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The problem of disenchantment: scientific naturalism and esoteric discourse, 1900-1939

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Publication date
2013

[Link to publication](#)

Citation for published version (APA):

Asprem, E. (2013). *The problem of disenchantment: scientific naturalism and esoteric discourse, 1900-1939*. [Thesis, fully internal, Universiteit van Amsterdam].

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Laboratories of Enchantment

Parapsychology in Search of a Paradigm

A collection of facts is not yet a science any more than a heap of stones is an edifice. They must be collated, sifted and ordered, according to a definite point of view, in order that we may draw conclusions from them.

Baron Albert von Schrenck-Notzing, *Phenomena of Materialisation* (1923), 28.

INTRODUCTION: THREE GENERATIONS OF PSYCHICAL RESEARCH

Psychical research's struggle to become a respected scientific discipline may be told as the history of three consecutive generations of researchers, facing different challenges and situations: the founding first generation (active ca. 1870 to 1900), the second generation (ca. 1900-1930), and the third generation (ca. 1930-1960s). This periodization marks some major points of transition in the history of psychical research programmes, and their relation to wider society. Thus, the first generation saw the establishment of the field by a network of Cambridge friends, and ended with the death of the core members of this network around the year 1900. The second generation, which will occupy us at length in the present chapter, was troubled by schisms and disagreements following the passing of those who had previously knit the field together and ensured its stability. This generation was characterised by conflicting attempts to establish a paradigm for the study of psychic phenomena. It was only with the third generation, the inception of which may conveniently be dated to the beginning of the 1930s, that such a paradigm was more or less successfully established. While the paradigm which took shape at the beginning of the 1930s is pretty much still in place in parapsychological communities today, we may date the end of the third generation to the 1960s, when the field opened up to new alliances and cultural constellations in Cold War America. That story is, however, beyond the scope of the present work, which only

considers developments up until the beginning of the Second World War. I will focus on the second generation in particular (ca. 1900-1930), its search for a proper paradigm, and the associated struggles for authority in the field. The search for a paradigm is the logical next step from the questions that concerned us in the previous chapter: once one has established that a science of the “supernormal” is both possible and desirable, and that “agnosticism” concerning such phenomena is premature, a new set of questions arise: just *how* should one go about studying the supernormal? What are the appropriate methodologies? How do these relate to the widely differing first assumptions in the field? This chapter provides a detailed look at how these questions have been answered. We shall see that the main challenge encountered by the prospective “laboratories of enchantment” has been to negotiate between *qualitative* and *quantitative* research methods. I will suggest that the major conflicts in the field arise from the tension between these two strands of research, and the basic hypotheses that have typically accompanied them.

* * *

The three generations faced different historical challenges, and each new one built upon the successes and failures of the previous. Accordingly, the debates of the second generation cannot be properly understood without reference to a broader historical framework.

The first generation of psychical researchers formulated the goals and agendas for their prospective discipline, calling for a naturalisation of the supernatural. They pioneered methods for researching “supernormal” phenomena, they collected data both of an ethnographic and historical character, and explored theoretical frameworks. They also created the first *institutions* for organising, conducting, and promoting such research, of which the English Society for Psychical Research (1882) was without doubt the most important one. In Britain, where the project was at its strongest, the first generation is notable for its *social status*: psychical research was an elite phenomenon, initiated and run by renowned academics, members of high society, and the

intelligentia.¹ There were even notable links to the political establishment, especially through the influential Balfour family.

The social status of the members involved with the project is significant, for it was no doubt part of the reason why the SPR was relatively successful during the first two decades of its existence. Thus it is also intriguing to note that the demise of the most notable and respected spokespersons of the field during the first decade of the 20th century, including Henry Sidgwick (died 1900), Frederic Myers (1901), and William James (1910), was also the beginning of the end to the social capital that had been built up by the society. In the first two decades of the new century, the SPR became associated with the spiritualist leanings of Oliver Lodge rather than the careful philosophical criticism of Sidgwick, or the psychological discourse of Myers and James. Although Lodge had originally been one of the intellectual beacons of the society in the 1880s and 1890s, and a leading physicist of his generation, his drifting towards the spiritualistic lecture circuit and steadfast adherence to ether metaphysics made him look increasingly cranky in the eyes of established academic communities. His ideas were, however, still widely received by the general population, and he was no doubt seen by these readers as a spokesman of legitimate science. Nevertheless, the status of the field was changing from that of a late-Victorian gentlemanly intellectual pursuit, to a topic of broad popular appeal.

A number of popularising writers contributed to this trend. One prominent example is the journalist and member of the American SPR, Hereward Carrington (1880–1958), who authored more than one hundred books, primarily on psychical research, spiritualism, magic and magical traditions, and occult phenomena such as astral projection.² By the 1920s, those who presumed to know something about psychical research were quite likely to have their knowledge from Carrington's writings.

¹ Cf. *ibid.*, 643-644; Gauld, *Founders of Psychical Research*.

² Relevant titles include Carrington, *The Physical Phenomena of Spiritualism* (1907); *Eusapia Palladino and Her Phenomena* (1909); *Personal Experiences in Spiritualism* (1913); *The Problems of Psychical Research* (1914); *True Ghost Stories* (1915); *Psychical Phenomena and the War* (1918); *Modern Psychical Phenomena* (1919); Carrington with Sylvan Muldoon, *The Projection of the Astral Body* (1929), and so forth.

In fact, his popularisations went far beyond the American and British markets, some of his books being translated into languages such as Japanese and Arabic.³

When it comes to actual research, the second generation was characterised by fragmentation and strife. Despite their successes in making psychical research socially acceptable for academics, the first generation had still failed to agree upon a theoretical framework that rendered the ostensible phenomena intelligible. Even the empirical foundation of the field had been too weak to convince the sceptics that further research was needed. This left a difficult situation for the second generation, which not only had to continue convincing sceptics, but lacked a secure ground upon which they could build their case. Psychical research between 1900 and 1930 was characterised by *conceptual*, *theoretical*, and *methodological* fragmentation. A number of different schools and research groups existed in countries such as the UK, the US, the Netherlands, Germany, and France, but there was no general agreement on how to conduct research, how to relate and interpret data, or even on what ought to be considered as proper data for the discipline in the first place. To borrow a concept from the history and philosophy of science, the second generation was torn between a number of different and diverging psychical *research programmes*, the scientific rigour of which varied considerably.⁴ Some of these programmes were university sponsored, most were the initiatives of private patrons, while others were direct outshoots of the spiritualist movement. Sometimes these boundaries were blurred entirely. Indeed, another point of disagreement was whether or not the scientific path was worth following in the first place.

Focusing on the second generation, this chapter describes a situation in which a number of competing “laboratories of enchantment” were attempting to establish a proper science of the supernatural. The main focus is on the schools’ respective take on proper scientific conduct – that is, on their actual laboratory *practice*. My main argument is that second-generation psychical research can be characterised as a “pre-paradigmatic science”, and the researchers involved in it as a generation of would-be scientists in search of a paradigm. With “paradigm” I am thinking of the most specific of

³ See the publisher’s note in Carrington, *True Ghost Stories*, 7.

⁴ The technical sense of scientific “research programmes” was developed in Lakatos, ‘Falsification and the Methodology of Scientific Research Programmes’. Compare this with my discussion of Thomas Kuhn’s concept of “paradigm” below.

the many senses in which the term has been used since the publication of Thomas Kuhn's *Structure of Scientific Revolutions* (1962), namely, as a "shared example" from which scientists in a given discipline are able to perform "normal science" in accordance with specific standards of rationality that are encoded in a common "practice" rather than "laws".⁵ As Kuhn emphasised, such paradigms are often embodied in groundbreaking textbooks, such as Newton's *Principia*, or in a specific series of experiments and papers, such as Maxwell's work on electromagnetism.⁶ It is through the shared acceptance of *exempla* of this type that a given special science can become mature and, as it were, "progressive". It is, I argue, precisely the *lack* of such a unifying *exemplum* that characterises psychical research in the second generation. We shall see that a number of works *tried* to put psychical research on a firm methodological footing, often backed up with an arsenal of neologisms to denote the field and its effects, and with theoretical frameworks for understanding the phenomena in question. However, no broad consensus was ever reached. Instead we see decades of competition and quarrels between sometimes widely different and even incompatible approaches. It is this diversity that shall occupy us at present: the search for paradigms and the social and discursive battles to enforce them.

A main fault line will be drawn between *qualitative* and *quantitative methodologies*, representing two separate lines of inquiry, favoured by different branches of psychical research throughout the period. Broadly defined, qualitative approaches focus on the close study of single cases, evaluating the minutia of a séance, or the details of a given "anomaly". By contrast, quantitative approaches are concerned with statistical relationships, probabilities, and the design of repeatable experiments. As

⁵ Kuhn's important book was a bit too liberal with its use of the term "paradigm". In an early criticism, Margaret Masterman counted as many as twenty-one different uses of the term in *Structures*, listing them all before suggesting, as a good analytic philosopher, that they could be subsumed to a total of three general types: metaphysical paradigms, sociological paradigms, and the exemplar type that I am adopting here. Kuhn himself, responding to critics in the 1969 postscript to the second edition, conceded that there were primarily *two* main types of paradigms: 1) 'the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community'; and 2) 'the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science'. Again, it is in this latter, restricted sense I am using the term at present. See Masterman, 'The Nature of a Paradigm', 61-65; Kuhn, 'Postscript - 1969', 175.

⁶ Kuhn, 'Postscript - 1969', 187-193.

we shall see later, quantitative and qualitative approaches to psychological phenomena were typically separated by different explanatory hypotheses as well: for example, the spiritualist and animist strategies that we discussed in the previous chapter were usually (although not exclusively) connected to *qualitative* research practices, while the “positive” reductionist ones proved more compatible with *quantitative* methods. In short, research communities were divided on a great number of initial hypotheses and theoretical assumptions: are psychic phenomena common or rare? Do they have a biological basis, or are they due to the activity of actual, disembodied spirits? Can they be produced at will, or do they only occur spontaneously? How one answered these questions would have great implication for the adoption of methodology, and they became the foundation for increasingly polemical battles about the proper conduct of psychological research.

* * *

This chapter, then, presents an “internalist” narrative of the fragmentation of psychological research in terms of what might be called its “rational component”.⁷ The reader should be warned that it is largely a history of failures: the very struggle to find rational common ground for the discipline led to a long series of bitter conflicts, schisms, and disappointments. I will start by assessing the failure of the first generation, told particularly through the witness of William James. The story then continues to a thorough assessment of the scientific conditions prevailing in the second generation.

This chapter’s internalist analysis will be complemented in the following chapter by an *externalist* consideration of the social, cultural, and political contexts of psychological research during the inter-war period. Despite the frustration, fragmentation, and highly elusive rational foundations of the field, a paradigm for psychological research *did* finally emerge in the beginning of the 1930s. This event ushered in the *third* generation – a

⁷ Here I make a separation between “internalist” and “externalist” aspects of scientific development, and describe the internal aspects as composing the “rational component” of a given discipline. It refers to the cluster of issues often discussed in the philosophy of science as part of science’s special “rationality”, which is thought to demarcate science from non-science in a philosophical sense. Thus, principles of verification and falsification, the logic of discovery and justification, methodological issues related to inferences to the best explanation, or questions bearing on experimentation, such as the use of controls, randomization, single and double blinds, repeatability, statistics and probability analyses, properly belong to an internalist analysis.

generation which could, for the first time in the field's history, consider themselves true *professionals*, trained in universities and working under the disciplinary banner of "parapsychology". This sudden development suggests that internalism alone is not enough to determine the success or failure of a scientific discipline: external factors (social, political, economic) must also be taken into account. But to appreciate the exact role and importance of external factors, it is necessary to first know something about the "rational" situation of the discipline. Thus, the present chapter also sets the stage for the next one, in which we shall explore the emergence of a paradigmatic science in the third generation.

1 WILLIAM JAMES AND THE FAILURE OF THE FIRST GENERATION

Between the foundation of the SPR in 1882 and the year 1900, about 14,000 pages of research reports, theorising, and experimental notes were published in the society's journal, proceedings, and reports.⁸ About half of these pages were written by the small group of people at the core of the society, essentially Henry and Eleanor Sidgwick, Richard Hodgson, Edmund Gurney, and Frederic Myers.⁹ With the exception of Eleanor Sidgwick (1845–1936; née Balfour – the sister of Arthur), they were all dead by 1905. When this highly influential and productive network of old Cambridge friends collapsed, the SPR was left without a clear direction. Considering that the heritage they left behind was also highly ambiguous at best, as far as one considers any actual progress in the endeavour they had defined, this was no small challenge.

William James' testimony from 1909, given just one year before he too deserted the ranks of the living, illustrates the situation at the demise of the first generation. Reflecting on the role of the "Sidgwick group", and their fortunes and results in psychical research, James testified to a feeling of failure:

These men [the founders of the SPR] hoped that if the material were treated rigorously, and, as far as possible, experimentally, objective truth would be elicited, and the subject rescued from sentimentalism on the one side and dogmatizing ignorance on the other. Like all founders, Sidgwick hoped for a certain promptitude of result; and I heard him say, the year

⁸ Especially Gurney et al., *Phantasms of the Living*, and Myers, *Human Personality*.

⁹ See Gauld, *Founders of Psychical Research*, 313.

before his death, that if anyone had told him at the outset that after twenty years he would be in the same identical state of doubt and balance that he started with, he would have deemed the prophecy incredible. It appeared impossible that that amount of handling evidence should bring so little finality of decision.¹⁰

James, who had been the main intellectual proponent of psychical research in the United States, shared Sidgwick's disappointment:

My own experience has been similar to Sidgwick's. For twenty-five years I have been in touch with the literature of psychical research, and have had acquaintance with numerous "researchers". I have also spent a good many hours (though far fewer than I ought to have spent) in witnessing (or trying to witness) phenomena. Yet I am theoretically no "further" than I was at the beginning; and I confess that at times I have been tempted to believe that the Creator has eternally intended this department of nature to remain *baffling*, to prompt our curiosities and hopes and suspicions all in equal measure, so that, although ghosts and clairvoyances, and raps and messages from spirits, are always seeming to exist and can never be fully explained away, they also can never be susceptible of full corroboration.¹¹

This quotation contains the essence of the situation of psychical research at the beginning of the 20th century. It expresses the futility felt by those sincere and thorough researchers who had invested most in the project thus far. But it also betrayed an underlying conviction that the evidence *ought* to be there regardless, that the phenomena – or some of them at least – were genuine even if the quest to prove so had turned out frustratingly elusive. In what was to be his last reflections as a psychical researcher, James confessed that he did indeed hold some of the phenomena to be genuine, and attributed quite a lot of significance to the fact.¹² In particular, James expressed how psychical research, through evidence such as the personalities emerging in automatic writing and mediumistic phenomena, led him to adopt a psychological and metaphysical position that was, in essence, identical to Frederic Myers':

¹⁰ James, 'Final Impressions of a Psychical Researcher', 174.

¹¹ *Ibid.*, 174-175.

¹² 'The next thing I wish to go on record for is *the presence*, in the midst of all the humbug, of *really supernormal knowledge*. By this I mean knowledge that cannot be traced to the ordinary sources of information – the senses, namely, of the automatist.' *Ibid.*, 200.

Out of my experience, such as it is (and it is limited enough) one fixed conclusion dogmatically emerges, and that is this, that we with our lives are like islands in the sea, or like trees in the forest. The maple and the pine may whisper to each other with their leaves, and Conanicut and Newport hear each other's foghorns. Just so there is a continuum of cosmic consciousness, against which our individuality builds but accidental fences, and into which our several minds plunge as into a mother-sea or reservoir. Our "normal" consciousness is circumscribed for adaptation to our external earthly environment, but the fence is weak in spots, and fitful influences from beyond leak in, showing the otherwise unverifiable common connection. Not only psychic research, but metaphysical philosophy, and speculative biology are led in their own ways to look with favour on some such "panpsychic" view of the universe as this.

It was, James argued, in this "panpsychic" framework that the most intriguing questions about consciousness ought to be asked, and he therefore urged that the whole field of investigation become eponymous with Myers:

Assuming this common reservoir of consciousness to exist, this bank upon which we all draw, and in which so many of earth's memories must in some way be stored, or mediums would not get at them as they do, the question is, What is its own structure? What is its inner topography? This question, first squarely formulated by Myers, deserves to be called "Myers' problem" by scientific men hereafter. What are the conditions of individuation or insulation in this mother-sea? To what tracts, to what active systems functioning separately in it, do personalities correspond? Are individual "spirits" constituted there? How numerous, and of how many hierarchic orders may these then be? How permanent? How transient? And how confluent with one another may they become?¹³

Looking at these questions through the lens of psychical research would lead one close to a new worldview along the lines of psychic enchantment, in which the whole cosmos is understood through fundamental psychical and spiritual realities:

What again, are the relations between the cosmic consciousness and matter? Are there subtler forms of matter which upon occasion may enter into functional connection with the individuations in the psychic sea, and then, and then only, show themselves? – So that our

¹³ Ibid., 204-205.

ordinary human experience, on its material as well as on its mental side, would appear to be only an extract from the larger psychophysical world?¹⁴

While these were all phrased as questions, there is no doubt that James considered them as suggestive of what might one day become established knowledge. Despite his open confession of disappointment in the results and progress of psychical research in its first generation, James ended his essay on a positive note, which, once again, urged one not surrender to dogmatic agnosticism but continue to push further into the unknown:

Vast, indeed, and difficult is the inquirer's prospect here, and the most significant data for his purpose will probably be just these dingy little mediumistic facts which the Huxleyan minds of our time find so unworthy of their attention. But when was not the science of the future stirred to its conquering activities by the little rebellious exceptions to the science of the present? Hardly, as yet, has the surface of the facts called "psychic" begun to be scratched for scientific purposes. It is through following these facts, I am persuaded, that the greatest scientific conquests of the coming generation will be achieved.¹⁵

Despite the problems, the hope of a future science remained. It is intriguing that James placed the hopes of that future science with the phenomena of *mediumism*, rather than the more strictly *quantifiable* trials that had been run with alleged "psychics", guessing playing cards and describing objects at a distance. This emphasis on the *qualitative* study of spiritualistic events in place of the quantitative study of isolated and well-defined "supernormal" faculties did become a vogue in the psychical research societies in the decades following James' death. However, the verdict of history must be that this was not the "progressive" line that the first generation had longed for.

With the hindsight of history, James' view of the future of psychology at large was quite different from the direction it would eventually take, and it all connects up to psychical research and particularly the place of Myers. As James had written in his obituary of Myers, published in the *SPR Journal* in 1901, he found it very probable 'that Frederic Myers will always be remembered in psychology as the pioneer who staked out

¹⁴ Ibid., 205-206.

¹⁵ Ibid., 206.

a vast tract of mental wilderness and planted the flag of genuine science upon it.¹⁶ More than a century later it is clear that this prediction failed, and James himself (or at the very least a sanitised version of his thinking) has, in part, taken the slot in history that had been reserved for Myers. The reasons for this fate can be better understood by looking at the development of psychical research in the second generation.

2 THE FRAGMENTATION OF PSYCHICAL RESEARCH: THE SECOND GENERATION

As mentioned, the second generation of psychical researchers were left without a clear paradigm for pursuing their work. Instead, a number of diverging schools were founded, and competing research projects vied for authority over things supernatural. In a paper on 'Psychical Research and Philosophy' read in 1926, Hans Driesch, then one of the leading intellectual figures in the field and the president of the SPR, observed the fragmentation of psychical research:

All so-called sciences, the word taken in the widest sense, are branches of philosophy which live, as it were, an independent life. Psychical Research is one of these branches, or, rather, the word "Psychical Research" or "Para- or Meta-psychology" – to introduce the terms used in Germany and France – denotes *several* branches of philosophy that have become independent to a certain extent. For at the first glance at least, there is not one Parapsychology but there are several Parapsychologies, which may one day unite into one, there being several groups of psychic phenomena which, at first in any case, are as different from one another as, e.g., chemistry is from optics.¹⁷

Driesch's focus here was on the heterogeneous status of the phenomena themselves; they seemed to belong to a number of different classes and categories, not necessarily subsumable to one and the same science. It is indeed a serious problem for an aspiring science to not be able to agree with itself on what kinds of phenomena it is supposed to study. This is a key characteristic of a non- or pre-paradigmatic science. It was, however, only one of a series of problems splitting the psychical research communities. The conference in which Driesch's statement was read was dedicated to an even more

¹⁶ James, 'Frederic Myers' Service to Psychology', 170.

¹⁷ Driesch, 'Psychical Research and Philosophy', 161.

serious problem: disagreements about the very *genuineness* of the phenomena that psychical research took as its objects of study.

The general fragmentation of psychical research is illustrated clearly by the symposium held at Clark University in November–December 1926.¹⁸ The concept for the symposium, collaboratively designed by William McDougall, Harry Houdini, and the psychologist Carl Murchison, was to convene spokespersons of opposing views within the field in order to bring the fundamental question of the “genuineness” of psychic phenomena into focus. Houdini, the arch sceptic of the period, and McDougall, a critical and scientifically-minded supporter of psychical research, although the best of friends, had their obvious disagreements concerning the issue, and it was clear to the three conveners that this disagreement was reflected in the field of psychical research at large.¹⁹ The idea for the conference was precisely to bring the fundamental tensions in the field to the fore.

The proceedings, published as *The Case for and Against Psychical Belief* (1927), presents fourteen essays distributed in four categories: those ‘Convinced of the Multiplicity of Psychical Phenomena’, those ‘Convinced of the Rarity of Genuine Psychical Phenomena’, researchers ‘Unconvinced as Yet’, and finally, those ‘Antagonistic to the Claims That Such Phenomena Occur’. Houdini, whose untimely death had prevented him from actually participating in the conference, clearly belonged in the latter category, together with the sceptical psychologist Joseph Jastrow, author of such books as *Fact and Fable in Psychology* (1900) and *The Psychology of Conviction* (1918), and, as we saw in the previous chapter, a pioneer of the “psychology of the occult”.²⁰ McDougall took the position of someone who was convinced of the *rarity* of genuine psychical phenomena, and was joined by Driesch, the Boston psychical researcher and Episcopal minister Walter Franklin Prince (1863–1934), and the pragmatist philosopher F. C. S. Schiller (1864–1937). The group of the “convinced” was unsurprisingly dominated by spiritualists, such as the author Arthur Conan Doyle and the physician Le Roi Crandon – husband to the famous medium “Margery” (Mina Crandon). A paper by Oliver Lodge was included in this camp as well, representing a

¹⁸ The papers read, and a couple of additional pieces, were published in 1927 by Murchison (ed.), *The Case for and against Psychical Belief*.

¹⁹ Murchison, ‘Preface’, vii.

²⁰ See discussion in the previous chapter.

pro-spiritualist view coming from within organised psychical research. The “unconvinced as yet” were two psychologists: Stanford’s John Edgar Coover (1872–1938), and Gardner Murphy (1895–1979), then working at Columbia University.

As we shall see later in this chapter, Coover and Murphy had both conducted important research into telepathy and clairvoyance in the decades prior to the conference, and were astute experimentalists. The fact that both of these experimentalists were in the group of the unconvinced reflects another of the problems of psychical research during the period: the conflict concerning methodologies for research. It is not a coincidence that the “convinced” group was dominated by those who advocated *qualitative* methodologies, based largely on the observation of mediums during séances, or at best the testing against external evidence of “psychic” information that could only be obtained in such situations.²¹ These correlations point to a basic and suggestive bifurcation that we shall return to shortly. First, however, we should take note of the middle category of those who were convinced of the *rarity* of genuine phenomena. This category is important, for here we find a class of researchers who claimed to value the scientific method just as much as the unconvinced experimentalist, yet still thought that *some* phenomena were genuine, while others were not. It is notable, though, that the men in this class – McDougall, Driesch, Prince, and Schiller – had minimal experience with actual experimental work in the field. Nevertheless, I will argue in the next chapter that the professionalization of parapsychology emerged precisely from this class, although due to reasons that may be characterised as “political” rather than strictly scientific.

At present, the relative strengths of what I have called “qualitative” and “quantitative” methodologies in psychical research must be assessed. We should start by focusing on the qualitative ones, which were particularly dominant in the SPR and its daughter and sister movements in Europe and the United States. The scope is thus international. Each group must be situated in their respective local and institutional

²¹ Another spokesperson for the convinced was Frederick Bligh Bond, most famous for his invention of psychic archaeology. Bligh Bond explained in his paper that he considered ‘Proof from Buried Antiquities’ to be one of the stronger forms of evidence that could be sought in corroboration of knowledge claimed by “supernormal” means. His own excavation of Glastonbury Abbey in 1907 had been carried out precisely on these grounds. Bligh Bond, ‘The Pragmatist in Psychic Research’, 38-48. For Bligh Bond’s account of the Glastonbury excavation, see his *The Gate of Remembrance*.

contexts, while also drawing attention to the international connections between them. Above all, however, attention will be drawn to the “rational”, “science-internal” aspect of each of these schools. That is: how did they situate their researches vis-à-vis the established sciences? What were the theoretical frameworks in which they proposed to view, interpret, and explain the phenomena they studied? What were the methodological assumptions informing their research, and how did these influence the adopted methods? When the socio-cultural and the methodological aspects of psychical research are assessed together we start to see how a “progressive”, pro-science, and pro-professionalisation trend in psychical research began to emerge, positioned in opposition to a “pro-spiritualist”, qualitative, and mostly medium-focused research tradition.

THE SPR AND THE GHOSTLY RETURN OF FREDERIC MYERS

The first generation of psychical researchers had succeeded in establishing a large network which spanned a number of countries and was sustained by durable institutions and periodicals. In the second generation, many local branches of the SPR continued to do psychical research in much the same manner as before, with a continued interest in mediums and spiritualism. The outcome of these researches were recorded and published in *Journal* and *Proceedings* of the Society. In addition to these outlets, members of the SPR prolifically published books and pamphlets for popular audiences. While most of these publications were apologetic defences of psychical research and expositions of its programme, other publications gave accounts of new research conducted by the Society and its associates. Examples of the latter include Hereward Carrington’s publications of sittings with the medium Eusapia Palladino,²² and his collections of articles in works such as *Modern Psychical Phenomena* (1919), and *The Problems of Psychical Research* (1921). These reports tended to go much further than Carrington’s more sober colleagues, as he wrote supportively about controversial fields such as “spirit photography”, the instrumental measurement and weighing of spirits by physical apparatus, and even experiments in astral projection – complete with

²² E.g. Carrington, *Eusapia Palladino and Her Phenomena* (1909); idem, *Personal Experiences in Spiritualism* (1913).

photographs of astral bodies in flight (see figure 10 below).²³ Meanwhile, other lines of psychical research were published and popularised by members, including work on dowsing as a form of clairvoyance, and the entirely new field of “psychic archaeology”.²⁴

I will not spend too much time on the details of these continued efforts of the SPR, but some important general trends should be mentioned. The continued reliance on mediums is particularly notable, especially in light of the problematic relation to spiritualism and the spiritualist hypothesis. From the 1920s, these became the focus of a growing internal disagreement concerning the very idea of aligning psychical research with scientific method. As seen already, the spiritualist hypothesis was experiencing a revival in psychical research in the early decades of the century. An *external* reason for this revival was the Great War and the general upsurge in spiritualism that followed in its wake. However, a reappraisal of the spiritualist hypothesis is already noticeable among psychical researchers in the decades *before* the Great War. For example, Oliver Lodge had started considering it seriously already in his presidential address to the SPR in 1902, and was followed by William Barrett’s address two years later.²⁵ In his 1908 *Immortality of the Soul*, which mostly addressed the compatibility of science and Christian notions of the soul, Lodge enlisted the seeming ability of some mediums to ‘respond to a psychical agency apparently related to the surviving portion of

²³ All of these topics are included in Carrington, *Modern Psychical Phenomena*, a book aiming to ‘outline a few of the many relationships and bearings of psychical phenomena upon science and our thought’, and describing ‘some of the newer researches and speculations in this fascinating field’. Carrington, *Modern Psychical Phenomena*, x-xi.

²⁴ A major work on dowsing in the context of the SPR was published by the early member and former president of the society, physicist William Barrett, together with Theodore Besterman, as *The Divining Rod: An Experimental and Psychological Investigation* (1926). The foundational work of psychic archaeology was Frederick Bligh Bond’s *The Gate of Remembrance*, which first appeared in 1918. The book was based on psychical investigations going back to the excavations at Glastonbury Abbey a decade earlier. It was published with a prefatory note by William Barrett, who gave his endorsement and testimony to Bligh Bond’s sincerity, and the trustworthiness of his character. Significantly, Barrett implored the reader to recognise the courage shown by the author in publishing a work ‘which might possibly jeopardise the high reputation he enjoys’ (x).

²⁵ Lodge, ‘Presidential Address, March 1902’; Barrett, ‘Presidential Address, January 29th 1904’. See my discussion of Lodge’s ether metaphysics in chapter six.

intelligences now discarnate' as part of a case in favour of immortality.²⁶ The year after, in 1909, he published a broader survey of psychical research with the suggestive title *The Survival of Man*. In the concluding section of this book Lodge, made it clear that he thought it 'the best working hypothesis at the present time ... to grant that lucid moments of intercourse with deceased persons may in the best cases supervene'.²⁷ Thus, the external factor of the Great War seems to have been merely an extra catalyst acting on an already existing tendency to reconsider the hypothesis of survival as a serious possibility.

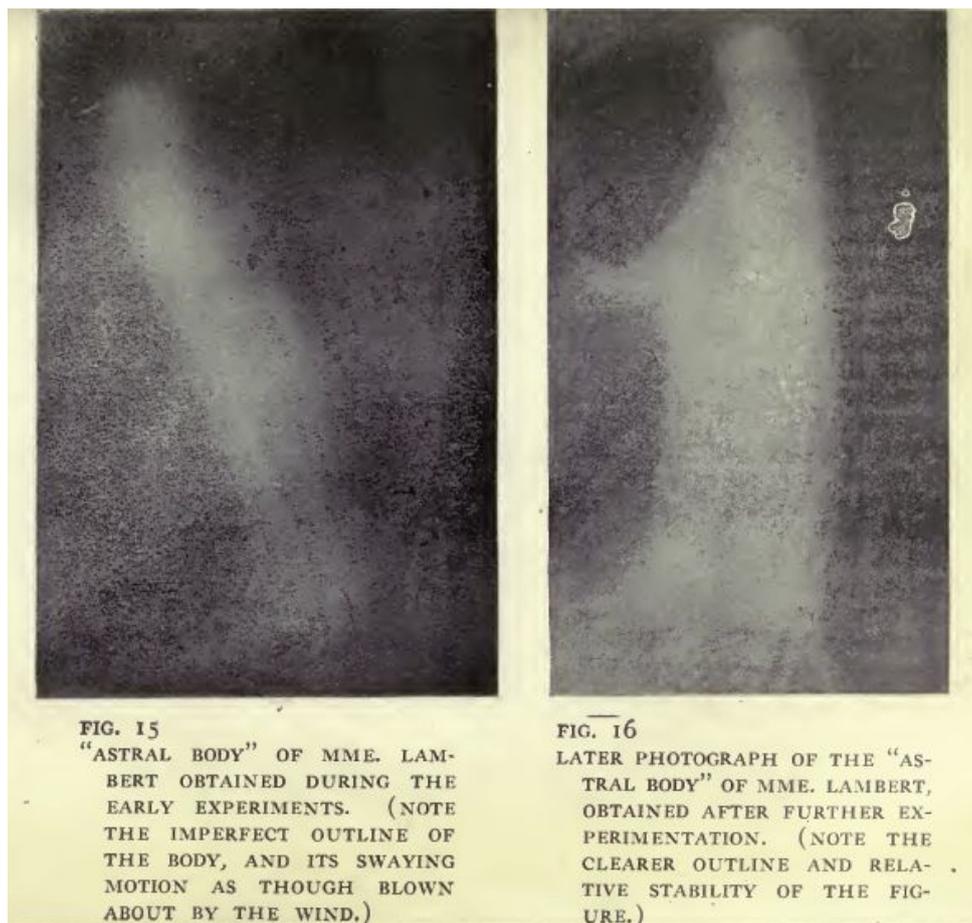


Figure 10: Photographic impressions of a projected astral body, with Carrington's explanations. Reproduced from Carrington, *Modern Psychical Phenomena*, 147.

²⁶ Lodge, *The Immortality of the Soul*, 72-73. Note, however, that he also voiced serious reservation about accepting such claims prematurely.

²⁷ Lodge, *The Survival of Man*, 337.

What, then, were the *internal* reasons to reconsider the spiritualist hypothesis? On the one hand, the re-evaluation of survival was connected with the failure of the mechanistic models of telepathy and clairvoyance that had been explored in the 1880s and 1890s, which had served as the guiding hypothesis of Gurney, Myers and Podmore's monumental *Phantasms of the Living* (1886). On the other hand, the return of spiritualism was facilitated by the final formulation and publication of an entirely new, essentially "romantic" way of theorising psychical phenomena in the work of Frederic Myers. Myers' perspective, which James fittingly had dubbed not only a "romantic" but also a "gothic" psychology,²⁸ had been familiar to regular readers of the SPR's *Journal* and *Proceedings*, but it was only as late as 1902 that his full-blown theory became generally available in the two-volume tome *Human Personality*. Myers' gothic psychology thus provided a unique framework for understanding and rehabilitating the spiritualist hypothesis in the decades following the publication of his work.

The influence of Myers in this development would, however, also take a quite different and "supernormal" form. In what must be one of the stranger anecdotes in the history of psychology, William James was with his friend Myers at James Baldwin's sanatorium in Rome the winter when the latter died. The two men had agreed on a pact that whoever would be the first to cross the threshold of death should return to the other with a message from the metetherial realm.²⁹ On January 17, 1901, it became clear that Myers would be the pioneer, and James his chronicler. According to an eyewitness, Dr Axel Munthe, who had treated Myers for the respiratory problems he was suffering, James got seated by Myers' deathbed when the moment was drawing near, with a notebook open in his lap and a pen in hand, ready to record the message.³⁰ Nothing happened that day, but the silence did not last for long. At the end of February, the famous Boston medium Leonora Piper was transmitting messages claiming to come from Myers' spirit to Richard Hodgson, one of the few members of the Sidgwick circles still alive. The spirit announced to his old colleague that he had established 'a society on this side of life for further pursuance of the ... Psychical work', and complained about

²⁸ James, 'Frederic Myers' Service to Psychology'.

²⁹ See the reconstruction of these events in Luckhurst, *Invention of Telepathy*, 253-254.

³⁰ The episode is recorded in the doctor's autobiography, Munthe, *The Story of San Michele*, 370-372. Cf. Luckhurst, *Invention of Telepathy*, 254.

the difficulties of communicating properly from the other side.³¹ That same winter, on February 19, 1901, Oliver Lodge and his wife were sitting with the medium Mrs Thompson in Birmingham, when, after dinner, “Myers” took over for Thompson’s regular control, “Nelly”.³² Thompson’s take on Myers was eager to name-drop other deceased researchers, and, like Piper’s, preoccupied with the difficulties of communicating from the other side. ‘Lodge, it is not as easy as I thought in my impatience’, Myers exclaimed when he first appeared, but added that ‘Gurney says I am getting on first rate’.³³ His quarrels with Henry Sidgwick about the genuineness of spirit communications apparently continued on the other side, too: ‘I want to convince Sidgwick. He says “Myers, now we are together, you convince me that I am sending my messages, and that she is not getting them from us some way.” He wants me to show him.’³⁴ Although he seemed to have forgotten, when asked, about the existence of the SPR in the world of the living, he appeared just as committed to its project in the world of the dead.

Thompson had known Myers and his circle very well in real life, which, Lodge carefully noted, meant that ‘no evidential importance can be attached’ to remarks about personal details and friendships.³⁵ While the communication from Myers could thus not be taken as ‘strictly evidential’, Lodge still held that it had been ‘as convincing as anything that could be imagined of that kind’.³⁶ That opinion was, however, not shared by the chief investigator across the Atlantic. As Roger Luckhurst has observed, the surviving notes and manuscripts from Hodgson’s early séances with Piper’s Myers control bear the signs of frustration and even anger on the part of the researcher. Hodgson had always been among the more sceptical of the SPR researchers, and when the medium confronted him with what she claimed to be the spirit of an old friend and master he was not easily impressed. Hodgson complained about the banality and lack of

³¹ Cited in *ibid.* The original manuscripts are kept in the Myers papers of the Wren library, Trinity College, Cambridge.

³² In the terminology of spiritualism, a “control” is a spirit personality ostensibly taking control of the medium during the séance or “trance state”. For Lodge’s report on this first encounter with Myers in the afterlife, see Lodge, *Survival of Man*, 289-295.

³³ In *ibid.*, 287.

³⁴ In *ibid.*

³⁵ Lodge, *Survival of Man*, 285.

³⁶ *Ibid.*, 290.

specificity in “Myers” communications, and wrote candidly about his suspicions of fraud and deceit on the part of the medium.³⁷

The first responses to Myers’ communications from the afterlife were thus mixed. It had precisely the same problems as other séances, with the only difference being that the person communicating was supposed to be a highly trained psychical researcher – and a personal acquaintance of the mediums themselves. In the longer run, however, Myers’ communications marked the beginning of an entirely new line of research on survival. With the “same” spirit entity apparently communicating with numerous mediums independently of each other, it became possible to *compare* messages coming from different mediums in search of intertextual evidence of an independent personality active behind the communications. The assumption was that Myers would leave hidden clues across a wide array of séances, which could only be deciphered by the sophisticated minds of highly trained psychical researchers. This approach became known as “cross-correspondence”, and was one of the strongest innovations of early 20th century psychical research.

The first systematic reports on cross-correspondences were published in the SPR *Proceedings* in 1908, and would continue to appear in following volumes. In 1910 the research was made available to a broader public through the concise exposition in Helen Alexandrina Dallas’ *Mors Janua Vitae?* (“Death, the Gate of Life”; a later American edition was given this translated title), heartily endorsed in an introduction written by William Barrett.³⁸ Dallas, a member of the SPR who had taken the trouble to carefully read and condense the already numerous and extensive reports and notes on the case, could now cite three mediums (or rather automatists) in particular, who seemed to be giving a voice and a pen to Frederic Myers, namely Mrs Piper, Mrs Verrall, and Mrs Holland.³⁹

Margaret Verrall was a lecturer in classics at Newnham College, Cambridge (where Eleanor Sidgwick was principal), and wife of the Cambridge classical scholar A.

³⁷ See Luckhurst, *Invention of Telepathy*, 255.

³⁸ A later and more thorough discussion of the cross-correspondences was published by Herbert Francis Saltmarsh, *Evidence of Personal Survival from Cross Correspondences* (1938).

³⁹ In fact there a few other female automatists were used in these experiments, including Mrs Verrall’s daughter, Helen, the Welsh suffragette and liberal politician Winifred Coombe-Tenant, Dame Edith Lyttelton (pseudonym Mrs King), and Mrs Stuart Wilson. We will meet some of these shortly.

W. Verrall. The Verralls had both known Myers personally while he was still alive, and participated actively in psychical research.⁴⁰ Margaret Verrall had also been running personal experiments with automatic writing, and it was through this method that she claimed to have obtained, a couple of months after Myers' death, a number of scripts written in Latin and some Greek, taking the form of a conversation. The second automatist, Mrs Holland, was known under a pseudonym: her real name was Alice Kipling, sister of the more famous Rudyard. She lived in India and had no relation with the SPR until she read Myers' *Human Personality* and consequently struck up a correspondence with the society's secretary, Alice Johnson, in June 1903. Holland/Kipling had been playing with automatic writing 'for her own amusement' since 1893, but after reading Myers' work, the style of these writings had suddenly changed.⁴¹ Before they had been mostly in the form of poetry; now, suddenly, they claimed to be messages from the deceased Myers, and sometimes also from Sidgwick and Gurney. Significantly, *Human Personality*, which had so much inspired Holland, was dedicated precisely to these two men.

Then there was Leonora Piper (1857–1950), who in contradistinction to the two other ladies was known as a professional psychic medium with a long, and generally good, reputation. The latter was no doubt a rare commodity among mediums. Piper had been "discovered" by William James in the mid-1880s, who recommended her for testing with his SPR colleagues in England. In 1890 she visited, and became personally acquainted with Myers, Sidgwick, and Hodgson who functioned as her investigators.⁴² In November 1906, Mrs Piper was shipped to England once more for testing with the SPR, now eager to ascertain the authenticity of the Myers and Sidgwick controls she had been exhibiting.⁴³ Richard Hodgson, who had first encountered Myers through Piper without being impressed, had passed in 1905; ironically, he too now appeared alongside the spirits of other researchers in Piper's gallery of controls. By 1906, some of the most prominent first-generation figures had returned from the afterlife, quite literally haunting the second generation from beyond the grave. From their new vantage point,

⁴⁰ See e.g. Saltmarsh, *Evidence of Personal Survival from Cross Correspondences*, 19-20.

⁴¹ *Ibid.*, 21.

⁴² For a review of Piper's mediumship, as it looked at the time she was involved with the cross-references, see Lodge, *Survival of Man*, 184-315; cf. Dallas, *Death, the Gate of Life*, 58-87.

⁴³ E.g. Dallas, *Death, the Gate of Life*, 88-94.

they were now prompting the new generation to go much further in the direction of spiritualism than they themselves had thought wise while alive.

Verrall, Holland, and Piper would produce hundreds of messages in total, with the major breakthrough occurring during Piper's stay in England. As mentioned, the methodological basis for cross-correspondence research was that there seemed to be correlations between messages "received" independently by different mediums. Correspondences were typically cryptic, and made use of literary references that required some erudition to spot. Thus, for example, Piper's "Myers" asked to look out for 'Hope Star and Browning'. As it turned out, a message from Verrall's "Myers", recorded already two weeks earlier, was replete with explicit references to hope and stars, including in Greek and Latin, and citations from Robert Browning's *The Ring and the Book*.⁴⁴ This episode was part of a longer series of responses to the so-called "Latin experiment" of 1906, in which a specific question with instructions intended for "Myers" had been translated into dense and difficult Ciceronian Latin, and read to Mrs Piper in one of her "trance states". The message should be extremely difficult to decipher without a proper training in Latin, which Piper lacked but Myers had possessed in real life. The message gave specific instructions to Myers' spirit about how to continue the cross-correspondences, which the researchers assumed he had consciously initiated from beyond the grave.⁴⁵ Working on the assumption that the instructions in Latin would have been received by Myers's spirit unbeknownst to the mediums he communicated through, the researchers spent the next months attempts to draw significant references out of the mediums' statements. In the quest to find what they were looking for, the psychical researchers seized increasingly abstruse methods of interpretation, where hidden anagrams and secret symbols were considered for clues, down to the letter.⁴⁶ While they were ultimately convinced by the evidence thus produced, in the form of symbolic and thematic correspondences across a wide set of séance notes, it is hard for the outsider to avoid observing that they also stretched their interpretations to the limits in order to get to that conviction. Through reliance on the cross-correspondences the SPR seems to have abandoned their earlier attempts to

⁴⁴ See Luckhurst, *The Invention of Telepathy*, 265-267; cf. Dallas, *Death, the Gate of Life*,

⁴⁵ Dallas, *Death, the Gate of Life*, 98-100.

⁴⁶ The two main investigators at this point were the businessman and SPR council member John George Piddington and the secretary Alice Johnson. See the summary in Dallas, *Death, the Gate of Life*, 100-107.

emulate strictly naturalistic methods, and instead developed an increasingly esoteric form of hermeneutics.⁴⁷ It was the most extreme yet sophisticated case of a qualitative methodology in psychical research to date.

The cross-correspondences continued, with more automatists and researchers getting involved in the following years. On the side of the mediums, Mrs Willett (pseudonym for Winifred Coombe-Tennant, a Welsh suffragette and liberal politician)⁴⁸ became the centre of some attention for further experiments involving the Myers control from 1910 onwards, while the Right Honourable (later Lord) Gerald William Balfour – brother of Arthur and thus another SPR member from the Balfour dynasty – became a chief investigator.⁴⁹

With the spiritualist revival following the Great War, the strictly science oriented faction of the SPR found itself in a rather difficult position. This was reflected above all by a number of institutional divergences during the 1920s. In England, the move away from a stricter scientific perspective towards a looser hermeneutic play designed to *interpret* rather than *explain* spirit conversations, and an increasing adherence to spiritualism within the society itself, provided an open niche which was filled by the National Laboratory for Psychical Research, established in London in 1925 by Harry Price.⁵⁰ Price is an ambiguous character; he had been interested in psychic phenomena since childhood, had developed a passion for stage magic and illusionism, and succeeded in convincing the University of London to support his establishment of a more scientific counterpart to the SPR.⁵¹ Price's Laboratory would play a significant role in activity that comes closer to high-profile "debunking" than to actual research. A highlight was his 1932 "Brocken experiment", in which a full "black magic" operation

⁴⁷ A similar observation was made by Luckhurst, *Invention of Telepathy*, 265.

⁴⁸ Coombe-Tennant was another central medium and automatist who preferred to keep her involvement in psychical research a secret to the public while alive. This, obviously, helped her pursue her for the times quite unusually successful political career for a woman – even though psychical research in fact furnished her with powerful contacts, such as the Lord Balfour who had been President of the Board of Trade, and member of the King's Privy Council. See also Graham Lloyd Rees, 'Coombe Tenant'.

⁴⁹ Balfour reviewed the experiments with Willett/Coombe-Tennant in volume 43 of the *Proceedings* of the SPR, in May 1935. Cf. Saltmarsh, *Evidence of Personal Survival from Cross Correspondences*, 24-25.

⁵⁰ For their early activities, see *Proceedings of the National Laboratory of Psychical Research*, Vol. 1, 1927-1929.

⁵¹ See e.g. Beloff, *Parapsychology*, 94.

was performed on the mountain Brocken in Germany, traditionally connected to the witches' Sabbath, on midsummer's eve.⁵² With the world press attending, the magical experiment attempted to transform a virgin male goat into a young man. Sensationally, nothing happened. What was to be gained by the experiment in terms of scientific knowledge remains entirely unclear.

Meanwhile in the United States, the American SPR was going through a schism precisely on the topic of whether or not to align strictly with science. This schism was nowhere more significant than in Boston, where the American SPR had its headquarters, and some of the most well-known American mediums were operating. William McDougall, who arrived in Boston in 1920 and was elected president of the ASPR the following year, came to have a decisive impact on the outcome of these controversies. First of all, his actions as president underscored the ongoing bifurcation between a pro-spiritualist and a pro-science wing within the society. Hailing from a strictly science-oriented research tradition, and possessing no personal leanings towards spiritualism or occultism whatsoever, McDougall was not impressed by the current influx of spiritualists to the ASPR. Consequently, he spent a public lecture in 1922 attacking committed spiritualist like Arthur Conan Doyle, whom, he complained, were apparently more interested in extravagant demonstrations of mediumship than in critical scientific investigations of the phenomena.⁵³ McDougall's clear statement, siding with the scientific wing of the ASPR, proved divisive and controversial. A year later the trustees of the society decided to dismiss his presidency – firing McDougall's ally, Walter Franklin Prince, from the editorial chair of the Society's journal while they were at it.⁵⁴

At this point the American conflict was no longer purely ideological, but became embodied in an institutional schism. Prince reacted to the spiritualist take-over of the SPR by establishing the rival Boston Society for Psychical Research (BSPR) in 1925 – incidentally the same year as Price had revolted against the British SPR in England.

⁵² For thorough documentation of this event, see the extensive press-clippings Price kept, which attests to the massive media coverage the ritual attracted from countries all over the Western world. Price, "Press cuttings book (vol. 14)", 1932; Harry Price Archive, HPF/3A/14; Senate House Libraries, University of London.

⁵³ McDougall, 'The Need for Psychical Research', 59-60.

⁵⁴ Cf. Moore, *In Search of White Crows*, 176. See my discussion in the following chapter, which considers McDougall's career in this period in some detail.

Apart from the deeper ideological differences, an important immediate context of this split was the handling of the controversial Margery case. As mentioned briefly in the previous chapter, the committee that had been put together by John Malcolm Bird, editor of *Scientific American*, eventually decided against the medium's genuineness, while Bird himself had become increasingly convinced of her powers. Kicking out critics like McDougal and Prince, while installing Bird as Research Officer of the ASPR, the society also came to disregard the verdict of the Margery report. Instead of following sticking with the report of the committee, the ASPR started publishing a number of positive reviews and defences instead. These were written largely by Bird himself, and by Margery's husband, the surgeon Le Roy Crandon.⁵⁵ The Society had thus effectively been hijacked by what Prince called 'the Crandon group'; it had become 'a propaganda platform', continuing to issue not only articles and reviews, but also books and pamphlets in the defence of Margery, while silencing and censoring opposing voices and criticisms in the Society's publications and in the public sphere. Prince, who wrote and published a thorough review of the case in *The American Journal of Psychology* in 1926 even documented how the Crandon group had systematically edited the original transcripts of the Margery sittings, omitting crucial pieces of evidence and also tinkering with the presentation in more subtle ways to direct the reader in favour of the medium's genuineness.⁵⁶

The year 1925 thus marks a significant shift in the history of psychical research. It was the year when the SPR was finally severed from the strictly scientific aspirations of its founders, while new spokespersons, movements, and institutions emerged with the intention of carrying on the torch. Some of these would explore more quantitative approaches to the subject, and introduce more rigid controls of their experiments. But before we get there, we must have a look at the institutions and research projects that were being pursued in continental Europe, largely independent of the SPR. Intriguingly, we shall see that very similar conflicts arose in the German and French research traditions, even coinciding in time with the developments we have considered for England and the United States.

⁵⁵ Cf. Prince, 'A Review of the Margery Case', 432. Cf. Crandon, 'The Margery Mediumship'.

⁵⁶ *Ibid.*, 438-440. The publication Price uses as an example for these documented alteration was the anonymously published pamphlet *Margery-Harvard-Veritas: A Study in Psychics* (1925).

Reflecting upon the fragmentation in psychical research in 1926, Hans Driesch invoked the many different names currently in use to denote specific approaches in the field.⁵⁷ In addition to the English “psychical research”, there was the French “*métapsychique*” and the German “*Parapsychologie*”. Both evoked similar associations about a special science related to mainstream psychology, but with their own peculiar histories and charged with special connotations. They denoted two major attempts at creating a paradigmatic framework for the discipline. In the French case more than in the German, there was also a determination to build institutions, and with it, a push towards internationalisation. In this section we shall briefly review these two schools, starting with German *Parapsychologie*.

* * *

The word “*Parapsychologie*” can be traced back to an article in the occult journal *Sphinx*, written by the philosopher Max Dessoir (1867–1947) in 1889. German psychical research had remained tightly connected to the general occult scene, sharing the journals *Sphinx* and *Psychische Studien* with Theosophists and spiritualists.⁵⁸ Dessoir belonged to a faction within German psychical research that wanted to take a more scientific approach, following in the vein of the British pioneers. He therefore suggested the term *Parapsychologie* to identify an emerging scientific field that studied the ‘pathological conditions’ of this ‘border area between the average and the abnormal’.⁵⁹ Dessoir’s proposal apparently fell on fallow ground, however, for the word was not adopted and a more scientific discourse on psychical research (referred to rather as ‘*Psychische Studien*’) did not manifest in Germany for many years still. The first real attempt to consolidate German psychical research around a set of ostensibly “scientific” procedures, partly institutionalised through the promotion of specialist journals, centralised offices and laboratories, slowly emerged in the years before the Great War. The leader of these efforts was Baron Albert von Schrenck-Notzing (1862–1929) in

⁵⁷ Driesch, ‘Psychical Research and Philosophy’, 161.

⁵⁸ For the German situation in this early period, see Bauer, ‘Periods of historical Development of Parapsychology in Germany’; cf. Wolfram, *Stepchildren of Science*, 33-82.

⁵⁹ Dessoir, ‘Die Parapsychologie’, 342. My translation.

Munich.⁶⁰ Originally trained as a physician, and versed in the controversial field of medical hypnosis,⁶¹ Schrenck-Notzing became a man of independent means after his aristocratic marriage to Gabriele Siegle in 1892. Eventually, this marriage and the financial capacity he thus acquired would be essential in establishing a laboratory for psychical research, and for further attempts at consolidation and institutionalisation of this field in Germany.⁶²

In the years before the Great War, Schrenck-Notzing established a laboratory in his palatial home on Karolinenplatz in Munich, furnishing it with all sorts of medical equipment, special lighting, and cameras. His intention was to work with spiritualist mediums, under conditions that were controlled and well-equipped for research, nonetheless it still had the atmosphere of a spiritualist séance room. As Wolfram writes, the Baron's laboratory room was 'a hybridisation of spiritualist and scientific space that posed less of a threat to its psychologically fragile subjects than those laboratories found in the hard sciences'.⁶³ The presence of instruments of measurement and observation were particularly important since Schrenck-Notzing had a marked preference for working with those notorious "physical" mediums who claimed to produce not only "mental" phenomena of telepathy, clairvoyance, or coded messages from beyond the grave, but various physically *tangible* effects as well. That is to say, the "Geisterbaron" was interested in moving objects, levitations, ectoplasm, and full-form materialisations. Indeed, one of the lasting contributions of the research programme in Munich is the great number of extraordinary photographs of famous mediums with

⁶⁰ On Schrenck-Notzing, see Wolfram, *Stepchildren of Science*, 131-189. A problem with our knowledge about this important phase of German psychical research is that most of the unpublished Schrenck-Notzing material has been lost. This goes for almost his entire scientific correspondence, as well as personal papers, diaries, and notebooks. Much appears to have been lost during the 1930s, when the baron's estate in Munich was expropriated by the Nazi government, seeking to establish offices there. Other parts were stored in a building in Munich that was bombed during the war; some material might also have been deliberately demolished. For these reasons, there is not so much evidence on Schrenck-Notzing's research except what he published. Conversation with Dr Andreas Fischer (IGPP, Freiburg), 28th October, 2010.

⁶¹ His main contribution to this field was published in 1888: Schrenck-Notzing, *Ein Beitrag zur Therapeutischen Verwerthung des Hypnotismus*.

⁶² Cf. Wolfram, *Stepchildren of Science*, 57.

⁶³ *Ibid.*, 132.

ectoplasm issuing from the orifices of their bodies and taking the shape of the spirits channelled. Some of these were published in the book *Materialisations-Phänomene* in 1914, including famous flash photographs from laboratory séances with the mediums Eva C. and Stanislaw P.⁶⁴

Schrenck-Notzing's reports on experiments with such "gifted" mediums show him quite unwilling to offer up clear explanations and hypotheses for the phenomena observed, beyond the fact that he considered them to be for the most part "genuine". He did, however, clearly identify with the "animist" as opposed to "spiritualist" camp that we discussed in the previous chapter.⁶⁵ For example, the preface to the first German edition of *Materialisations-Phänomene* started by noting that spiritualism had become completely 'discredited', and that this fact was causing a great deal of trouble for serious and intelligent men who had dedicated themselves to its study.⁶⁶ By agreeing that spiritualism was discredited, the baron interestingly positioned himself differently from most second-generation colleagues in the SPR. What his exact position was, however, is much harder to establish, as the baron remained vague about what animism really entailed. Formulating himself in a language reminiscent of earlier German romantic *Naturphilosophie*, particularly in the Mesmerist tradition, Schrenck-Notzing argued that his experiments might succeed in 'directing attention to a dark and unexplored side of human Soul Life, and in particular to certain problematical psycho-physical effects'.⁶⁷ Characteristically, Schrenck-Notzing ended a chapter on 'Facts and Hypothesis', which read pretty much as a "Stand der Forschung" for physiologically and psychologically oriented research on mediums up until the Great War, with the non-conclusion that: 'it is advisable to-day to verify, to observe, and to *refrain from conclusions*'.⁶⁸

In other words we have the presumption of pure experiment without theory – or, at the very least, a minimum of theory. Nevertheless, it is obvious that Schrenck-Notzing *did* presuppose certain hypotheses, and that these guided his experimental choices. For

⁶⁴ Translated into English in 1923, under the title *Phenomena of Materialisation*. The English edition also included certain later material that had appeared after the initial publication in 1914, and was furnished with no less than 225 illustrations.

⁶⁵ The preface to the first German edition of *Materialisations-Phänomene* started by reassuring that the spiritualist hypothesis was completely discredited, for example.

⁶⁶ See *ibid.*, v.

⁶⁷ *Ibid.*, vi

⁶⁸ *Ibid.*, 28-36, 35. Emphasis added.

example, supernormal abilities were due to unexplored functions of the human organism, and, as with other abilities with a physical basis, people might be unequally endowed, some being more talented than others. On this assumption, it made sense to select only those who seemed to possess some special talent, and the palatial laboratory in Munich became a centre for some of the most well-known, not to say infamous, physical mediums on the continent. The hypothesis of rare talent even led Schrenck-Notzing to start controlled *training* of some of his mediums, claiming thereby to increase their skills at producing certain phenomena under difficult experimental circumstances. This was achieved through the curious use of “medium contracts”: the baron would legally bind certain talented mediums to his laboratory for longer periods of time, so that he could help them “unlearn” their spiritualistic habits, and train their supposedly “natural” skills to work optimally under the desired conditions.⁶⁹

Incidentally, having top mediums in residence (Schrenck-Notzing would pay an allowance, provide lodging with well-off friends in Munich, and in some cases even provide basic health care packages) also had the effect of *monopolising access* to the “raw data” of psychical research in Germany. It became very difficult for anyone to work in the field independently of Schrenck-Notzing, since he possessed the legal right to some of the best mediums.⁷⁰ Over the course of a decade, the baron was building up near complete dominance of the field from his base in Munich. He acquired editorial control of the journal *Psychische Studien*, which had been around since 1874. In 1925 he renamed it *Zeitschrift für Parapsychologie*, a name that more appropriately signified the scientific pretensions of Schrenck-Notzing’s project.⁷¹ However, Wolffram has shown that the increasingly monopolistic and autocratic position Schrenck-Notzing took in German parapsychology eventually led to a split, as the dissatisfaction of scholars and researchers of a younger generation grew.⁷² His opponents felt that Schrenck-Notzing was using his money, influence, and social position to set the agenda for German parapsychology, in so far as he controlled the editorial staff of the field’s leading journal

⁶⁹ For example, he had Eva C. for four years, while the Polish medium Stanislava P. was held for observation over a six-month period. Cf. Wolffram, *Stepchildren of Science*, 169.

⁷⁰ See Wolffram, *Stepchildren of Science*, 162-169. Wolffram especially looks at the case of the Austrian boy and “gifted” medium Willy Schneider.

⁷¹ Cf. *ibid.*, 60-61, 133.

⁷² *Ibid.*, 165-169.

and dictated its content.⁷³ In 1925, a group of opposing researchers founded the *Zeitschrift für kritischen Okkultismus*, a journal staking out a quite different course for parapsychology, both in terms of content and methodology. It was run by a number of people whose expertise in the field had been somewhat pushed aside by Schrenck-Notzing's dominance, including the psychologist Richard Baerwald, the lawyer Albert Hellwig, and the ophthalmologist Rudolf Tischner.⁷⁴ Initially, the baron responded to this new initiative by attempting to become a member of the editorial board. This was, however, precisely the sort of thing the rebels protested against, and his candidacy was duly rejected.⁷⁵

Similar to what had happened that very same year in England and the United States, where old institutions broke up and new ones were created, the split between Schrenck-Notzing and the "critical occultists" became a battle over what counts as appropriate scientific conduct in psychical research. Although the name today rings odd, the critical occultists in fact took a hard-nosed naturalistic attitude to parapsychology, positioning themselves close to the utterly sceptical "psychology of the occult" that we encountered in the previous chapter. With reference to the strategies of naturalisation that we discussed earlier, the critical occultists were thus leaning towards *reductionist* strategies, including "negative" ones in which the phenomena were explained away by psychopathology, illusion, etc., per the research of Joseph Jastrow. In fact it was this rather confrontational strategy that prompted Schrenck-Notzing to change the name of *Psychische Studien*, and to give even stronger rhetorical emphasis on "science" in order to defend the status and reputation of his line of research.⁷⁶ The battles that ensued between the schools eventually led to the downfall of Schrenck-Notzing's Munich laboratory. When he died suddenly in 1929, his research programme faced the only destiny one would expect for an institution built on one man's autocratic leadership. Meanwhile, no other institution managed to take its place. Although a few individual researchers remained active, Germany would not have an experimental parapsychology

⁷³ *Ibid.*, 165.

⁷⁴ *Ibid.*

⁷⁵ *Ibid.*

⁷⁶ *Ibid.*, 165-168.

programme again until Hans Bender (1907–1991) established the Institut für Grenzgebiete der Psychologie und Psychohygiene in Freiburg in 1950.⁷⁷

* * *

The term *Métapsychique* was first suggested in a presidential address before the SPR in April 1905 by the Society's first French president, the physiologist and later Nobel prize winner Charles Richet.⁷⁸ The situation of psychical research in France at the beginning of the 20th century was somewhat different from that of Germany, due to the presence of research on “supernormal” phenomena in the context of established psychiatry and physiology, as well as a more active collaboration with England.⁷⁹ Richet, who had also coined the term “ectoplasm” in response to a series of sittings with Eusapia Palladino in 1894,⁸⁰ was an important scientific ambassador for psychical research in France. He had conducted research on somnambulism and suggestion in the context of mainstream psychiatry, and was a driving force for establishing a French journal for psychical research in 1891 (*Annales des sciences psychiques*).⁸¹ In the 1880s he also did some important quantitative work on thought transference and card readings that involved a use of probability analysis that was advanced for its time; however, he would soon turn away from this way of working and focus on qualitative aspects.⁸²

⁷⁷ Ibid., 195-177. Cf. Bauer, ‘Periods of historical Development of Parapsychology in Germany’. An additional reason for this delay is no doubt the rise to power of the NSDAP. As seen from the correspondence of people like Driesch and Bender in the 1930s, psychical research was made increasingly difficult in the Third Reich – despite some circumstantial evidence of a short lived attempt by the state to establish a parapsychological research group under the propaganda department. See e.g. Bender to J. B. Rhine, October 16, 1936 (Bender - Rhine correspondence, 1936-1950; IGPP-Archiv 10/5, A II 13).

⁷⁸ Richet, ‘La Métapsychique’, 13-14.

⁷⁹ For a historical overview of the developments in France, from “spiritism” to the various schools of psychical research and “metapsychics”, see especially Sofie Lachapelle, *Investigating the Supernormal*. Cf. John Warne Monroe, *Laboratories of Faith*; Bertrand Méheust, *Somnambulisme et médiumnité*.

⁸⁰ The sittings were held at Richet's cottage on a remote island off the southern coast of France, and was also attended by Oliver Lodge and Frederic Myers. See Grean Raia, ‘From Ether Theory to Ether Theology’, 19-20.

⁸¹ Cf. Lachapelle, ‘Attempting Science’, 4-5. Cf. idem, *Investigating the Supernatural*, 113-142.

⁸² Richet, ‘La suggestion mentale et le calcul des probabilités’. See my discussion of quantitative research below.

During the first two decades of the 20th century several attempts were made at institutionalising psychical research in France, but without much lasting success. The *Annales des sciences psychiques* became a central rallying point for these attempts, and from 1908 until the Great War the journal was officially connected with the Société Universelle d'Études Psychiques (SUEP), a small psychical research society based in Lille.⁸³ As documented by Sofie Lachapelle, a great number of regional journals and small societies popped up in France during this period, inspired by the efforts of the *Annales*. Among these were the Institut Psychique International in Paris (founded in 1900), which later morphed into the more mainstream psychological society Institut Général Psychologique (IGP).⁸⁴ While this society did host some famous experiments with Eusapia Palladino between 1905 and 1907,⁸⁵ it would generally abandon psychical research and focus on more respectable topics of psychology. This created a vacuum, which was to be filled only after the war by the Institut Métapsychique International (IMI).⁸⁶

The idea of this institute was conceived by the physician and committed spiritist Gustave Geley (1868–1924), with the help of Italian epidemiologist Rocco Santoliquido (1854–1930).⁸⁷ Similar to what we have seen in other national contexts, Geley and Santoliquido represented a faction that was dissatisfied with the lack of recognition of psychical phenomena within mainstream psychology on the one hand, and the lack of rigorous scientific thinking among “spiritists” and psychical researchers on the other. To remedy the situation, they wanted to build a fully equipped laboratory, a library with the best available literature, and an information office to disseminate knowledge to the public and to educate mediums.⁸⁸ In addition to this, international collaboration was emphasised, arguing that a unification of research efforts in different countries would

⁸³ Cf. Lachapelle, ‘Attempting Science’, 5.

⁸⁴ Ibid., 5.

⁸⁵ These are portrayed, together with her other continental sittings, in Hereward Carrington, *Eusapia Palladino*, 129-134. In Paris, the medium was tested by a number of highly esteemed scientists and academics, including Richet, Henri Bergson, and Pierre and Marie Curie.

⁸⁶ Cf. Lachapelle, ‘Attempting Science’, 5-7.

⁸⁷ Following convention, I use the term “spiritist” to denote the peculiarly French reception of mediumistic phenomena that were given a unique spin by Allan Kardec in the mid-19th century, thus distinguishing it from generic “spiritualism”. See e.g. John Warne Monroe, *Laboratories of Faith*, 95-149.

⁸⁸ Lachapelle, ‘Attempting Science’, 6.

be beneficial to the construction of a solid and progressive research programme.⁸⁹ The plan was finally realised after Santoliquido and Geley were introduced to Jean Meyer, a wealthy industrialist and spiritist. Meyer was convinced to finance the establishment of a spiritist institution (Maison des Esprits) and a scientific laboratory for psychical research.⁹⁰ The IMI was born in spring 1919, part product and part reaction to earlier attempts of psychical researchers, both in France and abroad. The IMI availed itself of similar strategies as those used by the early SPR: it created an organizational body on the model of the professional scientific association, as had the SPR; it established a laboratory, as had Schrenck-Notzing; and it recognized the importance of setting up strict boundaries to dissociate the group's activity from blatantly "unscientific" practices. The adoption of the term *métapsychique* was itself a part of such boundary-work at a time when the term "psychical research" (or "science psychique") was becoming increasingly tainted with associations of spiritualism and occultism.⁹¹

The IMI did not appear to introduce anything radically new in terms of research methodology. The main focus was on mediumistic phenomena, and Geley, who became the first president, directed research towards his own favourite topic, ectoplasm.⁹² Thus, judging from Geley's 1924 book, *L'ectoplasmie et la clairvoyance*, the IMI's research did not differ much from what was going on in Munich. A somewhat broader spectrum of interest is nevertheless reflected in the pages of the new journal that the Institute established in 1920, the *Revue métapsychique*. As Lachapelle summarises, the journal

published research on mediumistic productions and other faculties of the mind, on extreme manifestations of religiosity and mysticism, and on phenomena of a similar kind produced in the Orient; it provided discussions on ways of reconciling metapsychical phenomena with the laws of physics and biology; and it presented practical considerations on the role of metapsychics in medicine and justice. Moreover, the *Revue métapsychique* always contained book reviews and news of the field around the world. It put itself forward as a unifying venture into the scientific study of the psychical.⁹³

⁸⁹ Ibid.

⁹⁰ Ibid., 7.

⁹¹ See the next chapter for a thorough discussion of these strategies, and the way they would eventually make professional parapsychology possible.

⁹² Ibid., 8.

⁹³ Ibid., 7-8.

Apart from the president, the society was governed by a directing committee that represented a continuity with the past as opposed to a radical break from it. The committee included Richet and the famous physicist and spiritist Camille Flammarion, while celebrities such as Oliver Lodge and Hans Driesch were soon invited from abroad.⁹⁴

As to the theoretical standpoint of the IMI, Richet's notion of *métapsychique* unsurprisingly dominated. Richet himself gave the approach its fullest expression in *Traité de métapsychique* (1922), where it was defined as 'a science which object is the phenomena, mechanical or psychological, that are caused by forces that seem intelligent, or by unknown latent powers of the human mind'.⁹⁵ This, obviously, was an extremely open definition of a field. Characteristically, it focused on certain "phenomena", while providing nothing concrete in terms of theory. Richet's *métapsychique* appeared, indeed, even less theoretically rigid than Schrenck-Notzing's *Parapsychologie*, in that the definition gave room for "spiritualist" in addition to "animist" explanations (e.g. external "intelligences" as well as "latent powers"). Richet even wrote explicitly in the book's introduction that no theory would be proposed since all theories seemed to him equally insufficient.⁹⁶ This, however, did not keep the Nobel laureate from summing up the whole field by mention of three *concepts*: "cryptesthesia" (*cryptesthésie*), "telekinesis" (*télékinésie*), and "ectoplasm" (*ectoplasmie*).⁹⁷ These were presented as "facts", but it does not take too much competence in the philosophy of science to see that they were much more than that, each concept presupposing specific theoretical choices. For example, as illustration of what he termed "subjective metapsychics" of the cryptesthetic kind, Richet mentioned how the assassination of Queen Draga of Serbia had been announced by a medium in Paris at the very moment when it was committed in Belgrade, without there being any possible 'normal cognisance of the crime'.⁹⁸ But to

⁹⁴ Cf. *ibid.*, 7.

⁹⁵ My translation. French original reads : 'une science qui a pour objet des phénomènes, mécaniques ou psychologiques, dus à des forces qui semblent intelligentes ou à des puissances inconnues latentes dans l'intelligence humaine.' Richet, *Traité de métapsychique*, 5.

⁹⁶ Richet, *Traité de métapsychique*, i.

⁹⁷ *Ibid.*, ii.

⁹⁸ *Ibid.*, 4.

classify the event as “supernormal” already presupposes a “metapsychical” theory of perception, which allows the interpretation of the event as something else than a chance occurrence. Similar objections could be raised for the other categories presented by Richet as theory-neutral “facts”.

Indeed, a rather harsh criticism along these lines was put forward by the American psychologist Joseph Jastrow a few years later, portraying Richet’s *métapsychique* as no less theory-laden than Conan Doyle’s spiritualism:

Is it possible that M. Richet does not see that in crediting as *facts* the thousand and one things that transcend the scientific experience, and which he admits are wholly discredited by his scientific confrères, he is woefully begging the question or befogging the issue? Is he unaware that he is assuming, inferring, conjecturing, asserting, imagining or throbbing the theory that they are of supernormal origin? Is he unaware that while professing to refrain from theories, he is none the less theorising, subtly theorising, boldly theorising at every step? Unaware that the metapsychic position is no less a theory, indeed a highly speculative fantastic hypothesis, an extravagant conjecture, quite as much as the theory of spirit agency which Sir Arthur Conan Doyle holds with every defiance of elementary logic, but which Professor Richet regards as a needless or unfounded theory in the special sense of a detailed *modus operandi* or “mechanism” theory of how the effects are produced, but which effects he regards as of supernormal origin quite as much as does Conan Doyle?⁹⁹

It seems safe to say that Richet’s metapsychics did little besides updating Myers’ “supernormal” and adding another layer of sciency rhetoric. In other words, a more robust and successful methodological paradigm was still not in sight. Instead, from the mid-1920s IMI would go through exactly the same kinds of conflict between a pro-science and a pro-spirit(ual)ism branch as characterised the American, British, and German research communities in the same period. In 1926 René Sudre made an attempt to update metapsychics and place the discipline within a consistent and meaningful theoretical and methodological framework, but succeeded only in creating discord in

⁹⁹ Jastrow, ‘The Animus of Psychological Research’, 298. To illustrate the general tone of Jastrow’s critique – which was nothing short of scathing – here is his initial verdict on the introduction to Richet’s book: ‘In these less than six hundred words from a book of more than six hundred pages, there are involved enough logical fallacies ... to occupy a class of sophomores profitably and with only an elementary depth of analysis for six hours, and a class of graduate students prepared to enter into all the ramifications of the logical intrigue for six weeks.’ *Ibid.*, 298.

the society. His *Introduction à la métapsychique humaine* attempted to set the discipline on stricter naturalistic lines than before by completely dismissing the spirit hypothesis and going for an *animist* position, in which the universe was seen to contain irreducible creative forces – not dissimilar from the philosophy of Henri Bergson. For his efforts, Sudre was rewarded with a message declaring him *persona non grata* as contributor to IMI's journal, the *Revue métapsychique*.¹⁰⁰ Expelled from the journal for suggesting a modest pro-science view, Sudre withdrew from the society altogether, and was followed by others likeminded. Shortly after this episode, the internal ideological frictions became even clearer, as did the fact that the problem stemmed from the IMI's source of funding. An open conflict between the financial founder, the spiritist Jean Meyer, and Santoliquido, who was then president of the institute, erupted in 1928. Meyer, realising he was getting older, made certain institutional and financial arrangements for the future, which caused much concern. The new arrangements implied that the society's board of directors was stripped of their power to decide on financial matters. How the society was to spend its money was instead put solely in the hands of private shareholders who were for the most part active spiritists, handpicked by Meyer himself.¹⁰¹ The decidedly biggest shareholder was Meyer's old butler, a certain M. Forrestier, who had gradually gained more influence over his master, rising from butler to secretary before ending up as Meyer's private spirit medium.¹⁰² When Meyer's will turned out to leave the ex-butler a large part of the inheritance, the family contested the authenticity of the will. They gained the support of the IMI, fearing that the spiritists had succeeded in taking control of the resources which had until then been used to support the scientific and presumably nondenominational efforts of the institute. Sure enough, the conflict ended with Forrestier blocking IMI's funds, and the organisation was thrust into a state of serious financial insecurity.¹⁰³

The IMI's final demise was, however, due to quite different factors. Somewhat surprisingly, perhaps, the organisation's fall came as a result of its aggressive policy of internationalisation.¹⁰⁴ During the inter-war period, there was a total of five

¹⁰⁰ Lachapelle, 'Attempting Science', 11.

¹⁰¹ Ibid., 11-12.

¹⁰² Ibid, 12.

¹⁰³ Ibid.

¹⁰⁴ Ibid., 18-22.

international conferences of psychical research in Europe, the first held in Copenhagen in 1920, and the last in Oslo in 1935.¹⁰⁵ At that point, cooperation had already become difficult due to the Nazi take-over in Germany, and the triumph, once again, of local patriotism over internationalism. Local patriotism was, however, a problem for international psychical research already in the late 1920s, as seen in the conflict ensuing over IMI's attempt to establish and lead a permanent international congress centre in Geneva. People such as Driesch, Lodge, and Carl Gustav Jung had been suggested for the provisory committee of the centre, together with the Paris/IMI troika Osty, Richet, and Santoliquido.¹⁰⁶ Driesch, being the German delegate to this venture, was given a reprimand by Schrenck-Notzing, who saw behind the initiative a covert attempt by the French to secure international domination of the field. The suspicion was strengthened when the French proposed that the next international conference, which was being planned for Athens in 1930, should be cancelled in order to avoid a bifurcation of the milieu of researchers internationally.¹⁰⁷ Due to these controversies, more people began to express hesitation about contributing to the project. When both Santoliquido and IMI's financier Meyer died in 1930, the institute's enthusiasm and momentum dwindled completely. In the end, no conference was held in Geneva. The Athens conference proceeded as planned, albeit with no French participation. The result of these events was a boost to English dominance, as the proceedings of these conferences being published for the first time in English rather than French.¹⁰⁸ The IMI had lost its momentum, and yet another ambitious plan to save psychical research through unification and scientization failed.

3. THE STATISTICAL TURN:

QUANTITATIVE EXPERIMENTALIST PROGRAMMES IN PSYCHICAL RESEARCH

¹⁰⁵ The three middle conferences were in Warsaw (1923), Paris (1927), and Athens (1930). For a short notice on these five conferences, see William Mackenzie, W. H. Slater, and Thorstein Wereide, 'Statement Regarding the International Congresses Held Between the Two World Wars', 134-135.

¹⁰⁶ Lachapelle, 'Attempting Science', 19.

¹⁰⁷ *Ibid.*, 20.

¹⁰⁸ *Ibid.*, 20-21.

The psychical research programmes we have considered so far share a number of commonalities. The SPR in England and the United States, the IMI in France, and the Munich laboratory of Schrenck-Notzing in Germany were all situated in a theoretical field that oscillated between spiritualism and animism. Their methodologies were open, in the sense that they would typically claim to bring in as little theoretical baggage as possible. In reality, however, they all presupposed that psychical phenomena were connected to certain especially gifted individuals. From this assumption it followed that the method of choice should be of a *qualitative* rather than a *quantitative* type. While all these projects emphasised some form of experimentalism, their laboratories were largely coextensive with the séance room. Special procedures, protocols, equipment and methods would be brought in, such as Schrenck-Notzing's photographic cameras and medical equipment, or the hermeneutical tools of the SPR's cross-correspondence research. Nevertheless, their methods remained qualitative: knowledge was to be built on a careful assessment of specifically chosen extraordinary events, conducted on a case-to-case basis.

Meanwhile, a *quantitative* research tradition was also in the process of being developed.¹⁰⁹ The quantitative programmes differed from the qualitative ones on a number of counts. They moved from the study of the minutiae of special cases, to isolated and measurable phenomena that could be tested in repeated runs, with many subjects, which yielded results that could be analysed statistically. Intriguingly, most of this work was carried out in institutional and social settings that were quite different from the groups we have discussed thus far. With a few notable exceptions, the quantitative approach was developed by those who pursued psychical research in the setting of ordinary universities. In this section I shall give an overview of these programmes. But, before moving to the most important quantitative programmes, which we find in American universities from around 1912 onwards, we should first take a quick look at the ambiguous position of quantitative research in the traditional

¹⁰⁹ Once again I wish to make it clear that my use of "research programme" is intended to be read along the lines of Lakatos. In other words, the "quantitative research programme" that I speak of here is a "rational reconstruction", which identifies and discusses certain traits of theory formation and methodology that were developed, sometimes independently of each other, sometimes in a historical continuity. Thus, one should be careful not to confuse a "research programme" with a specific and unified "tradition".

psychical research societies. What was the relation between statistics, probabilities, and controlled experiments, on the one hand, and qualitative case studies, on the other? What kinds of theoretical and methodological problems were such practices connected with in the literature? Briefly considering these questions makes us better equipped to appreciate the parallel developments in university contexts, and evaluate the degree of novelty and innovation they represent.

* * *

Despite a preference for the study of mediums, quantitative studies of isolated effects such as telepathy had already been conducted in the early years of the SPR.¹¹⁰ In fact, there was even a prolonged debate about statistics and probabilities in the 1880s, sparked by Charles Richet's early work.¹¹¹ Richet appears to have been the first to apply probability calculus to the guessing of playing cards in a larger population, for which he found some very slight evidence of thought-transference – so slight, in fact, that he would soon enough conclude that card-guessing was a useless method, and that it was more fruitful to turn to the hypothesis that genuine supernormal faculty was rare.¹¹² Following Richet's publications, the economist F. Y. Edgeworth, an expert on statistics, contributed a series of papers to the SPR journal that explained the use – and *misuse* – of probability calculus.¹¹³ For example, Edgeworth warned that even when probabilities seemed to rule against a pure chance result, and thus indicating that there is *some* agency involved, '[t]he calculus is silent as to the nature of that agency – whether it is more likely to be vulgar illusion or extraordinary law'.¹¹⁴ In other words, probabilities can only be used to show that "something is going on"; that is, they may suggest some *correlations*, but say nothing about the mechanisms involved in causing the result.

The development of probabilities introduced a new and popular rhetorical tactic to the psychical research literature of the 1880s and 1890s, in which probabilities

¹¹⁰ Murphy, 'Telepathy as an Experimental Problem', 266. For early examples, see e.g. Eleanor Sidgwick's reports in the *Proceedings of the SPR*, volume 8 (1892). See also Lodge, 'Experiments in Thought Transference'.

¹¹¹ E.g. Richet, 'La suggestion mentale et le calcul des probabilités'.

¹¹² Cf. Hacking, 'Telepathy', 438-439.

¹¹³ Edgeworth, 'The Calculus of Probabilities Applied to Psychical Research', I and II.

¹¹⁴ Edgeworth, 'The Calculus of Probabilities Applied to Psychical Research [I]', 199; cf. Hacking, 'Telepathy', 441.

against chance were liberally invoked for any kind of phenomenon that was being discussed. These figures were, however, given without any standardised method of control, and we find some utterly ridiculous numbers put on the most unfitting type of material. Gurney et al.'s *Phantoms of the Living* is exemplary: in one case the authors wrote that 'the odds against the occurrence, by accident, of as many coincidences of the type in question ... are about *a thousand billion trillion trillion trillions to 1*'. Sometimes the authors did not even bother spelling the figures out because the number was so big: '[t]he argument for thought-transference ... cannot be expressed here in figures, as it requires 167 nines – that is, the probability is far more than the ninth power of a trillion to 1'.¹¹⁵ These early amateur uses of probability sparked a sharp debate in the very first volume of the *Proceedings* of the American SPR, where one of James' colleagues, the philosopher, logician and mathematician Charles Sanders Peirce, lashed out at Gurney et al.: 'I shall not cite these numbers, which captivate the ignorant, but which repel thinking men, who know that no human certitude reaches such figures of trillions, or even billions, to one'.¹¹⁶

Despite these early debates and controversies, new attempts to join qualitative and quantitative aspects together appeared in the psychical research societies of the early 1900s. Another notable series of experiments was run by the SPR researchers Clarissa Miles (whom we have briefly encountered as a council member of the Alchemical Society) and Hermione Ramsden in 1905 and 1907.¹¹⁷ As with the previous experiments, these were quite crudely constructed. The "targets" used for thought transference were typically everyday objects, such as "spectacles", or natural phenomena, such as "sunset", making it extremely hard to calculate chance expectations. Furthermore, the experiments made use of a very limited number of subjects. In fact, the Miles-Ramsden experiments simply consisted of the two women "sending" thought fragments to one another from a distance at set times, with the one at the receiving end drawing down whatever came to her imagination. The only truly quantitative aspect was that the experiments were *repeated*, so that one could calculate how many

¹¹⁵ Gurney et al., *Phantasms of the Living*, volume 2, 17; volume 1, 34. Similarly unbelievable figures are found throughout the two volumes.

¹¹⁶ Peirce, 'Criticism on Phantasms of the Living', 150. Cf. the discussion in Hacking, 'Telepathy', 444-445.

¹¹⁷ See the report in *Proceedings of the S.P.R.*, volume 22, pages 60-93. Cf. Murphy, 'Telepathy as an Experimental Problem', 266.

experiments out of the total number of runs had been successful. However, repetitions were made under different conditions every time, and with no possibility of controlling against chance. These are precisely the kinds of circumstances that make the application of probability analyses quite misleading.

The “targets” that were to be “transferred” from sender to receiver in these experiments constituted another serious methodological challenge. When the aim is to transfer whatever mental object comes into the mind of the sender, it becomes impossible to judge against chance: the images might not be random, after all, but could for example follow mental habits shared by the two experimenters. Another problem concerns the standard for what counts as a “hit”: if the sender is thinking of a horse, and the receiver draws something that *might* look like the head of an animal with a mane, does that constitute a success or a failure? Would the same drawing have been considered a success even if the target had really been a lion? When transferring complex mental images, the answer always becomes heavily dependent on interpretation in each case. The experiments remain, in other words, half way between the quantitative and the qualitative.

This problem can be exemplified further by some aspects of René Warcollier’s *La télépathie* (1921), another work that stands half way between the qualitative and the quantitative. Warcollier was a chemical engineer, and did his research in the context of the Parisian IMI and the *Revue métapsychique*. He focused on how different mental states influence telepathic abilities, with a special interest in drowsy and semi-sleeping states, and the effect of drugs such as alcohol, coffee, and various sedatives such as antipyrine.¹¹⁸ Experiments with psychical phenomena under different mental conditions was one of the major contributions of Warcollier’s research.¹¹⁹ It was potentially a very important line of quantitative research, as it looked for different

¹¹⁸ See e.g. Warcollier, *Experiments in Telepathy*, 165. Here, Warcollier followed a longer line of speculation in psychical research that certain mental states were more susceptible to supernormal functioning than others. Myers already speculated in this direction, and coined several new terms in his *Human Personality* (see e.g. the glossary on pp. xiii-xxii) to distinguish between such states. Miles and Ramsden had also worked with these hypotheses, particularly with the half-sleep states that Myers had called “hypnogogic” (from waking to sleep) and “hypnopompic” (from sleep to waking).

¹¹⁹ See the report in *Proceedings of the S.P.R.*, volume 22, pages 60-93. Cf. Murphy, ‘Telepathy as an Experimental Problem’. For a much later assessment by practicing parapsychologists, see Russell Targ & Jane Katra, ‘Interpretive Introduction’.

correlations between effect rates and other factors. However, Warcollier also contributed qualitative analyses that attempted to explore the ways supernormal perceptions worked, including how images transmitted by telepathy would be “disturbed” and morph into similar, but different forms in the mind of the receiver (see figure 11 for an example). Looking for disturbances in the signal, however, means that the experiments have ceased to be tests of telepathy as such: that telepathic transfers happen is assumed from the outset, while focus is shifted from gathering evidential *support* to explaining the *function* of telepathy. To those yet unconvinced of the veridical status of the phenomena in question, this procedure would obviously appear premature, and bound to lead to severe biases in interpretation.

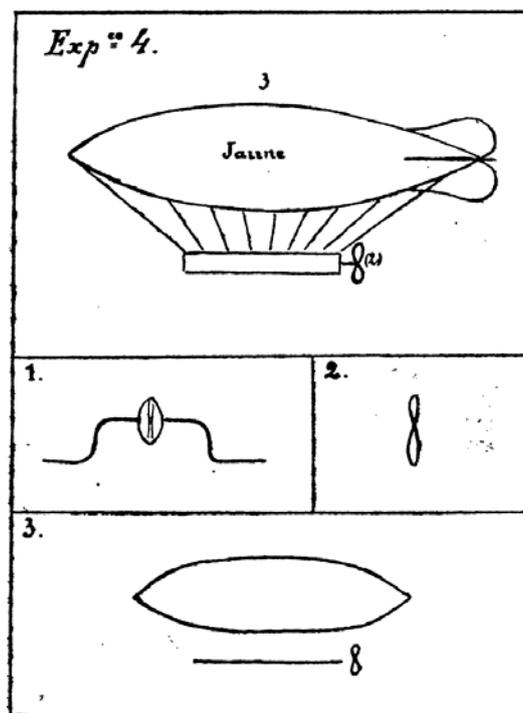


Figure 11: Example of Warcollier’s interpretation of disturbed telepathic signals. The top image of a zeppelin was the target, while the three abstract figures below were part of the “hit” – seen as elements detached from the whole of the target image. Reproduced from Warcollier, *La télépathie*, 238.

Gardner Murphy, who’s lecture at the 1926 conference at Clarke University amounted to an assessment of the relative strengths and weaknesses of previous experimental research, stated that the problem with Warcollier’s research was a

recurring lacked of sufficient statistical controls.¹²⁰ This, as I have suggested, seems to be a general trend for experiments that tried to bridge the quantitative and the qualitative.¹²¹ Attempts to mend these problems appear only to have been made once experiments in psychical research were conducted in the context of ordinary universities. Ironically, we shall see that Murphy's own attempts at improving earlier research ended up falling in the same trap. Other projects were methodologically more successful, however. In the following section I will describe four temporally consecutive research projects that all found their place within American universities in the period from 1912 to 1927. These are associated with the names John E. Coover, Leonard Thompson Troland, Gardner Murphy, and George Hoben Estabrooks. Of these four, I will devote the most space to Coover. The reasons for this will become apparent: Coover's was the first large-scale university sponsored research programme, and it was very significant in that it developed a sophisticated methodological framework that has even had an influence on methodology in the human and social sciences more generally. As we shall see, it also illustrates other problems and challenges in the field that are important. Coover, therefore, will be discussed in the first sub-section, while I will treat the three later projects together in the section following it. The four projects are united by an interest in "supernormal" *cognitive* functions (e.g. telepathy and/or clairvoyance), and a general commitment to *quantitative* methodologies. At the same time, the projects are divided on several crucial counts as well, including experimental design, rigour of controls, hypotheses advanced, and, importantly, the verdict reached on the phenomena's genuineness. What is more, the succession from one project to another

¹²⁰ Murphy, 'Telepathy as an Experimental Problem', 271.

¹²¹ But also in at least one case of university sponsored trials of telepathy, namely the research carried out by Dr H. J. F. W. Brugmans and his colleagues in the psychology department at the university of Groningen in the Netherlands, in 1921. Brugmans and his colleagues used only one test subject, a young physics student, and even though they took measures to avoid certain biases such as unconscious sensory cues, other methodological flaws are evident: the target used was not very well suited for probability analyses (mental habits not ruled out); an apparently naive form of randomisation; and the usual lack of controls to check against a null-hypothesis. As with the Warcollier experiments, Brugmans' was discussed in the psychical research literature primarily because it too had tested the impact on various types of stimulants, including alcohol and caffeine, and suggested that self-reported states of relaxation, and the intake of alcohol both increased telepathic performance. For descriptions and discussions of this experiment, see e.g. Murphy, 'Telepathy as an Experimental Problem', 268-270; Mauskopf & McVaugh, *Elusive Science*, 8.

cannot be understood in terms of “progress”; there is no straight-forward accumulation of data, and no direct line of improvement in experimental designs. Instead, the “black box” of telepathy has pretty much been reopened for every new project, each considering other hypotheses than the prior.

JOHN E. COOVER AND THE STANFORD FELLOWSHIP IN PSYCHICAL RESEARCH, 1912-1917

The most significant of the programmes that will be reviewed here is the one designed and carried out by psychologist John Edgar Coover at Stanford University between 1912 and 1917. Coover’s research is important for at least three reasons. To begin with, it was the very first time that psychical research was set up within a university programme, and led by a professional psychologist who was not committed to any prior agenda. Second, it was the first attempt to employ rigorous statistics and experimental controls on par with the best research methods available in psychology at the time. In fact, Coover even contributed to the development of stronger research methods in the human and social sciences at large, by creating more satisfactory controls and randomisation in the course of his telepathy research.¹²² Third, the Stanford Fellowship in Psychical Research is notable because it illustrates the tensions between personal agendas, funding of research, and scientific integrity. As we shall see, the Stanford position was only made possible by a generous contribution from a wealthy spiritualist and businessman, Thomas W. Stanford, who was also brother of the university’s founder, the Californian railroad tycoon Leland Stanford.¹²³

I will begin with this latter point, since it concerns the institutional and historical contexts in which Coover’s research was made possible. Similar to research societies outside of university structures, the Stanford Fellowship in Psychical Research was funded by someone with strong interests in a positive outcome. Thomas Wilton Stanford, who had emigrated to Australia in 1859 and made a fortune selling sewing

¹²² See Ian Hacking, ‘Telepathy: The Origins of Randomization’, 446-449. Hacking even suggests that Coover may have been the originator of the very term “control experiment”, in the technical sense it is used in clinical trials today. *Ibid.*, 449. The standard work on randomisation and control in experimental design was published only as late in 1935, in the statistician and geneticist Ronald A. Fisher’s *The Design of Experiments*. It is noteworthy that Fisher, too, published an article in the *SPR Proceedings* in 1924, where he suggested methods for working out the finer difficulties of analysing scores in runs of guessing playing cards. Fisher, ‘A Method of Scoring Coincidence in Tests with Playing Cards’.

¹²³ For a brief contemporary review of the circumstances, see Frank Angell, ‘Introduction’, xix-xxiii.

machines, was a dedicated spiritualist and a co-founder of the Victorian Association of Progressive Spiritualists in 1870.¹²⁴ In fact, Thomas was not the only member of the Stanford family to have invested heavily in spiritualism: Leland Stanford's wife, Jane, had a well-known interest in the subject, and both she and her husband had participated in séances after their son died in 1884.¹²⁵ The contribution from Thomas Wilton Stanford created some headache for the university administration, which on one hand wanted to avoid associating itself with spiritualism, but on the other had to pay respect to the founders and financial benefactors. Using the money to create a new Fellowship in Psychical Research under the psychology department in 1912 was a compromise of sorts, providing a way to do something scientifically useful while remaining faithful to Thomas Stanford's wishes.¹²⁶

Certain tensions between the research group and its benefactor would arise over the years that followed. Stanford seemed on the whole more interested in creating a room for spiritualism in the university than a centre for critical and constructive scientific work related to psychical research. One of his first requests after Coover had been appointed to direct the new research effort was that a number of books on spiritualism and occultism be purchased for the library: writing to the university in 1913, Stanford's secretary noted with pleasure that Andrew Jackson Davis' *Harmonial Philosophy* (perhaps the most important "theological" work to have emerged from the spiritualist movement), had been acquired, describing it as 'a work with which he [T. W. Stanford] is most familiar & commends its value in the highest terms of approval'.¹²⁷ The university staff were forced to handle the benefactor's personal interest more intimately when, in 1913, Thomas Stanford invited the chancellor and previous president of the university, David Starr Jordan, to see him in Melbourne and attend the weekly spiritualistic "Circles" that Stanford arranged at his offices with the medium

¹²⁴ E. Daniel Potts, 'Stanford, Thomas Welton (1832-1918)'.

¹²⁵ See, e.g., Theresa Johnston, 'Mrs. Stanford and the Netherworld'. The involvement of the Stanford family with spiritualism, and the relation to the early administrators and staff at the university – especially its first president, David Starr Jordan – is a fascinating topic that deserves further research.

¹²⁶ Cf. Angell, 'Introduction', xix-xx.

¹²⁷ Wm.J. Crook (T.W. Stanford's secretary) to W. E. Caldwell (secretary of The Board of Trustees, Leland Stanford University), June 10, 1913, John Edgar Coover Papers, Stanford University Library, Folder 1.

Charles Bailey. In a letter to Coover, Jordan wrote that he had decided to take this upon himself, and sarcastically remarked that he was 'likely to be a witness to "mysteries"'.¹²⁸

Jordan did travel to Melbourne in January 1914, and kept a long correspondence with Coover through the winter and spring. Their correspondence illustrates the anxieties spurred by the situation. Stanford was clearly a "true believer", and had enough faith in the medium Bailey to organise sittings in his office every week. Coover and Jordan, however, both knew that Bailey had in fact been exposed as a fraud several times in the past, including by the SPR.¹²⁹ This created a delicate situation, since Stanford expressed a wish to invite *both* Jordan and Coover to examine Bailey – clearly expecting that the extraordinary phenomena were genuine, and some real proof could be put on the table in his very own office.¹³⁰ Coover ended his letter to Jordan expressing reservations about having to conduct experiments with Stanford's favourite medium: 'I should like very much if you would tell me just what the situation appears to you to be; I may have to go over, and I am assuming exposure would be very unfortunate.'¹³¹

The worries proved premature, however, for when Bailey learned of Stanford's intentions to invite researchers to test him, he suddenly vanished for a longer period, and drifted off from his circle in Melbourne. Jordan was convinced that the medium was avoiding him, and also that the prospect of Coover's arrival had 'scared a rascal out of a prosperous trade'.¹³² With reference to these events, Coover himself would later write to Stanford, taking on a face of disappointment:

I was sorry to learn that Charles Bailey, who is credited with so much psychic power, has so ruthlessly cut himself off from those who could have helped him put the phenomena upon a scientific basis. It would seem that his voluntary action runs counter to the intent and purposes of the "messages" which he is instrumental in delivering.¹³³

¹²⁸ Jordan to Coover, December 8, 1913, John Edgar Coover Papers, Stanford University Library, Folder 1.

¹²⁹ Coover to Jordan, December 26, 1913, John Edgar Coover Papers, Stanford University Library, Folder 1.

¹³⁰ Bailey's speciality was to make live hummingbirds appear during séances. The SPR had suggested that he could keep them in a nest that could be 'concealed within natural cavities of the body'. Coover to Jordan, December 26, 1913.

¹³¹ *Ibid.*, 3.

¹³² Jordan to Coover, July 16, 1914.

¹³³ Coover to T.W. Stanford, June 28, 1917.

In the spring of 1916, when Coover was already well under way with his research, he filed a long report to the President of the university, Ray Lyman Wilbur, explaining the situation with T. W. Stanford.¹³⁴ The report made the delicate aspects of the matter explicit, particularly concerning Stanford's likely wish to donate his fortune to the university after his death (he was already in his 80s). Although Coover emphasised that Stanford had never interfered directly with research policies, he found it problematic that the benefactor showed so much interest in the results, and appeared to anticipate a vindication of spiritualism. Such expectations were in stark contrast to the realities of Coover's research: 'This anxious waiting for gratifying news, while results of research so far as the supernormal goes, are uniformly negative, creates an increasingly delicate situation', he wrote to Wilbur.¹³⁵ It was also clear that Stanford had wanted research into séance phenomena, while the line of approach taken by Coover was on experimental runs of telepathy. While he had also done some minor séance research on the side in San Francisco, Coover noted that this work was only useful as 'contributions to the psychology of deception, of delusion, of credulity, of inference, of belief, of scientific method'.¹³⁶ In other words, they conformed to the psychology of the occult in the tradition of Joseph Jastrow.¹³⁷

One final aspect in this difficult relation between the funder and the researcher is evident from a correspondence between Coover and president Wilbur in August 1916. This shows that Thomas Stanford was pressing the university to hurry the publication of results in psychical research.¹³⁸ Coover was put under pressure by Wilbur in this respect, and the research had to be concluded and put into a publishable form earlier than Coover himself wanted. After some additional delay, it was finally published as a monograph in December 1917. The question of how Stanford would respond to the final outcome of his investment was the central topic of Coover and Wilbur's correspondence

¹³⁴ Coover to Wilbur, March 16, 1916.

¹³⁵ *Ibid.*, 1.

¹³⁶ *Ibid.*, 2.

¹³⁷ Cf. Jastrow, *Fact and Fable in Psychology*.

¹³⁸ Wilbur to Coover, 28 August, 1916.

on the publication.¹³⁹ Despite earlier differences and worries, Coover expressed certainty that Stanford would approve:

I am confidently expecting Mr. Stanford to express his satisfaction with the work that has been done here in psychical research, the principal investigations of which are reported in the monograph, -not because he realizes that we have done but little in the line of his special interest (the investigation of "apports" and other séance phenomena), but because he, like any other intelligent and sound-minded business-man, cannot fail to see that we have attacked *genuine* "psychical" problems not only with vigor but with the *scientific* method, and he cannot fail to value such work even if he must leave its adequate comprehension to the specialist. I believe that Mr. Stanford will appreciate what others regard as solid work.¹⁴⁰

These assertions may have been designed primarily to relieve the president. In any case, we do not know of Stanford's final reaction to the work: he died in his home in Melbourne in August 1918, apparently leaving no statement on the work that has survived.

Whatever Stanford's final satisfaction with his investment may have been, it is clear that he would not find any vindication of the spiritualist hypothesis on any of the monograph's 641 pages. Instead, *Experiments in Psychical Research* contained endless tables of numbers and variables detailing the quantitative results of thousands of telepathy experiments done with lotto blocks, playing cards, and even "the sense of being started at". It was furnished with long and detailed discussions about methodology, about the protocols used, the controls of the experiment, and the randomisations used to rule out unconscious biases. If this was not enough already to put off the committed spiritualist, Coover had also added reports on research into cognitive mechanisms that could explain *away* apparent supernormal phenomena, including one on "mental habit", and a report on completely original experiments on people's tendency to hear meaningful messages in nonsense syllables.¹⁴¹ The monograph ended with an appendix that included a critical review of earlier quantitative approaches, tellingly entitled 'Grounds for Scientific Caution in the

¹³⁹ Wilbur to Coover, 28 Dec. 1917; Coover to Wilbur, 31 Dec. 1917.

¹⁴⁰ Coover to Wilbur, 31 Dec. 1917, 1.

¹⁴¹ Coover, *Experiments in Psychical Research*, 230-410,

Acceptance of the “Proof” of Thought-Transference’.¹⁴² No further comfort would be gained from reading Coover’s conclusion on the most elaborate series of experiments that he ran, namely the approximately 10,000 experiments with 100 subjects attempting to guess playing cards “telepathically”. ‘Statistical treatments of the data fail to reveal any cause beyond chance’, was Coover’s calm conclusion after four years of research.¹⁴³ Additionally, his 1,000 experiments with self-declared “psychics” or “sensitives” showed ‘no advantage for the psychics over normal reagents as claimants for the capacities of telepathy or clairvoyance’.¹⁴⁴ Furthermore, and this is significant in terms of the innovative methods employed, Coover reported that there was no difference between target and control groups – the latter meaning the cases in which, following randomised procedure, a card was guessed by the “reagent” even when no card had been read by an “agent”.¹⁴⁵ This was the first time that telepathy tests had been run against a control, and possibly the first time that a “null-hypothesis” had been tested systematically in any quantitative experiment whatsoever.¹⁴⁶ In this case, the null-hypothesis had been strengthened, meaning that there was no statistical difference between control and effect group, and hence no trace of any effect that needed to be explained by telepathy in the first place. A hypothesis of no effect would do.

Experiments in Psychological Research was well received by leading academic psychologists, including James Rowland Angell, and Edward B. Titchener.¹⁴⁷ Titchener no doubt saw in it a powerful weapon to use against psychological researchers and spiritualists who continued to latch on to the field and threaten its scientific credentials. He looked forward to see more such ‘debunking’ work being published.¹⁴⁸ Indeed, Coover’s monograph was supposed to be only the first volume in a series, and had been numbered ‘Psychical Research Monograph No. 1’. As it turned out, none would follow. Writing to the president of Stanford University in April 1918, Coover expressed a strong wish to take up a different line of research, making it clear that he thought it rather

¹⁴² *ibid.*, 461-502.

¹⁴³ *Ibid.*, 123.

¹⁴⁴ *Ibid.*, 142.

¹⁴⁵ *Ibid.*, 124.

¹⁴⁶ Cf. Hacking, ‘Telepathy’, 447-449. The expression “null-hypothesis” was only introduced about twenty years later, in Fisher, *The Design of Experiments* (1935).

¹⁴⁷ Cf. chapter five for the place of these men in American psychology in the early 20th century.

¹⁴⁸ Cf. Mauskopf & McVaugh, *The Elusive Science*, 53-54.

pointless to continue looking at psychical research after four long years of experimentation had borne singularly negative results. As he expressed it to president Wilbur, 'psychical research at Stanford University has avoided the precipitation of an academic scandal', by the thoroughness of its methodology.¹⁴⁹ However, 'I should have been better satisfied', Coover continued,

with an opportunity to put part of my time upon research the material of which is not so meagre and elusive, not so offensive in the nostrils of my fellow psychologists, and more directly applicable to problems in psychology, education, or psychotherapy.¹⁵⁰

With the United States now at war with Germany overseas, Coover wanted to join the Sanitary Corps of the Army, which he was able to do in November of 1918, with the rank of captain.¹⁵¹ In the meantime, he had arranged a plan to restructure the California Psychical Research Society in San Francisco, to have it function along responsible scientific lines, and watched over by council members that Coover handpicked himself (David Starr Jordan being the most prominent one).¹⁵² It is intriguing to note the rationale for this move: 'The presence of this Society should act as a deterrent to a revival of mediumistic charlatanry, likely to follow the sorrows and grieves of war as it has in England'.¹⁵³ This attests to some accurate cultural foresight on Coover's part; as we have seen already, spiritualism would flourish again in the United States in the 1920s as it did in England, and would lead to schisms in the Psychical Research Societies on the east coast.

THE RICHARD HODGESON MEMORIAL FUND: PSYCHICAL RESEARCH AT HARVARD, 1916–1926

Despite the negative verdict on telepathy in what was, from a scientific perspective, the decidedly strongest and most elaborate test ever to have been performed on a psychic phenomenon, Coover had not settled the matter as far as most psychical researchers were concerned. In 1924 a replication attempt was made by a recent PhD graduate from

¹⁴⁹ Coover to Wilbur, April 10, 1918, 1.

¹⁵⁰ Ibid.

¹⁵¹ Ibid.; Coover to Wilbur and Board of Trustees, November 8, 1918.

¹⁵² Coover to Wilbur, April 10, 1918; see attachment.

¹⁵³ Ibid., 1-2.

Columbia, Hulsey Cason, producing the same null result as Coover had: this was published in the *SPR Journal*, but the interest in telepathy continued to linger.¹⁵⁴ A major reason why researchers in the field could choose to distrust these negative studies was no doubt the theoretical elusiveness that characterised psychical research. Coover's experiments had for the most part used ordinary psychology students as its test subject, they could argue, and thus it had only tested the hypothesis that telepathy is a widely distributed and "normal" mental faculty, rather than a rare faculty which is possessed only by certain "gifted" individuals. As we saw, this had been Richet's conclusion already in the 1880s, turning to the rarity thesis when he failed to convince himself that the probability analysis of his own card guessing trials had yielded any satisfactory results. This would however not be an entirely fair objection against Coover, since he had run equally unsuccessful experiments with self-professed psychics, a group where one would expect to find at the very least *some* psychic ability if it existed at all.¹⁵⁵ But even in that case, there would be enough theoretical flexibility for the psychical researcher to avoid despair: there was also the hypothesis that psychic phenomena were not only *rare*, but also *spontaneous* in nature – a suggestion which had been at the basis of some of the first SPR work, and illustrated well in Gurney et al.'s *Phantasms of the Living*. On this hypothesis, any repetitious and tiring run of tests would fail to find the effect, because this kind of setting was a natural inhibition to the phenomenon itself. By analogy, the spontaneity objection could hold quantitative research on telepathy to be as futile as researching the capacity of falling in love by repeatedly exposing a test subject to pictures of attractive persons of the appropriate sex. That line of research might easily come to the conclusion that love is not proven to exist, as it had failed to appear under properly controlled test conditions. Any evidence of it would remain anecdotal.

It is thus no surprise that more research would be carried out in the context of the societies which sole reason for existence was to explore these alleged phenomena. It is perhaps more surprising that it continued within the university setting as well. This, however, was made possible exclusively by more private funding. Stanford was not the only place to have received private money for psychical research: two other American universities, Clark and Harvard, found themselves in a similar position. Clark University

¹⁵⁴ Cason, 'A Simple Test for Thought-Transference'; cf. Mauskopf & McVaugh, *Elusive Science*, 323 n. 33.

¹⁵⁵ Coover, *Experiments in Psychical Research*, 125-143.

had been given a donation by one J. A. Battles, the son-in-law of Joseph Smith – the founder of Mormonism.¹⁵⁶ Battles' will was however quite open about how the money was to be spent, and the university administration, then headed by the grandfather of American academic psychology G. Stanley Hall (1844–1924), got away with simply arranging some public lectures on the topic.¹⁵⁷ In fact, it was this money that would enable Carl Murchison's collaboration with William McDougall and Harry Houdini on the conference 'For and Against Psychical Belief' at Clark in 1926.¹⁵⁸ It would appear that this conference was the most significant result to come out of the Clark money.

Another grant, the Richard Hodgson Memorial Fund, was given to sponsor psychical research at Harvard University in 1912. Unlike the Clark endowment, the Harvard money was specifically tailored to fund actual research; and unlike the fellowship at Stanford, the grant was to be given on an *individual* basis to fund specific projects.¹⁵⁹ Over the years, the Hodgson grant would make three other significant series of experimental work possible. The first sponsored project at Harvard was conducted by Leonard Thompson Troland (1889–1932), in 1916/1917. Troland was an interesting character with a strongly interdisciplinary approach: he earned a BS in optics and theoretical physics at MIT in 1912, and continued to be engaged in physics research while he studied for a doctorate in psychology at Harvard between 1912 and 1915.¹⁶⁰ He was an early supporter of psychoanalysis, and would eventually be the inventor, vice-president and creative force behind Technicolor, Inc.¹⁶¹ Together with his old physics mentor from MIT, Daniel Frost Comstock, Troland even published a book that

¹⁵⁶ See Mauskopf & McVaugh, *Elusive Science*, 49.

¹⁵⁷ Cf. *ibid.*

¹⁵⁸ In the foreword to the proceedings, Murchison seems to have referred to this money when he wrote that: 'The President and Trustees of Clark University were favourable to the idea [of the conference], and voted the use of certain funds left to Clark University some years ago for such purposes.' Murchison, 'Preface', vii.

¹⁵⁹ Cf. Mauskopf & McVaugh, *Elusive Science*, 54.

¹⁶⁰ See his obituary in *American Journal of Psychology*: J. G. Beebe-Center, 'Leonard Thompson Troland: 1889–1932', 817.

¹⁶¹ *Ibid.*; cf. Troland, 'The Freudian Psychology and Psychical Research'.

included one of the very first expositions of relativity and early quantum theory in the United States, entitled *The Nature of Matter and Electricity* (1917).¹⁶²

Troland's background in physics clearly informed his work in psychology as well. Writing exactly at the time when behaviourism was emerging and winning ground in the United States, Troland's research was oriented towards psychophysiology, reflexions, and the analysis of cognitive, perceptual and motor actions in terms of the purely physical interactions involved. In another radical innovation of research methodology, Troland applied this wholly mechanical approach to the study of telepathy. He was even determined to get rid of the possible influence of the *experimenter*, and devised a set of machinery to replace the human factor.¹⁶³ This machinery consisted of a 'stimulus field' where a lamp would light up one of two square blocks in a completely randomised fashion. The agent (or "sender") would perceive the light in one room, while the physically separated reagent (or "receiver") would move a switch to try and indicate which lamp had been lit up in the other room. This introduction of mechanical instruments, and the "double blinding" that resulted from removing the experimenter himself, made the experiment even more rigidly controlled and randomised than Coover's had been.¹⁶⁴ As for the results produced, Troland found that his test subjects scored slightly, but significantly, *below* chance expectations.¹⁶⁵ This was in itself considered an interesting result; for example, a systematic error could arise from an *actual* "telepathic" communication gone bad somehow, not unlike the studies of distorted signals that we saw in Warcollier's research. Nevertheless, Troland did not find it interesting enough to warrant further research, and never returned to psychical research after these experiments.

¹⁶² It was also clearly inspired by the work of J. J. Thomson, whose portrait was included at the beginning of the book. The title alludes to Thomson's popular book from 1904, *Electricity and Matter*.

¹⁶³ His experiment design and results were published as Troland, *A Technique for the Study of Telepathy and Other Alleged Clairvoyant Processes*.

¹⁶⁴ Ian Hacking's study of randomisation and control in psychical research does not mention Troland's experiments. Nevertheless, they seem equally important as Coover's, seeing that they were designed only a little later, and introduced an important new feature in the double blind. It is to be noted that double blinds only became usual in medical science as late as the post-war era.

¹⁶⁵ Cf. Mauskopf & McVaugh, *Elusive Science*, 56.

While Troland's research may have been significant on a number of counts, it has largely passed into oblivion, even in the parapsychological literature.¹⁶⁶ A more famous product of Harvard's Hodgson Fund was the trials performed by Gardner Murphy (1895–1979) between 1922 and 1925. Murphy had been a research assistant in Troland's experiments, while he was still doing his MA, and was thus well acquainted with the methods.¹⁶⁷ His own research, however, differed widely, and can be seen as a more conservative approach to the subject than that of his immediate predecessors, both of whom had worked on the methodological cutting edge of experimental psychology. While Troland had implicitly been testing the hypothesis that psychic functioning was widely distributed, and Coover had emphasised it while also testing the rarity thesis, Murphy started from the hypothesis that psychic phenomena are *rare*, and furthermore, that they tend to occur *spontaneously*. His agenda was to establish methods that were able to take both rarity and spontaneity seriously. As he reported in 1926, just as the researches were completed:

I devoted about fifty per cent of my time during those years [1922–1925] to the search for individuals who claimed to have telepathic gifts, – my theory being that such gifts, if genuine, are rare, and that it is among those reporting extraordinary psychic experiences that experimental results are most likely to be obtained.¹⁶⁸

Murphy generally selected people who claimed to have experienced strange coincidences involving mental impressions and unconnected external realities. He

¹⁶⁶ On the short term, it received criticism by the philosopher and psychical researcher F. C. S. Schiller ('Review of *A Technique for the Study of Telepathy*'), and was included more favourably in the methodological review written by Coover's successor as Stanford Fellow in Psychical Research, John L. Kennedy, in 1939. The latter only mentioned Troland's study briefly in passing as an example of research that had been designed to exclude various biases, such as mental habits, unconscious perceptual cues, and recording errors. See Kennedy, 'Methodological Review of Extrasensory Perception', 64, 66. In the later historical literature, Beloff's sympathetic survey did not mention Troland's work at all, focusing on the (from a pro-parapsychology perspective) "progressive" events only. More curiously, Hacking does not discuss Troland in his otherwise very thorough discussion of developments in experimental design in telepathy research. The best discussion in secondary literature thus appears to be two-and-a-half pages in Mauskopf & McVaugh, *Elusive Science*, 54-56.

¹⁶⁷ *Ibid.*, 60.

¹⁶⁸ Murphy, 'Telepathy as an Experimental Problem', 271.

would then select these subjects for his experiments, which involved tests of the “supernormal” transfer of mental objects, ‘especially geometrical figures of various shapes’.¹⁶⁹ In this sense, his design was much more primitive than those of his immediate precursors, looking more like the earlier designs of Warcollier or the Miles-Ramsden experiments, relying on targets that were hard to subject to a fair probability analysis, and which, moreover, lacked the control for “mental habits” that had just recently become customary.¹⁷⁰ Murphy’s expressed goal was to ‘ascertain the statistical as well as the qualitative analysis’ of telepathic and clairvoyant events¹⁷¹ – in other words to get the best of both worlds when compared to the existing research traditions in psychical research.

Concerning the results, Murphy commented:

The great bulk of my telepathic work has yielded results closely comparable to those of Dr. Coover; that is to say, the vast majority of subjects give results which offer no difficulties of explanation in terms of coincidences. Some rather marked exceptions remain unexplained.¹⁷²

This statement reveals a fundamental ambivalence in Murphy’s experimental approach to psychical research. It indicates that Murphy allowed himself certain irregularities in the interpretation of his data, in order to accommodate the belief that psychic phenomena were unevenly distributed and hard to catch. Thus, a very few “high scoring” experiments are allowed to be seen *separate* from all the runs that were consistent with chance. From a methodological point of view this could easily lead to a form of selection bias, keeping the results that fit with the hypothesis while discarding the ones that do not. In the end, Murphy’s struggle to get the best out of two worlds, namely the quantitative and the qualitative, remained unresolved, the demands of one cancelling those of the other.

¹⁶⁹ Ibid., 272.

¹⁷⁰ I.e., controlling for the possibility that certain shapes suggest themselves more often than others to certain people. Both Coover and Troland had controlled for mental habits, and Coover even published a long discussion of this particular bias. See Coover, *Experiments in Psychical Research*, 230-368.

¹⁷¹ Murphy, ‘Telepathy as an Experimental Problem’, 273.

¹⁷² Ibid.

Whatever might have come of it, Murphy's research was cut short when he suffered a series of illnesses following a severe flu in 1925.¹⁷³ Murphy's successor at Harvard was George Hoben Estabrooks (1885–1973), who had been one of William McDougall's graduate students at Harvard.¹⁷⁴ It was also McDougall, now a professor of psychology at Harvard, who secured the grant money for Estabrooks to conduct his research while finishing his PhD. McDougall seems to have been a little disappointed with Murphy's achievements, or lack of such, and it is notable that, even as Murphy himself would admit, Estabrooks immediately set out to improve on the methods that Murphy had used.¹⁷⁵ These improvements, however, consisted primarily in making the experiments more similar to Coover's standards. Most importantly, Estabrooks changed the initial selection criteria, now going from the assumption that telepathy was ordinary, and hence accepting much larger samples of subjects. Estabrooks designed experiments that were strictly quantifiable – reverting again to experiments using decks of cards – and introduced more rigid statistical methods. He followed Troland on making use of a degree of instrumentation to avoid the human factor of the experimenter by introducing a telegraphic signalling device connecting the two rooms where the agent and reagents were seated, providing a purely mechanical way to decide when a “transfer” was to take place.¹⁷⁶

Estabrooks also introduced certain novelties of his own, primarily in the analysis of the data. For example, he conducted three series of experiments, the first one using a group of personal friends as test subjects, while the others used students pooled from psychology classes at Harvard. Estabrooks noted that the first series had been much more successful than the latter ones, even though the method had been the same: his novel suggestion was that the personal relationship with the test subjects created a laid-back attitude that made the subjects less mentally preoccupied with the experimental

¹⁷³ Following his illness, Murphy started building a solid career in mainstream psychology, writing two popular volumes, *An Historical Introduction to Modern Psychology* (1929), and *An Outline of Abnormal Psychology* (1929).

¹⁷⁴ Cf. Mauskopf & McVaugh, *Elusive Science*, 66-69.

¹⁷⁵ Murphy, 'Telepathy as an Experimental Problem', 275-276.

¹⁷⁶ For Estabrooks' description of his methods, see 'A Contribution to Experimental Telepathy', 194-197. Cf. Mauskopf & McVaugh, *Elusive Science*, 66-67. Estabrooks' report was first published by the Boston Society for Psychical Research in 1927; after having been out of print for years, it was reprinted in the *Journal of Parapsychology* in 1961. I am quoting from this later reprint.

situation – this, it was suggested, led to a conducive state for telepathy.¹⁷⁷ This assumption led to a second novelty in the *analysis* of such quantitative studies, namely that each run seemed to start off with more successes, and then run into *decline* as the experiments continued. This, Estabrooks suggested, could be a “fatigue effect”, meaning that the rapport between the communicating minds gradually disappears as the subjects get bored and mentally tired from too many repetitions.¹⁷⁸ In a sense, this analysis seemed more successful in reconciling the wishes Murphy had expressed; namely, to of uniting the quantitative with the qualitative and give justice to the “spontaneity thesis” without sacrificing in experimental control. It also seemed to imply that larger quantitative studies, like Coover’s, would be of little promise.

Estabrooks’ research at Harvard is also important in that it was a quantitative, well-designed experimental approach that actually seemed to produce positive results.¹⁷⁹ Particularly his first run, with friends as subjects, had come out very positive.¹⁸⁰ While this no doubt pleased psychical researchers, including McDougall, who even convinced the influential psychologist Edwin G. Boring to support Estabrooks for a while, the situation was now getting increasingly confusing: which studies to trust? What was the final verdict of quantitative experimental work on telepathy? With other well-designed and thorough experiments showing no effect, including in replication studies, it seemed that the only thing Estabrooks really achieved was to once again open the door of possibility for laboratory psychologists interested in psychical phenomena. But the reasons for doubt were equally, if not even better justified.

¹⁷⁷ Estabrooks, ‘A Contribution to Experimental Telepathy’, 199-200.

¹⁷⁸ *Ibid.*, 201.

¹⁷⁹ It was, however, methodologically weaker than Coover’s on at least two counts that I can see. First, the sizes were a lot smaller than those used by Coover. Secondly, Estabrooks had not been able to do satisfying control runs, such as Coover had. Intriguingly, it appears that McDougall had insisted that Estabrooks did such a control, but it did not happen because his test subjects refused to cooperate: ‘On the suggestion of Professor McDougall the writer attempted to retest as many as possible of his former subjects under conditions which would rule out all possibility of telepathy and use these results as a check. Unfortunately the men thought otherwise and would not cooperate. While they were perfectly willing to give a half hour once, they were by no means willing to do so twice much to the chagrin of the operator.’ Estabrooks, ‘A Contribution to Experimental Telepathy’, 205. This means that in practice the null hypothesis was not seriously considered in these experiments.

¹⁸⁰ Cf. Mauskopf & McVaugh, *Elusive Science*, 67.

4 CONCLUSION: ENCHANTMENT AND THE REIGN OF QUANTITY

We started this chapter by noting the disappointments of the first generation of psychical researchers at the dawn of the 20th century. To Henry Sidgwick it had seemed incredible that a couple of decades of organised psychical research should not have managed to bring any certainty whatsoever to even the most basic research questions. What would Sidgwick, Myers, James and the rest have thought, had they been able to witness the next few decades? The Sidgwick circle had been dedicated to salvaging what they thought was useful from spiritualism and occultism, and elevate the sanitised concepts of “telepathy” and “clairvoyance” to the finest levels of scholarship. The first two decades after their passing were characterised by the opposite direction: popularisation rather than the building of expertise, and a return to grass-root spiritualism in place of the careful search for scientific explanations of it. If that was not enough, mediums were now claiming to capture the souls of the first-generation pioneers themselves. Dependent on the bodies of spiritualist mediums to voice their opinions, the old researchers suddenly appeared much more supportive of spiritualism than they had ever been in real life.

I have suggested 1925 as the year when psychical research finally bifurcated into a “progressive” scientific wing, and a “conservative” spiritualistic wing.¹⁸¹ Before this point, the SPR and its sister and daughter societies across the Western world housed everything from simple spiritualist demonstrations, to purely philosophical reflections, to mildly quantitative experiments with specific effects. By 1925, it had become increasingly clear that to pursue the latter type of strictly scientific research, one had to leave the context of the traditional psychical research societies behind. One reason for this, I suggest, is that the expertise needed to design and conduct scientifically valid experiments and data analysis was getting increasingly sophisticated and specialised.

¹⁸¹ Please note that I do not intend this distinction in a *political* sense; politically speaking, spiritualists could be socialists while science-oriented psychical researches might just as well place themselves on the political right. A good example of the complexity of the political issue can be found in the next chapter, in the case of William McDougall. The distinction between “progressive” and “conservative” is only meant as a convenient short-hand for two different attitudes to psychical phenomena: one is primarily interested in building a “progressive science” around such phenomena, keeping and systematising the positive evidence, whatever it is and in whichever direction it points; the other is primarily interested in *conserving* the religious and doctrinal content of the spiritualist movement.

Experimentalism was becoming more refined, and the requirements for producing valid knowledge in the human and social sciences were getting more difficult for the amateur to meet. Above all, this reflects the fact that the academic discipline of psychology had become well-established by the 1920s, and characterised by extensive and sophisticated methodological debates that amateurs could not be expected to keep up with. It was, in other words, quite a different situation from the one in which psychical research had first been defined.

Adding to the problem of specialisation, the popular upsurge of spiritualism also led to controversies of a more personal and emotional kind. Post-war spiritualism, responding to the emotional need of a generation of bereaved people, had a curious ability to bridge the popular level and high society. It became difficult to approach the field with scepticism without appearing both elitist (contra the masses) and libellous (contra the bourgeoisie) at the same time. The case of Mina “Margery” Crandon and her bourgeois husband in Boston is an illustrative example: in this case, the very attempt to subject the medium to scientific testing led, when the genuineness was questioned and the hypothesis of fraud suggested, to the split of the American SPR, with the Crandon family and new SPR leadership actively curtailing critical voices.

While the field was fragmented at large, the “camps” themselves were also far from unified. The “conservative” wing remained necessarily heterogeneous, allowing the curious exploration of anything that seemed remotely supernormal as long as true scepticism was kept at arm’s length. The “scientific” wing, however, needed a larger degree of unification if it was to hope for any kind of progress. This was not so easy. The tendency among the scientifically oriented was clearly to abandon the controversial and perpetually unfruitful study of mediumship, and focus instead on specific effects, capable in some degree of being isolated and tested separately under laboratory conditions. They tended to favour the quantitative methods over qualitative ones, although sometimes the boundaries between the two were blurred. However, even when the séance room was replaced by the psychology laboratory, several fundamental questions remained to be answered: are supernormal faculties rare or common? Can they be reproduced at will, or do they happen spontaneously? What kinds of supernormal faculties are there, anyway? What are the most likely *normal* explanations, and how can they be adequately checked for? The answer to each of these questions had a bearing on how experiments should be constructed. No matter which choice was

opted for, the results yielded, whether positive or negative, could always in principle be questioned by researchers who answered the initial questions differently. The result, as we have seen, was that even within the strictly quantitative research tradition—spanning the contributions of Richet, Warcollier, Coover, Troland, Murphy, and Estabrooks – no straight-forward “progressive” course can be traced. Instead, fundamental questions are brought up time and again, with an endless return to the main fundamental hypotheses.

We could expect that at least in *research design* there might be some sort of “progress” to trace, but even here the matter is not clear-cut. The fundamental difficulties with using probability and control correctly were brought up already in the 1880s by people such as Richet, Edgeworth, and Peirce; yet in the twenty years that followed there is little trace of their insights having any real impact on actual research design. Coover appears as the first psychical researcher to truly take these insights seriously, while meticulously adding ways to implement controls and randomisation in the experimental situation. His work was well received in the psychological literature, and influenced some of the very few professional psychologists who ventured into psychical research in the coming years, notably Troland in 1916/1917 and Hulsey Cason’s 1924 replication attempt. Since these studies were generally negative with respect to the anticipated effects, there could be no more psychical research on these lines. Thus, it was not long until the fundamental questions were asked once again, with Murphy going back to the rarity thesis, scrapping Coover’s strict controls, and focusing on spontaneity. Estabrooks mediated between the two, producing positive results if a *spontaneity* thesis was assumed, but with a somewhat weaker methodology than Coover’s.

* * *

The pattern that emerges is clear enough: the stricter the methodology, the weaker the results. To researchers who had not invested any personal prestige in the *veracity* of psychic events, such as Coover, the most obvious conclusion to draw was that the phenomenon itself was likely to be spurious – an illusion created by the attribution of meaning to random patterns. Coover’s own research into the illusions of perception created when listening to nonsense syllables was an excellent demonstration of this

hypothesis.¹⁸² That evidence for telepathy disappeared when tests were repeated would, from a statistical point of view, appear as a simple application of the law of large numbers. It was comparable to the normal tossing of a coin: do it ten times and you may see a preference for heads, do it 10,000 times and the preference drowns in the average value. Those who were not ready to embrace this conclusion, including Murphy and Estabrooks, still had other strategies available. These largely depended on defining the phenomena in ways that made them impossible to measure by strict applications of quantitative experiments: only a unique type of individuals should be used as subjects, for example, or the spontaneous quality of the effects should be recognised. Researchers like Murphy and Estabrooks attempted to get the best of these two worlds; the question, however, is whether those worlds are at all reconcilable. Taking the uniqueness of the phenomenon seriously, both in terms of confining the “gift” to a very few “special talents”, and by emphasising the spontaneity of the events themselves, makes both standardisation and replication of experiments close to impossible in practice. Considering that psychological research methodologies were becoming stricter every day, this meant that it would be very hard to be taken seriously from a scientific point of view.

It was precisely with reference to this apparent “paradox”¹⁸³ that Carl Gustav Jung, following his collaboration with Wolfgang Pauli (and, to a lesser extent, Pascual Jordan), would later suggest that psychic phenomena are what he called *synchronistic* events.¹⁸⁴ On this reading, psychic events are non-causally connected “meaningful coincidences”, and one would expect them to be very hard to produce on demand in a laboratory. Since synchronicity focuses on “meaning”, with Jung even admitting to its ‘anthropomorphic’ character,¹⁸⁵ the synchronistic view of experiments on telepathy is in fact not so far removed from the “sceptical” one: meaning is only possible in “unique” situations, and will disappear with quantity. The difference is, of course, that Jung would not *reduce* this meaning dimension to something illusory, but instead establish the principle of acausal, meaningful connections as an irreducible feature of nature. With

¹⁸² Coover, *Experiments in Psychical Research*, 369-410; cf. Joseph Jastrow, *Fact and Fable in Psychology*.

¹⁸³ Paradoxical, perhaps, to the believer in psychical phenomena: the disbeliever sees no paradox in these tensions, only what one would expect on the hypothesis that the phenomena do not exist.

¹⁸⁴ See e.g. Jung, ‘Synchronicity’, 14-19, *passim*.

¹⁸⁵ *Ibid.*, 69.

the input of Pauli, he even suggested an analogy between telepathy experiments and quantum mechanics: the “uniqueness” of quantum mechanical processes and their fundamental indeterminacy was indisputable; yet, physics at large was safe because order and lawfulness emerges from quantity on the macro-level.¹⁸⁶

In conclusion, the collision between quality and quantity appears as the most serious *scientific* problem faced by second-generation psychical research. The problem becomes even more evident when we consider psychical research programmes as “laboratories of enchantment”, bent on studying precisely those natural, human, or spiritual phenomena that seemed to run counter to the “disenchanted” world picture that had been propagated by certain late-19th century popularisers of science. Whether they corroborated spiritualism’s claim of the existence of disembodied spirits, or suggested simply that non-mechanical powers of the mind were operative across great distances, psychic phenomena promised more life, more agency, more meaning, and mystery in nature than “classical” physics, mechanistic biology, and Daltonian chemistry had to offer. Putting them on a solid scientific footing therefore bore the promise of overturning disenchanted naturalism. Those seeking genuine enchantment through a science of the supernatural were, however, faced with a serious dilemma. Through the early decades of the 20th century it became increasingly clear that such a science had to play by the rules of *quantity*. To be taken seriously in a scientific context, studies of psychic phenomena had to go through experimental control, randomisation, statistics and probabilities. Anything personal, situated, and subjective had to be removed – that was the only way from unreliable “anecdotes” to reliable “data”. However, while science was a quantitative endeavour, psychic experiences appeared to be of a qualitative nature. As studies piled up, it became hard to avoid the conclusion that whatever else they might be, “psychic phenomena” were of an essentially subjective character. One could opt to interpret this subjectivity as indications of bias and personal meaning-making, or as expressing a genuine “spiritual tease”: phenomena that, in the style of William James, were intended to ‘remain *baffling*, to prompt our curiosities and hopes and suspicions all in equal measure’.¹⁸⁷ In either case, the laboratories of enchantment were looking in the wrong place when attempting to quantify the unique. The consequence would be that no scientifically legitimate psychical research is possible.

¹⁸⁶ Cf. Gieser, *Innermost Kernel*, 289-298.

¹⁸⁷ James, ‘Final Impressions of a Psychical Researcher’, 175.

The project would be forced to return to puzzles over anecdotes and personal experiences; at best, it could survive as a hermeneutics of supernormal experiences rather than as a science of supernormal experiments.¹⁸⁸

Given such harsh verdict, one might think that McDougall and other anti-agnostics in psychical research would have gotten their final answer: the methods of psychical research had now been tried and tested, in accordance with a variety of hypotheses and methodologies, but results were not forthcoming. Perhaps it was time to close the book and conclude, now with *weighted evidence* rather than by a priori reasoning, that the supernormal constitutes a genuine case of *ignorabimus* – something that, if genuine at all, we *cannot* know scientifically.

No such verdict was passed. In fact, the decidedly largest programme in experimental psychical research was still to come, and McDougall himself would play a decisive role in bringing it about. In 1934, less than ten years after the international schisms, a book appeared that would finally succeed in becoming the “paradigmatic” foundation text for a professionalised, parapsychological “normal science”: Joseph Banks Rhine’s *Extra-Sensory Perception*. This book introduced yet another new set of scientific nomenclature, made a new taxonomy of effects, described methodological protocols, provided fresh interpretation of earlier research, and, above all, presented the results of years of quantitative experiments that seemed to yield an overwhelmingly *positive* verdict. Furthermore, the research it presented had been funded through a university budget, carried out in the psychology department of the newly established Duke University in North Carolina. The second generation’s long and disappointing search for a paradigm was finally over. Rhine’s work ushered in the third generation, which became the generation of *professional parapsychology*. Given the extraordinary history of failure we have just described, and the increasing certainty by which

¹⁸⁸ This, in fact, would come close to the position taken more recently by historian of religion Jeffrey Kripal: ‘paranormal phenomena are semiotic or hermeneutical phenomena in the sense that they signal, symbolize, or speak across a “gap” between the conscious, socialized ego and an unconscious or superconscious field’. What precisely is meant by this “superconscious field” remains unclear. The form of hermeneutics imagined by Kripal appears deeply inspired by a psychological (or even “psychical”) approach on the lines of Myers and Jung, while also drawing inspiration from the phenomenological school in the history of religion, particularly Mircea Eliade and Ioan Couliano. See Kripal, *Authors of the Impossible*, 1-34, 25.

professional psychologists could disregard psychical research after Coover, how could this happen? This is the question we must turn to next.