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Debunking and the Psychology of Error: A Historical Analysis of Psychological Matters

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*The psychology of the paranormal has always reflected wider themes about the nature and
status of psychology. This paper, in examining these themes, seeks to show how qualitative
research in the history of psychology can contribute to the understanding of psychological
topics, ones that have fundamental relevance to the discipline of psychology as a whole. The
paper examines the construction of psychological expertise by analysing criticisms of
unorthodox areas of psychological knowledge. It shows how psychological scientists have
long deployed certain rhetorical strategies, and that these have been designed not only to
reject certain claims but also to construct both their opponents as unscientific and, in the
process, themselves as scientific. Furthermore, through the construction and deployment of a
psychology of error, critics have warranted both the need and value of psychological
explanations for such beliefs and, in the process, constructed the superiority of scientific
expertise over public views about psychological matters.*

Introduction

While most psychologists recognise the worth of understanding past psychological
research, it can also be argued that history is essential to psychological understanding
(e.g. Graumann & Gergen, 1996; Richards, 2002; Smith, 2008). The need for
historical knowledge within psychology can be seen as following from the reflexive
nature of psychological knowledge, which is the product of people (because
psychologists are people too) who think and behave within a wider social context, and
whose knowledge is the result of thinking and behaving in a particular way that is

shaped by that context. Because psychological knowledge, in turn, also shapes how we think and behave (both as psychologists and people), we are constantly in the business of recreating over time both our understanding of ourselves (Psychology) and what we are as thinking and behaving beings (its subject matter). Thus, only through history can we understand how psychological knowledge has come to be what it is, and how it has shaped what we are.

However, if the history of psychology is to be a part of psychological research, rather than a related but separate area of enquiry that provides a useful, but ultimately unnecessary, historical background to contemporary ('real') psychological research, history needs to contribute directly to psychological as well as historical knowledge. To do this in practice, psychological topics need to be examined via methods that are available to psychologists, and which can be employed in the study of historical sources. Discourse analysis offers one practical solution to the problem of method (Lamont, 2007a). This paper seeks to address the problem of topic, by using discourse analysis to examine a topic of not only historical but also psychological relevance: the role of belief, as a discursive social phenomenon, in the construction of psychological expertise.

It is, after all, essential to the existence of the discipline of psychology that it is recognised as the most reliable source of psychological knowledge, i.e. superior to the claims of others who claim expertise on psychological matters and to the understanding of the wider public who also tend to have views about such things. Thus, for example, the argument is invariably made in introductory psychology texts that psychology is a science and, therefore, more reliable than unscientific theories and commonsense. What counts as science is, of course, a matter of consensus at any given time, and so there has been an ongoing attempt by psychologists to construct such a consensus, not only among psychologists and scientists but also among the public, who need to realise the limits of their own lay expertise. How the boundaries of scientific knowledge and expertise have been negotiated has been a significant theme in the history of psychology (e.g. Bunn et al., 2001; Coon, 1992; Derksen, 1997; Gieryn, 1983; Lamont, 2007a; Wolframm, 2006), and is a matter of fundamental importance to understanding the history of the discipline.

However, this is not only a question of social or historical context. It is in itself a psychological topic, one that might be considered in terms of persuasion or belief. Thus, to examine how psychologists have constructed, and continue to construct, the scientificity of their discipline, need not be viewed as an inherently critical approach. It might be seen as nothing more than a simple recognition that psychologists are also people. In the interests of warranting the worth of their discipline, they have sought to persuade others that what they do is science, that what others do is not, and that this distinction provides them with greater authority to pronounce upon psychological topics. In doing so, as we shall see, they have deployed psychological categories in order to persuade others. This, then, is a matter of discursive social psychology (cf. Edwards, 1997; Potter, 1996) that has fundamental relevance to the discipline of Psychology. It is also one that stresses the need for historical understanding, as history allows us to examine psychological discourse over time, and to consider the significance of continuity and change. And, as we shall see, while changes in psychological categories may seem of more obvious significance (e.g. Danziger, 1997; Smith, 2005), there are also rhetorical continuities that are relevant to psychological understanding.

Indeed, what I hope to show is that psychological scientists (i.e. scientists with an interest in psychological matters, including those prior to the emergence of academic psychology) have long deployed certain discursive strategies in ways designed to achieve very similar ends. In doing so, I wish to show how historical analysis can address matters of contemporary psychological interest, by showing how psychological scientists have constructed themselves as authoritative in relation to both rivals and the wider public, and how they have in the process regularly deployed a psychology of error (in particular, of false belief). In order to make the case that there have been continuities in the form this has taken, the extracts analysed below are taken from a wide range of periods and contexts. This is not to suggest for a moment that the form of discourse is not shaped by the particular context in which it occurs; rather, it is to focus on those similarities in discourse that have oriented towards very similar matters of context, albeit at different periods of history. After all, while the extracts that follow appeared in quite different historical contexts, all of them addressed a similar rhetorical context, one in which rival claims about psychological

matters were in dispute. And, despite differences in time and place, these disputes were similar in certain key respects.

First, the dispute was always ostensibly about the reality or not of certain 'unexplained facts'. Proponents argued that there were observed facts that could not be explained by current scientific knowledge, and that these were facts despite the lack of a scientific explanation. Critics argued that there were no unexplained facts, that the observation was inadequate and that, therefore, existing theories (relating to deception and self-deception) were adequate to explain what had really happened. Disputes about unorthodox phenomena, so far as they dispute the reality of the phenomena, have invariably taken this form. Second, however, in disputing the facts, the discussants also disputed matters of psychological expertise and authority. Indeed, this was unavoidable, because these disputes rapidly became disputes over perception and belief. Proponents argued that they were competent observers, and denied accusations that they were driven by prior beliefs or by a desire to believe. Conversely, critics claimed superior competence in observation as they themselves denied accusations of being driven either by prior beliefs or by a refusal to believe. Finally, these disputes over the facts were bound up with disputes not only over individual expertise but also over wider issues relating to science and society. Proponents argued that such facts, though unexplained, were nevertheless compatible with science, and that knowledge of them was beneficial for society. Critics, as they rejected the facts, argued that they were incompatible with science, and that beliefs in them were detrimental for society (Lamont, 2008). These consistent patterns of argumentation, though they have been deployed in particular ways at particular times, suggest an ongoing rhetorical context within which one might examine the role of belief, as a discursive social phenomenon, in the construction of psychological expertise over time.

Data and methods

The data used in this analysis are drawn from a range of publications by those I have termed psychological scientists, namely, John Forbes, William Benjamin Carpenter, Joseph Jastrow, E. G. Boring, and James Alcock. Some individual background details

will be given prior to the relevant extracts, but the key point is that, whatever differences might separate the individuals themselves, all of the extracts below come from texts that shared at least three features. First, they all explicitly argued against the reality of phenomena that were generally viewed, by both proponents and critics, as unexplained by contemporary scientific knowledge. Second, they were all explicitly aimed at a non-specialist audience. Third, they are all well known texts in the history of debunking – indeed, their significance is evident from the fact that earlier texts are cited by subsequent authors (for example, Alcock cites Hansel, Hansel cites Jastrow, Jastrow cites Carpenter and Carpenter cites Forbes).

The extracts were chosen as examples of more common themes that recur throughout such publications. The form of discourse analysis resembles that used previously with naturally occurring written texts (e.g. Macmillan & Edwards, 1999; Wallwork & Dixon, 2004; Lamont, 2007a), in that it focuses primarily on rhetorical strategy. In this case, the specific focus was on how authors engaged in the debunking of the reality of certain phenomena by making use of a discourse of expertise and belief. For reasons of space, the analysis will be restricted to the core argument, which is concerned with the presence of certain themes, and some of the extracts are slightly abridged.

Analysis

The first extract comes from a book entitled *Illustrations of Modern Mesmerism from Personal Investigation*, written by John Forbes in 1845. Forbes was editor of the *British and Foreign Medical Review*, the most widely read medical journal in Britain at the time, and a prominent debunker of mesmerism. Though a medic by profession, his criticisms of mesmerism, not surprisingly, engaged in psychological discourse, and his book was regularly cited by later sceptics. In the preface, he explained the purpose of his *Illustrations*:

'If received simply as specimens or illustrations of the sort of things which mesmeric professors daily hold forth to the world, and which the world receives, as marvels of the highest order and as truths admitting of no question, they must surely give rise to

reflections that may lead to some beneficial results. … If the professors do not condescend to supply the public with evidence of a more satisfactory kind, the public must cease to be satisfied with the evidence they do supply … If they refuse to adopt the rigid system of observation required in the sciences, and repudiate all the ordinary rules of induction and rational inference deemed essential to establish facts on other departments of knowledge, they have no right to quarrel with those who persist in disbelieving [things] which, for the most part, have no other evidence in their favour than the bare assertions of ignorant, interested and, it may be, very unprincipled persons … It is also hoped that the perusal of the exposures contained in this little book, may teach a useful lesson to those numerous unscientific persons, who are accustomed to attend mesmeric exhibitions … Such persons, it is believed, must now feel convinced that no reliance whatever is to be placed on the results presented at such exhibitions, as evidencing the truth and powers of mesmerism. As these results are witnessed by the ordinary visitor, it is quite impossible to discriminate the true from the false (Forbes, 1845: vi-ix).

These extracts from the preface, which ostensibly explain the purpose of the book, also reveal a number of discursive themes. First, the author presents the book as an attempt to show that the facts of mesmerism are false: the evidence is less than 'satisfactory'; the rules 'deemed essential to establish facts' have been repudiated; and the reader should place 'no reliance' upon them. Second, in doing so, he presents mesmerists as lacking basic scientific expertise: they are quacks ('professors', the term then having been used regularly to refer to itinerant lecturers with no formal qualifications); they do not use the 'rigid system of observation required in the sciences'; they are uninformed ('ignorant) and biased ('interested'). Third, in his rejection of both the facts of mesmerism and the expertise of mesmerists, the author presents himself as scientific: as one who can assess whether evidence is 'satisfactory', can recognise whether scientific procedures have been employed, and can 'teach a lesson' to 'unscientific persons'. Fourth, the author constructs his own expertise over not only the mesmerists but also the public: who receive such facts 'as marvels of the highest order and as truths admitting of no question'; and who, as 'ordinary' people, cannot 'discriminate the true from the false'. Thus, as he constructs boundaries between the true and the false, he also does so between his own scientific expertise and that of not only mesmerists but also the public.

It is also notable that the false beliefs of the public in mesmerism are attributed not only to the mesmerists but also to the limits of ordinary thinking and observation: to the gullibility of the world (which accepts marvels without question) and to the unreliable observation of the 'ordinary' witness. Furthermore, the challenging of such beliefs is presented as having wider social and moral value: they are based on evidence presented by those who are not only 'ignorant' and 'interested' but also 'it may be, very unprincipled'; and more critical reflection is described as being 'useful' and liable to produce 'beneficial results'. Thus, the author rejects the phenomena in question by constructing a psychology of error that warrants not only the superiority of his own scientific expertise over that of mesmerists and the public, but also its deployment as necessary and valuable. As we shall see, all of these themes recur in subsequent debunking discourse.

Forbes' debunking of mesmerism appeared shortly before the advent of Modern Spiritualism, after which some scientists came to take an interest in the phenomena of the séance room. One of the most important was, William Crookes, a well-known chemist and Fellow of the Royal Society (and later its President), who famously tested the medium, D. D. Home, and came to the conclusion he had discovered a new 'psychic force'. One of the witnesses was William Huggins, vice-President of the Royal Society. Crookes' experiments were published in the *Quarterly Journal of Science* in 1871, a journal he himself edited, along with a supporting letter from Huggins, and the rhetoric of his articles was significant (Lamont, 2004; Luckhurst, 2001; Noakes, 2004). Indeed, discourse analysis of Crookes' articles (Lamont, 2007a) has shown how, by constructing a version of science that made relevant the importance of accurate observation and the absence of preconceptions about what is possible, Crookes presented himself as an ideal scientist who was both an expert observer (in contrast with both spiritualists and the public), and an open-minded thinker (in contrast with those scientists who refused to enquire into the subject). By presenting spiritualist beliefs as wrong and harmful, Crookes constructed the scientific study of spiritualism as necessary and valuable.

However, while Crookes was in the business of warranting the reality of his new psychic force in a scientific forum, his key critic, William Benjamin Carpenter,

responded in a more public arena. Carpenter was a well-known psycho-physiologist, a Fellow of the Royal Society and winner of its gold medal. He had been a critic of mesmerism, had proposed a theory of 'ideo-motor action' to explain table-turning at séances in 1852, and had become the most prominent scientific critic of spiritualism in mid-Victorian Britain. His response to Crookes, which appeared in the *Quarterly Review* in 1871, did not question Crookes' claim that the phenomena of spiritualism were worthy of scientific investigation, accepting that science should be based on a lack of preconceptions. Rather, he rejected the charge of narrow-mindedness, noting that 'it was only after [repeated failure] that we, and our scientific friends associated with us, abandoned the pursuit, as involving a waste of time that might be profitably employed upon worthier objects of investigation'. Neither, of course, did he undermine the importance of scientific expertise. Rather, he distinguished between general and specialist scientific expertise, claiming that:

'a man may have acquired a high reputation in one department of science, and yet be utterly untrustworthy in regard to another. This is what not merely the general public, but men who claim to guide its judgements, seem unable to understand. Any 'scientific man' is properly supposed to be a competent authority upon obscure questions, for the elucidation of which are required discrimination and acute discernment of the sources of fallacy, which can only be gained by a long course of experience, based on special knowledge, particularly when the enquiry is psychical, rather than physical, and involves a knowledge of the modes in which the Mind of the observer is liable to be misled either by himself or by the arts of an intentional deceiver … there are *moral* sources of error, of which Dr Huggins, with his simple trustingness, would never dream, and that one of the most potent of these is a proclivity to believe in the reality of spiritual communications, which places those who are not constantly on their guard against its influence under the two fold danger of deception – alike from *within* and from *without*.' [original italics] (Carpenter, 1871: 340-342).

In this passage, Carpenter engages in similar rhetorical strategies to those used by Forbes. First, he warrants his rejection of spiritualist phenomena by presenting scientific proponents as lacking proper scientific expertise: he distinguishes between expertise in 'one department' and 'another'; argues that competence in this area

requires 'special knowledge'; and presents such competence as requiring knowledge about sources of error of which Huggins 'would never dream'. Second, in doing so, he presents himself as possessing such expertise: as one who is aware of such sources of error, who can publicly remark upon Huggins' ignorance of these, and who knows of the dangers of the 'proclivity to believe'. Third, he presents his own expertise as superior not only to scientific proponents but also the public, who are 'unable to understand' such distinctions, whose minds are 'liable to be misled', and who, without 'a long course of experience based on special knowledge', are not 'competent authorities'. Thus, as above, in constructing boundaries between the true and the false, he also does so between his own scientific expertise and that of his opponents and the public. Furthermore, his rejection of the phenomena is also bound up with a psychology of error in which beliefs in spiritual phenomena are not only wrong (being based on sources of fallacy – indeed, a tendency to believe is itself a source of error) but also a 'moral' issue and a 'danger' against which one must be constantly 'on their guard'. Thus, as above, in constructing his own scientific expertise in such matters as superior to that of his opponents and the public, Carpenter warrants its deployment as necessary and valuable.

It is worth noting that Carpenter went on to publish a number of articles on the psychology of belief (Carpenter, 1873, 1876, 1877), in which these themes recurred, and in which they were spelt out more explicitly. In doing so, of course, his descriptions of what counted as proper scientific expertise, of the causes of belief and its implications, and of the need and value of a psychology of belief, were bound up with wider themes within contemporary mid-Victorian discourse about the nature of scientific and religious evidence, which had been prompted by Biblical criticism and Darwinism, and had led to a growing distinction between external (scientific) evidence and the internal evidence of revelation (Cerullo, 1982; Lamont, 2004; Oppenheim, 1985). However, we are concerned here with continuities in rhetorical strategies over time, rather than with differences between different historical contexts. As noted above, while the social context within which these arguments took place clearly changed significantly over time, the rhetorical context was similar in that it remained that of a psychological scientist engaging in the debunking of 'unexplained facts' within the public arena.

Such was also the case with Joseph Jastrow, the first American to obtain a PhD in Psychology, who went on to found one of the earliest Psychology departments in the United States, at the University of Wisconsin in 1888. He was one of the most prominent critics of spiritualism and psychical research in late nineteenth century America, writing several articles on these topics, many of which were aimed at a general rather than a scientific audience. The extract below comes from a brief article he published in *Popular Science Monthly* entitled 'The Psychology of Spiritualism':

'it is likely that the marvels of spiritualism will be, by believers in them, incorrectly and insufficiently reported. The first reason is to be found in the mental condition of the observer; if he be excited or deeply moved, his account can not but be affected, and essential details distorted … The fact that scientific examination everywhere reveals deception makes it extremely probable that, when exposure has not taken place, it is because there was no scientific examination … Let him understand that under the shelter of spiritualism men and women in all our large cities are daily and hourly preying upon the credulity of simple-minded folk, and obtaining money by means for which the law provides the jail' (Jastrow, 1889: 730-732).

As we can see, Jastrow rejects the phenomena of spiritualism as the product of deception and self-deception, and the reported evidence as the result of incorrect and insufficient reporting. In doing so, he presents those who fail to detect this as being unable to conduct a 'scientific examination', and himself as one who not only recognises a proper scientific examination but also understands the psychological reasons for beliefs in such phenomena. His expertise is presented as superior not only to those unqualified to conduct a scientific examination but also more generally to 'simple-minded folk' in 'all our large cities'. His rejection of the phenomena is also bound up with a psychology of error in which such beliefs are not only wrong (based on 'essential details' being 'distorted') but are themselves the result of a 'mental condition' that caused such distortion. Such beliefs are also presented as a moral issue in which the credulity of the public is 'preyed upon' by those who are in the business of 'obtaining money' by illegal means. Thus, Jastrow, as others before him, rejects the reality of the phenomena by constructing not only the unscientific status of proponents of such phenomena but also his own scientific expertise over them and the

wider public, and warrants the need and value of his own psychological expertise by presenting such beliefs as both wrong and harmful.

The fourth example is E. G. Boring, the well-known historian of psychology, and director of the psychological laboratory at Harvard from 1924 to 1949. In addition to other countless publications, most notably his *History of Experimental Psychology* (1929), Boring wrote the introduction to C. E. M. Hansel's *ESP: A Scientific Evaluation* (1966), a well-known critique of parapsychology's claims about the reality of psi. In that introduction, Boring wrote:

'there are insidious logical weaknesses when concepts of probability are applied to the validation of ESP. This introduction is, however, not the place to discuss the details of this complex problem; yet no harm can have been done in exhibiting the rather special dissonance to which some keen thinkers adhere without realising how insecure is the support these elaborate statistics and gigantic probabilities in parapsychology afford *
* There is an extensive literature on the application of the theory of probabilities to empirical data, and the reader can enter it by way of the studies cited (Boring, 1966: xx).

In this brief extract, Boring rejects the validity of ESP as a claim based on 'logical weaknesses' and insecurely supported statistics. In doing so, he presents parapsychologists as lacking appropriate scientific expertise, being guilty of such logical weaknesses in their application of concepts of probability, and not realising how insecure their evidence is. At the same time, the author constructs himself as possessing the appropriate scientific expertise, being one who recognises such weaknesses and insecure evidence, and who is knowledgeable about 'the details of this complex problem'. His expertise is constructed as superior not only to parapsychologists but also to 'the reader', who can 'enter' the 'extensive literature' by way of studies the author has provided. This rejection of ESP is also bound up with a psychology of error, in which believers exhibit 'special dissonance' to which they 'adhere without realising' the weakness of their case. Such thinking is not only wrong but 'insidious', while exhibiting such flaws in thinking can do 'no harm'. Thus, in his rejection of ESP, the author constructs his own expertise over parapsychologists and

the lay reader, and warrants the need and value of such expertise by presenting such beliefs as wrong and harmful.

The final example comes from James Alcock's book, *Parapsychology: Science or Magic?* (1980). Alcock has been a prominent critic of parapsychology in recent years, and his book has been one of the best-known critiques of the field. In the conclusion, he writes:

'To the extent that existential fears serve to make parapsychology attractive as a kind of surrogate faith, paranormal belief will continue to flourish in one form or another. Yet, as I pointed out in Chapter 2, I do not believe that this is the only or even the major reason for belief in the paranormal. Much of this belief is based either on a negative reaction to the mechanistic view of humankind that science seems to provide, or conversely, on the view that scientific evidence supports the claims for the existence of psi. We can do something about these two factors. Scientists need to educate the public about science' (Alcock, 1980: 195).

Thus, Alcock rejects the claims for the existence of psi by presenting those attracted to parapsychology as unscientific people (who are driven by 'existential fears' to seek 'a surrogate faith'), and himself as someone with proper scientific expertise (who can 'point out' the various reasons for belief, and can explain what scientists can and need to do). His expertise is constructed as superior not only to parapsychologists but also the public, whose 'view that scientific evidence supports the claims for the existence of psi' means that they need to be educated about science. His criticisms of parapsychology are bound up with a psychology of error in which paranormal belief is not only wrong but also based on a 'negative' view of science, and is something that scientists 'need' to do something about, thus warranting the worth of his own psychological expertise.

Discussion

There is, of course, a significant body of literature on the psychology of belief in general, and on paranormal belief in particular (see Irwin, 2009). In the provision of

psychological explanations for belief in the paranormal, psychologists may dismiss paranormal claims, and they may present themselves as experts on the subject. That these themes are compatible is hardly surprising. The argument here is that, in the process of rejecting certain claims, psychological scientists have constructed their expertise over both proponents and the wider public, and have deployed a psychology of erroneous belief in the process. The latter has not been a disinterested attempt to understand why people believe in certain phenomena but has itself been part of the argument against the reality of the phenomena in which people believe. It has also been a way of warranting the need and value of psychological expertise to a non-specialist audience. In short, debunking has been used as an opportunity to persuade others of the importance of psychological knowledge, and the discursive deployment of a psychology of erroneous belief has been key to this.

As a historical argument, this offers a quite different interpretation of events from that of others. For example, Coon (1992) has argued that early American psychologists, in defence of their newly born science, engaged in combat with psychical research, by testing psychic claimants and by creating a new area - the psychology of deception and belief. Wolframm (2006) has made similar claims about early German psychology. However this view fails to consider the following points. First, what counted as psychology or psychical research was itself part of the argument. Thus, so far as 'psychologists' engaged in the testing of psychic claimants, far from combating psychical research, they were, by definition, doing psychical research. Second, a psychology of deception and belief was neither new (e.g. Carpenter, 1873) nor an 'area' in any meaningful sense. After all, while a few eminent psychologists published articles on deception and belief at the end of the century, they appeared largely in non-specialist journals and in such an ad hoc fashion that Norman Triplett, when he published a doctoral dissertation on the psychology of deception in 1900, was not even aware that Joseph Jastrow had published on the topic just a few years earlier (Triplett, 1900). Thus, Jastrow's publications, rather than being part of a new area of psychology, are better understood as part of an ongoing discourse of debunking, one in which individuals deployed a psychology of belief in ways designed to construct the worth of their expertise.

A more conventional historical article could have made similar points, perhaps stronger claims, and no doubt in a more eloquent narrative form. However, it is hoped that by adopting a form closer to that used by discourse analysts, there are certain advantages. First, by providing extracts, rather than isolated quotes within a general historical narrative, it can be shown more clearly that these distinct but related rhetorical moves were intricately bound up together. Second, by providing an open analysis of these extracts, it allows the reader to assess the analytical process and, therefore, to agree or disagree with the interpretation. Such open analysis, which is not employed in a typical historical narrative, thus allows for reader evaluation to act as a form of reliability (Potter, 1996). Third, by making the argument in this form, it may be more familiar to colleagues within psychology and, it is hoped, more convincing as a psychological argument.

In making this psychological argument, one must acknowledge that there is a significant difference between using sources from different historical periods and drawing upon contemporary data, but it has been argued that, while the historical contexts clearly varied, the rhetorical context was similar in those respects most relevant to the analysis. In drawing upon a range of historical sources, one might be more open to the accusation of 'cherry-picking', but those familiar with the historical sources relating to disputes over psychic phenomena will know that the rhetoric of debunking has shown remarkable consistency over time, and that very similar arguments have long been deployed in disputes over unexplained phenomena (e.g. Hess, 1993; Lamont, 2008; McLenon, 1984; Wallis, 1979; Wooffitt, 1992; Zingrone, unpub'd). For example, what might be called the Galileo argument - 'they laughed at Galileo too' - was being employed by mesmerists (and refuted by critics) in the 1840s, and is still being deployed by proponents of controversial claims (and refuted by sceptics) to the present day (e.g. 'Animal Magnetism', 1838; Sagan, 1979). That there has been continuity in the form of argumentation might suggest that subsequent generations have simply drawn upon their predecessors. However, that these debunking attempts were all designed in a way that constructed expertise and deployed a discourse of belief does not mean that later attempts were drawing upon earlier ones in their construction and deployment of these categories. Indeed, the form in which these strategies were used, and the degree to which they were intricately bound up together suggests otherwise. Rather, these debunking attempts can be seen

as examples of how psychological categories can be, and have been, discursively deployed in ways designed to persuade others of the worth of psychological expertise.

In a broader sense, what this analysis seeks to show is that historical analysis can contribute to understanding of psychological matters that relate to the place of psychology as a discipline. Whatever the cognitive status of beliefs, the expression of belief is a social phenomenon (cf. Edwards, 1997; Potter, 1996), and expressed beliefs about the paranormal, like descriptions of the paranormal, are produced in ways that are functional within particular social contexts (Lamont, 2007b; Wooffitt, 1992). However, a discourse of belief can also be deployed as part of a wider social argument that is designed to construct expertise in psychological matters. This deployment of psychological expertise can be seen, in turn, as being designed to persuade an audience that such beliefs are wrong and harmful, and that certain practices (such as attending demonstrations of psychic phenomena, or the scientific study of them) are equally so. In this sense, these authors were engaging in the deployment of psychological expertise in a way that was designed to change thought and behaviour. There is, of course, nothing surprising about this; indeed, it could be seen as the main purpose of much of applied psychology. Nevertheless, that psychological knowledge not only represents thought and behaviour but also, in representing its objects of study, is part of the process through which they change over time, is one reason why historical analysis is essential to psychological understanding (e.g. Danziger, 1997; Richards, 2002; Smith, 2007). But history is about continuity as well as change, and this analysis suggests that there are also continuities in how psychological scientists have not merely represented psychological phenomena but have discursively constructed and deployed them in ways that have been designed to achieve particular social ends. What history offers is a way of examining how both continuity and change in the form and function of psychological discourse have shaped both the discipline and its subject matter.

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