Psychokinesis and the Philosophy of Science

The incident took place at the home of Lady Poulett. Mr Home was there. We all saw the supper table, on which there was a quantity of glass and china full of good things, rise to an angle of 45 degrees without anything slipping in the least, and then relapse to its normal position. There was also a centre-table in the room, round which we were seated, and as we joined hands it moved and we followed it. There was Baron Reichenbach, the discoverer of paraffin, present, who laughed at us, and challenged us to move the table if we would let him get under it and hold it. He was a rather tall and powerfully-built man, and he got under the table and clasped it with both his arms, but it moved as before, dragging him all round the room'. (From a first-hand account.)

Levitations, materializations, healings, earthquake-like rocking. The evidence of large-scale spontaneous psychokinesis (PK) throughout history is a neglected and unjustly maligned aspect of the science of parapsychology. In The Limits of Influence, Stephen E. Braude argues that the familiar card-guessing experiments and statistical 'proofs' of psi will never elicit important or revealing facts about the phenomena. Instead, he turns our attention to the substantial (if sometimes bizarre) qualitative evidence. Not only does this pose no threat to the fabric of science, but it holds out hope for substantial progress in psychology, as well as in the philosophy of science and the philosophy of mind — which so far have failed to solve fundamental problems of consciousness and intentionality.

The cases of physical mediumship, associated with the rise of spiritualism at the turn of the century, provide a rich mine of historical documentation, especially the cases of D.D. Home and Eusapia Palladino. In the light of the materializations exhibited by mediums, Braude compares and evaluates the leading theories of apparitions and considers the idea that collective apparitions are a further type of psychokinetic phenomenon. Through a discussion of theoretical approaches to PK, Braude exposes the poverty of mechanistic theories of the mind. The Limits of Influence concludes with an analysis of the evidence for precognition as a manifestation of PK, in the process presenting a novel reason for rejecting the possibility of retrocausation.

'Although psi might not make us gods, we might be more powerful than we realize. But with that power, of course, comes responsibility for the things it can achieve'. (From the Conclusion.)

'Braude's study offers a load of evidence from a fresh quarter showing how difficult it is to get an honest hearing for deviant phenomena and how compliant the model of science actually is. I'm delighted to see it in print. Braude is surely one of the best-informed and most readable philosophical guides, and his book offers the most orderly entry into the inevitable quarrels. The anecdotal material is absolutely fascinating. But Braude's careful speculations about its bearing on causality and empirical confirmation are even more intriguing and much more important.' — Joseph Margolis, Temple University
The Limits of Influence
Psychokinesis and the Philosophy of Science

Stephen E. Braude

ROUTLEDGE & KEGAN PAUL
New York and London
For my parents
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This book documents, among other things, my growing disenchant-ment with the intellectual community. Not too long ago, before I began to investigate the evidence of parapsychology, I still believed that intelligence was a weapon in the war against evil, that my colleagues in academia (especially in philosophy and science) were committed to discovering the truth, and that intellectuals would be pleased to learn they had been mistaken, provided the revelation brought them closer to this goal. I now realize how thoroughly naive I was.

Since dipping into the data of parapsychology, I have encountered more examples of intellectual cowardice and dishonesty than I had previously thought possible. I have seen how prominent scholars marshal their considerable intellectual gifts and skills to avoid honest inquiry. I have seen how intelligence can be as much a liability as a virtue – in particular, how it sometimes affords little more than complicated ways of making mistakes, entrenching people in views or opinions they are afraid to scrutinize or abandon. I have seen, in effect, how intelligence often expands, rather than limits, a person’s repertoire of possible errors.

I have also come to realize that members of academic and other professions tend to be strikingly deficient in the virtue that, ideally, characterizes their field. I have seen how scientists are not objective, how philosophers are not wise, how psychologists are not perceptive, how historians lack perspective – not to mention how physicians are not healers, and how attorneys are not committed to justice.

Some of my revelations (however long overdue they may be) spring from personal experiences. I have observed with amaze-
ment – and, I suppose, ill-concealed disdain – how academics are able to proclaim confidently that the evidence of parapsychology is insubstantial, and then display that they don't even know what the evidence is – a lacuna about which they could hardly have been unaware. I have seen college professors, and also a well-known, ambitious, and (I believe) thoroughly unscrupulous magician move from relative obscurity to considerable notoriety, by cultivating reputations as debunkers and defenders of clear-headedness – in large part through their sedulous avoidance of evidence they know they cannot explain. More disappointing still, I have discovered from my investigations of nineteenth-century mediumship that none of this is new, and that prominent intellectuals have been behaving in these dishonest ways all along.

I must add, however, that there is a further and somewhat embarrassing personal reason for the present clarity of my perceptions. Frankly, I cannot pretend always to have achieved the sublimity of thought whose absence I criticize in my colleagues. Some of what I now understand about the varieties of intellectual dishonesty and cowardice I owe to having observed them in myself; they are demons with whom I am intimately acquainted. For many years I was content to dismiss reports of ostensibly paranormal phenomena as, at best, the result of confusions or delusions of various sorts. Of course, I hadn't bothered actually to read any of the evidence and assess it for myself. My opinions were fashioned after those of my mentors, who (I later learned) were equally ignorant of the evidence, but with regard to whom I was too insecure and intimidated to display much independence of thought (especially on a matter that so easily provoked their derision). Even after I became tenured and finally began to study the experimental evidence of parapsychology, I continued to accept uncritically the received view that laboratory evidence was inherently cleaner, and more respectable and reliable, than the non-experimental evidence.

Admittedly, I had no idea at the time how few of those who promulgated this wisdom had bothered to examine the latter body of evidence with any care. In fact, only recently have I come to appreciate how few parapsychologists are familiar with the material. But at no point along the way was my ignorance benign; it was, in fact, a lazy and craven expedient. For one thing, it facilitated the disgracefully scornful attitude I occasionally
Preface

adopted, initially toward parapsychology in general, and then later toward those who defended the non-experimental evidence. For another, it simply reinforced the complacency with which I held my beliefs. Even after I began to study the evidence of parapsychology and develop a respect for the field and its data, it allowed me to remain smugly comfortable with my moderate radicalism. I made no effort to examine the non-experimental evidence for myself; I was content not to have to admit into my universe phenomena that seemed to me bizarre and frightening (both personally and professionally). Of course in my heart I knew what I was doing. But at that stage in my career I lacked the courage to challenge, not only an increasing number of orthodox academicians, but also the majority of active parapsychologists. Because of my sympathetic interest in parapsychology, my alliance with the former was in a state of flux — collapsing in some places and solidifying in others; and I was insecure about its future. And my alliance with the latter was new, and presumably fragile.

I have now spent more than five years carefully studying the non-experimental evidence of parapsychology — in fact, just that portion of it which is most contemptuously and adamantly dismissed by those academics who all along have been blithely ignorant of the facts. I started with the expectation that the received wisdom would be supported, and that my belief in the relative worthlessness of the material would merely be better-informed. But the evidence bowled me over. The more I learned about it, the weaker the traditional skeptical counter-hypotheses seemed, and the more clearly I realized to what extent skepticism may be fueled by ignorance. I was forced to confront the fact that I could find no decent reasons for doubting a great deal of strange testimony. It became clear to me that the primary source of my reluctance to embrace the evidence was my discomfort with it. I knew that I had to accept the evidence, or else admit that my avowed philosophical commitment to the truth was a sham.

I am hardly comfortable about announcing to my academic colleagues that I believe, for example, that accordions can float in mid-air playing melodies, or that hands may materialize, move objects, and then dissolve or disappear. I have taken abuse and ridicule for the far more modest opinions expressed in my previous book. But I have reached my recent conclusions only after satisfying myself that no reasonable options remain. Actually, I
Preface

find that my discomfort tends to diminish as I discern more clearly how little the most derisive and condescending skeptics really know about the evidence, and how their apparent confidence in their opinions is little more than posturing and dishonest bluffing. In fact, I am less comfortable about stating my present views on parapsychology than I am about confessing how my intellectual independence was won, in part, through learning not to respect my colleagues.

It is no accident, then, that the tone of this book is occasionally polemical and antagonistic. In the past, those who defended the evidence for large-scale psychokinesis have too easily allowed themselves to be put on the defensive. In my opinion they have responded too timidly — or graciously — to their most vocal opponents, especially to those motivated more by the love of publicity than by the love of knowledge. I believe, however, that the skeptic must be put on the defensive. The more evangelical of the lot inveigh against the forces of irrationalism. But I believe that their greatest enemies might be full information and an open mind. It is a simple (and often profitable) matter to be a professional skeptic about parapsychology, especially when one suppresses the best cases and perpetuates misconceptions among those who know even less about the field. I hope, therefore, to inject some relevant data and clear reasoning into a debate where those commodities have been in short supply. I believe that the evidence I present will seem respectable — if not coercive — to anyone without a scientific or metaphysical axe to grind. And I hope that my discussion will make it more difficult for the self-styled debunker to dismiss the evidence with feigned confidence, bogus or irrelevant facts, and facile arguments.

The empirical part of this book deals primarily with a rather restricted portion of the total evidence for psychic (now called ‘psi’ or ‘paranormal’) phenomena — namely, the evidence from physical mediumship. A medium is an ostensible psi agent, who purports to be an intermediary between the spirit world and this world. A mental medium is a medium who apparently transmits and receives communications from the world of spirits. A physical medium is a medium in whose presence psychokinesis (PK) is observed.

I shall take no stand here on whether mediums really are what they purport to be. Although I shall make some comments on the
topic of survival in Chapter 3, I shall postpone an in-depth treatment to a later occasion. This book is concerned primarily with psychokinesis – particularly, large-scale PK – and its significance for the world of souls incarnate. I want to challenge certain widespread beliefs about the nature and quality of the data, and consider the implications of a sensible appraisal of the evidence for familiar views about the world. Many commentators (parapsychologists included) have been ignorant of the full evidence for large-scale PK, although that has not deterred them from expressing firm opinions on the subject and promulgating various standard misconceptions. And many others have simply been confused. Their assessments have been colored by unwarranted assumptions about the possible range or efficacy of PK (and psi phenomena generally), as well as the utility of laboratory experimentation. And they have misunderstood the importance of psi phenomena for our received body of scientific knowledge.

My first task, then, will be to compare the evidence for large-scale PK to the allegedly superior laboratory evidence. In the process, I shall expose the defects in the principal skeptical arguments against the former body of data, and argue that psi phenomena do not pose a clear or serious threat to the fabric of science. Then, in Chapter 2, I shall examine in detail some of the best evidence for PK, paying particular attention to the cases of D. D. Home and Eusapia Palladino. In Chapter 3 I survey the leading theories of apparitions, and consider the extent to which the evidence – especially the evidence for collective apparitions – can be explained by an appeal to PK. Chapter 4 examines the central issues in PK theory, and contains a sustained critique of mechanistic theories of action and cognitive phenomena. Finally, in Chapter 5, I consider the viability of explaining the evidence for precognition in terms of real-time large-scale psi, and present reasons for rejecting the possibility of retrocausation.

Although I am writing primarily for philosophers and parapsychologists, this book (like my last) is intended for a diverse audience. I would like it to be useful to the broad range of academics, students, and educated laypersons who are unfamiliar with the evidence, and who have innocently (or not so innocently) accepted the many received misconceptions about it. But certain sections of the book presuppose familiarity with background philosophical issues, while others will be most fully appreciated by those
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acquainted with the problems and data of parapsychology. Since the topics I address should interest readers in many disciplines, I have tried to make the book as generally accessible as possible without making it trite. Nevertheless, it is inevitable that some will find parts of it too advanced, elementary, or cursory. Unfortunately, I know of no way to maximize the book’s utility and avoid this problem. I can only hope that readers will be tolerant of passages tailored for those at some other level of philosophical or parapsychological sophistication.

One of my major tasks in preparing this book was to locate and digest the mountains of obscure and out-of-print documents on physical mediumship. I would like to express my gratitude to those who helped me to acquire the relevant materials, and who guided me through the imposing maze of cases. In particular, I wish to thank Carlos S. Alvarado, Rhea A. White, George Zorab, Eleanor O’Keeffe, and Patrice Keane. For their valuable comments on portions of the manuscript, I wish to thank John Beloff, G. Lee Bowie, Alan Gauld, and Michael Martin. And I am especially grateful to Jule Eisenbud and Bruce Goldberg for their stimulation, encouragement, and conceptual guidance. Finally, I must express my debt to the late Laura A. Dale, whose research assistance and criticism fueled my early enthusiasm for this project, and whose ideals of clarity and accuracy I found both intimidating and inspiring. Laura would probably have disagreed with some of my views; but I hope my scholarship would have received her blessing.
1

THE IMPORTANCE OF NON-EXPERIMENTAL EVIDENCE

1.1 INTRODUCTION

The evidence for psi phenomena generally, and for psychokinesis in particular, may provisionally be divided into three broad categories: experimental, semi-experimental, and anecdotal. Into the first category fall twentieth-century laboratory studies, as well as the more tightly designed investigations of the nineteenth century — for example, some French studies of mesmerism and ESP. The second category consists of investigations of what are sometimes called recurrent spontaneous psi phenomena — i.e., psi phenomena that (a) occur outside a laboratory setting, and (b) occur repeatedly in connection with either a certain person or a certain place. Mediumistic phenomena and most poltergeist phenomena are person-centered members of this class, while some poltergeist phenomena and most haunting phenomena are place-centered members.¹ I call the evidence for these phenomena semi-experimental because, although the phenomena are recurrent and therefore lend themselves to investigation (and not merely after-the-fact inquiries), the investigations are typically not controlled or designed in a way generally regarded as characteristic of scientific experimentation. This is not to say that they are therefore more careless or more vulnerable to trickery or fraud than laboratory experiments. In fact, certain of the semi-experimental studies — for example, some investigations of the great superstar mediums of yesteryear (see Chapter 2) — seem to be at least as well-guarded against fraud as most contemporary laboratory experiments (even in orthodox science). But the general consensus about the study of mediums, poltergeists, etc., is that these investi-
The importance of non-experimental evidence

gations are not full-blown experiments, either because they are exploratory and post-hoc in nature and are not intended to test specific hypotheses, or because they don't allow for the proper isolation and manipulation of experimental variables, or else because they are not conducted in a laboratory and accordingly more closely resemble a field investigation than an experiment. Whether or not such judgments are justified (especially with regard to the study of physical mediums) is a matter I prefer to postpone for now; and so I am willing to regard such investigations as semi-experimental.\(^2\) Into the final category of evidence for psi fall what have been termed *sporadic spontaneous psi phenomena*—i.e., psi phenomena that (a) occur outside a laboratory setting, and (b) are unique or almost unique occurrences in the life of the person(s) involved (either as agent or as subject). Most reports of apparitions belong in this category.

The reader might wonder why the term 'spontaneous' should ever have been applied to mediumistic phenomena—particularly those intentionally provoked or encouraged for the scrutiny of prepared observers. As far as I can determine, there are two reasons for that use of the term. First, the phenomena studied in the lab tend to be more or less contrived events; often they are of a kind that would occur only or primarily in a laboratory setting. Before electronic tests for PK became the rage, laboratory psychokinetic phenomena were principally nonrandom falls of carefully controlled tosses of dice, coins, or other objects. Now they tend to be nonrandom outputs of electronic random event generators (see Braude, 1979). By contrast, the phenomena apparently produced by mediums occurred originally in real-life situations. Levitations, materializations, and apports, for example, had been reported centuries before laypersons and scientists made organized attempts to evoke and study them. Second, the arguments leveled against the semi-experimental and anecdotal evidence are quite similar, and differ from those typically advanced against the experimental evidence. Here, perhaps, we find the main reason for including mediumistic phenomena with the rest of the so-called spontaneous phenomena, recurrent and sporadic. The received wisdom, even among parapsychologists, seems to be that only the experimental evidence is (or can be) reputable, and that the other two classes of evidence share serious common weaknesses. Supposedly, the two types of non-exper-
The importance of non-experimental evidence

Experimental evidence are inherently defective, and for more or less the same general reason — namely, that they are not as 'clean' (i.e., reliable or trustworthy) as the experimental evidence with its associated contrived phenomena.

In any case, we need not detain ourselves over the use of the term 'spontaneous.' That seems really to be a side issue. Of more central concern is the piece of received wisdom that simultaneously condemns the semi-experimental and anecdotal evidence for psi. I want to examine carefully whether the customary opposition to non-experimental evidence is justified. And here I must part company, not only with most academics, but also with many (perhaps the majority of) parapsychologists. I cannot support the prevailing view that the non-experimental evidence of parapsychology is either inherently weak or at least inferior to the laboratory evidence. Interestingly, this common attitude toward non-experimental evidence is a somewhat new development. Only in recent years has the data of parapsychology been heavily weighted in favor of the experimental. Most of the evidence preceding the work of Rhine around 1930 is semi-experimental and anecdotal. It is only relatively recently, then, that parapsychologists have been able to luxuriate in the apparently clean evidence drawn from laboratory studies. Of course, Rhine and many others felt that this was all for the good. But for reasons I shall mention below, I must challenge this judgment. The belief in the superiority of experimental evidence seems to me to be based on several major confusions.

My current favorable assessment of the non-experimental evidence for psi is by no means unprecedented. But no doubt many will consider it to be either reactionary or radical (depending on their point of view). Certainly, my present views are more extreme than those I took in my previous book on philosophy and parapsychology (Braude, 1979). At that time, I was (at best) only mildly attracted to some of the views I now hold. I now believe that the non-experimental evidence of parapsychology has been unjustly maligned. I now consider it to be an extremely valuable source of information concerning the nature and limits, and even the reality, of psi functioning. Even more important, I now believe that such material is at least as valuable and reliable as the evidence gathered from laboratory experiments, and probably more so.
The importance of non-experimental evidence

No doubt many will be surprised by this evolution of my position. After all, they might say, experimental evidence is gathered under controlled conditions, and offers the promise of replication of results. Non-experimental evidence, on the other hand, tends to be collected under non-controlled conditions, and suffers from the inevitable unreliability of human testimony and perception. Moreover, the phenomena reported in these cases are often so bizarre as to be antecedently incredible, and probably the result of ingenious chicanery and conjuring. This is precisely the received opinion regarding non-experimental material, and there is enough truth in it to render the response initially plausible. But I have come to believe that the received view is ultimately superficial, and that it withers under close scrutiny. In what follows I shall explain why I believe this to be the case.

1.2 THE LIMITATIONS OF EXPERIMENTAL RESULTS

In some ways, experimental parapsychology has been very disappointing. Most laboratory evidence appears to suffer from two major — and as far as I can see, ineliminable — weaknesses: (a) its inability to convince skeptics of the reality of psi functioning, and (b) its inability to reveal vital facts or data concerning the nature of psi.

As far as (a) is concerned, the problem is not that there have been no good or impressive experiments. In my opinion, there have been quite a few (some discussed in Braude, 1979). Often the designs have been ingenious and clean, and sometimes the results have been quite striking. Nor is the problem the alleged paucity of repeatable experiments. For reasons I have explored elsewhere (Braude, 1979), I think both skeptics and believers are deeply confused about this issue, and have made far too much of it. To see what is really wrong with experimental evidence, we must begin by placing recent psi experimentation into historical perspective.

Most parapsychologists today work in the shadow of J. B. Rhine, and either knowingly or implicitly adopt certain of his assumptions about the nature of science and the value of experimentation (in the behavioral sciences generally, and in parapsychology in particular). And although it is certainly false (contrary
The importance of non-experimental evidence
to what some believe) that Rhine almost singlehandedly made parapsychology scientific, many nevertheless believe that his experimental, quantitative procedures afforded the best possible opportunities for obtaining clean evidence for psi, and hence for silencing the skeptic.

My own view, on the other hand, is that Rhine's so-called revolution has failed, and in just those respects in which it was expected to succeed. Skeptics have rarely (if ever) been converted, or even deeply impressed by laboratory experiments. Nor have they been more effectively swayed than they were in the past by séances or anecdotal reports. The reasons all concern the fact that the laboratory evidence is quantitative. Most successful experiments boast odds against chance, at best, of 100 or 1,000 to 1. And although some reach staggering levels of significance, they are rare enough for the skeptic or parapsychological fence-sitter to wonder whether they represent nothing more than a statistical anomaly, rather than occasional spasms of outstanding – or conspicuous – psi. Certainly, laboratory results in PK are never as viscerally compelling or mind-boggling as experiences – or even mere reports – of large-scale physical phenomena (e.g., object levitations or materializations). Not even Schmidt's experiments with pre-recorded targets (see, e.g., Schmidt, 1976, and the discussion in Braude, 1979) carry the same impact as tangible object movement under good conditions. As important as Schmidt's tests may be, the items of data themselves – signals recorded on tape – are colorless. The experiments are arresting only in the light of statistical arguments and abstract considerations about the nature of causality.

The problem, in my opinion, is that applying the laws of chance to real-life situations is a complex and subtle business. Under nonexistent ideal conditions the laws are relatively straightforward. But when it comes to actual cases it is often extremely difficult to pinpoint nonrandom occurrences with any confidence. (That is why parapsychologists abandoned PK tests with dice and coins and turned to comparatively simple experiments with random number generators.) But even in the more straightforward cases, one can still wonder whether enough trials have been conducted to iron out present anomalies. By contrast, however, an object flying slowly and gracefully through the air in good light poses a clear-cut problem, and therefore tends to be more impactful right