of the construction of neutrality in research practices,

6.3 Constructing neutrality.

This section will focus on how participants described the neutrality of their research, orienting to scientific research as the neutral representation of facts independent of factors such as personal ideas, emotional involvement or self-interest (Mulkay, 1979:20). These constructions allowed participants to show themselves to be independent of the production or interpretation of their research outcomes. This independence making business seems to orient particularly to the constructed role of researchers' belief in relation to the *psi* claim. Belief in *psi*, as was discussed in Chapter 2, is a ubiquitous theme in both the parapsychological literature and informal settings of discourse, and can be thought of as a symptom of the controversial status of the *psi* claim. It has also been constituted as a central variable in the explanation of parapsychological outcomes. The demarcation between sceptics (i.e. opponents or disbelievers in the *psi* claim) and believers (i.e. proponents of the *psi* claim) is currently used as a short hand for categories of researchers within parapsychology, and such categories have been employed as an explanation for (respectively, negative and positive) results in experiments.

The construction of experimental work is commonly concerned with the business of neutrality and, as Gilbert and Mulkay (1984) put forward, "organized in a manner which denies its character as an interpretative product and which denies that the author's actions were relevant to its content" (p. 56). These authors argue that descriptions of scientific practices are characterised by a set of grammatical, stylistic and lexical resources which achieve versions of scientific action and belief as neutral mediums through which "empirical phenomena make themselves evident" (Gilbert and Mulkay, 1984:56). These parapsychologists' accounts are therefore remarkable in making relevant, and directly warranting, the influence of personal factors in relation to experimental outcomes and their interpretation. Neutrality, rather than being constituted in the organization of their talk, is made explicitly relevant and, above all, explicitly accountable. In a sense, these accounts of neutrality seem to attempt to resolve a conflict between the acknowledgement of this "belief talk" (within the context of their research discipline) and the expected impersonal nature of scientific work. The explicit declaration of neutrality seems to function as a way of neutralizing the influence of individual characteristics (such as the internal state of belief in *psi*) that "might distort the researchers' perception of the external world" (Mulkay, 1979:20), and thus the validity of their claims. In being directly engaged in the business of accounting for a transparent position based upon beliefs, preferences or claims, these parapsychologists both orient to, and manage to mitigate, its undermining potential. The section will focus on the analysis of two extracts in which participants PA 12 and 15 engaged in explicit declaration and assessment of their neutrality.

Extract 5 – PA 12, lines 24-64

24. C What would you say and, I mean, you talked about it already,
25. [ehm, are sort of the =
26. PA [Yeah.
27. C = main e:hm (.) theoretical, experimental models that sort of underlie your work?
28. PA **E:hm, (.) I don't know what you mean by model...**
29. C Ah (.) f- for instance, theories assumptions, I mean you talked about personality
30. and probably [those are, are things that you are working with.
31. PA [Yeah
32. PA **Ehm (1) I don't know. Wha- with regard to *psi*?**
33. C Well, [with regard to what you do.
34. PA [With regard...
35. C = I mean, y- y-you know you said, ok, "why do these people have
36. [more experiences?", therefore you know you're already saying, ok,=
37. PA [Hm.
38. C = "I'm looking at them through a particular" =
39. PA = **Ok, ehm, [I think I was looking at it as neutrally as I could.**
40. C = [(angle)...
41. C Hmhm.
42. PA I think I was I was saying (.) **there is an experience and there are numerous**
43. **explanations for this experience** and then this, an experience is related to belief,
44. and that was the thing its gonna bring it (round in) a whole circle. **But I was**
45. **trying to look at ehm (.) *psi* from all levels at the same time.**
46. C Hmhm.
47. PA I (should not) say, ehm ok there's an explanation, **I was trying to be ehm (.)**
48. **equal in my expectations for the reasons.** So I was I spent a long time talking
49. about ehm (.) [1] how reality and imagination confusions **might** relate to *psi*
50. experience, =
51. C Yeah.
52. PA = and that **might that might** relate because ehm they are hallucinating and
53. they're not real experiences.
54. C Hmhm.
55. PA **[2] Or it might be that the confusion is actually conducive to genuine *psi*,=**
56. C Hmhm.
57. PA = ehm or **[3] something else or a combination of both. So I think I was trying to**
58. **look at it (.) and I think I probably was looking at it quite equally.**
59. C Right.

60. PA **And I I wanted to find *psi* but I was, ehm, I didn't mind if I found *psi* or not.**
61. C Yeah, yeah.
62. PA I didn't mind **at all**, and eh (.) I didn't find *psi* [(laughs)].
63. C [(laughs)]
64. PA = No, but it doesn't, it doesn't ehm effect my interest in it.

In this extract, PA 12 engages in a declaration and assessment of her neutrality in relation to the interpretation of her outcomes. I will focus of three notable features of this construction: a) the participant's orientation to models and theories as intensely problematic topics; b) the ensuing claim to, and account of, her neutrality, and c) the upshot of these claims to neutrality being her impartiality about the empirical verification of the *psi* hypothesis. In a similar way to the analysis of PA 17's Extract 2 (section 6.1.4), the analysis of this extract will include the exploration of both PA 12's and C's turns. The analysis of the organisation of the pairing between PA 12's design of the topics as problematic ones and C's pursuit of a response, is again particularly relevant here.

6.3.1 Constructing, again, "models" into a problematic topic.

In the opening sequence of this extract (lines 24-38), and similarly to the sequence between PA 17 and C in Extract 2 of this chapter, PA 12's response to the interviewer's request to talk about "theoretical, experimental models" (line 27) is designed in a way that constitutes this topic into an intrinsically and intensely problematic topic. In this opening sequence it is possible to observe again features of this designing of this topic into a problematic one. In line 28 (in bold), PA 12 does not provide a direct reply to C's question (line 25-27). Instead she arguably accounts for this absence, identifying the problem with the question to be in the meaning of the word "model". Thus, PA 12 seems to orient to the word "model" as one that is not obviously accessible to her or its meaning shared by her and the interviewer. This account is, in turn, heard by the interviewer as a request for a clarification (lines 29 and 30), who, in pursuing a response, offers a set of words of equivalent meaning to the troublesome "models" (Pomerantz, 1984b:153). This is followed by a specific but tentative example ("probably", line 30), making reference to PA 12's previous account of her research work, thus building up the reasonableness of the question and shared accessibility of the word "model". The interviewer's clarification is followed by PA 12's reply to the "clarified" question (line 32, in bold). However, the reply starts with a "ehm", a delay and, ostensibly, a declaration of lack of knowledge about the topic of the question. This reply and its "I don't know" can be observed here as an interesting features of this delay. Edwards (1995) showed how the expression "I don't know" has important interpersonal functions. The author observed how a speaker engaged in a description of his partner as a "flirt"

(Edwards, 1995:332) used it to manage implications of exaggerated watchfulness or suspicious jealousy, and thus constitute his description of his partner's actions as a disinterested and (therefore) factual (Edwards, 1995:333). Rather than looking at these expressions as a literal declaration of a cognitive state of uncertainty or ignorance about a question or an event, he showed how these expressions can be used by speakers to "mark a lack of concern for a potentially sensitive object or topic" (Wooffitt, 2005:124), and thus handle or play-down a stake or interest in the content of a description (Edwards and Potter, 2005:246). Here, the expression "I don't know" comes after C's clarification attempt and immediately before a further request for clarification. It is argued that this is, first of all, a clear expression that the interviewer's clarification did not work as one, and thus can be seen, on the one hand, as a way of marking the problematic and not-straightforward nature of this topic, and, on the other, (closer to Edwards' sense) a way of distancing herself from having any thoughts on the matter, by again marking a lack of concern with the topic.

The second request for clarification follows, and, this time, provides a specification of the trouble with the word models (line 32, in bold) – models in relation to *psi*. This further request for clarification is acknowledged by the interviewer, and, it is interesting to see that the interviewer builds up an "explanation" for her initial question that ostensibly does not directly address the question PA 12 asked. Rather, C offers an account of PA 12's previous description of her work, which again works as a support the reasonableness of her question and the straightforwardness of the topic – this clarification amounts to saying "look, you have actually talked about this". In doing so, the interviewer makes use of the active voicing (Wooffitt, 1992:161) of PA 12's account of her research (line 35, in bold), constituting C's reproduction of what PA 12 said into what PA 12 actually said. These interactional problems in the first few segments of this extract illustrate how PA 12, as PA 17 did in Extract 2, constructed models or theories into an intensely problematic topic. As it will be possible to see in the next sub-section, this seems to support the analytical point made earlier (in relation to Extract 2) that talking about models or theories is, in itself, a problematic activity, inviting implications which compromise claims to neutrality. The forcefulness of these claims to neutrality will be examined next.

6.3.2 Making an explicit claim to neutrality.

This negotiation of what counts as a "question" or "answer" is closed in line 39, where the participant explicitly signals – "Ok, ehm (...)" (line 39, in bold) – the beginning of a reply. The reply amounts to a declaration of neutrality - "I think I was looking at it as neutrally as I could". This reply orients directly to the interviewer's earlier use of reported speech (lines 35 to 38) as a suggestion that her research is guided by a particular "angle" (line 40). In fact, the reply comes before the conclusion of

the interviewer's turn (line 40), and could be seen as a correction of the interviewer's summary of her research (thus managing the interactional business of disagreeing with the previous assessment of her research practices). PA 12's early clear-cut declaration of neutrality (line 39, in bold) is designed to set the record straight by explicitly accounting for her neutrality. The account thus builds up the robustness of her claim to neutrality, particularly when the interviewer's previous assessment of her research could have been reasonably construed as an unsympathetic hearing (Pomerantz, 1986:221) of that claim.

The construction of her position as a neutral researcher continues to be the focal point of the next segment of analysis. The listing of these various instances of neutrality constitute being neutral into a regular feature of her research. Her role as a researcher is described as a response to the construed imperative of being transparent, balanced and impartial, yet this remains unwarranted, i.e. the benefits of acting in this way are not put forward or justified. The participant's pursuit of neutrality is therefore presented as a demand that "just is" imposed upon her. In the next few segments of analysis, I will explore three instances in which PA 12 makes a direct assessment of her actions in relation to this imperative, making relevant her attempts to comply with it.

In lines 42 to 45, PA 12 provides a description of her research practices in which she describes her view of the relationship between her research outcomes and the various available explanations for them. In making relevant these various possible correspondences between undisputed fact and speculative explanation, the participant seems to be constructing her position as an "open-minded researcher". Despite putting forward a possible relationship between *psi* experiences and belief in *psi*, the explanation is immediately accounted for in lines 44-45 (in bold). This account, although retrospectively correcting the meaning of the hypothesis of relationship between belief and *psi* into one of the possible ways in which she was looking at *psi*, seems to function here as an almost retrospective disclaimer [i.e. an inverted form of a disclaimer, in the sense that Hewitt and Stokes (1975) described]. This account both signals the problematic nature of the above hypothesis, as it logically disrupts her position of neutrality in this social interaction, and allows her to align herself back with it, preserving the claim to a completely neutral stance in relation to *psi*. The extreme way in which PA 12 puts forward her efforts to look at *psi* "from all levels at the same time" (line 45, in bold) is also effective in constructing not only a robust claim to unprejudiced analysis of the phenomena, but also as a way of marking her investment in this position (Edwards, 2000:364).

The use of this extreme case formulation seems to have been treated by the participant as an insufficient persuasive resource as she goes on to a second instance of neutrality making (lines 47-48, in bold), by almost repeating the same claim. However, in this instance, it initially takes the form of an imperative on her own research – "I (should not) say (...)" – which is again left unwarranted as a self-evident criterion of good scientific practice. This is followed by a demonstration of the "balance" of

her position, as the participant lists a set of possible relationships between various phenomena that she addressed in her work (the three components of this list are identified in the extract with numbers in bold square brackets). The use of a three part list has been identified as a rhetorical device that “allows the speaker to indicate that all three parts of the list exemplify some common feature” (Jefferson, 1991, cited in McKinaly and Dunnett, 1998:38). It is thus argued that this list allows PA 12 to build up the general and systematic character of her unbiased treatment of *psi* phenomena and explanations, and to hearably index a general feature of her research practice. The tentative way in which these possible ways of explaining the phenomena were considered in her work – note the repetition of “might” (lines 49, 52 and 55, in bold) – also helps to construct these suggestions as temporary, uncommitted ideas or hypotheses that she could easily let go of. This list is completed in lines 57 and 58, when PA 12 provides an upshot of it, i.e. that she treats all possible hypotheses equally. This third self-assessment of her compliance with the imperative of neutrality, together with the previous two, orients to the role of the researcher as a neutral medium through which the truth (or explanations of empirical phenomena) make themselves evident (Gilbert and Mulkay, 1984:56).

6.3.3 Orienting to the undermining power of the investment in *psi*.

In the last segment of this extract, PA 12 seems to turn this neutrality-making work to the construction of a personal position in relation to the *psi* hypothesis. As discussed above, the parapsychology literature attributes a fundamental agency to researchers’ “investment” or “belief” in the *psi* claim. In line 60 (in bold) – “**and I wanted to find *psi* (...)**” – the participant explicitly links her account of neutral exploration of her data to an account of her position in relation to the *psi* hypothesis. PA 12 had, until then, been engaged in the business of accounting for her transparent position in relation to explanations for her data, but here she naturally switches to a direct account of her “investment” in the *psi* hypothesis. This is, in turn, oriented to as compromising the validity of her work. This seamless switch seems to index both the relevance and the topicality of her investment in “finding *psi*” to the discussion. It seems that the construction of neutrality that was repeatedly built up beforehand is being treated here as unfinished work, and this declaration of “wanting to find *psi*” is added on, unaccounted for, as something that has to be made clear before the topic is closed.

The potentially compromising statement of wanting to find *psi* (line 60, in bold) is promptly, using Potter’s term, inoculated against by another construction of impartiality (line 60, in bold). In line 62, this initial direct account of a personal motivation is hearably oriented to as an undermining element to her claim of neutral research, and as she goes on to say “I didn’t mind **at all**” (line 62, in bold), PA 12 makes relevant the factuality of not having achieved her goal and the complete (note the “**at all**” above) lack of negative feelings about this outcome (thus inoculating the pr assessment of her research

as involving some sort of stake or quest). Given that this segment started with a question about “theoretical, experimental models” (line 27), which was followed by a lengthy account of her neutrality, this final inoculation against her desire “to find *psi*” constitutes an interesting and remarkable characteristic of PA 12’s construction of neutrality. The next extract will examine more fully the relationship between the experimenter’s neutrality and his belief or investment in the reality of the *psi* claim.

Extract 6 – PA 15, lines 591-617

591. C Following on from that,=
592. PA = Yeah =
593. C = what would you consider, this is to you [as a as a researcher. What would you=
594. PA [Yeah.
595. C = consider a successful outcome?
596. PA Ooh -hat’s a goo- ehm it depends, I mean, **I actually (1) don’t mind if there**
597. **isn’t such a thing as *psi*.** [Doesn’t really bother me either way.
598. C [Hmhm.
599. PA = **I mean, that might sound, you might not believe me, but actually I don’t**
600. **really care.** [What I mind is not knowing =
601. C [Hmhm.
602. PA = so people [making claims about [things, you know like (*psi*) healing =
603. C [Yeah [Yeah.
604. PA = and things and-. I I don’t care sort of, **I’m not gonna sit there and say I**
605. **believe or I disbelieve.** I mean I’m- eh I-. If you read Broughton’s textbook and
606. he says “well I don’t say I believe or a disbelieve, y’ know, I’m a scientist” and I
607. sort of, I go by that **I think that actually personally I am a scientist I don’t**
608. **invest my emotions too much in this,** ehm, and I just think I’ve come up with a
609. theory (which I’m) testing. But if I -, you know, it depends how lon- how many
610. years you do it for =
611. C Yeah, [of course.
617. PA = [and not find things.

The final extract begins with a question about PA 15’s assessment of experimental success, which explicitly requests the participant to construct categories of experimental outcomes. The analysis will explore how he disclaims the idea that experimental success is obtaining results that support the *psi* hypothesis. This construction of “success” is thus oriented to as problematic, dependent upon the

researcher's belief in *psi*, which is construed as potentially compromising his neutrality and, therefore, the validity of his research work.

6.3.4 Building up neutrality.

The construction of PA 15's claim to neutrality can be found in lines 596 and 597, as a direct reply to the question about experimental success. The construction of this reply is interesting in two ways. First, it orients to a hearing of "successful outcome" as being dependent (line 596, in bold) on an internal state of belief or interest in the *psi* claim. Second, it engages in the business of directly inoculating against his interest or investment in the reality of this claim, with an explicit declaration of impartiality in relation to it (Potter, 1996a:126). This construction seems to appeal to the idea that what constitutes a successful outcome is directly related to how much or how little you are committed to the verification of the existence of *psi* phenomena. As it was mentioned in the introduction to this sub-section, belief in *psi* is not only frequently invoked as a central issue in parapsychologists' assessments of the reality of the *psi* claim, but is also invoked as a causal variable in the possibility of obtaining results which support the reality of this claim. However, on the other hand, in any account of scientific action the assertion of investment in what ever and how much it may be is a problematic assertion, as it may compromise a construction of research which is ideally neutral and unengaged. This, it is argued, leaves parapsychologists in the uncomfortable position of, on the one hand, acknowledging empirical work that brings to the fore the beneficial role of belief and desire to verify the existence of *psi*, and, on the other, maintaining the empirical commitment to neutrality or impartiality. The question of what to a parapsychologist constitutes a successful outcome is, thus, a complicated question, in which a fine balance between the expression of desire to obtain outcomes supportive of *psi* and the empirical commitment to impartiality needs to be struck. It is this fundamental conflict that PA 15 is potentially orienting to as he constitutes the category of "successful outcome" in his research as an accountable issue, in relation to the desirable neutrality or impartiality of empirical research.

In line 596 (in bold), PA 15 builds up the factuality of this claim to neutrality, in his use of the word "actually", and later with the trivialization of the claim – "doesn't really bother me" (line 597, in bold). This initial claim to impartiality is followed by an insertion in which the participant orients to the reasonableness or credibility of this claim (lines 599-600, in bold). He puts forward a potential reaction to this claim to absolute impartiality. The anticipation of C's reaction of incredulity seems to be oriented to as a rhetorical piece. By advancing a reaction of incredulity from C, PA 12 is able to manage an actual response of disbelief from C. In dealing with this sort of "straw man response" (Ashmore, 1989, in Edwards and Potter, 1992:22), PA 15 is able to take up this self generated

challenge and, in responding to it, to manage the brittleness of his claim to neutrality and further build up its factuality. In a sense, this anticipated challenge functions also as a stake confession (Potter 1996a:130), as he rhetorically recognizes his interest in *psi* and, in the process of denying it, disarms the challenge. The factuality of this claim is further worked up when PA 15 contrasts his investment in the reality of *psi* with his investment in the validity of knowledge claims (lines 600-604). In appealing to the primacy of the latter, he constructs his disinterest, and thus, his neutrality.

6.3.5 Making “belief in *psi*” explicitly relevant.

The initial claim to impartiality is warranted in the next segment, when PA 15 makes explicitly relevant his position on the “belief in *psi*” question. In line 604 (in bold), he links the earlier statement of neutrality to a dichotomy of “believing or disbelieving” the *psi* claim, reinforcing the centrality of the believer/skeptic distinction. Having built up these fundamental categories, however, he then locates himself in neither. Given that these categories have been presented as based upon belief, this refusal is consistent with his claim to neutrality. Nevertheless, it is oriented to as an accountable action. PA 15 justifies this by appealing to the authority of a well known author in the field, making use of reported speech (line 650-606) in order to construct a direct quotation, and thus achieving the factuality of this position. He also makes particularly relevant his identity as a “scientist” and, as an entitlement of this categorization, he defines himself as someone who does not invest his “emotions too much in this [obtaining *psi* supportive outcomes]” (line 608, in bold). This identity making statement seems to provide a sufficient account of his reluctance to engage in “belief talk”. In constituting this locally relevant social category (Wooffitt, 1992:71) of “a scientist”, PA 15 appeals to the “understood” entitlement of this membership, i.e. neutrality. Thus, the troublesome issue of doing “neutral” science in a context where belief is central is managed.

Summary.

This chapter has discussed three ways in which parapsychologists constructed their research as doing empiricism: a) the construction of extreme empiricism, i.e. the primacy of data and the dispensability of theory; b) their self-presentation as “bottom-up” researchers; and c) their explicit constructions of neutrality. All of these parapsychologists constructed versions of scientific inquiry that were hearably

in line with a “standard view of science”, i.e. as an “intellectual enterprise concerned with providing an accurate account of objects, processes and relationships occurring in the world of natural phenomena” (Mulkay, 1979:20). As such, they can be seen as further accounts of how they do safe science in a less than perfect field. What is remarkable about them, however, is their extremity or forcefulness and their variety. This, it is argued, illustrates the extent to which the empirical quality of their research is oriented to by them as something that is not taken for granted, and therefore needs to be accounted for. Also, these descriptions can be seen as performances of their investment in doing science in ways that are consistent with common and normative views of what science is. The next chapter will examine how issues such as neutrality and primacy of data are negotiated when participants are asked to construct categories of *psi* and anomaly.

Constructing and making sense of *psi* and anomalies.

Introduction.

This chapter is concerned with how parapsychologists built up categories of *psi* and of anomaly, and explores these in relation to themes raised in previous chapters. Chapter 4 examined participants' constructions of evidence in parapsychology as essentially ambiguous, and of *psi* as an elusive and non-replicable research object. Chapter 5 analysed the ways in which participants constructed the scientific safety of their research work within parapsychology, by asserting its ordinariness, rigour and competence of their own research practices, and by building up a special or qualified position in relation to the field. In putting forward these accounts of scientific practice, these parapsychologists managed a paradoxical position in relation to standard accounts of scientific action: while they actively appealed to the ordinariness and rigor of their own experimental practices, they also constructed *psi* as a research object and evidence in parapsychology in ways that breach such accounts of science. Chapter 6 examined how the problem posed by these compromising elements to the sustainability of scientific statements about research (and about *psi*) is oriented to in the participants' extremity in constructing empiricism, neutrality and the priority of data. They thus achieved versions of their research as being based on neutral experimental practices, with little or no theoretical assumptions or expectations. In the context of a field that investigates a set of phenomena that are experimentally and conceptually construed as anomalies to psychological and physical observations and theories, this chapter considers how an anomalous outcome and its relationship to *psi* are constituted by these parapsychologists. Anomalies and their relationship to *psi* constituted a particular focus during the interviews with parapsychologists (see Appendix 1), during which there were two moments in which participants were specifically asked to engage in the categorization of outcomes.

The chapter will focus on four ways in which participants built up categories of *psi* and anomaly (and the relationship between them). The first is the straightforward identification between anomaly and *psi*, i.e. *psi* as a statistical anomaly. The second is the problematisation and also straightforward rejection of the identification between anomaly and *psi* (appealing to the constructions of *psi* as a difficult research object, explored in chapter 4). The third is the constitution of anomaly into an

impossible category in parapsychology (orienting to accounts of extreme empiricism, explored in chapter 6). The fourth and final construction is the distinction between *psi* and anomalous *psi* (i.e. outcomes supporting the reality of *psi* that go beyond some boundary of expectation). This last construction is particularly interesting as it appeals to a normal track record of experimental outcomes (thus reconciling the use of some form of expectations with both the constructions of extreme empiricism and the versions of *psi* as an ambiguous and elusive object, put forward in previous chapters). These four constructions of *psi* and anomaly will be looked at in sequence.

7.1 The identification between anomaly and *psi*.

The identification of *psi* as an anomaly was constituted by some parapsychologists by equating *psi* with a statistical departure from an expected performance in an experimental setting. As Chapter 2 noted, this identification was present in Palmer's definition of *psi*:

“psi is a statistically significant departure of results from those expected by chance under circumstances that mimic exchanges of information between living organisms and their environment, provided that a) proper statistical models and methods are used to evaluate the significance and b) reasonable precautions have been taken to eliminate sensory cues and other experimental artifacts” (Palmer, 1983b:54, italics in the original).

There are two significant aspects to this definition. The first is that *psi* is constructed as a statistical departure from expected results in an experimental setting – what Palmer calls “chance”. *Psi* is defined as a probability that is different from chance, though the direction is not specified, i.e. it might be either greater or less than chance. The second is that Palmer makes use of a construction of empirical control (explored in chapters 5 and 6). He appeals both to experimental rigour (as a means of ruling out non-*psi* causes of the outcome) and to statistical rigour (as a means of assuring the quality of the measurement). *Psi* is therefore built up as a probabilistic entity reliant upon a specific methodological set up that ensures that a given outcome is indeed *psi*. The definition thus provides *psi* with both probabilistic and methodological properties. In the next three extracts, the straightforward identification between *psi* and anomaly is achieved in a way that is consistent with this definition – *psi* is identified as the anomaly in parapsychological research precisely because *psi* is defined as a statistical deviance from chance. The categories are thus constituted and presented by these participants as thoroughly empirical, based upon data, and free from assumptions regarding the direction or magnitude of the deviation.

Extract 1 – PA 17, lines 113-119.

113. C Now, in your experience of using this particular set up for instance, what would
114. be, and linking now with the idea of anomalous outcome, what would be an
115. anomalous outcome within this, within this setting?
116. PA Anomalous outcome here would be very straightforward, it would be the
117. person's ehm physiologic- ehm physiological responses being higher or lower,
118. significantly higher or lower when they're being [] than when they're not
119. being []. That that would be a straight anomaly.

Extract 2 – PA 8, lines 34-68

34. C = ahm, working from those assumptions, what would be an anomalous outcome,
35. in relation to that?
36. PA Right, (I th-), an anomalous outcome, **obviously it's all in relation to the chance**
37. **e- expectations.** So, **essentially** what we're doing (we're) saying, ok, by chance
38. the person will identify the target, in this case the dream ESP and the *Ganzfeld*,
39. 25% of the time, if nothing is going on. And the idea was to see whether, ah, (I
40. mean) **essentially** in our research we're looking for *psi* hitting. So, I mean, eh, for
41. me ESP is *psi* hitting. I know some other researchers would see ESP as being *psi*
42. hitting or missing. For me *psi* missing doesn't s-, doesn't seem to make so as much
43. sense. So, for me, I work on (research) I'm looking for *psi* hitting.
44. C Yeah.
45. PA Ehm, so basically we're looking for people to identify the target, overall, more
46. that what you'd expect by chance.
47. C Hmhm.
48. PA And, you know, following on from that (.) the explanation for the anomaly, or that
49. deviation from chance expectation, is that ESP is operating.
50. C [Alright.
51. PA [But again, some would say you can't necessarily go that far. But, the way you
52. design an experiment, is is to to kinda pick up on ESP. So (1) I would say, yeah,
53. maybe **it's suggestive of ESP**, but there's always a small amount is n- in there
54. might be an alternative.
55. C Right, wha- what sort of an alternative (would) be? Just, just
56. [to (explore that a bit)
57. PA [Well, it can all- it can always be a **fluke**.
58. C Yeah.

59. PA Ahm, any set of results no matter how [significant can always be a fluke,
60. C [Hmhm.
61. PA = it's just a question of how unlikely is it,=
62. C Yeah.
63. PA = you know, and I think **it would be wrong** a this stage to say **I'm 100% sure**
64. that this is ESP, [because there **there could always be** an alternative explanation=
65. C [Hmhm.
66. PA = could be some, kind of flaw that (you) don't know about. **So I'm always wary**
67. **of saying anything is definite. [In fact I most scientists are ((smiley voice))**
68. C [Yeah

Extract 3 – PA 10, lines 266-301.

266. C Eh what would it be in relation to that type of paradigm, that type of- your
267. understanding of of of how this process works, what would be an anomalous
268. outcome, in relation to them?
269. PA (1) Well anomalous outcome is basically anything that e.h is statistically
270. significant, in other words you know doesn't fit (.) the random model, under eh
271. conditions that ah, (there in a way) might be another way to put that, that was
272. information there. Eh (.) that's either being received or sent. A:h (1) a:nd e:h you
273. have ruled out conventional mechanisms that would produce that.
274. C Yeah, yeah.
275. PA **So, of course it's it's it's negatively defined**, (certainly), **but that's the way it**
276. **is.**
277. C [((sniggers)) Yeah ((laughing voice)) ((laughs))
278. PA [You know, **it's not good**, **but that's what it is**. So ehm and it's of course is the
279. purpose of doing theoretical oriented research is to try to make that a positive
280. [explanation] instead of a negative and (it's one that) we're not there yet, so ()
281. defined, and eh in some ways, although I've never really done this, nobody else
282. has ever done it, the probably-, what is you probably do in a *psi* task is eh (1) is
283. **basically simply** do a goodness of fit test to that distribution,=
284. C Hmhm.
285. PA = (.) **if it doesn't fit, you know it's *psi***, which may manifest as hitting, and it has
286. its variance, and it has displacement (), but I think any of those, eh assuming
287. you've controlled for (the mode of) analysis and all that, but assuming that you've
288. solved that kind of problem, then **any significant distortion** you get from that
289. normal distribution under the proper kinds of controls against artifactual (.) means

290. of means of communication is *psi*, is the anomaly that we're interested in, =
291. C Yeah.
292. PA = and **of course you're talking about a particular kind of anomaly**
293. [and again one of the complaints about that term is, =
294. C [Yes.
295. PA = well anything can be anomalies, is a red herring, cause **obviously we're**
296. **talking about a certain class of anomalies** eh, that basically mimic
297. communication processes.
298. C Right, right, right. And so that distinction is sort of clearly made then? (.)
299. Between...
300. PA Well I mean I know what I mean by the term, [yeah ()
301. C [Yeah, yeah, exactly, yeah.

7.1.1 Constructing *psi* outcomes as an anomaly.

Extract 1 illustrates the straightforwardness of this construction of *psi* as an anomaly. In this extract, when asked directly about what would be an anomalous outcome within the setting of his research work (lines 113-115), PA 17 constructs his reply as an obvious, “very straightforward” (line 116, in bold) matter. Indeed, this theme can be found across all three extracts. In this case, the anomalous outcome is built up to be one that supports the possibility of remote staring being detected autonomically, represented by the “person’s physiological responses” (line 117). The appeal to the statistical character of anomalous outcome is clear when PA 17 qualifies the representation of this detection – and therefore of *psi* – as a statistically “significant” (line 118, in bold) outcome. In making the link between outcomes that are anomalous and those that support the reality of a specific *psi* phenomenon, the construction of this link as an obvious one shows just how readily available it is. PA 17’s reply appears to orient to this identification as a consensual “fact” in parapsychology, something that is known to be the case and, therefore, warranted.

In the second extract, PA 8 provides a similar, though more extensively warranted, construction. The construction of *psi* as the anomaly is again made, by appealing to a comparison with “chance expectations” (lines 36-37, in bold). This construction is interesting as it unpacks the identification between *psi* and anomaly through the use of a statistical criterion in a similar way to that proposed by Palmer’s definition. In lines 36 to 37, PA 8 provides a description of the way in which an anomalous outcome is constructed, i.e. in comparison with “chance expectations”. In doing so, he constructs, as did PA 17 above, the straightforwardness and obviousness (line 36, in bold) of this definition. The obviousness of what constitutes an anomaly allows PA 8 to go beyond the fact making properties of

this formulation, and build up this category as one which exists “out-there”. In unpacking this definition (in lines 37 to 41) the participant outlines what he means by “chance” and what the deviation from it should be. In this explanation, he again makes use of markers of the normative way of hearing this definition. In using “essentially” (lines 37 and 40, in bold), PA 8 signals that these definitions are to be taken just so, as “definitions”. On the one hand, chance is explained as corresponding to the expected outcome in an experimental situation if “nothing is going on” (line 39). On the other hand, the deviation is explained as corresponding to “*psi* hitting”, i.e. a particular type of deviation from chance, one that is already identified with ESP.

This step by step definition is concluded in lines 45 and 46, when the deviation is qualified as being a positive deviation from chance. Again, this formulation seems to retain the same norm-making features of the previous steps, presenting a bottom-line definition of anomaly – note the use of “basically” (line 45). Like some of the participants in chapter 6, when describing their versions of research practices, PA 8’s construction of the simple notion of anomalous outcome and “*psi* hitting” reduces assumptions to a single one expressed here as chance or probability. The streamlined description of anomaly and its identification with *psi* is concluded in lines 48 and 49, as PA 8 introduces ESP as the “explanation for the anomaly”. Anomaly is now reduced to the idea of deviation, and ESP is introduced as a way of making sense of it, i.e. as what this anomaly means.

7.1.2 Qualifying the identification between *psi* and anomaly.

The construction of “ESP operating” as the meaning of this deviation and, therefore, of anomaly in parapsychology, is followed by a series of qualifications. Thus, the initial straightforward identification between anomaly and *psi* is increasingly disclaimed. This displayed sensitivity towards the initial identification seems to focus on the one-to-one correspondence between anomaly and *psi*, which (in lines 51 to 54) is softened through a succession of rhetorical moves which: a) first concede the problematic status of the straightforward identification (line 51); b) provide a reprise of the initial identification with its experimental warranting (lines 51-52); and c) finally returns to a softened version of the initial claim. These seem to orient to ambiguity and elusiveness as essential characteristics of *psi* (discussed in Chapter 4). The softening begins with PA 8 identifying and locating the reservations of “some” (line 51) about the correspondence between anomaly and *psi*. His footing as a narrator of these reservations allows the introduction of this near disclaimer, without risk of a potential contradiction. What follows is presented as an argument against these reservations (lines 51-52). In a sense, PA 8 is constructing two sides of a story, which culminates with the achievement of the softened version of the identification (lines 52-54), when he introduces the qualification “suggestive of ESP” (line 53, in bold) as a downgraded version of “the explanation for the anomaly

(...) is that ESP is operating” (lines 48 to 49) above. In lines 63 to 67, he further downgrades the initial assertion by describing the risk involved in an unwarranted identification of anomaly with *psi* (lines 63-66, in bold). In some of these expressions of risk – “ it would be wrong (...) to say that I’m 100% sure” (line 63), “there could always be an alternative explanation” (line 64) and ““I’m always wary of saying anything definitive” (line 66-67) – PA 8 conveys that an assertion of certainty about *psi* and anomaly would amount to jumping to a conclusion. This allows PA 8 to downgrade the assertion about the correspondence, and construct this downgrading process as an achievement of the scientific character of his research practices (lines 66-67, in bold). The location of these doubts as a feature of being a “scientist” allows PA 8 to forestall an accusation of inconsistency between the identification of anomaly as *psi* and subsequent doubts about this correspondence. In doing so, PA 8 manages this inconsistency by making it part and parcel of being “a scientist”.

7.1.3 Warranting the identification – anomaly as a definition of *psi*.

In extract 3, PA 10 makes the same identification between anomaly and *psi*. The straightforward construction of anomaly as *psi* is also warranted here by appealing to a statistical norm or baseline. The same streamlined description of anomaly and its identification with *psi* that was made above is explored in this extract in greater detail. PA 10’s version of the identification is formulated twice as a statistical construct, but in the end is qualified and restricted to a particular class of anomalies, those “that basically mimic communication processes” (line 296).

In lines 270 to 274, PA 10 constructs his version of an anomalous outcome, using the same referential as both PA 8 and Palmer. It is constructed as a rhetorically simple, streamlined comparison with a statistical norm, what PA 10 calls “the random model” (line 271). As PA 8 talked about deviation from chance, PA 10 talks about not fitting “the random model”. Thus, he appeals both to the simplicity of this demarcation (line 270, in bold) and to its meaning as an outcome supportive of the *psi* hypothesis (lines 270-271, in bold). The construction of the correspondence between anomaly and *psi* is continued (in line 276 to 278) as the negative definition of *psi* (line 276). Although constructed as straightforward and basic, this identification has been made through oblique references (Wooffitt, 1992:104) to *psi*. Thus, in “not naming” *psi*, the participant seems to orient to the term as problematic, displaying his sensitivity to a personal commitment to the label, or to the direct correspondence with anomaly proposed earlier.

Another interesting feature of PA 10’s construction of this correspondence is that it is oriented to as an unsatisfactory or challengeable definition of *psi* (lines 275-278, in bold). The concession that is made about the usefulness or status of this correspondence seems to be rhetorically effective in managing its

disputability (Antaki and Wetherell, 1999:8). By drawing attention to the “negative” and “not good” (line 278, in bold) character of the correspondence between anomaly and *psi*, PA 10 seems to attend to a troublesome feature of the definition. This seems to be fulfilling a rhetorical function that is concerned with the negotiation of this definition of *psi* as the existing one, though not the desired one. Looking back at Chapter 4, it was possible to explore that this negative definition of *psi* was oriented to as part of the current problematic status of *psi* as a research object lacking either theoretical or empirical content. The identification between anomaly and *psi* allows a content free definition of *psi*. Although this assures a desirable, empirically pure definition of *psi*, it is also (as it will be possible to examine below) one that runs the risk of being empty or too encompassing to be meaningful.

7.1.4 Focusing on conceptual simplicity.

In line 282, PA 10 continues to identify *psi* with anomaly. A noticeable feature of this second wave is how it again appeals to the straightforwardness and simplicity of the identification (line 283, in bold). PA 10 seems to work towards its factuality, its simplicity implying that it is out-there to be acknowledged. The construction of anomaly is now clearly the construction of *psi* as an anomaly, and PA 10 again makes use of a statistical construct of a “goodness of fit test” (line 283) between chance and the probability of a *psi* event. In using this kind of concept, the participant allows the “simple” construction of *psi* as an anomaly to enjoy the entitlements that come with being placed within the category of statistically based concepts. The construction of this definition of *psi* is clinched in line 285 (in bold), and made again into a simple matter, one that follows logically from the premise “it doesn’t fit” (line 285, in bold). The construction of *psi* as a simple conclusion of statistical thinking, construes it into a logical, mundane and, above all, not-storyable phenomenon (Sacks, 1984:419). The repeated use of this rhetorical resource in this extract provides the closest thing to a bottom-line definition of *psi*. It becomes an object that, despite its troublesome features (explored in Chapter 4), has an identity that can be straightforwardly assessed and, more importantly, empirically established.

7.1.5 The construction of *psi* as a “certain class of anomalies”.

The last segment of this analysis focuses on how PA 10 constructs a boundary to this bottom-line, and therefore potentially over-inclusive, definition of *psi*. In lines 288 to 299, the participant constructs a limit to what anomaly (as a definition of *psi*) includes, orienting to the definition as potentially troublesome, precisely because of its bottom-line character. In lines 288 to 290, PA 10 upgrades the construction of *psi* as anomaly, as he puts forward that “any significant distortion” (line 288, in bold)

from the normal distribution defines *psi* as an experimental phenomenon. Thus, PA 10 constructs a category boundary that has the potential to include “any” distortion, and thus confer upon any anomaly the meaning of representing *psi*. The appeal to experimental rigour, i.e. the exclusion of “artifactual means of communication” (lines 289-290), is provided initially as the only condition for the application of this definition to any anomaly. The use of this extreme formulation of inclusion seems to be oriented to by PA 10 as a troublesome matter, as in line 292 (in bold) it is repaired into a less inclusive boundary. “Any anomaly” is repaired into “a particular kind of anomaly” (line 292, in bold), which itself is constructed as obvious, almost an unnecessary distinction to make. The second feature of this repair sequence happens in lines 295-296 (in bold), when he appeals to a community of people to whom the definition of *psi* as a particular kind of anomaly is obvious. This boundary, which chooses which anomalies can be identified with *psi* [i.e. those that “mimic communication processes” (lines 296-297)], is made when PA 10 makes use of a “report” of a “complaint” (line 294) about this identification. In doing so, he brings it in as a “red herring” (line 295), i.e. as a diversion from the argument, then verbalises the problem of the definition of *psi* as any anomaly. This move allows PA 10 both to downgrade this argument into an irrelevant criticism, and to inoculate the construction of the identification between anomaly and *psi* against such a criticism. The construction of *psi* as the anomaly that “they’re looking for” is a further, but necessary, qualification of the straightforward identification with which PA 10 began his reply.

In summary, these participants constructed a straightforward identification of anomaly with *psi*. Anomalies were thus made sense of as the experimental evidence for *psi*, (indeed, PA 10 effectively worked up the equivalence between the two). Both participants constructed *psi* as a meaningful deviation from what would be expected by chance. By warranting *psi* as a statistical anomaly, participants built up its factuality and out-there-ness, defined it in relation to accepted scientific processes and, in doing so, provided it with an identity based upon this analogy. The next section, however, will explore how the straightforwardness of this identification of anomaly with *psi* was disputed by others.

7.2 “*Psi* is the anomaly” rejected – anomaly and *psi* as a troublesome match.

This second construction of anomaly contrasts directly with the straightforwardness of the identification between anomaly and *psi* above. The common feature of these participants’ replies is the presentation of a version of anomaly that orients directly to the above identification, but actively rejects or contests it. The rejection is made through disputing the coherence of the demarcating criteria

that locate *psi* in the category of anomaly. In constructing a clash between *psi* as an unexpected or anomalous outcome (in terms of “normal science”), and *psi* as an expected outcome of a parapsychology experiment, these participants warranted why *psi* is not a straightforward anomaly. This was done by responding to the question, “what would be an anomaly in your research?”, as if it were a slightly different question, “is *psi* an anomaly?”. In doing so, these constructions work towards a version of *psi* as an epistemologically tricky, elusive and ambiguous object (as explored in chapter 4).

The analysis in this section will focus on two extracts in which participants put forward responses that orient to the “alternative” question above. The analysis of extract 4 will look at the way PA 16 constructs and warrants the clash between two epistemological positions for *psi*. The first is that of *psi* as a successful outcome of an ordinary experimental process (supportive of the reality of *psi*, expected in parapsychology, and therefore not an anomaly). The second is *psi* as an outcome that is a statistical diversion, or is ontologically unexplained (and therefore an anomaly). PA 16 seems to orient both to the normalization of *psi*, constructing it as just like any other outcome (as discussed in chapter 5), and to an overlapping construction of *psi* as a deviant scientific object. It is the incompatibility between these that warrants *psi* as not a straightforward anomaly. In extract 5, PA 5 constructs a version of why *psi* is not a straightforward anomaly by attending to another type of incompatibility, i.e. the clash between the largely unknown status of *psi* and the possibility of expectations.

Extract 4 – PA 16, lines 194-229.

194. C Working from that and I suppose I eh (.) again it's it's ehm, suppose we could
 195. look at the two contexts of if if you if you want to talk more about the Ganzfled
 196. (one), [because you're now working [on it. What would be and from your =
 197. PA [Ok, [Right.
 198. C = experience of, you know, thinking about these things, what would be an
 199. anomalous outcome to this type of research?
 200. PA (1) Ehm:: **anomalous in what way?**
 201. C That's the thing, [exact- yeah ((laughs))
 202. PA [yeah, you're asking me that or is that ehm...
 203. PA .hh (.) **I mean if you're using the term anomalous** in a general se- in its more
 204. **general sense**, (1) in that (.) and you, you know, **and some people have done**
 205. **they've kind of labeled the term *psi* as being simply an anomaly, =**
 206. C Yeah.
 207. PA = then the anomalous outcome is if you do seem to get some kind of (.), eh,
 208. outcome which (.) is difficult to explain in terms of chance, =

209. C Hmhm.

210. PA = and is difficult to explain in terms of, eh, essentially non-*psi* explanations, =

211. C Hmhm.

212. PA = eh, so called conventional explanations, ehm, (.) so that cou- y'know, - . I

213. don't if it would be (pointing) to what an explanation might be, 'cause you're

214. saying well ok, that's anomalous in the sense that, y' know, ok that's the anomaly

215. that then we would try to explain. But they do nothing, y' know, about anomalous

216. in terms of what I would expect would happen [in the expe- in the experiment.

217. C [Yeah, yeah.

218. PA Because part of me is want- wanting to expect, given that, if there's something

219. real going on there, **if we have a successful experiment in much the same way**

220. **as other successful experiments have done it, =**

221. C Hmhm

222. PA = then **we should be expecting** to try to get similar effects. Which (.) -. So in a

223. sense it's still anomalous, in that we don't know what, what the explanation of

224. them is, you know, we're labeling it as *psi*, but not necessarily anomalous in

225. terms of, y' know, I'm (.) I'm kinda expecting it to happen, =

226. C Hmhm.

227. PA = given that we're trying to, y' know we're trying to seek out that outcome rather

228. than get something that's ha- happened without us (.) eh expecting it or wanting

229. it, or (something).

Extract 5 – PA 5, lines 323-491

323. C Yeah, yeah. Coming up from that, [from that type of research that you're doing,=

324. PA [Hmhm

325. PA Hmhm.

326. C = and try-, starting to link it with the idea of an anomalous outcome, what would

327. be, for example within a DMILS set up, an if you have an example of a specific ,

328. you know, a specific set up that you're working with. What would be an

329. anomalous outcome in relation to that? [You think.

330. PA £ [Now you're really not gonna like me at

331. all £ [(laughs))

332. C [(laughs)) £ Go for it! £

333. PA **I don't, I I mean anomalous, I don't know what is meant by anomalous.**

334. C [Hum.

335. PA **[In the context you're using it. Ehm, if I adopt *psi* as a working hypothesis,**

336. **then** I cannot call it anomalous, if it has actually occurred.
337. C Yeah, yeah.
338. PA So, I don't what an-, you know **if** it doesn't happen, I'm not gonna call that
339. anomalous either. I think we're dealing with a hugely complex system, we don't
340. understand the moderating variables yet, you know I'd love to (bite in), to tease
341. out a few pieces and put that, you know, that puzzle.
342. C Hmhm, hmhm.
343. PA Ahm, but I'm I'm not sure of what anomalous means **in this circumstance**.
344. C Right, right.
345. PA A:hm. So I I don't know how I would define an anomalous outcome.

7.2.1 Constructing the relationship between anomaly and *psi* as a troublesome topic.

In responding to a request for their categorization of what would be an anomalous outcome in relation to their research, both PA 16 and PA 5 construct the topic of the question – the identification of what an anomaly is in their research – as a troublesome topic. In Extract 4, the delay and the prolonged use of “Ehm:” (line 200) and, more importantly, the returning of the question back to the interviewer (line 200, all features in bold), seem to work as markers of this topic as one which is difficult, i.e. marking anomaly as a not straightforward topic. The interviewer’s response to PA 16’s returned question constitutes the return as a request for clarification, and pursues the response by reaffirming the question, yet making use of laughter, suggesting this insistence could be troublesome. As Jefferson (1984:350) pointed out, this seems to be a recurrent conversational configuration in troubles talk, in which the use of laughter after a troublesome assertion or request is not taken up as a humorous event by the recipient, who instead “produces a recognizably serious response” (Jefferson, 1984:346). In line 202, it is possible to see the insistence followed by laughter being taken up as simply an insistence. This is followed in line 203 by a prolonged exhalation, which seems to function, again, as a non-verbal performance of difficulty or reluctance to engage in this topic. Turning to Extract 5, we find that PA 5 a similar response to a similar question. In line 330, PA 5 prefaces her reply with a “warning”, not only marking but showing to be marking that her response is not going to be the desired one. In this sense, a borrowing a term from Heritage (1984:269), she warns the interviewer that what follows is not an affiliative or supportive response. This warning seems to be taken up by the interviewer who joins PA 5 in his laughter, and constructs with the participant what Jefferson calls a “buffer” sequence (1984:352). The alignment of jokes and laughter between PA 5 and C again provides a break from the troublesome preface to PA 5’s response.

This characterization of PA 16's and PA 5's initial responses contrasts strikingly with the straightforward replies discussed in the last section. The opening lines of both extracts here orient to anomaly as a troublesome topic. PA 16 (extract 4, lines 203 and 204) builds up anomaly as a less than straightforward concept (line 203, in bold) by making relevant the multiple, and potentially idiosyncratic, meanings that anomaly might have in parapsychology. Similarly, PA 5 (extract 5, lines 333 to 335, in bold) orients directly to the personal nature of the definition of anomaly that was used in C's question, and makes use of this personal meaning to construct the term anomalous as a less than straightforward term. In extract 4, PA 16 begins his reply by orienting directly to the construction of *psi* as an anomaly as a "simple" construction held by some (lines 204-205, in bold). In doing so, PA 16 seems to make available that, while this construction exists out-there in the community of parapsychological researchers, its simplicity might be misleading, given the potential multiple meanings of anomaly. In extract 5, PA 5 also orients early in her response to talking about *psi* (line 335, in bold) as a less than straightforward anomaly. These participants seem to orient to this identification as an available, albeit unsatisfactory, way of constructing *psi* and its relationship to anomaly, and above all, one that is not a consensual let alone normative way of talk about this relationship.

7.2.2 Two versions of *psi* – experimentally expected but conceptually anomalous.

In extract 4, the first objection to the identification of *psi* with anomaly is related to the construction of two versions of *psi* as a scientific object. The analysis of this incompatibility will focus on the final segment of PA 16's response (lines 218 to 229). Here we find the main claim in the participant's construction of incompatibility. PA 16 constructs, on the one hand, *psi* as a normal scientific object expected in experimental situations but, on the other hand, as an anomalous object precisely because it remains unexplained in terms of its process and nature. In lines 218 to 222, *psi* is constructed as an ordinary scientific object, i.e. in an experimental situation, *psi* "should" (line 222, in bold) be expected, as the desired or successful outcome in much the same way that other experimental processes isolate their desired or targeted outcome (lines 219-220, in bold). In doing so, PA 16 makes, borrowing again Edwards (1998:15) expression, "the relevant thing about (...)" *psi* its ordinariness as an experimental outcome and orients to it as a rhetorical resource in order to construct the incompatibility between this ordinarily expected outcome and the concept of anomaly (lines 224-225). The use of the formulation "not necessarily anomalous" (line 224, in bold) allows PA 16 to make available the reasonableness of this argument, presented as a possible point of view rather than as a normative account of what *psi* is. This reasonableness allows PA 16 to downplay the particularity of parapsychological phenomena, and constitute *psi* as an ordinary experimental outcome (the construction of ordinariness was already explored in Chapter 5, extract 1). However, PA 16 also

makes available another version of *psi*, one that would warrant its categorisation as an anomaly. This version is presented in lines 222 to 224, and seems to orient directly to the construction of *psi* as a conceptual anomaly (introduced earlier in lines 204-205), in that though it is expected, it is nevertheless ontologically unexplained. The presentation of this alternative version of *psi* could be seen here as an instance in which PA 16 shows to be conceding this point of view (that *psi* is an anomaly), and possibly strengthening the initial position (that it is not). According to Antaki and Wetherell (1999) such a device is associated with a three-part structure – an initial proposition or claim, the concession itself and a reprise of the initial proposition. This can be seen in extract 4, in PA 16's reprise of the claim that *psi* is an expected experimental outcome (lines 224-229) allowing the participant to strengthen the claim that there is indeed a fundamental incompatibility.

7.2.3 Constructing the incompatibility between *psi* and anomaly.

Turning now to Extract 5, PA 5 constructs the same type of objection to the identification of *psi* with anomaly, i.e. a basic incompatibility between the two. In doing so, PA 5 appeals to the construction of *psi* as an ambiguous and elusive experimental effect (explored in Chapter 5) and also to its status as an “unknown” phenomenon. In lines 335 to 339, the participant constructs in two acts a fundamental incompatibility between *psi* and anomaly. This incompatibility is constructed almost as a logical axiom, as PA 5 uses the format of a logical deduction to exclude *psi* from the category of anomaly (lines 335-336 and 338, in bold). The gist of PA 5's articulation of this incompatibility seems to be: *psi* cannot be an anomaly, as the definition of an anomaly requires the existence of knowledge or expectations about the phenomenon; since *psi* is a highly complex and largely unknown object, the possibility of its identification with an anomaly is absurd. This first claim is clearly oriented towards the warranting of why *psi* is not an anomaly. The second half of this segment allows PA 5 to warrant this incompatibility with the construction of the status of *psi* as an unknown set of phenomena. It is interesting to note that the participant presents *psi* as a “hugely complex system” (line 339), yet makes clear her dedication to the investigation of this largely complex and unknown system of variables. In doing so, PA 5 makes available a construction of her work as a scientist who, undeterred by such difficulties, is willing to change the status of *psi* into a known phenomenon (this is similar to what was explored in Chapter 6 as the construction of “doing extreme science”). Finally, the last segment of analysis of this extract is concerned with lines 343 and 345, as PA 5 offers a gist of her response to the initial question. In orienting back to the original question, the participant seems to use these instances to preserve the central function of the previous talk constructing the difficulty of defining what an anomaly is in the context of the largely unknown *psi* (line 343, in bold).

In summary, this section examined how these participants oriented to the question about anomalous outcomes as one that asked whether *psi* was an anomaly. In doing so, they oriented to the identification of anomaly with *psi* as an existing, but problematic, definition of *psi*. Having constructed *psi* as elusive and largely unknown (as discussed in chapter 4), PA 5 then builds up the incompatibility between an anomaly (which depends upon expectations) and *psi* (as an unknown phenomenon). PA 16 builds up the incompatibility between an (unexpected) anomaly and *psi* (as an expected outcome in an experiment) by orienting to *psi* as a normal experimental outcome (as discussed in chapter 5). The next section will examine how the idea of anomaly was itself constructed as a troublesome category of outcomes.

7.3 Anomaly as a troublesome category.

This section will focus on how participants constructed the category of anomaly itself as a dispreferred one. It will explore how “anomaly” was built up into a meaningless category in parapsychology, by orienting to a constructed lack of expectations (in line with the versions of extreme empiricism analysed in Chapter 6), and to a version of research in parapsychology that is concerned with doing ordinary science (discussed in Chapter 5). The section includes the analysis of two extracts in which anomaly is constructed as a dispreferred category of outcomes. In extract 6, the analysis will explore how PA 6 problematized the category of anomaly, constructing his research as an extreme case of doing empiricism (in line with his version of research practices analysed in Chapter 6). In this extract, the participant puts forward a version of anomaly that is dependent upon the existence of expectations, and thus is problematic in relation to the notion of extreme empiricism. In extract 7, the analysis will address how PA 1 constructs the meaning of anomaly in parapsychology as implying an assumption of paranormality. The troublesome status of this category is built up by appealing to the troublesome status of “paranormality”, which in turn orients to his version of doing research in parapsychology as doing ordinary science (as explored in Chapter 5).

7.3.1 The construction of anomaly as a meaningless category of outcomes.

Extract 6 – PA 6, lines 112-140

112. C Yeah, yeah, ok. And considering that, considering y- that particular view, that
113. particular perspective, what would be, in your view then, using-, because of you

114. using these assumptions in this method, what would be an anomalous or
 115. unexpected outcome?
116. PA **There is no unexpected outcome, because I don't care.**
117. C Yeah.
118. PA **((laughs)) So, [because I don't care, =**
 119. C [Yeah.
120. C Hmhm.
121. PA **= you know, every outcome is fine with me.**
122. C Ok.
123. PA Eh, anomalous, an anomaly is in- is always in relation to somebody **believing**
 124. something.
125. C Hmhm.
126. PA You know, let's say the ehm, some scientist would believe that there can not be
 127. such a thing as x, y or z.
128. C Hmhm
129. PA And if he finds x in his data , of course, this cannot be.
130. C Hmhm, hmhm.
131. PA And then that it's an anomaly.
132. C Yeah.
133. PA But because I don't, I personally have not such eh **stereotypes** on my data,=
 134. C Hmhm.
135. PA **= any pre- pre-assumptions, I can simply look at the data and say "oh that's**
 136. **quite interesting, =**
137. C yeah.
138. PA **= there is such a fact".** So, from this point of view there is no ano-, ane-,
 139. anomaly [((exhalation))
140. C [((sniggers)).

The analysis of extract 6 focuses on PA 6's construction of anomaly as a meaningless category of outcomes. The straightforward answer that is given to the interviewer's question (line 116, in bold) relates the meaningless value of the category to a total lack of expectations (in relation to outcomes) in his research. In Chapter 6 (extract 1), the analysis (using an extract from the same interview with PA 6) showed how he constructed his research as doing extreme empiricism, i.e. experimentation with no assumptions or theoretical underpinnings. The construction of anomaly as a meaningless category is therefore coherent with that version of research. In the present extract, PA 6 constructed not only the absence of expectations, but also constituted them as unwanted and troublesome elements in research.

In line 116, PA 6 presents the straightforward answer “there is no unexpected effect” (line 116, in bold). This is followed by an equally straightforward justification “because I don’t care” (lines 116 and 118, in bold) and the consequence from this lack of caring – “every outcome is fine with me” (line 121, in bold). The noticeable aspect of these formulations is precisely their straightforwardness. This simplification of research into a process where nothing is unexpected (because nothing is expected in the first place) seems to work as a way of closing down the issue that was raised in the question. In doing so, PA 6 also offers a construction of his research practices that is (like PA 17, examined in Chapter 6, extract 2), an extreme formulation of simplicity. This simple construction of lack of expectation implies that anomaly is a meaningless category, and is coherent with his version of research as doing extreme empiricism.

This opening claim is followed by a warranting sequence (lines 123 to 131) in which PA 6 seems to take the interviewer through a sequence of simple reasoning steps, during which he explains the relationship between “belief” (line 123, in bold) and the interpretation of an outcome as an anomaly. The use of “always” (line 123, in bold) establishes this relationship as an enunciation of “a rule of thumb” and, in doing so, provides an account of an abstract notion of anomaly, rather than engage in the detail of his own research. In constructing the dependency between “belief” (or expectation) and anomaly, the participant provides the necessary link to the reprise of the initial claim – “I don’t care” – in lines 133 to 139. This segment is rhetorically rich, in the same sense of constructing extreme simplicity, and orients quite clearly to the neutrality of his position and the extreme empirical accounts of his practices that he made before. In line 133, PA 6 upgrades beliefs and expectations into “stereotypes” (in bold) and “pre-assumptions” (line 135, in bold). This, it is argued, seems to be a performance (Edwards, 2000:364) of his neutrality and empiricism: first, by making available the undesirable and compromising nature of beliefs (by upgrading them into stereotypes and pre-assumptions); and second, by positioning himself as a researcher who is totally free from such compromising elements (line 135, in bold). The performative quality of PA 6’s self-presentation as a researcher with no expectations or assumptions about his data is continued in lines 135 to 138, where the use of his own reported speech builds up the factuality of this position, and, in doing so, another instance of simplification in which his activity as a researcher is reduced to that of an observer who spots the occurrence of “facts” (line 138, in bold). Having established this simple neutral position, the participant can safely conclude that, in this context, “there is no anomaly” (line 138-139). By constructing the meaninglessness of anomaly in relation to his version of extreme empiricism, however, the implication is that his version of research excludes the possibility of testing hypotheses, making decisions about success or failure, or relating the data to some form of explanation or theory. In short, and interestingly, this extreme version of doing empiricism runs contrary to common views of doing science, which demand that researchers make decisions about the meaning of data. In the next extract it will be possible to examine another way in which anomaly was constructed as a troublesome category.

Extract 7 – PA 1, lines 448-503

448. PA I'm trying to to verbalize now the what our leading ideas are of of of ur:m
449. our research efforts. (1) But er, (.) still I was (.) I've been speaking for quite a
450. long time but, as far as I can recall, I'm quite sure the word anomaly =
451. C = no =
452. PA = haven't occurred, haven't, ya? I =
453. C Yeah.
454. PA = er er during my **long talk** there was (.) **no need** to use the word anomaly.
455. C U-hum.
456. PA Why anomaly? (1) anomaly in what sense? .h You know what anomaly of water
457. is? (1) er (.) help yourself. [presents a packet of cigarettes]
458. C Oh, thank you.
459. PA Er (.) well as you know (1) as you know most substances increase their volume
460. (.) at higher temperatures.
461. C U-hum
462. PA And reduce their volume at lower temperatures, so there is a monotonic function
463. of temperature right? (1) A an iron rod will shrink =
464. C When it's cold =
465. PA = when frozen would extend when heated, (.) it's monotonic function of
466. temperature, but there are exceptions. One of those exceptions is water. As you
467. know the highest density, .h er water reaches its highest density at 4° of Celsius
468. C U-hum.
469. PA +4.
470. C U-hum.
471. PA Ya? (.) so if you have a say a a gallon of water, (.) and you cool it down it
472. shrinks and shrinks and shrinks, not observably but measurably.
473. C Yes.
474. PA (2) Reaches attains its its minimum volume at about 4° Celsius, and then it
475. expands again. (.) And if you, if it starts to to freeze, that is if you if you obtain a a
476. piece of ice, (.) again it expands further, ya? With decreasing temperature. .h So
477. this is something which is strange. (.) Which is not usual. (.) Most substances do
478. not behave that way and this is why it is called, even in textbooks of physics,
479. anomaly of water
480. C Yeah.
481. PA But you know this is no para para-physics. (1) This such a word for something
482. which is unusual but not (.) violating any laws of physics. Just to understand it we
483. need to understand a little bit more about the molecule structure and so on and so

484. on. (1) So:: I am not against using the word anomaly if something strange is going
485. on, =
486. C = Yeah.
487. PA = but, again, I wouldn't attribute the metaphysical meaning to those anomalies
488. which I unfortunately can see this tendent- this tendency, er sometimes even
489. among my esteemed colleagues in P(h)A circles ((sniggers)).
490. C [yes, yes.
491. PA E:r you know, they are hoping for something more for a divine revelation, you
492. know. (.) To be distilled out of the anomalies. Wrong [leans forward and speaks
493. this word into the microphone] ((sniggers)). Ok, ya? This is m(h)y (h)firm con-
494. ye- convinced opinion. This is wrong, ya? .h If there is something strange going
495. on, well [1] let's follow it, [2] let's nail it down and [3] let's show where is the
496. place of this phenomenon in the normal =
497. C Yes.
498. PA = landscape of all those already understood phenomena. You see I said
499. understood, not explained. I I .h I avoid the word explanation as as much as
500. possible. .h Er hh Ok so, this is in very very condensed form, my opinion about
501. the, about (.) the adjective [anomalous, =
502. C [anomaly u-hum
503. PA = about the noun anomaly.

7.3.2 Problematising the meaning of “anomaly” in parapsychology.

This sub-section examines how anomaly was made into a troublesome category, not because it was meaningless, but rather because it was *meaningful* within the context of parapsychology. The above extract is part of PA 1's initial long description of his research activities, in which he laid out the types of phenomena, methodologies and epistemological assumptions that have concerned and guided his own research work throughout the years. Towards the end of this description, PA 1 addresses spontaneously the issue of anomaly, and orients to it as his take on the overall theme of the interview. This rather long extract seems to have been structured as an almost rehearsed account of why anomaly is to him a problematic term. PA 1 presents this argument with features that include an introduction, an example, the claim itself (i.e. the problematisation of the term anomaly) and a conclusion that, in interactional terms, closes down the theme. For analytical convenience, the exploration of this extract will focus on these parts in turn.

In the introduction, the topic of anomalies is raised by PA 1 himself, who stresses that he has not mentioned the topic despite having spoken of his research at length (lines 450 and 454). This introduction therefore constructs the topic of anomaly into one that can be avoided, i.e. which is not central to his version of research (lines 449-452). This is repeated in line 454, using extreme formulations like “long talk” and “no need” (line 454, in bold). A series of questions follow (line 456), which set out something problematic about the word “anomaly” from the start, and crucially undermine the significance of this topic (namely, as a topic for a research interview). The structure of this introductory segment suggests that the switch to talking about the trouble with the term anomaly is neither accidental nor improvised.

PA 1 follows this introduction with an “example” of an anomaly, which is used to claim that anomalies can be found in normal areas of science (in this case physics), and are therefore normal elements of the process of science. In lines 456 and 457, PA 1 asks the interviewer a question that she takes up as rhetorical – note that C (line 458) does not answer the question. This seems to function not only as an introduction, but also as an early marker of the normalization of the term anomaly. By introducing an account of what “the anomaly of water” is (line 456), PA 1 makes relevant that the term anomaly applies to a phenomenon as normal as water. This example (lines 459-479) is presented in a didactic fashion, in which the participant “educates” the interviewer. In the conclusion of this example (lines 476 to 479), PA 1 makes relevant that anomalies can be found “even in textbooks of physics” (line 478). This builds up the category of anomaly as including phenomena that, while strange or unusual, are not beyond normal, ordinary and mundane physical laws (line 482). It also locates the trouble with the use of the term ‘anomaly’ within the context of parapsychology (i.e., the attribution of a paranormal value to anomalous phenomena). The meaning of anomaly is thus constructed here as being problematic because its context of use is problematic, i.e. by appealing to its indexical properties (Potter and Wetherell, 1987:23),

In lines 484 to 492, PA 1 goes on to contrast the correct and desirable “natural” meaning of anomaly with the troublesome “paranormal” or “metaphysical” (line 487) meaning. On the one hand, the correct and desirable meaning is indexical of normal and valid science while, on the other hand, the troublesome meaning is indexical of parapsychologists’ controversial views of *psi*, which locate it beyond the realm of physics or psychology (and potentially located in the real of metaphysics). The introduction of this contrast is made in line 481, where the participant uses the sentence “this is no para-physics” (line 481). It also allows PA 1 to do some positioning work, aligning himself with the correct and desirable use of the term anomaly (line 484), and further distancing him from the troublesome use in the context of parapsychology (line 489) (PA 1’s distancing work from this community was already explored in Chapter 5). Later we will see how the use of this contrast allows PA 1 to construct a safe meaning of the term anomaly, in which its normal character is emphasised.

Going back to the construction of the community of parapsychologists to whom this metaphysical view on anomalies is attributed, it is interesting to note PA 1's specification that this erroneous interpretation of anomaly can be found "even among (...) colleagues in PA³¹ circles" (lines 488-489). This seems to make available three implications: a) that this is a common view amongst parapsychologists; b) that it is so pervasive that it can be found "even" in "circles" (lines 488-489) where he would not expect to find it; and c) that these fellow members of the PA (line 489) would, like him, be expected to hold the correct, natural and, therefore, scientifically warranted view that anomalies are nothing more than natural, although strange, phenomena. This constructed disappointment with fellow scientists allows PA 1 to build up (as in chapter 5) a special membership of parapsychology in which such problematic attributions are always at a safe distance. In lines 491-492, PA 1 puts forward an extreme version of the attributed meaning of anomaly. The effect of this sarcastic formulation is the building up of the attributed view on anomalies into a fundamentally problematic one that clearly does not fit the context of a scientific research field. Thus, PA 1 is again able to construct the meaning of anomaly as problematic because its context of use is problematic.

7.3.3 Making sense of anomalies – their normalisation.

The final sub-section is concerned with the concluding segment of PA 1's construction of the problematic value of anomalies in parapsychology. After the positioning work in the previous segment, PA 1 outlines his own way of making sense of anomalies (lines 492-498). As before, the participant builds up a safe meaning of the term anomaly, in which its normal character is emphasised. As in Chapter 5, PA 1's description of the correct way of dealing with anomalies seems to attend to the discursive effect of constructing the ordinariness or usualness of this process (Sacks, 1984:414). In doing so, he locates his view of anomalies in contrast with the "metaphysical" and "divine" properties of other parapsychologists' views. McKinlay and Dunnett's (1998:41) work as already cited in a previous chapter as having explored the use of contrastive pairs (gun-owners vs. criminals and vigilantes) in speakers' constructions of their identities as gun-owners as "just average people" (McKinlay and Dunnett, 1998:40). Here, PA 1 uses the category of those who attribute metaphysical qualities to anomalies as the contrast that allows him to construct his view of anomalies in parapsychology as, using McKinlay and Dunnett's phrase, "just an average anomaly". The other relevant feature of this description is its straightforward and didactic format. PA 1 builds up a sequence or list of three actions (lines 495-498, numbers in bold) that would allow anomalies to be made sense of. This sequence constructs a normative process, which goes beyond his personal opinion and takes the form of a prescription for how to deal with anomalies. The normalisation of the term

³¹ The Parapsychological Association annual conference.

“anomaly” is thus achieved. PA 1 concludes his argumentation with a clear ending (lines 500-503), closing down the subject, achieving once more the effect that his views on the matter of anomaly have been thought through, and are definitive.

In summary, this section focused on how PA 6 and PA 1 constructed the category of anomaly as a dispreferred one. On the one hand, the term was constructed as meaningless in the context of a lack of expectations. While consistent with a notion of doing extreme empiricism, the brittleness of such a position (suggested at the end of the last chapter) is clear, its implications actually run contrary to a standard view of science, in which scientists are expected to provide meaning for their data. On the other hand, anomaly was constructed as indexical, and problematic because its meaning within parapsychology is problematic (being suggestive of paranormality). Unlike other parts of science, where anomalies are normal, anomalies in parapsychology imply paranormality, hence the need to be normalized.

7.4 Anomalous *psi*.

This section will examine the fourth and final construction of anomaly put forward – that of anomalous *psi*. In doing so, it will also examine the construction of expectations in experimental parapsychology, i.e. it will look at these parapsychologists’ descriptions of anomalous *psi* as a way of constructing expectations that are compatible with a) their versions of scientific practice as doing extreme neutral, bottom-up empiricism (examined in Chapter 6), and b) versions of *psi* as an essentially elusive, difficult and largely unknown object of research (examined in Chapter 4).

The accounts of scientific action explored in Chapter 6 accomplished versions of doing science as an extremely empirical process, where the experimenters’ commitments and interpretations were discursively removed from the act of producing knowledge. Within these accounts of extreme empiricism, these parapsychologists not only noticeably removed expectations from their descriptions of doing research but, more importantly, intensely problematized them (i.e. talked about expectations, models, theories or assumptions as unwanted elements in doing research). This section examines how parapsychologists constructed a category of anomalous *psi* by making use of a track record of expected experimental outcomes regarding their magnitude. In doing so, it is argued, they constructed a safe kind of expectation, which is: a) related to magnitude of effect, rather than quality or content; b) factual and available to all as shared knowledge, i.e. not personal opinion; and c) products of experimental action, therefore compatible with their descriptions of research as extreme empiricism.

The section also examines how anomalous *psi* can be seen as a way of managing the incompatibility between *psi* as a difficult and elusive object and the need to establish categories of outcomes (such as anomalies) which rely upon expectation of some sort. In extract 8, the analysis will focus on PA 18's construction of anomalous *psi*, by making use of a track record of magnitude of effects. In extract 9, the analysis will examine how PA 5 builds up a rhetorically reluctant category of anomalous *psi* from "huge phenomena" (line 417), one that is consistent with her previous description of *psi* as largely unknown (and logically, therefore, of unknown magnitude), in extract 5 of this chapter.

Extract 8 – PA 18, lines 504-804

504. C Is there anything that would be an anomalous outcome (1) to you?
 505. [() in a particular project or a... =
506. PA [It's hard...
 507. PA = Yeah (.) ehm (.) it's hard isn't it because parapsychology is, by definition, is
 508. happy with the ano- anomalies.
509. C Yeah.
 510. PA Ehm (3). I suppose (1) anomaly would be something like (.) where (.) we got
 511. **really big** changes in what we would expect in an ESP study. =
512. C Hmhm
 513. PA = if we just got **absolute** 100% that would be a **real anomaly** ((smiley voice)),
 514. [because you know it **shouldn't** work like that. [So, you know, we've got a (.) =
 515. C (((laughs)) [Yeah.
 516. PA = we've got a fairly well accepted body of knowledge that says (.) it happens as a
 517. **slight influence** on your perception of things and your cognition of things, so that
 518. you can **pick up the odd thing**, e:hm, which may, you know, **bear** resemblance to
 519. the target or [whatever was con- whatever was considered to be, you know, =
520. C [Hmhm.
 521. PA = part of the study. But if you've got something that was really **absolutely spot**
 522. **on**, then probably you kind of -. I s- I suppose almost the anomaly would be
 523. pointing there to different interpretations. You'd have to say, "oh well that's just
 524. too good", =
525. C Yeah.
 526. PA = you know there must be something going on here that either, you know, we've
 527. completely got it tapped now, we know what it is, **or**, y'know, people are cheating
 528. or ehm there's some other kind of explanation I don't even have any kind of grip
 529. on.
 530. C Hmhm, hmhm.=

531. PA = Because straight away, you know, I mean, we're looking for anomalies
 532. ((smiley voice)) =
533. C = Yeah, yeah. =
534. PA = so you know **we're happy**. But probably as long as they don't
 535. [(1) as long as they = don't become so: kind of extreme that it looks like it's =
 536. C [yeah
537. PA = happening 100%, you know, mind to mind communication or something
 538. ((laughs)). Then **we'd probably all get worried**.
539. C ((laughs))
540. PA Probably lots of parapsychologists ((smiley voice)) would be **very worried if**
 541. **they got that kind of thing**.
- [Suppressed lines 542-782]
782. PA Ehm. I think it's ehm (.) is is a **very hot one [to handle**. I think most =
 783. C [yeah.
784. PA = parapsychologists would **absolutely be really scared** if they got **100% all the**
 785. **time**.
786. C Yeah.
787. PA They really would, because, (.) eh I mean, you can just imagine, I mean probably
 788. people are quite happy with parapsychologists (still) going along doing **their little**
 789. *Ganzfeld* experiments as along as they never get above 33% hit [rate. You know,=
 790. C [Yeah
791. PA = that's ok it's not very, it can still **just be noise and randomness** in the data,
 792. there's no problem, or, you know, **little bit** of fraud tinkering with it, that's not a
 793. problem, but if you get 100% it's either real fraud, =
794. C Hmhm.
795. PA = you know, and you've brought everything into disrepute, or if it happens so
 796. well that eh, you know, and you've ruled fraud out you're left with the even more
 797. difficult thing to handle which is the fact that it's real, =
798. C Yeah
799. PA = and it stresses something so radically different from our normal experience, the
 800. experience that's up here rather than down there, =
801. C = yeah ((smiley voice)) =
802. PA = and we don't know what to do with it, =
803. C Yeah.
804. PA = and we're worried about it.

In extract 8, PA 18 presents the category of anomalous *psi*. The analysis will focus on two main moments in this extract: first, the construction of anomaly as a discernable category of outcomes, and the criteria for inclusion in the class of anomalous *psi* (making use of a track record of usual magnitude of effects); second, the discussion of the value of this kind of anomaly (i.e., its meaning for the claim that *psi* is a real effect)

7.4.1 The construction of anomalous *psi*.

While this extract was selected for its clear construction of the category of anomalous *psi*, PA 18 nevertheless starts his response by making relevant the difficulty of defining ‘anomaly’ in parapsychology (lines 506-507). He seems at first to orient to the request of categorisation as a difficult request, related to the overlap between the meaning of *psi* (as an anomaly to normal science) and the meaning of anomaly (within the realm of *psi* phenomena), similar to the constructions of incompatibility explored in section 7.2 above. Although PA 18 does move on to the construction of the category of anomalous *psi*, this prefacing to his response, again, signals something problematic about this question in the context of parapsychological research. In lines 510-513, he builds up a category of anomalous outcomes in parapsychology, which includes effects that are expressed in hearably large terms – “really big changes” (line 511, in bold), “absolute 100%” (line 513, in bold). The extremity of this category of “real anomaly” (line 513, in bold) seems to construct these very large effects into outcomes with minimal probability of actually happening (Hutchby and Wooffitt, 1998:209). This extremity is a rhetorical practice that PA 18 uses recurrently during this extract – for instance in “absolutely spot on” (line 521-522, in bold) and “100% all the time” (line 784-785, in bold) – and is effective in constructing this category of anomaly in parapsychology as one that is identifiable, i.e. clearly different from the track record of “normal” *psi* outcomes. In doing so, arguably PA 18 transforms anomalous *psi* into a rhetorically viable category, even in relation to an ambiguous, elusive or difficult *psi* (as explored in chapter 4).

This initial construction of anomaly in relation to *psi* is warranted in lines 514 to 521, where PA 18 puts forward expectations that are specific to experimental situations in parapsychology, and which appeal to a body of knowledge about normal magnitude and relative probability. In using the sentence “because it shouldn’t work like that” (line 514), PA 18 makes available the existence of a set of expectations that establish the size of effect in an almost prescriptive way (“shouldn’t” in line 514, in bold). Contrary, for example, to PA 6’s contribution in extract 6, PA 18 makes relevant here a context or “body” (line 516) of knowledge and, therefore, of expectation. This “body of knowledge” (line 516) is constituted and warranted when PA 18 presents it as a “well accepted” (line 516) set of expectations, building up the factuality and authority of this “body of knowledge” (Potter, 1996a:159).

A second significant aspect of this construction is that it is oriented to as a body of empirical findings, i.e. a set of observations resulting from experimental studies concerning the magnitude of effects (lines 517 and 518, both in bold). Thus, PA 18 builds up a track record of magnitudes of *psi* effects, and anomalies as outcomes that go beyond this boundary of expectation (being too large or its probability too high). In doing so, PA 18 constructs a safe kind of expectation, in the sense that it is: a) consensual and factual; b) a product of experimentation; c) concerning magnitude of phenomena (rather than content or nature); d) thus compatible with descriptions of research as extreme empiricism; and e) also compatible with descriptions of *psi* as a largely unknown phenomenon.

7.4.2 Constructing the value of anomalous *psi* – making sense of too much evidence.

This segment of analysis is concerned with PA 18's account of the potential empirical value of anomalous *psi*, as the participant elaborates on what it might mean in relation to the question of *psi*'s reality. In Chapter 5, the analysis explored participants' constructions of the consequences of *psi* being real. In PA 18's discussion of what anomalous *psi* might mean, he focuses on its potential to be a radical form of evidence for the reality of *psi*. However, the participant also included in this discussion other options for making sense of these outcomes, such as "real fraud" (line 793) or "cheating" (line 527), i.e. as effects that are "just too good" (lines 523 and 524) to be true.

These alternative ways of understanding these effects make available their interpretation as outcomes that support the *psi* hypothesis too much. In lines 531 to 538, PA 18 introduces a contrast between just enough evidence and too much evidence for *psi*. This is done by constructing the extremity of anomalous *psi* – "100% (...) mind to mind communication" (line 537). This allows PA 18 to make available the latter category of outcomes as a problematic one for parapsychology. The construction of this contrast relies upon the starting point that parapsychologists actively look for anomalies and are "happy" (line 534, in bold) to find them (appealing to the identification between *psi* and anomaly that was explored in section 7.1). The sort of anomalous anomaly (*psi*) that he then constructs is thus built up as more than a deviation from a norm or rule. This class of outcomes seems to have been rhetorically "cut out" (Smith, 1978:50) from the normal type of experimental situations in parapsychology, and made into a problematic exception, by making relevant that the community of parapsychologists would be "worried" (line 538, in bold) about them. This reaction is repeated and upgraded into "very worried" in line 540 (in bold). Then, in line 782, such an outcome is upgraded to "a very hot one to handle" (in bold), and the community's reaction being "absolutely be really scared" (line 784). The upgrading of the meaning of these anomalous outcomes builds up both the significance and the problematic consequences of this class of outcomes.

The potential problems are further elaborated in a second contrast. On the one hand (lines 787 to 793), PA 18 belittles the current output of parapsychology. On the other hand (lines 793 to 804), he constructs a dramatised set of consequences for the anomalous outcome, i.e. the scientifically compromising assumption of “real fraud” (line 793), or a radical reshaping of scientists’ world views. In contrast with the “quiet” version of parapsychology, he constructs this type of evidence for *psi* to be either radically compromising or revolutionary to science, i.e. either causing irreparable damage to the reputation of parapsychology as a whole (lines 793-795), or providing indisputable proof of the reality of *psi*. The upshot from the latter is the radical challenge to the current scientific context, and thus anomalous *psi* is constructed as an outcome that is too troublesome to handle either way (line 802). It is interesting to note that the participant locates the first half of this contrast in a vague “people are quite happy (...)” (line 788), and seems to orient to the authorship of this view of comfortable and quiet parapsychology as being external to the community. This version, as mentioned above, is greatly discounted and, in the process, the work and achievements of parapsychology minimised to their most trivial properties – note the rhetorical markers of this discounting like “doing their little *Ganzfeld* experiments (...)” (lines 788-789, in bold), “it’s ok it can still just be noise (...)” (line 791, in bold). Similarly to what Smith identified as the contextual work that provides the “instructions” for a behaviour to be heard as an anomalous behaviour (Smith, 1978:39), PA 18’s description of the run of the mill work and results in parapsychology provides the “instructions” for the meaning of anomalous *psi* to be heard as critically significant and intensely problematic, either as a result of real fraud (line 793) or as a reality that is difficult to understand and explain (lines 796-797). In the next extract, it will be possible to see how the value of anomalous *psi* is further problematised.

Extract 9 – PA 5, lines 415-436

415. PA [Again, I have problems with the word. [And I have-, and what's anomalous?
 416. C [Yeah. [Yeah
 417. PA Well **huge phenomena is anomalous**, but it happens from time to time, so it's
 418. not that unusual.
 419. C Hmhm, hmhm.
 420. PA In that respect,=
 421. C Yeah, yeah.
 422. PA = but not outrageously so.
 423. C Yeah, [yeah.
 424. PA [Hm? (((laughs))
 425. C [No, that that yeah.
 426. PA You welcome it, it's nice. You welcome, come into my lab, you invite (him)
 427. (((laughs)). Ehm, but, yeah.

428. C ((laughs)). Hmhm, hmhm. No that makes sense, makes [eh sense].
429. PA [So. ((laughs)) Well, I
430. don't know. But you know, it's it's it's a horrible contradiction to say that if you
431. see huge displays of phenomena you're looking for, you start thinking, wow, that's
432. unexpected.
433. C A-ha.
434. PA Eh, but at the same time I think (.) that is a part is a part (.). Is it a part of my
435. assumption? I don't know if it's my assumption, cause it's happened often enough
436. that I know it does happen.

This extract is the continuation of PA 5's construction of anomaly, who previously (in extract 5) rejected the identification of *psi* with anomaly by problematizing the meaning of the term 'anomalous'. In this extract, PA 5 builds up another version of anomaly, described above as anomalous *psi*. The analysis will focus on how PA 5 builds up this version of anomaly as a category that includes "huge phenomena", but one which is a reluctant and unsatisfactory construction. The analysis will be in two parts, the first concerned with the construction of anomalous *psi*, and the second with its problematisation.

7.4.3 The reluctant construction of "huge phenomena" as anomalous *psi*.

In line 417, PA 5 constructs "huge phenomena" (in bold) in parapsychology as anomalous. In the lines that immediately precede this, it appears that this is oriented to as a reluctant concession. In line 415, the participant prefaces this construction of anomaly with a clear indication of her "problems" with the word, and also repeats the initial question asked by the interviewer (question included in extract 5). This prefacing, along with the introductory particle "Well" (line 417) seem to be markers of a reluctant reply, as if providing a performance of "humouring" the interviewer. The construction of the meaning of anomaly as "huge phenomena" follows, in which PA 5 puts forward a version of anomaly that appeals to (as PA 18 did in the last extract) the magnitude of *psi* effects. However, this construction of anomalous *psi* is again oriented to by PA 5 as a reluctant construction, as she immediately presents (lines 417-418) a problem with this category of outcomes, i.e. that it does not satisfy the criterion of rarity (line 418). It seems that PA 5 is making a performance out of conceding this point (Antaki and Wetherell, 1999), this shortcoming of the definition being insisted upon in line 422. By appealing to her reservations about the correctness of this definition (i.e. of anomaly as large *psi* phenomena), she undermines its potential to be convincing. Indeed, the discounting of this construction of anomaly is continued in the next moments of this extract.

Having discounted, and thus undermined, the category of anomalous *psi*, in lines 424 and 425 PA 5 then assesses it as a category of “welcomed” outcomes, in terms of its support for the *psi* hypothesis. Nevertheless, in lines 429 to 431, she quickly orients to this positive value of anomalous *psi* (i.e. as a sought-after outcome) as being somehow incompatible with its unexpectedness. This implies that the difficulties with this definition of anomaly are bound to the issue (already explored with extract 5 in section 7.2) of how outcomes that are expected can be anomalous. PA 5’s reluctant construction of anomalous *psi*, and downplaying of its value as a sustainable definition, allows her to concede that huge phenomena would be anomalous, while maintaining coherence with her previous attributions of *psi* as a largely unknown research object.

In summary, this section focused on participants’ construction of anomalous *psi* in relation to a track record of experimental outcomes of an expected magnitude. Anomalous *psi* was thus constructed as a category of unexpectedly large *psi* effects. The analysis of accounts explored their versions of anomalous *psi* in relation to their research as doing extreme empiricism (chapter 6), and of *psi* as a largely unknown research object (chapter 4). It showed how they constructed a safe kind of expectation which would arguably be reconcilable with both formulation of extreme empiricism and the unknown nature of *psi*, explored earlier in the thesis.

Summary.

This chapter explored four main constructions of anomaly in parapsychology. When asked what would be an anomaly or anomalous outcome in their research, these participants provided radically different accounts not only of anomaly but also of *psi*. Some provided a straightforward identification of anomaly with *psi*, constructing *psi* as the anomalous outcome in parapsychological research, and anomalies as experimental evidence for the reality of *psi*. Others, however, rejected the identification of anomaly with *psi* as fundamentally incompatible, on the basis that a research object cannot be both sought after and unexpected. Some rejected anomaly in parapsychology either as a meaningless category (in the absence of expectations), or as an undesirable one (since, in that context, it implies paranormality). Others, however, explicitly constructed a category of anomaly in parapsychology, that of anomalous *psi*, by appealing to a normal track record of (i.e. expected) experimental outcomes.

This variability of constructions within a relatively small community of researchers who meet regularly at conferences signals that there is nothing straightforward or unproblematic about the

category of anomaly in parapsychology. What is particularly significant, however, is that all of the participants' made sense of anomaly by appealing to their own constructions of *psi*. Clearly, then, there is nothing straightforward or unproblematic about the construction of *psi* itself. Given that this is the central object and claim of the field, such fundamental differences are hugely significant (and, perhaps unsurprisingly, not reflected in the literature of the field). What analysis of parapsychologists' accounts has shown is that there are disparities not only in how they talk about the most basic concept in their field, but also between this variability in discourse and the relative consistency in the literature. Such disparities therefore need to be made sense of. What does appear to be consistent is that these different versions of anomaly and *psi* appear to be in line with the different versions of research practises that they put forward. By identifying anomaly with *psi*, participants provided *psi* with a straightforward empirical identity (i.e. a statistical deviation from chance), one that is consistent with doing strict empiricism (as discussed in Chapter 6). In building up the fundamental incompatibility of *psi* with anomaly, it can be argued that participants appealed both to the status of *psi* as a largely unknown phenomenon (as discussed in Chapter 4) and its status as a normal scientific object and, therefore, to their doing of ordinary science (as discussed in Chapter 5). In constructing anomaly as a meaningless category, one can put forward the relevance of versions of research practice as doing extreme empiricism (as discussed in Chapter 6), and in constructing anomaly as an undesirable category, versions of research as doing ordinary science. As the last section has shown, in the construction of anomalous *psi*, participants appealed to a track record of results (i.e. a minimal version of expectations), which is coherent both with their research practice as doing extreme empiricism, and with constructions of *psi* as a largely unknown phenomenon. The variability in how a small close-knit community of researchers described the most basic concept in their field can thus potentially be made sense of in relation to different ways of accounting for their research practice and their discursive achievements.

Introduction.

This thesis has examined interviews with experimental parapsychologists, and, using a form of discourse analysis interested mainly in fact construction, examined how parapsychologists constituted their field, concepts, research object, practices and outcomes. It can be seen as a contribution to the field of parapsychology: a) as a reflexive, functional and situated analysis of the competing versions of reality to which parapsychologists constantly orient and with which they deal in their research lives; and b) as a way of revitalizing the practice of what was described in Chapter 1 as the 'Zetetic perspective' within parapsychology, i.e. the examination and self-reflection upon epistemological and social issues within the field. In a wider sense, it also aims to contribute to the context of social studies of science (specifically in relation to the examination of areas of scientific controversy), by revisiting the discourse analytic project within it in the light of recent forms of DA research developed within the field of social psychology. This final chapter will first briefly summarise the main issues that were addressed in the thesis and analytical arguments that were made in each of the previous chapters (section 8.1). It will then present the main empirical findings of this study (section 8.2). Finally, in section 8.3, it will discuss reflexively relevant methodological issues and assess the thesis' findings (sub-section 8.3.1) and discuss theoretical and substantive implications of the thesis, and point to possible research alternatives or developments (sub-section 8.3.2).

8.1 Summary of chapters.

As it was described in Chapter 1, the examination of controversies in science was used by scholars such as Harry Collins, Trevor Pinch and Andrew Pickering as ideal arenas in which to explore how scientific knowledge and practice is accomplished. These authors' examination of accounts of instances of scientific controversy formed a thread of research within SSK which argued that what

comes to count as scientific knowledge is a social product, right down to its most technical core. As Pickering put it, the production of consensual knowledge is “the outcome of ‘negotiations’ between social actors” (Pickering, 1992:1). Controversial aspects of science, these authors propose, are instances in which it is not clear what should count as a successful outcome, a competent experiment or evidence for a model or proposition (Collins, 1981b:34). Such settings allowed these scholars to examine how a community of researchers negotiates and effectively constructs, for instance, the empirical factuality of a disputed phenomenon. Parapsychology was considered to be, in this sense, a rich context of analysis, where the reality of its research object and the justification for the field’s existence are issues that are to this day fiercely disputed.

Methodologically, the present thesis followed up on the so called “turn to language” in social studies of science. In Chapter 1 it was described how the discourse analytic project in social studies of science put forward a) a radically critical assessment of the naïve use of language and accounts in previous sociological work, and b) proposed that scientists’ accounts in themselves be made into the primary topic of research in sociological analyses of science. Michael Mulkay and Nigel Gilbert proposed a shift in the analysis of the social construction of scientific knowledge, which focused “on describing how scientists’ accounts are organized in ways which portray [their] actions and beliefs in a variety of specifiable and contextually appropriate ways” (Mulkay et al., 1983:197). This, of course, implied an alternative research question, moving its focus from asking “what is going on in science?” to “how do scientists construct their versions of what is going on in science?” (Mulkay and Gilbert, 1982a:314). The analysis of scientists’ discourse was used in a considerable number of social studies of science, including such topics as theory choice (Gilbert and Mulkay, 1982), the construction of scientific consensus (Lynch, 1984), practices of social categorisation in psychology (Potter, 1988), and Gilbert and Mulkay’s seminal text “*Opening Pandora’s Box*” (1984), which examined a controversy in chemistry. However, despite the fundamental implications of Gilbert and Mulkay’s discursive project for the practice of SSK (and for sociology in general), it did not have a sustained impact on the discipline, and the use of DA in SSK was more or less abandoned (Wooffitt, 2005a:48). The development of DA into a methodological alternative took place, however, within social psychology and, as described in Chapter 3, led to many different (though related) approaches based on diverse intellectual traditions. These two broad empirical traditions were thus addressed in Chapter 1 as the substantive and methodological background of this thesis, and it was argued that discourse analysis would be this thesis’ choice of method, as it would afford important insights into how experimental parapsychologists realise and achieve the properties of their field, research work and object. As such, the thesis was identified as a contribution to parapsychology, adding to the discursive project within it, and reviving the Zetetic tradition of parapsychology’s self reflection upon its own epistemological and social issues.

Chapter 2 presented a possible characterization of parapsychology as a field of internal controversy. According to the parapsychological literature, their object of research, *psi*, has a) uncertain properties, b) is described using terminology that is not consensual, and c) is characterized as a “reluctant” scientific object. Furthermore, what counts as evidence for *psi* and, therefore, as an appropriate methodology for its investigation, is fiercely disputed. It was argued that, since parapsychology’s central object of study is not consensually “real”, and parapsychology itself is by no means a straightforwardly warranted field of research, experimental parapsychologists’ accounts of their field, of what counts as good practice or as a valid outcome (or as an anomaly) are fruitful sources for examining how scientists account for the scientificity of their work in what they themselves acknowledge to be a controversial field.

In Chapter 3, the study’s methodology was described as a form of DA which had as its main analytical concern fact construction, i.e. the analysis of the organisation of the interviewed parapsychologists’ factual language. The aim was thus the examination of how these parapsychologists presented their field of research, what they do and their research objects, using ostensibly factual accounts of the way things are or of what they do, and how they managed issues such as accountability, interest or bias in these descriptions. It was further argued that there is a clear link between the study of fact construction and the study of scientific discourse. The starting point for the analytical concerns in this thesis, and its main reference, was Jonathan Potter’s “*Representing Reality*” (1996a), specifically his articulation of two sets of questions that can be asked of factual accounts: a) “how is it that descriptions are made so that they are treated as factual?”, or “how can a factual account be undermined as biased or concocted” (i.e. questions regarding the empirical orientation of accounts); and b) “what kinds of actions are these factual accounts accomplishing?”, or “why are descriptions used?” (i.e. questions regarding the action orientation of accounts). Given the way parapsychology was described above, the thesis’ interest in how parapsychologists construct what they do as doing science, i.e. as representing something real in the world, and their field as a warranted area of research into this aspect of the real world, is particularly relevant to an analytical approach concerned with factual discourse. Parapsychologists, it was argued, are in the business of representing a contested part of the real world, through the construction of what they do as scientific practice and their outcomes as scientific knowledge. The analysis of their factual discourse is therefore crucial to the understanding of how parapsychologists are able to achieve this goal or, conversely, how their descriptions are undermined.

Chapter 4, the first analytical chapter, examined how parapsychologists constructed their field as a community, as a body of evidence, and as a field with a particular relationship to science as a whole. It examined how participants constructed the field as one made up of sceptics and believers, and how the role of belief was both managed and transformed from a contingent into an empirical variable. It also examined how participants constructed the field as a body of evidence, and how the lack of

replicability was managed by essentialist formulations of the evidence as ambiguous, and of *psi* as elusive. In doing so, implications of incompetence or pointlessness were attended to, and their research practice constructed as a particularly difficult (almost heroic) quest. Finally, it examined participants' accounts of the role of *psi* and parapsychology in a wider context of "science", and how both were located outside the realm of "normal" science, but with inherently revolutionary implications in relation to it. Such implications, however, were nevertheless constructed as dependent upon "unchallengeable data". The analysis showed how, in these various constructions, participants attended to the (scientific) problem of a lack of consistent evidence for *psi*, by appealing to the way things are, rather than to what they do. By transforming potentially compromising features – belief, the difficulty in obtaining results, and the clash with "normal science" – into essentialist characteristics of the field, they were able to build up the difficulty and the importance (and therefore the scientific quality) of their work within a less-than-perfect-field.

Chapter 5 focussed on how participants' versions of what they do as researchers in parapsychology were concerned with the presentation of their work as doing safe science in a less-than-perfect scientific field. This was examined through four analytical themes. First, the construction of doing ordinary science in parapsychology was achieved by: a) appealing to expertise in psychophysiology; b) using ordinary scientific, rather than parapsychological, terminology; and c) appealing to the mainstream relevance of such ordinary work in parapsychology. Second, the construction of parapsychological research as particularly rigorous was achieved by appealing to external criticism and internal difficulties within the field, and by contrasting the emphasis upon methodological rigour in parapsychology with the emphasis upon obtaining results in "other fields". In this sense, the problems and controversies in parapsychology guaranteed the scientificity of the researchers' work in this field, and the lack of consistent results was built up as evidence of rigorous methodology. Third, participants contrasted "traditional" parapsychology with their own parapsychological research, and good science (and scientists) with not-so-good science (and scientists), even in relation to use of the *Ganzfeld* procedure (which is central to "traditional" parapsychology). Finally, participants negotiated a qualified membership of parapsychology, and thus their independence from it, in a variety of ways, building up positions in relation to the field such as that of a consultant, or that of a temporary or part-time member. The chapter showed how, throughout all of these themes, participants constructed their own research in parapsychology as doing safe science, even if parapsychology in itself is not a safe field. Thus, in describing what they do, they oriented to the way things are in parapsychology, but appealed to what they themselves do with reference to what is done in other areas of science.

Chapter 6 examined how parapsychologists constructed their research as doing empiricism. Their accounts of their work were constructed as extreme empiricism (in which they emphasised the primacy of data over theory), presented themselves as "bottom-up" researchers, and engaged in explicit constructions of neutrality. In doing so, they all described versions of scientific inquiry that

were hearably in line with a “standard view of science”, and can be seen as further accounts of how they do safe science in a less-than-perfect field. The chapter further argued that the variety and extremity (and therefore rhetorical brittleness) of these formulations illustrates the extent to which the empirical quality of their research is oriented to by them as something that is not taken for granted, and therefore needs to be accounted for. They can therefore be seen as performances of their investment in doing science in ways that are consistent with common views of what science is. However, in describing how they do science, they appealed to extreme versions of how empirical science is done, excluding speculative or theoretical aspects from their work. These aspects, although presented as secondary to “data”, are present in common accounts of scientific action. That the extremity of their empiricism was such that it breaches the “standard view of science” suggests there is a particular problem of theory in parapsychology, namely, one that it is associated with expectations.

The problem of expectations became explicit in the last analytical chapter (chapter 7), which examined how parapsychologists constructed radically different versions of anomaly and *psi*. Some provided a straightforward identification of anomaly with *psi*, presenting anomalies as experimental evidence for the reality of *psi*, while others rejected this identification as fundamentally incompatible (since *psi* cannot be both sought after and unexpected). Some rejected anomaly either as a meaningless category (in the absence of expectations), or as an undesirable one (since, in the context of parapsychology, it implies paranormality). Others still explicitly constructed a category of anomalous *psi*, by appealing to a normal track record of (i.e. expected) experimental outcomes. The chapter argued that such variability shows the problems parapsychologists themselves have in defining the central object and claim of parapsychology. It was also suggested that, since this crucial variability is not entirely reflected in the literature of the field, the analysis of parapsychologists’ accounts provides a more complex picture of this field of internal controversy, and argued that such variability can be understood in relation to different ways of accounting for their research practice.

8.2 Review of main empirical findings.

In this concluding stage, it is possible to identify 5 main empirical findings from the analysis of these parapsychologists’ accounts: a) the construction of doing safe science, through a bottom-line distinction between i) content and (lack of) outcomes of research, and ii) practice of research in parapsychology; b) the implications of “doing good methodology”; c) the construction of a version of science that can be called “X-treme science”, and the fundamental problematization of theory and expectation in parapsychology; d) the construction of a discursive paradox by these parapsychologists;

and e) the variability in defining *psi* and anomaly. These findings will now be explored in turn, and as it will be possible to verify, some of these claims reflect issues that were addressed in Chapter 2.

There is, of course, nothing new about recognising parapsychologists as having a controversial standing in science, and one would expect this to be an issue addressed in their accounts of research practice. This analysis has shown, however, that in their own descriptions of how they do their research, parapsychologists oriented to notions of demarcation, i.e. normative views of what “comes to count” (Collins, 1985:267) as science. Specifically, they presented parapsychology (i.e. the field, their object of research and their experimental evidence) as essentially problematic, and their own research work (i.e. their practice) with reference to what is done elsewhere in science, and in line with normative accounts of empirical research, and therefore unproblematic. Thus, this thesis argues that “doing safe science in a less-than-perfect field” is a discursive action that: a) orients to a bottom-line distinction between what they do and the content of their research; and b) constructs the scientific quality of the former, and locates the compromising elements to the scientific quality of their work in the latter.

Collins (1981b:34) argued that, in normative accounts of science, what comes to count as good methodological practice is necessarily linked to the production of “successful outcomes”. In areas of controversy, however, in the absence of agreement about what a successful outcome is (the detection or non-detection of *psi*), “arguments concerning the existence of the phenomenon turn, not upon experimental results, but upon what comes to count as a ‘well done experiment’” (Collins, 1981b:34). Similarly, one can understand parapsychologists’ appeal to the doing of correct and rigorous methodology in itself, rather than its products, as a discursively effective way of building up the validity of their work. In doing so, they construct a purpose to doing scientific practice that is sufficient by itself, independent from (and thus inoculated against) the potential implications of incompetence or pointlessness that the lack of consistent outcomes might suggest. Thus, doing research is constructed as “doing methodology”. In this sense, as was examined in Chapter 5, the lack of consistent results can be constituted into evidence for rigorous methodology. Additionally, it can be argued that by orienting to methodology as a guarantor of scientificity, they exploit the implications that the doing of “good methodology” affords, i.e. what counts as good methodology elsewhere counts as good methodology in parapsychology (despite the absence of its product). It is interesting to note that the appeal to methodology as an isolated criterion that could ensure parapsychology’s status as a truly scientific field is one that is commonly articulated in the literature. For instance, Morris (1987), as a central figure in the field, proposed that the issue regarding the assessment of parapsychology’s scientific status be dependent upon the comparison of parapsychological methodological practices with those employed in other fields consensually considered to count as science. Morris (1987:247) argued that the symmetry of status between fields should be articulated in terms of the symmetry in their practices: as long as the research practices are equivalent, so should be their evaluation in terms

of scientific quality. These participants' construction of "doing methodology" as a stable and independent guarantor of "doing science" in parapsychology seems to thus find an echo in the field's literature.

The extremity with which they constructed their research as doing empiricism (with no expectations, assumptions or theory) reflected, it was argued, the extent of the separation of methodology from knowledge. While there is nothing remarkable about scientists separating the two, and giving primacy to the achievements of methodology (data) over expectations, assumptions or theory, what is remarkable is the way in which parapsychologists deeply problematized the presence of these latter elements. It is remarkable precisely because such elements are not problematic in normal accounts of science, they are simply secondary to "data". In the absence of data, however, such elements become deeply problematic in a controversial field (e.g. because of potential implications of bias). The appeal to methodology in itself as the sole guarantor of scientificity therefore avoids this problem. However, the exclusion of expectations, assumptions and theory from their research practice runs contrary to the standard views of what science is about.

As was pointed out in Chapter 2, experimental parapsychology has developed into an area of research in a uniquely paradoxical position in science "in that it forcefully defends the primacy of the experimental method, but has so far been unable to fully develop a research programme in the manner of the other experimental sciences" (Mauskopf and McVaugh, 1980, p. xiv). This thesis argues, however, that these parapsychologists have themselves constructed another type of paradox in relation to normative accounts of doing science, i.e. a discursive paradox. While they actively appeal to the primacy and rigor of experimental methodology as guarantors of the scientific quality of their research practices, they have constructed a set of essential characteristics for their research object that compromise their appeals to empiricism. In other words, the construction of "doing safe science in a less-than-perfect field" was based on the differentiation between the way things are in parapsychology and what parapsychologists do. On the one hand, the construction of *psi* and its evidence as essentially difficult (i.e. ambiguous and elusive in their nature) allowed these parapsychologists to achieve versions of scientific practice that were hearably in line with normative accounts of doing science (in some extracts, almost as a scientifically heroic practice). On the other hand, however, these constructions present both evidence in parapsychology, and *psi* specifically, as inherently resistant to the very practices that sustain their claims to scientificity. The rhetorical value of accounting for their research as doing strict, empirical methodology is thus undercut by constructions of evidence and an object of research that are elusive to strict, empirical methodology. A similar theme was addressed in Chapter 2 when, in review of parapsychological literature, it described the tension between parapsychology's method and object.

Finally, the remarkable variability in how a small close-knit community of researchers described the most basic concept in their field, and the variety of accounts that parapsychologists provided of their research, demonstrates that such accounts of doing science cannot be taken as unproblematic representations of the way things are. Rather, they need to be understood as ways of pragmatically attending in their accounts to the interactional problem of presenting their work as doing science. In doing so, however, they created a discursive paradox in their accounts of their work, which constructs them as crucially different from the consensus of how scientific work “should” be described. From a discursive point of view, and to paraphrase PA 4 (Chapter 4, Extract 9, line 287), perhaps this is a possible description and explication of the “conundrum that parapsychology is in”.

8.3 Reflection upon methodological and substantive issues.

The evaluation of discourse analytic work (and of qualitative research in general) is an issue of some contention. Several authors have attempted to list potential criteria for assessment (e.g. Smith, 1996; Taylor, 2001b; Willig 2001). Others have directed their criticisms of DA as a methodology towards its lack of an integrated system of assessment and validation of its claims (Hammersley, 2003:764). This is often pointed out by CA authors, who propose that CA’s aim and format of observation and explication of the organisational features of talk carries within itself the format for its validation (e.g. Wooffitt, 2005a:88). It would not be contentious to put forward that such issues as the validity, representation, generalisation or inferential properties of DA claims are particularly thorny and not sufficiently worked out (Hammersley, 2003:764). Furthermore, epistemologically, because of DA’s inherently constructivist theoretical underpinnings, as Hammersley (2003) points out, the legitimacy of DA claims about social phenomena “*as they are*” (p. 764, italics in the original), and the ontological status of the analytical claims themselves as constructed statements about the real world are also issues that, in many instances of analysis, remain unexplained or not satisfactorily articulated. Overall, because of DA’s involvement in traditional psychological themes, and its self presentation as an alternative methodological and epistemological approach to social life, its findings are regularly assessed with reference to the measures used elsewhere in psychology. The frequent reply that such measures are incompatible with or irrelevant to DA’s products is commonly received with scepticism. If DA research wishes to make claims about social phenomena, such issues as generalizability need to be addressed. The evaluation of this thesis and reflection upon issues of methodology and theory will be a tentative one, but it will nevertheless try to comment upon such issues. It will distinguish between the evaluation of the practice of DA itself and the evaluation of the relevance of its products for parapsychology (based on Potter and Wetherell (1987:169-172) seminal work “*Discourse and*

Social Psychology”). Thus, subsection 8.3.1 will focus on the reflection upon methodological issues, and subsection 8.3.2 will reflect upon the substantive relevance of the thesis.

8.3.1 Reflection upon methodological issues.

This thesis began as an examination of the juxtaposition between anomaly and *psi* in parapsychological research. As has been discussed already, the identification between *psi* and an anomaly, particularly in the context of the experimental operationalisation of the former (i.e. as a statistical anomaly), poses a categorisation problem for parapsychologists. Parapsychology is therefore a particularly complex and challenging context in which to investigate the construction of anomalous outcomes, and the initial analytical focus of this thesis was to look at the various and functionally meaningful ways in which parapsychologists constructed categories of *psi* and of anomaly. As can be seen in Appendix 1, the interview protocol was designed primarily to allow insights into how these parapsychologists made sense out of these two central categories of outcomes. The context of their field, and their individual scientific practices within the field, were initially regarded as secondary themes. However, as the analysis progressed, the focus increasingly shifted towards parapsychologists' constructions of their context of research as a primary theme. As described in Chapter 3, the analysis was a recursive process, involving sequential readings of the transcripts and the ongoing construction of a coding scheme. As it developed, the most frequent, extensive and functionally remarkable features of the participants' accounts were related to constructions of their work that oriented to normative ideas of what "counts" as science (such as replication, empiricism, neutrality). These, then, became the focus of the arguments developed here, and the analysis became increasingly concerned with these accounts as accounts of "doing science". The relationship between *psi* and anomaly was nevertheless examined, but became a less significant theme than had initially been anticipated. The analytical progression of this thesis therefore points up an important characteristic of DA as an empirical approach to text, i.e. that valid discourse analysis should be guided by the participants' own orientations (Potter and Wetherell, 1987:170; Taylor, 2001b:321). Without such flexibility many of the analytical claims made in this thesis might not have been made at all.

To be guided by participants' orientations is not, of course, to remove the role of the analyst from the process, and the shift in focus necessarily took a particular analytical viewpoint that reflected my research concerns. There is no intention to make claims about neutrality or, as Hammersley (2003:758) calls it, or claim to have deployed any form of "primitive empiricism". This study does not claim to provide a neutral representation or analysis of the discursive practices of these parapsychologists. It includes a set of analytical claims that is contingent to my own analytical

concerns and to what these participants made relevant and, using Sack's term, "storyable" (Sacks, 1984:419) in their accounts. The nature of DA as a methodology is clearly interpretative, i.e. its products are products of individual interpretation by individual researchers. Any claim to the validity of the line of analytical arguments developed in this thesis rests largely on a claim to its ability to provide coherence to ostensibly contradictory constructions of what these parapsychologists do, their research object, their field and their community. It is argued that the sequence of analytical arguments in this thesis provides such coherence, through the set of discursive actions described as doing 'safe' science in parapsychology, for example, or through the paradox between these parapsychologists' constructions of practice and evidence in parapsychology [this issue of coherence is also a criterion put forward by Potter and Wetherell (1987:169) as an important element in the assessment of the practice of DA].

In terms of the status of the claims developed in these arguments, the analytical focus was placed not on the representation of how parapsychologists do their work or see themselves or their community – in Hammersley's (2003) words "about social phenomena *as they are*" (p.764) – but rather on how parapsychologists construct these issues as their ostensible reality, and what such constructions afford. Thus, the conclusions described above and the potential insights that such interpretations allow are relevant only to how these parapsychologists represent their reality, and not to their (assumed) reality. This, it is argued, provides important insights in itself. If one accepts the basic discursive constructivist idea that discourse is constitutive of realities, one must then consider that the way these parapsychologists describe what they do, their field and object (not only in these interviews but also elsewhere in journal articles or conferences) is crucial in constituting its 'real' properties. Thus, this examination of such discursive practices (as it will be discussed in the next subsection) can be an important contribution to the field of parapsychology.

Another related issue that needs to be addressed is that of whom this thesis refers to, i.e. the issue of the possibility of inference from its claims. Unlike other methodologies in psychology, DA does not provide instructions for the generalisation of its claims. The building up of a research project invariably implies the delimitation of a group of participants, but also the delimitation of a wider category in which this group of participants can be included (and to which the interpretations that the group of participants afford may also apply). These are then preliminary concerns in any research project, which are generally referred to as issues of sampling and inference in quantitative psychological research. The present thesis worked with data provided by a group of 20 parapsychologists and, as was described in Chapter 3, a great deal of attention was given to the characterisation of the category into which participants might be included – experimental parapsychologists with a background in experimental psychology. The specification of this category was useful in considering abstract sampling issues. However, although the 20 interviewed parapsychologists constitute a large part of the currently active group of parapsychologists with a

background in experimental psychology (as defined by the Parapsychological Association member index), this cannot be taken at face value as a representative group of this larger category. Moreover, sampling and generalizability issues in DA are further thrown into question when combined with the need to follow participants' own orientations to what to them are the relevant categories in the particular context of the interview. As the analysis in Chapter 5 showed, membership of the category of experimental parapsychology was oriented to by several participants as deeply problematic, as they stressed their qualified membership to the interview's pre-specified category, and negotiated how they should be seen by the interviewer. Therefore, these issues of sampling and inference cannot be addressed in the way that is done elsewhere in psychology. An alternative way is to consider this issue, as the analysis did from time to time, in relation to the extent to which the analysis reflects issues and questions addressed in the field's literature or in dialogues between a wider group of researchers, for instance, in ongoing internet base discussion forums. Thus, although the argument of inference to a wider category of people is a difficult one to make, in developing the analysis and selecting the arguments to be included in the final thesis, the question of their relevance to the analysis of how parapsychologists construct their field, object and practices as science, was regularly reflected upon and used as a guide for the inclusion of analytical claims. A related issue that also warrants reflection here is the way in which some claims that were chosen to be included in this thesis were based on the contributions of only a few participants within the group. Although the verification of repetition and pattern within the group was the main and most immediate decision making tool, some other claims were chosen not because of their recurrence (an inherently quantitative argument for selection), but because they seemed to reflect practices found in other contexts of discourse in parapsychology. The claim regarding the construction of parapsychology as 'ordinary' science included in Chapter 5, is a good example of a relevant claim that is mainly based on the contribution of two participants only. On the other hand, the claims regarding the constructions of the ambiguity of parapsychological evidence and the inherent elusiveness of *psi*, were based on almost all participants' patterned use of these constructions, and are also ubiquitous in the field's literature.

Related to issues of validity and inference are the choices of data and form of analysis that were made. The use of interviews and its justification was addressed in Chapter 3. The specific context that interviews construct is intrinsically linked to the conclusions that they allow. Although the specific form of analysis of this interview data resulted in interesting and relevant conclusions, in retrospect it would have been beneficial to the analysis to use data from different discursive contexts. For instance, in the manner of Gilbert and Mulkey's (1984) work, one could examine these constructions of parapsychological research, object, field and community in experimental papers, perhaps during periods of particular controversy within the field (such as the *Ganzfeld* debate discussed in Chapter 2). Also, in the manner of McKinlay and Potter (1987) or Potter (1988), the use of data from discussion periods at conferences such as those during which these interviews were carried out, or the use of data from existing internet discussion forums, would be ways of cross-checking the relevance and

diversifying the interpretative claims that were afforded by the interviews. The larger scope of such analysis might warrant larger and more sustainable conclusions that could arguably be inferred as relevant to a greater category of parapsychologists.

Finally, having fact construction as the main focus of the analysis was justified in Chapter 3. It was argued that given parapsychology as a context of research, the thesis' interest in how parapsychologists construct what they do as doing science, i.e. as representing something real in the world, and their field as a warranted area of research into this aspect of the real world, is particularly relevant to an analytical approach concerned with factual discourse. Parapsychologists, it was argued, are in the business of representing a contested part of the real world, through the construction of what they do as scientific practice and their outcomes as scientific knowledge. Furthermore, it was argued that the analysis of their factual discourse is crucial to any understanding of how parapsychologists are able to achieve this goal or, conversely, how their descriptions might be undermined. It is felt that this form of analysis did accomplish this goal, and that the analytical claims - regarding the doing of 'safe' science in parapsychology in Chapter 5, the rhetorically robust reductions of doing research in parapsychology into 'doing methodology' in Chapter 6, and the paradox between constructions of practice and evidence pointed up by the analysis in Chapter 6 and 4 (respectively) – sustain the adequacy of this form of analysis.

However, as with the collection of data, it would have been possible to shift the analysis' focus from the broader issues of the wider interpersonal functions and actions served by these accounts, in order to characterize in greater detail just how these factual versions were accomplished, in the moment-by-moment unfolding of the interaction between interviewer and participant. This focus would be closer to the empirical tradition of conversational analysis, which has as its object the description and explication of the patterned sequential structures in talk through which activities are accomplished (Wooffitt, 2005a:79). There was throughout the analysis a concern to use, as Potter (1996b:132) recommends, references from conversation analysis for technical and empirical support for the claims. However, greater detail in the characterization of features such as turn organization, pairing of actions and more extensive referencing of existing studies, would have greatly enhanced the empirical justification and our understanding of how sequences of discourse normatively accomplished actions within this group of parapsychologists. In summary, while fact construction as a form of DA research was a fruitful and adequate focus for the analysis of these parapsychologists' talk, a more extensive use of conversation analysis references and practice would have provided this analysis with stronger empirical support.

8.3.2 Reflection upon substantive issues.

This thesis was characterized as being concerned with the field of parapsychology as an experimental science. It was based on discourse analysis of experimental parapsychologists' own accounts of their field, research practices and experimental outcomes. It examined how, in these accounts of what they do, their field and their object these parapsychologists constructed the ostensible reality of parapsychology as a field of scientific research, of what they do as doing science, and of their research object as a real (although controversial and disputed) object of research. This thesis' introductory chapter identified both a conceptual and methodological background and an empirical location for this thesis. It was argued that while this thesis may be seen as part of the DA project in social studies of science, it was specifically located as part of a self-reflexive tradition within parapsychology itself and, in doing so, characterized its contribution as a discourse based analysis and reflection upon how parapsychologists constitute their scientific practices and knowledge. This last section will attempt to outline the substantive implications of this thesis for: a) the analysis of parapsychology in social studies of science, as a field of controversial science; b) the discursive project within social studies of science; and c) the field of empirical parapsychology itself, understood as an inclusive field of research beyond the experimental exploration of *psi*.

Chapter 1 included a description of how parapsychology was examined within the discipline of sociology of scientific knowledge (SSK) as an exemplar of scientific controversy, playing an important part in this discipline's exploration of the social processes through which scientific knowledge is accomplished. The reason for this interest was that, despite parapsychology's formal symmetry with other accepted fields of experimental science, parapsychological claims are often seen as incompatible or violating the contents and practices of science. Studies by authors such as Harry Collins and Trevor Pinch examined parapsychologists from the analytical position of empirical relativism within SSK. These authors explored how claims about the ostensible reality of paranormal phenomena, and the practices of parapsychologists, are made into, or not made into, part of the realm of science. In other words, these authors used the examination of parapsychology as a way of analysing demarcation practices that would allow them: a) to challenge the traditional view of demarcation as the result of the successful deployment of definitive and socially invariant criteria (e.g. experimental demonstration, replication of results); and b) to demonstrate the social contingency of these demarcation practices, i.e. their description as social processes of judgement, manufacturing and negotiation, similar to other processes of interaction. The overview of these authors' studies verified that they relied on testimonials from parapsychologists, an examination of their literature, observation of their practices and even participation in parapsychological experimentation (Collins and Pinch, 1982).

The radical criticisms and propositions of the linguistic turn in social studies of science were described and illustrated with reference to one of the papers within the SSK exploration of parapsychology (Collins and Pinch, 1979). It used the criticisms made of this paper by Mulkay, Potter and Yearley (1983), main proponents of the turn to language in social studies of science. Their criticisms focused on how Collins and Pinch empirically supported the analysis of the processes involved in the construction of parapsychological claims in two contexts of scientific action – the constitutive and the contingent forums (Collins and Pinch 1979:240). Mulkay and colleagues' main criticism was that the authors had uncritically used parapsychologists' own interpretations as data with which to furnish their own analytical claims about the reality of parapsychology.

Chapter 1 thus isolated these two sets of empirical work as the conceptual and methodological background to this thesis. On the one hand, it took from Collins and Pinch's explorations of parapsychology the inspiration for a critical look at how the scientific properties of this field are accomplished and rebuked in socially relevant activities. On the other hand, it referred to the linguistic turn in social studies of science – i.e. the analysis of scientists' discourse itself – as its general methodological background. In relation to the first set of studies, following Mulkay and colleagues lead, this thesis can be seen as offering a further explication of some themes present in Collins and Pinch's analyses. These authors seem to gloss over issues that were addressed and worked through in the present analysis, which can and should be examined further. For instance, their identification of parapsychologists' metamorphosis into scientists by acquiring university posts (Collins and Pinch, 1979:253) as a "tactic" for the legitimisation of their work, seems to gloss over how parapsychologists get and maintain these academic posts in the first place, if what they do is so clearly construed by these authors as outwith the norm of scientific activity. Looking at the issues that were explored in Chapter 5, such as the construction of 'scientifically ordinary' parapsychology or the participants' construction of positions in relation to the field such as that of a 'part-timer' or a 'consultant', these can be seen as potential explications of the ways in which constructions of 'safe' parapsychology (i.e. aligned with normative accounts of what counts as science), can arguably sustain the metamorphosis of parapsychologists into ordinary scientists. Another example of an issue that Collins and Pinch systematically gloss over in their analyses of parapsychology (e.g. Collins and Pinch 1982:237) is their reliance on the existence of two easily identifiable communities within parapsychology – parapsychologists, i.e. proponents of the phenomena, and orthodox scientists, i.e. opponents of the phenomena – for the development of their arguments concerning the negotiations regarding the scientific legitimacy of the field. This analysis has shown that membership of such categories is deeply problematic and not a straightforward matter at all. In Chapter 5, participants regularly worked on their membership of the field, negotiating how and in relation to what contexts they should be seen as parapsychologists or as conventional scientists. It is thus argued that these categories are in themselves part of the negotiations in the construction of rebuttal of the scientific properties of parapsychology, and should not be taken as 'real', identifiable and disparate

communities. Rather, these parapsychologists' orientations to these identities serve interpersonal purposes, such as: the use of the 'parapsychologist' identity, to sustain a claim of an insider status to parapsychology, in order to warrant the factuality of a description of an outcome; versus the use of a psychologist or psychophysicist identity in order to accomplish a distancing from troublesome implications of doing parapsychology

The second set of empirical work – the analysis of scientists' discourse – was identified as the methodological background to this thesis. This thesis sees itself as following the tradition of the analysis of scientists' discourse, as Gilbert and Mulkay first proposed it. However, as was discussed in Chapter 3, the present thesis' use of DA, when compared to Gilbert and Mulkay's form of analysis, has important methodological differences, given the evolution and development of DA within the field of social psychology during the last 15 years. While the initial issues and questions that interested the present investigation were akin to those that the above authors posed of their own data, the way in which the data was analysed was considerably different, focusing specifically on the strand of DA research concerned with 'fact construction'. Gilbert and Mulkay's critical analysis of the reliance on the realist use of participants accounts and the proposition of the analysis of these accounts as the focus of social studies of science, despite its lack of development and adherence within this field, remains an important epistemological reflection and methodological shift. This thesis seeks only to bring up to date the DA project in social studies of science, having used recent developments of DA research into fact construction. Instead of following Gilbert and Mulkay's use of the broad conceptual tool of 'linguistic repertoire', the DA developed in this thesis focused on finer aspects of the construction of science, i.e. on the how descriptions are made into facts, built up as robust ostensible representations of reality, and on what particular accounts accomplished. The use of this sort of analysis, as was argued in Chapter 3, suits the analysis of scientists' discourse in general, and the analysis of parapsychologists' discourse in particular. In retrospect, as was mentioned above, a finer analysis (for instance, more extensively inspired by conversation analysis) could have been beneficial, but the claims afforded by this thesis' analytical process do contribute to an exploration of parapsychology that is not only new but, it is argued, functionally important.

While this thesis may be seen as part of the DA project in social studies of science, it identified itself more specifically as being part of a self-reflexive tradition within parapsychology. This thesis sought to contribute to the field of parapsychology which is, as was so often argued by the late Prof. Bob Morris, much more than the experimental study of the reality of *psi*. It allowed the analysis and the explication of the complex relationships between competing versions of reality to which parapsychologists regularly orient, and with which they constantly deal in their research lives (such as the discursive paradox between these parapsychologists' constructions of their research practices and their evidence). Furthermore, it argued that parapsychology would benefit from the revitalization of the practice of a "Zetetic perspective" [i.e. a habit of continuous dialogue and self-reflection

“interested not only in the adjudication of the claims, but with the sociology and psychology of the disputes themselves” (Truzzi, 1978:2)], for which this sort of discursive work would be a suitable tool. The expansion of this sort of analysis could include not only different sorts of analytical work, but also the use of data from different contexts of parapsychological discourse. It would also be able to incorporate novel research questions in the field, which would have more to do with the representation of an ostensible paranormal reality (be it in the laboratory or outwith it), and less with obtaining proof for the reality of the paranormal in itself. The development of discursive and conversational analytic work in parapsychology, as was mentioned above, is already under way, based on the efforts of Robin Wooffitt. His rigorous conversation analysis of descriptions of anomalous experiences (Wooffitt, 1992), or of the negotiations between experimenter and subject in *Ganzfeld* contexts (Wooffitt, 2003), set a high quality example of how parapsychology would benefit from a systematic empirical examination of how talk constitutes the properties of these experiences. After all, most parapsychological work is (as it is the case in psychology and other social sciences) heavily reliant on just that – talk.

In a similar way, this thesis argues that the analytical focus developed here – i.e. on the accounts of parapsychologists themselves – would afford this wider field of parapsychology a built-in way of examining how its practitioners’ talk constitutes the properties of their scientific statements about its practices, outcomes or concepts. After all, if one accepts that the ways in which parapsychologists account for their knowledge claims and research practices in varying contexts – from an informal chat to a journal paper or a conference presentation – are ultimately crucial in constituting what experimental parapsychology is about, then exploring the discursive practices of parapsychologists may be crucial in understanding, and potentially informing, how this (arguably) most controversial of fields of academic experimental research, its object, concepts and practices are presented.

Bibliographical References

- Akers, C. (1984). Methodological criticisms of parapsychology. In Stanley Krippner (Ed.), *Advances in parapsychological research: Volume 4*, p. 112-164 Jefferson, North Carolina: McFarland & Company, Inc. Publishers.
- Alcock, J. E. (1981). *Parapsychology: science or magic?* Foundations & Philosophy of Science & Technology Series. Oxford: Pergamon Press.
- Allison, P. D. (1979). Experimental psychology as a rejected science. In Roy Wallis (Ed.), *On the Margins of Science: the social construction of rejected knowledge*, *Sociological Review Monograph*, n. 27, p. 271-291. Keele: Keele University Press
- Amir, Y., Sharon, I. (1991). Replication research: a “must” for the scientific advancement of psychology. In James W. Neuliep (Ed.), *Replication Research in the Social Sciences*. p. 51-70. London: Sage Publications.
- Antaki, C., Wetherell, M. (1999). Show concessions. *Discourse Studies*, vol 1, 1, p. 7-27.
- Antaki, C., Billig, M., Edwards, D., Potter, J., 2003, Discourse Analysis Means Doing Analysis: A Critique of Six Analytic Shortcomings. *Discourse Analysis Online*, vol.1, no.1. Retrieved 14th December 2004 from <http://www.shu.ac.uk/daol/previous/v1/n1/index.htm>
- Arber, S. (1993). Designing samples. In, Nigel Gilbert (Ed.), *Researching social life*, p. 68-92. London: Sage Publications.
- Atkinson, J.M., Heritage, J. (1984). Transcription notation. In, Maxwell Atkinson and John Heritage (Eds.), *Structures of Social Action: Studies in conversation analysis*, p. ix-xvi. Cambridge: Cambridge University Press & Paris: Editions de la Maison des Sciences de l’Homme.
- Bauer, E (1984) Criticism and controversies in parapsychology – an overview. *European Journal of Parapsychology*, vol 5, 2, p. 141-165.

- Beloff, J. (1973). *Psychological Sciences – A review of modern psychology*. London: Crosby Lockwood Staples.
- Beloff, J. (1977). Historical overview. In B.B. Wolman (Ed.), *Handbook of parapsychology*, p. 3-24. Jefferson, North Carolina: McFarland & Company, Inc. Publishers.
- Beloff, J. (1980). Seven evidential experiments. *Zetetic Scholar*, 6, p. 91-94.
- Beloff, J. (1993). *Parapsychology – a concise history*. London: The Athlone Press.
- Beloff, J. (1994). Lessons of History. *The Journal of the American Society for Psychical Research*, vol. 88, 1, p. 7-22.
- Bem, D. (1993). The Ganzfeld experiment. *The Journal of Parapsychology*, vol. 57, 2, p. 101-110.
- Bem, D. and Honorton C. (1994). Does psi exist? Replicable evidence for an anomalous process of information transfer. *Psychological Bulletin*, n 115, p. 4-18.
- Bem, D., Palmer, J. and Broughton, R. (2001). Updating the Ganzfeld database: a victim of its own success. *The Journal of Parapsychology*, vol. 65, 3, p. 207-218.
- Blackmore, S. J. (1983). Unrepeatability: Parapsychology's only finding. In Betty Shapin and Lisette Coly (Eds.), *The repeatability problem in parapsychology: proceedings of an international conference held in San Antonio, Texas, October 28-29, 1983*, p. 183-196. New York: Parapsychology Foundation, Inc.
- Blackmore, S. J. (1985). The adventures of a psi-inhibitory experimenter. In Paul Kurtz (Ed.) *A skeptic's handbook to parapsychology*, p. 503-518 New York: Prometheus Books.
- Braud, W. (1983). The two faces of psi: psi revealed and psi obscured. In Betty Shapin and Lisette Coly (Eds.), *The repeatability problem in parapsychology: proceedings of an international conference held in San Antonio, Texas, October 28-29, 1983*, p. 150-175. New York: Parapsychology Foundation, Inc.
- Braude, S. E. (1979). *ESP and Psychokinesis: A philosophical examination. Philosophical monographs, third annual series*. Philadelphia: Temple University Press.
- Broughton, R.S. (1987). Parapsychology on the couch. *Behavioural and Brain Sciences*, 10, p. 575-576.
- Bunge, M. (1987). Why parapsychology cannot become a science. *Behavioural and Brain Sciences*, 10, p. 576-577.

- Carpenter, J. C. (1977). Intrasubject and subject-agent effects in ESP experiments. In, B.B. Wolman (Ed.), *Handbook of parapsychology*, p. 202-272. Jefferson, North Carolina: McFarland & Company, Inc. Publishers.
- Coelho, C. (2001). *Concepts and attitudes of researchers in psychology and parapsychology towards scientific anomalies: An exploratory interview study*. Unpublished MSc. thesis, University of Edinburgh.
- Collins, H. M. (1981a). Introduction: Stages in the empirical programme of relativism. *Social Studies of Science*, vol. 11, 1, Special Issue: "Knowledge and Controversy: Studies in modern natural science", p. 3-10
- Collins, H. M. (1981b). Son of the seven sexes: the social destruction of a physical phenomenon. *Social Studies of Science*, vol. 11, 1, Special Issue: "Knowledge and Controversy: Studies in modern natural science", p. 33-62.
- Collins, H. M. (1982). Knowledge, norms and rules in the sociology of science. *Social Studies of Science*, vol. 12, 2, p. 299-309.
- Collins, H. M. (1983a). The sociology of scientific knowledge: studies of contemporary science. *Annual Review of Sociology*, vol. 9, p. 265-285.
- Collins, H. M. (1983b). An empirical relativist programme in the sociology of scientific knowledge. In, Karin D. Knorr-Cetina and Michael Mulkay (Eds.), *Science observed – Perspectives on the social study of science*, p. 85-113. London: Sage Publications.
- Collins, H. M. (1985). *Changing order: replication and induction in scientific practice*. London: The University of Chicago Press, Ltd.
- Collins, H. M., Pinch, T. J. (1979). The construction of the paranormal: nothing unscientific is happening. In Roy Wallis (Ed.), *On the Margins of Science: the social construction of rejected knowledge*, *Sociological Review Monograph*, n. 27, p. 237-270. Keele: Keele University Press
- Collins, H. M., Pinch, T. J. (1982). *Frames of Meaning*. London: Routledge & Kegan Paul Limited.
- Dickerson, P. (2000). 'But I'm different to them': constructing contrasts between self and others in talk-in-interaction. *British Journal of Social Psychology*, vol. 39, p. 381-398.
- Edge, H. L. (1986) Psi and Science. In Hoyt L. Edge, Robert L. Morris, John Palmer, Joseph H. Rush

- (Eds.), *Foundations of Parapsychology: exploring the boundaries of human capability*, p. 296-324. Boston: Routledge & Kegan Paul.
- Edwards, D. (1991). Categories are for talking: on the cognitive and discursive bases of categorisation. *Theory and Psychology*, vol.1, 4, p. 515-542.
- Edwards, D. (1995). Two to tango: script formulations, dispositions, and rhetorical symmetry in relationship trouble talk. *Research on Language and Social Interaction*, vol. 28, 4, p. 319-350.
- Edwards, D. (1998). The relevant thing about her: social identity categories in use. In Charles Antaki and Sue Widdicombe (Eds.), *Identities in talk*, p. 15-33. London: Sage.
- Edwards, D. (2000). Extreme Case Formulations: softeners, investment and doing nonliteral. *Research in Language and Social Interaction*, vol. 33, 4, p. 347-373.
- Edwards, D., Potter, J. (1992). *Discursive psychology*. London: Sage Publications, Ltd.
- Edwards, D., Potter, J. (1995). Attribution. In Rom Harré and Peter Stearns (Eds.), *Discursive Psychology in Practice*, p. 87-119. London: Sage Publications.
- Edwards, D., Potter, J. (2005). Discursive psychology, mental states and descriptions. In H. Molder and J. Potter (Eds.), *Conversation and Cognition*, p. 241-259. Cambridge: Cambridge University Press.
- Fielding, N. (1993). Qualitative interviewing. In, Nigel Gilbert (Ed.), *Researching social life*, p. 135-153. London: Sage Publications.
- Gieryn, T.F. (1983). Boundary-work and the demarcation of science from non-science: strains and interests in professional ideologies of scientists. *American Sociological Review*, vol. 48, p. 781-795.
- Gilbert, G. N., Mulkay, M. (1982). Warranting scientific belief. *Social Studies of Science*, vol. 12, p. 383-408.
- Gilbert, G. N., Mulkay, M. (1984). *Opening Pandora's box – a sociological analysis of scientists' discourse*. Cambridge: Cambridge University Press.
- Gill, R. (1996). Discourse analysis: practical implementation. In, John T. E. Richardson (Ed.), *Handbook of qualitative research methods for psychology and the social sciences*, p. 141-155. Leicester: British Psychological Society Books
- Goffman, E. (1974). *Frame Analysis*. Harmondsworth, Middlesex: Penguin Books, Ltd.

- Goffman, E. (1979). Footing. *Semiotica*, vol. 25, ½, p. 2-29
- Hair, J. F., Anderson, R. E., Tatham, R. L., Black, W. C. (1998). *Multivariate Data Analysis – fifth edition*. London: Prentice-Hall International (UK) Limited.
- Hammersley, Martyn (2003). Conversation Analysis and Discourse Analysis: methods or paradigms? *Discourse and Society*, vol. 14, 6, p. 751-781.
- Hansel, C. E.M. (1985). The search for a demonstration of ESP. In, Paul Kurtz (Ed.) *A skeptic's handbook to parapsychology*, p. 97-127. New York: Prometheus Books.
- Hansel, C.E.M. (1987). Experimental evidence for paranormal phenomena. *Behavioural and Brain Sciences*, 10, p. 590-592.
- Hansen, G. (2001). *The Trickster and the paranormal*. Philadelphia: Xlibris Corporation.
- Harré, R., Stearns, P. (1995). Introduction: psychology as discourse analysis. In, Rom Harré and Peter Stearns (Eds.), *Discursive Psychology in Practice*, p. 1-8. London: Sage Publications.
- Hendrick, C. (1991). Replications, strict replications and conceptual replications: are they important. In James W. Neuliep (Ed.), *Replication Research in the Social Sciences*, p. 41-50. London: Sage Publications.
- Heritage, J. (1984). *Garfinkel and ethnomethodology*. Cambridge: Polity Press
- Hess, D. J. (1993). *Science in the New Age: the paranormal, its defenders and debunkers, and American culture*. Madison: The University of Wisconsin Press.
- Hewitt, J. P., Stokes, R. (1975). Disclaimers. *American Sociological Review*, vol. 40, 1, p. 1-11.
- Honorton, C. (1977). Psi and internal attention states. In B.B. Wolman (Ed.), *Handbook of parapsychology*, p. 435-472. Jefferson, North Carolina: McFarland & Company, Inc. Publishers.
- Honorton, C. (1985). Meta-analysis of psi Ganzfeld research: a response to Hyman. *The Journal of Parapsychology*, vol. 49, 1, p. 51-91.
- Hövelmann, G. H. (1983). Seven recommendations for the future practice of parapsychology. *Zetetic Scholar*, 11, p. 128-138.
- Hutchby, I., Wooffitt, R. (1998). *Conversation Analysis – principles, practices and applications*. Oxford: Blackwell Publishers, Ltd.
- Hyman, R. (1980). Pathological Science: towards a proper diagnosis and remedy. *Zetetic Scholar*, 6, p. 31-39.

- Hyman, R. (1985). The Ganzfeld psi experiment: a critical appraisal. *The Journal of Parapsychology*, vol. 49, 1, p. 3-49.
- Hyman, R. (1987). Parapsychology: the science of ostensible anomalies. *Behavioural and Brain Sciences*, 10, p. 593-594.
- Irwin, H. (1999). *An Introduction to Parapsychology* (Third Edition). Jefferson, North Carolina: McFarland & Company, Inc. Publishers.
- Jefferson, G. (1984). On the organization of laughter in talk about troubles. In, Maxwell Atkinson and John Heritage (Eds.), *Structures of Social Action: Studies in conversation analysis*, p. 346-369. Cambridge: Cambridge University Press & Paris: Editions de la Maison des Sciences de l'Homme.
- Kennedy, J. E. (2003). The capricious, actively evasive, unsustainable nature of psi: a summary and hypotheses. *The Journal of Parapsychology*, vol. 67, 1, p. 53-74.
- Kennedy, J.E., Taddonio, J.L. (1976). Experimenter effects in parapsychological research. *The Journal of Parapsychology*, vol. 40, 1, p. 1-33
- Knorr-Cetina, K. D., Mulkay, M. (1983). Introduction: emerging principles in social studies of science. In, Karin D. Knorr-Cetina and Michael Mulkay (Eds.), *Science observed – Perspectives on the social study of science*, p. 1-17. London: Sage Publications.
- Koestler Parapsychology Unit Glossary of Parapsychological Terms (n.d.). Retrieved 15th July 2003 from http://moebius.psy.ed.ac.uk/Glossary_H.html
- Krippner, S. (1983). Critical commentary on Hövelmann, G.H. (1983) Seven recommendations for the future practice of parapsychology. *Zetetic Scholar*, 11, p. 151-153.
- Kurtz, P. (1985). Is parapsychology a science?. In, Paul Kurtz (Ed.) *A skeptic's handbook to parapsychology*, p. 503-518. New York: Prometheus Books.
- Lakatos, I. (1978). *The methodology of scientific research programmes*. Philosophical papers, volume 1. John Worrall, Gregory Currie (Eds.). Cambridge: Cambridge University Press.
- Lantz, N.D., Luke, W.L.W., May, E.C. (1994). Target and sender dependencies in anomalous cognition experiments. *The Journal of Parapsychology*, vol. 58, 3, p. 285-302.
- Lynch, M. (1984). *Art and artifact in laboratory science: a study of shop work and shop talk in a research laboratory*. London: Routledge and Kegan Paul.

- Mabbett, I. W. (1982). Defining the paranormal. *The Journal of Parapsychology*, vol. 46, 4, p. 337-354.
- Mauskopf, S. H., McVaugh, M. R. (1980). *The elusive science: origins of experimental psychical research*. Baltimore: The Johns Hopkins University Press.
- May, E.C., Spottiswoode, S.J.P., James, C.L. (1994). Managing the target-pool bandwidth: a possible noise reduction for anomalous cognition experiments. *The Journal of Parapsychology*, vol. 58, 3, p. 304-313.
- Mazeland, H., ten Have, P. (1998). *Essential tensions in (semi-)open research interviews*. Retrieved 11th August 2002 from <http://www.pscw.uva.nl/emca/ET.htm>. (Original publication: Mazeland, H., ten Have, P. (1996). Essential tensions in (semi-)open research interviews. In I. Maso and F. Wester (Eds.), *The deliberate dialogue: qualitative perspectives on the interview*, p. 87-113. Brussels: VUB University Press).
- McClenon, J. (1984). *Deviant Science: the case of parapsychology*. Philadelphia: University of Pennsylvania Press.
- McKinlay, A., Potter, J. (1987). Model discourse: Interpretative repertoires in scientists' conference talk. *Social Studies of Science*, vol. 17, p. 443-463.
- McKinlay, A., Dunnett, A. (1998). How gun owners accomplish being deadly average. In Charles Antaki and Sue Widdicombe (Eds.), *Identities in talk*, p. 34-51. London: Sage Publications.
- Milton, J. (1988). Critical review of the displacement effect. *The Journal of Parapsychology*, vol. 52, 1, p. 29-55.
- Milton, J. (1999). Should Ganzfeld research continue to be crucial in the search for a replicable *psi* effect?. *The Journal of Parapsychology*, vol. 63, 4, p. 309-333.
- Milton, J., Wiseman, R. (1999). Does *psi* exist? Lack of replication of an anomalous process of information transfer. *Psychological Bulletin*, n 125, p. 387-391.
- Milton, J., Wiseman, R. (2001). Does *psi* exist? A reply to Storm and Ertel (2001). *Psychological Bulletin*, 127, p. 434-438.
- Morris, R. L. (1987). Parapsychology and the demarcation problem. *Inquiry*, vol. 30, 241-251.
- Mulkay, M. (1979). *Science and the sociology of knowledge*. London: George Allen & Unwin, Ltd.
- Mulkay, M. (1981). Action and belief or scientific discourse? A possible way of ending intellectual

- vassalage in social studies of science. *Philosophy of the Social Sciences*, vol. 11, p. 163-172.
- Mulkay, M., Gilbert, G. N. (1982a). What is the ultimate question? Some remarks in defence of the analysis of scientific discourse. *Social Studies of Science*, vol. 12, 2, p. 309-319.
- Mulkay, M., Gilbert, N. (1982b). Accounting for error: how scientists construct their social world when they account for correct and incorrect belief. *Sociology*, 16, p. 165-183.
- Mulkay, M., Gilbert, N. (1982c). Joking apart: some recommendations concerning the analysis of scientific culture. *Social Studies of Science*, vol. 12, 4, p. 585-613.
- Mulkay, M., Potter, J. Yearley, S. (1983). Why an analysis of scientific discourse is needed. In Karin D. Knorr-Cetina and Michael Mulkay (Eds.), *Science observed – Perspectives on the social study of science*, p. 171-203. London: Sage Publications
- Oxford English Dictionary Online (n.d.). Retrieved 27th July 2005, from http://dictionary.oed.com/cgi/entry/50291119?single=1&query_type=word&queryword=Zetetic&first=1&max_to_show=10
- Palmer, J. (1983a). Critical commentary on Hovelmann, G.H. (1983) Seven recommendations for the future practice of parapsychology. *Zetetic Scholar*, 11, p. 164-166.
- Palmer, J. (1983b). In defence of parapsychology: a reply to James E. Alcock. *Zetetic Scholar*, 11, p.39-70.
- Palmer, J. (1986a). Terminological poverty in parapsychology: Two examples. In D. H. Weiner, D. I. Radin (Eds.), *Research in parapsychology 1985*, p. 138-141. Metuchen, NJ: Scarecrow Press.
- Palmer, J. (1986b). ESP research findings: the process approach. In Hoyt L. Edge, Robert L. Morris, John Palmer, Joseph H. Rush (Eds.), *Foundations of Parapsychology: exploring the boundaries of human capability*, p. 184-222. Boston: Routledge & Kegan Paul.
- Palmer, J. (1989a). Confronting the experimenter effect: part I. *Parapsychology Review*, vol. 20, 4, p. 1-4.
- Palmer, J. (1989b). Confronting the experimenter effect: part II. *Parapsychology Review*, vol. 20, 5, p. 1-5.
- Palmer, J. (1997). The challenge of experimenter psi. *European Journal of Parapsychology*, vol. 13, p. 110-125.
- Parapsychology Foundation, Inc. Basic Terms in Parapsychology (n.d.). Retrieved 14th of July 2003,

- from <http://www.parapsychology.org/dynamic/070100.html>
- Parapsychological Association. Glossary of Psi Terms (n.d.). Retrieved 14th of July 2003, from http://www.parapsych.org/glossary_a_d.html
- Parapsychological Association. Member Index (n.d.). Retrieved 14th of October 2004, from http://www.parapsych.org/member_index.html
- Pickering, A. (1980). Critical commentary on Ray Hyman (1980) "Pathological Science: towards a proper diagnosis and remedy. *Zetetic Scholar*, 6, p. 60-63.
- Pickering, A. (1981). Constraints on controversy: the case of the magnetic monopole. *Social Studies of Science*, vol. 11, 1, Special Issue: "Knowledge and Controversy: Studies in modern natural science", p. 63-93.
- Pickering, A. (1992). From science as knowledge to science as practice. In, Andrew Pickering (Ed.), *Science as practice and culture*, p. 327-342. London: The University of Chicago Press.
- Pinch, T. J. (1979). Normal explanation of the paranormal: the demarcation problem and fraud in parapsychology. *Social Studies of Science*, vol. 9, 3, p. 329-348.
- Pinch, T.J. (1983). Critical commentary on G. H. Hövelmann (1983) "Seven recommendations for the future practice of parapsychology". *Zetetic Scholar*, 11, p. 166-168.
- Pinch, T.J. (1987). Some suggestions from sociology of science to advance the psi debate. *Behavioural and Brain Sciences*, 10, p. 603-605.
- Pinch, T. J., Collins, H. M. (1984). Private science and public knowledge: the Committee for the Scientific Investigation of the Claims of the Paranormal and its use of the literature. *Social Studies of Science*, vol. 14, 4, p. 521-546.
- Pomerantz, A. M. (1984a). Agreeing and disagreeing with assessments: some features of preferred and dispreferred turn shapes. In, Maxwell Atkinson and John Heritage (Eds.), *Structures of Social Action: Studies in conversation analysis*, p. 57-101. Cambridge: Cambridge University Press & Paris: Editions de la Maison des Sciences de l'Homme.
- Pomerantz, A. M. (1984b). Pursuing a response. In, Maxwell Atkinson and John Heritage (Eds.), *Structures of Social Action: Studies in conversation analysis*, p. 152-163. Cambridge: Cambridge University Press & Paris: Editions de la Maison des Sciences de l'Homme.
- Pomerantz, A. M. (1986). Extreme case formulations: a way of legitimizing claims. *Human Studies*,

- 9, p. 219-229.
- Potter, J. (1984). Testability, flexibility: Kuhnian values in scientists' discourse concerning theory choice. *Philosophy of the Social Sciences*, 14, p. 303-30.
- Potter, J. (1988). Cutting cakes: a study of psychologists' social categorizations. *Philosophical Psychology*, vol. 1, 1, p. 17-33.
- Potter, J. (1996a). *Representing reality: Discourse, rhetoric and social construction*. London: Sage Publications.
- Potter, J. (1996b). Discourse analysis and constructionist approaches: theoretical background. In, John T. E. Richardson (Ed.), *Handbook of qualitative research methods for psychology and the social sciences*, p. 125-140. Leicester: British Psychological Society Books.
- Potter, J. (2002). Two kinds of natural. *Discourse Studies*, vol. 4, 4, p. 539-542.
- Potter, J. (2003). Discursive Psychology: between method and paradigm. *Discourse and Society*, vol. 14, 6, p. 783-794.
- Potter, J. (2004). Discourse Analysis. In, M. Hardy and A. Bryman (Eds). *Handbook of Data Analysis*, p. 607-624. London: Sage Publications.
- Potter, J., Mulkay, M. (1985). Scientists' interview talk: interviews as a technique for revealing participants' interpretative practices. In, Michael Benner, Jennifer Brown and David Canter (Eds.), *The research interview: uses and approaches*, p. 247-271. London: Academic Press, Inc.
- Potter, J., Wetherell, M. (1987). *Discourse and Social Psychology: beyond attitudes and behaviour*. London: Sage Publications.
- Potter, J., Edwards, D. (1990). Nigel Lawson's tent: discourse analysis, attribution theory and the social psychology of fact. *European Journal of Social Psychology*, 20, p. 24-40.
- Proctor, R. W., Capaldi, E. J. (2001). Empirical evaluation and justification of methodologies in psychological science. *Psychological Bulletin*, vol. 127, 6, p. 759-772.
- Rao, K. R. (1981a). On the question of replication. *The Journal of Parapsychology*, vol. 45, 4, p. 311-320.
- Rao, K. R. (1981b). Science and the legitimacy of psi. In Betty Shapin and Lisette Coly (Eds.), *Parapsychology and the Experimental Method: proceedings of an international conference held in New York, November 14, 1981*, p. 100-111. New York: Parapsychology Foundation, Inc.

- Rao, K. R., Palmer, J. (1987). The anomaly called psi: recent research and criticism. *Behavioural and Brain Sciences*, 10, p. 539-551.
- Rosenthal, R. (1969). Interpersonal Expectations. In R. Rosenthal and R. L. Rosnow (Eds.). *Artifact in behavioural research*, pp. 181-277. New York: Academic Press.
- Rosenthal, R. (1976). *Experimenter effects in behavioural research (Enlarged edition)*. New York: Irvington Publishers Inc.
- Rosenthal, R. (1991). Replication in behavioural research. In James W. Neuliep (Ed.), *Replication Research in the Social Sciences*, p. 1-30. London: Sage Publications.
- Sacks, H. (1979). Hotroder: a revolutionary category. In, George Psathas (Ed.), *Everyday Language: studies in ethnomethodology*, p. 7-14. New York:
- Sacks, H. (1984). On doing "being ordinary". In, Maxwell Atkinson and John Heritage (Eds.), *Structures of Social Action: Studies in conversation analysis*, p. 413-440. Cambridge: Cambridge University Press & Paris: Editions de la Maison des Sciences de l'Homme.
- Seale, C. (1999). *The quality of qualitative research*. London: Sage Publications.
- Shapin, S. (1979). The politics of observation: cerebral anatomy and social interests in the Edinburgh phrenology disputes. In, Roy Wallis (Ed.), *On the Margins of Science: the social construction of rejected knowledge, Sociological Review Monograph*, n. 27, p. 139-178. Keele: Keele University Press
- Silverman, D. (2001). *Interpreting qualitative data: methods for analysis talk, text and interaction* (2nd Edition). London: Sage Publications.
- Smith, D. E. (1978). "K is mentally ill": the anatomy of a factual account. *Sociology*, 12, p. 23-53.
- Smith, J. A. (1996). Evolving issues for qualitative psychology. In, John T. E. Richardson (Ed.), *Handbook of qualitative research methods for psychology and the social sciences*, p. 189-201. Leicester: British Psychological Society Books.
- Speer, S. (2002). "Natural" and "contrived" data: a sustainable distinction? *Discourse Studies*, vol. 4, 4, p. 511-525.
- Storm, L., Ertel, S. (2001). Does psi exist? Comments on Milton and Wiseman's (1999) meta-analysis of Ganzfeld research. *Psychological Bulletin*, 127, p. 424-433.
- Taylor, S. (2001a). Locating and conducting discourse analytic research. In Margaret Wetherell,

- Stephanie Taylor, Simeon J. Yates (Eds.), *Discourse as data: a guide for analysis*, p. 5-48. London: Sage Publications.
- Taylor, S. (2001b). Evaluating and applying discourse analytic research. In Margaret Wetherell, Stephanie Taylor, Simeon J. Yates (Eds.), *Discourse as data: a guide for analysis*, p. 311-330. London: Sage Publications.
- Ten Have, P. (2002). Ontology or methodology? Comments on Speer's "natural" and "contrived" data: a sustainable distinction? *Discourse Studies*, vol. 4, 4, p. 527-530.
- Thalbourne, M. A. (2003). *A glossary of terms used in parapsychology*. Charlottesville: Puente Publications.
- Truzzi, M. (1978). Editorial. *The Zetetic Scholar*, vol. 1, 1, p. 2; 34.
- Truzzi, M. (1978). On the extraordinary: attempt at clarification. *The Zetetic Scholar*, vol. 1, 1, p. 11-22.
- Truzzi, M. (1981). Reflections on paranormal communication: a zetetic's perspective. In, Thomas A. Sebeok and Robert Rosenthal (Eds.), *The Clever Hans phenomenon: communication with horses, whales, apes, and people*, *Annals of the New York Academy of Sciences*, vol. 364 . New York: The New York Academy of Sciences.
- Truzzi, M. (1987). Anomaly versus artifact, or anomalous artifact? *Behavioural and Brain Sciences*, 10, p. 614-615.
- Truzzi, M. (1987b). Introduction. In Stanley Krippner (Ed.), *Advances in parapsychological research: Volume 5*, p. 4-8. Jefferson, North Carolina: McFarland & Company, Inc. Publishers.
- Truzzi, M. (2001). Letter to the Editor. *The Journal of Parapsychology*, vol. 65, 3, p. 320-322.
- Tversky, A., Kahneman, D. (1971). Belief in the law of small numbers. *Psychological Bulletin*, vol. 76, 2, p. 105-110.
- Valentine, E.R. (1992). *Conceptual issues in psychology* (Second Edition). London: Routledge.
- Watt, C., Brady, C. (2002). Experimenter effects and the remote facilitation of attention focusing: two studies and the discovery of an artifact. *The Journal of Parapsychology*, vol. 66, 1, p. 49-71.
- Westrum, R. (1982). Science and social intelligence about anomalies: the case of meteorites. In H.M. Collins (Ed.), *Sociology of Scientific Knowledge: A Source Book*, p. 185-217. Bath: Bath University Press. [reprint of Westrum, R. (1978). Science and social intelligence about

- anomalies: the case of meteorites. *Social Studies of Science*, vol. 78, p. 461-493.]
- Widdicombe, S. (1998). 'But you don't class yourself': The interactional management of category membership and non-membership. In Charles Antaki and Sue Widdicombe (Eds.), *Identities in talk*, p. 52-70. London: Sage Publications.
- Widdicombe, S., Wooffitt, R. (1995). *The language of youth subcultures: social identity in action*. Hemel Hempstead: Harvester Wheatsheaf.
- Willig, C. (2001). *Introducing qualitative research in psychology – adventures in theory and method*. Buckingham: Open University Press.
- Wiseman, R. (2002). Letter to the Editor. *The Journal of Parapsychology*, vol. 66, 2, p. 211-212.
- Wooffitt, R. (1992). *Telling tales of the unexpected: the organization of factual discourse*. Hemel Hempstead: Harvester Wheatsheaf.
- Wooffitt, R. (2001). A socially organized basis for displays of cognition: procedural orientation to evidential turns in psychic-sitter interaction. *British Journal of Social Psychology*, vol. 40, p. 545-563.
- Wooffitt, R. (2003). Conversation analysis and parapsychology: experimenter-subject interaction in Ganzfeld experiments. *The Journal of Parapsychology*, vol. 67, 2, p. 299-323.
- Wooffitt, R. (2005a). *Conversation Analysis and Discourse Analysis: a comparative and critical introduction*. London: Sage Publications.
- Wooffitt, R., Allistone, S. (2005b). Towards a Discursive Parapsychology: language and the laboratory study of anomalous communication. *Theory and Psychology*, vol. 15, 3, p. 325-355.
- Woolgar, S. (1980). Discovery, logic and sequence in a text. In, K.D. Knorr, R. Krohn, R. Whitley (Eds.), *The Social Process of Scientific Investigation*, p. 239-268. Dordrecht: Reidel.
- Woolgar, S. (1981). Interests and explanation in the social study of science. *Social Studies of Science*, vol. 11, 3, p. 365-394.
- Woolgar, S. (1988). *Science: The very idea*. London: Routledge.
- Woolgar, S. (1992). Some remarks about positionism: a reply to Collins and Yearley. In, Andrew Pickering (Ed.), *Science as practice and culture*, p. 327-342. London: The University of Chicago Press.
- Woolgar, S. (1996). Psychology, qualitative methods and the ideas of science. In, John T. E.

Richardson (Ed.), *Handbook of qualitative research methods for psychology and the social sciences*, p. 11-24. Leicester: British Psychological Society Books.

Yearley, S. (1981). Textual persuasion: the role of social accounting in the construction of scientific arguments. *Philosophy of the Social Sciences*, 11, 409-435.

Zingrone, N. (2002a). Controversy and the problems of parapsychology. *The Journal of Parapsychology*, vol. 66, 1, p. 3-30.

Zingrone, N. (2002b). Letter to the Editor. *The Journal of Parapsychology*, vol. 66, 2, p. 212-216.

Zingrone, N., Alvarado, C. (1987). Parapsychological Terminology. *The Journal of Parapsychology*, vol. 51, 1, p. 49-74.

Appendix 1

Interview protocol.

To start with, I would like to talk about what you do, as an experimental researcher, that is, about your work...

[Theme: experimental model; characterisation and assessment of type of research]

1. Within experimental parapsychology, what has your work mainly focused on?
2. What types of conceptual and experimental models or assumptions underlie your research work?
3. What do you see as their main strengths and their main weaknesses?
 - 3a. For example, to what extent do you think that they provide a useful basis for understanding the type of phenomena you're researching?

Given your research experience (with this type of model) ...

[Theme: unexpected and anomalous outcomes; dealing with anomalous outcomes]

4. Just to give me an idea, what would be an unexpected or anomalous outcome?
5. Generally, how would you describe the way you deal with anomalous outcomes in your work?
 - 6a. How should they be dealt with?
6. How flexible do you think your assumptions / model are in order to deal with anomalous outcomes?

Also in your research experience...

[Theme: experimental success/failure; unexpected success/failure; perception of the "field's" construction of success/failure]

7. What to you counts as a successful outcome?
 - 7.a What type of outcome would you say constitutes a successful outcome?
8. What counts as a failure?
 - 8.a What type of outcome would you say constitutes a failed outcome?
9. To what extent do you think your views on what is a successful outcome or failed outcome are

shared by your colleagues in the field?

10. Is there any type of outcome that you would consider an unexpected/surprising successful outcome?

10.a Do you have any examples of one from your research experience?

10.b In what sense would it be/was it unexpected?

11. Would you call that an anomalous outcome?

11.b Why?

OK, so how about the flip side of that...

12. Is there any type of outcome that you would consider an unexpected failure?

12.a Do you have any examples of one from your research experience?

12.b In what sense would it be / was it unexpected?

13. Would you call it an anomalous?

Why?

In relation to how you go about dealing with these sorts of outcomes...

[Theme: dealing with unexpected successful and failed outcomes]

14. When you had/if you had an unexpected success in an experiment, how do you deal with this outcome?

14.a What kinds of questions did it/ would it raise for you as an experimental researcher?

OK, and, the flip side of that...

15. When you had/if you had an unexpected failure in an experiment, how would you deal with this outcome?

15.a What kinds of questions does that raise for you, as a researcher?

Appendix 2

Parapsychological Association annual convention participant recruitment letter.



Dear Conference Delegate,

My name is Claudia Coelho, I am a PhD student at the Koestler Parapsychology Unit (University of Edinburgh). I am developing a study of how experimental parapsychology understand and respond to anomalies, in their own research and practice. I am attending this conference because I am keen to talk to parapsychologists with a psychology background and who are involved in experimental work. If you are one, I would greatly appreciate some of your time.

Parapsychologists, due to the nature of the research area, are well aware of the relevance and challenges raised by anomalous outcomes. How experimental parapsychologists understand and deal with anomalous outcomes is, therefore, of particular interest to any study looking into the problem of anomaly and exception in science. In order to do this, I need your help. I would like to interview you – this would normally take less than 60 minutes – about aspects of your experimental research work, such as, the conceptual model(s) you use, outcomes you obtain and expectations you hold. If you do wish to take part, we can set up a time that best suits you at any point during the four days of the conference.

If you have any questions about this research project, please don't hesitate to ask. Once again, your participation will be greatly appreciated.

Thank you very much for your attention, Claudia Coelho.

Appendix 3

Society for Psychical Research annual conference participant recruitment letter.



Dear Conference Delegate,

My name is Claudia Coelho, I am a PhD student at the Koestler Parapsychology Unit (University of Edinburgh). I am developing a study of how experimental parapsychologists understand and respond to anomalies, in their own research and practice. I am attending this conference because I am keen to talk to parapsychologists with a psychology background and who are involved in experimental work. If you are one, I would greatly appreciate some of your time.

Parapsychologists, due to the nature of the research area, are well aware of the relevance and challenges raised by anomalous outcomes. How experimental parapsychologists understand and deal with anomalous outcomes is, therefore, of particular interest to any study looking into the problem of anomaly and exception in science. In order to do this, I need your help. I would like to interview you – this would normally take less than 60 minutes – about aspects of your experimental research work, such as, the conceptual model(s) you use, outcomes you obtain and expectations you hold. If you do wish to take part, we can set up a time that best suits you at any point during the three days of the conference.

If you have any questions about this research project, please don't hesitate to ask. Once again, your participation will be greatly appreciated.

Thank you very much for your attention, Claudia Coelho.

Appendix 4

Example of email to participants recruited individually.

Dear Dr (),

My name is Claudia Coelho, I am a PhD student at the Koestler Parapsychology Unit, at Edinburgh University. I am developing, as part of my PhD research project, an interview based study focusing on how parapsychologists understand and respond to anomalies, in their own research and practice. This project is being supervised by Prof. R. Morris, and Dr Andrew McKinlay.

I began conducting these interviews during this year's PA and SPR conferences. Regrettably, although you were at the PA, I didn't get a chance to meet you. I'm writing now to ask you if it would be possible and if you would be interested in doing an interview with me at some point in early October.

The interview (with your permission, audio-taped) normally takes less than 60 minutes and focuses on aspects of your experimental research work (such as, conceptual model(s) you use, research outcomes, expectations) relating them to the occurrence or the possibility of occurrence of scientific anomalies.

If you would be interested in doing it, we could set up a time and place where I could meet you that would best fit you.

I'm sending in attachment the project's consent form, in which you can find a bit more detailed information about it. If you can and are interested in participating in an interview, your contribution would be greatly appreciated indeed.

I look forward to hearing from you,

All the best,

Claudia Coelho.

Appendix 5

Participant consent form.

Participant Consent Form

My name is Claudia Coelho, I am a Psychology Ph.D. student at the School of Philosophy, Psychology and Language Sciences, at the University of Edinburgh. I would like to invite you to participate in this study, which is part of my Ph.D. research project, being developed with the academic supervision of Prof. Robert Morris and Dr. Andrew McKinlay.

The project is an investigation into experimental parapsychologists' constructions of scientific anomalies. It is concerned with how parapsychologists describe their understanding of and their responses to anomalous outcomes, in their own research work.

This is a qualitative research project, using semi-structured interviews. If you agree to take part in this project, I will conduct an interview with you at a time and location of your choice. The interview will involve questions about aspects of your research work, such as the conceptual models you use, types of outcomes and expectations you have, relating these aspects to the occurrence, or the possibility of occurrence, of scientific anomalies.

The interview data will be later analysed through discourse analysis. The aims of the analysis in this project are, initially, to attempt to find patterns and differences in the way the interviewed researchers, a) describe and account for the characteristics of anomalies, and b) describe and account for their responses to anomalies. It will also, attempt to gain insight into fundamental conceptual and practical aspects of parapsychological experimental research. The interview lasts up to 60 minutes, *on average*, as this also depends on how much you want to contribute to it. *With your permission, I will audiotape the interview.* This will allow for its full transcription and enable a thorough and meaningful analysis.

The only person who will hear the interview's audiotape and produce the original transcript is me. I will make the transcript anonymous by attributing it a code designation. Thereafter, the only other person who will read the transcript is Dr. Andrew McKinlay, who will only see the transcript in its anonymous form.

Later, after the analysis is complete, I will use excerpts from the interviews in my Ph.D. thesis. In doing so, I assure you that all the content from this interview will be kept thoroughly anonymous. In I case I use it, I will make the excerpts from your interview as unidentifiable as possible. Furthermore, I will not use your name or other identifying information (e.g. academic affiliation) in any reports resulting from this research project.

Finally, the tape recordings, transcripts and notes on this interview will be stored safely and anonymously. After this research project is completed, I will save the tape recordings, transcripts and notes for other potential uses by me in future research work and publications. In any case, the same anonymity and confidentiality guarantees given here will apply to the storage and future use of the materials.

It is entirely up to you if, at any point, you wish to refuse to answer any questions or stop taking part in this interview. Also, if you wish to do so, you have full access to the transcript of the interview and, when it is ready, the resulting thesis. If you have any further concerns before, during or after the interview, please, don't hesitate to share them with me.

Although, I expect to conduct only one interview, additional clarification may be needed. If so, with your permission, *I may need to contact you later* by email or phone.

Again, if you have any questions about this research project don't hesitate to ask (contact details below). If you agree to take part in this research project, please sign this form below. Please keep a signed copy of this form for future reference.

I have read this consent form and agree to take part in this research interview.

Name _____ Date ____/____/2003

Signature _____

Thank you so much for your participation in this project.

Contact details:

Claudia Coelho, Psychology.

School of Philosophy, Psychology and Language Sciences

University of Edinburgh

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Appendix 6

Transcription notation.

The following features of talk were transcribed. This is a selection of notation conventions from the Jefferson system based, in turn, on the selections presented in: Atkinson and Heritage (1984: ix-xvi), Wooffitt, (1992:xi-xii) and Antaki, Billig, Edwards, Potter (2003).

- **Pauses:** numbers in parenthesis indicate elapsed time in seconds (2) or a dot in parenthesis indicates a tiny (less than 1 sec.) but noticeable pause (.)
- **Continuing speech:** (=) when there is no discernable gap between one utterance and another, and when the speech runs on.
- **Overlaps:** When two sections of talk overlap, square brackets ([]) are used before the overlapping sections. The overlaps between what is said and the signalling of agreement or understanding were not notated except when they were meaningful – e.g. when the participant pauses after the overlap to give the interviewer time to express her agreement or understanding.
- **Breathing:** (,h) indicates an audible intake of breath (the length of the row of h's indicates roughly how long it is); (h) indicates an audible outbreath.
- **Stretching of a sound:** (:) are used to indicate when a sound in a word is prolonged.
- **Cut off of a sound or word:** (-) a dash indicates the sharp cut to a prior word or sound.
- **Separation of a word into sections:** (-) dashes within words indicate that a word was cut into sections (syllables or not) – e.g. “extra-sen-so-ry”, to convey emphasis or clarity; or “c-c-complex”, to indicate hesitation or stammering.
- **Intonation:** (. , ? !) are used as they are in current text to indicate the speakers intonation.
 - ? indicates a raising inflection
 - ! indicates an animated tone
 - . indicates a natural pause
 - , indicates a breathing “comma-like” pause
 - £ denotes a phrase said with a smile
- **Emphasis:** the underscoring indicates some form of stress (either by volume or pitch) given to a word or fragment.
- **Loud:** capitals indicate louder sounds in contrast to the rest.
- **Inaudible sections:** () empty brackets are used when a word or sentence is inaudible (because of overlap or noise).

- **Unclear sections:** (word) when there are doubts about what a word or section is parentheses indicate the transcriber's possible hearing (the best guess) of that word or section.
- **Descriptions of behaviour or non-speech sounds:** (()) when describing sounds that add but are not part of "proper" speech double parenthesis are used ((laughs)) ((sniggers)) ((swallows)). A distinction was made between a "laugh" and a "snigger". The former is used to describe loud audible laughter, the latter, a lighter, less audible laugh, done mostly by expelling air through the nose.
- **Descriptions of events:** [] when describing events during the interview (e.g. a pause to light a cigarette) square brackets are used.
- **Additional information:** [[]] when it is deemed necessary to clarify a term or the subject of an ambiguous sentence, double square brackets are used.
- **Identifying features:** These are substituted by a description within square brackets (e.g. [the speaker's name]), or be deliberately omitted using empty brackets (e.g. []).
- **Other words:** there are a variety of sounds used in talk (uhm, eh, erm, ahm, hm-hm) which are notated as close as possible to the sound that is heard on the tape.