

# LONDON EXPERIMENTS WITH MATTHEW MANNING

Edited by Anita Gregory

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INTRODUCTION

ANITA GREGORY

Biographical details in this introduction are mainly based on conversations with Matthew Manning and his parents and on Matthew's books.

The following conventions are used to indicate the source of the information: (T1) refers to a taped interview with Mr. and Mrs. Manning and Matthew, 12.11.1978. During virtually the entire investigation described in this paper, tape recordings were made in the main experimental area at the Bio-Electricity Laboratory, City University, London. These tapes were labelled A1/1, A1/2, A2/1, etc., and references marked in the form (A1/1) denote these recordings. In the Introduction references to Matthew's own books are indicated by an initial letter followed by the page number; thus (L3) refers to *The Link* page 3, (M3) to *In the Minds of Millions* page 3, (S3) to *The Strangers* page 3.

The investigation of Matthew Manning, born 17.8.1955, to be described took place in the summer of 1978, after he had written to me earlier that year offering his services as a subject of research. He has published three books about his experiences to date (Manning 1974, 1977, 1978). What follows is based on these. Terms such as 'alleged' are avoided and should be taken for granted.

OUTLINE SKETCH OF PHENOMENA

Poltergeist phenomena began in the Manning household on 18.2.1967 when Matthew was 11, his sister Rosalind 8 and his brother Andrew 6. The onset was marked by the displacement of a tankard from a shelf, and of a vase of flowers on a table. The phenomena increased in frequency and intensity, being at their height between 7 and 7.30 am. Knocks, other unexpected taps, creaks and a batlike 'pinging' were heard in the modern detached house, doors would open and shut, and objects of varying size flew about. At about Eastertime 1967 a mislaid rubber that slowly rose in the air and gently floated down and landed beside its owner, Matthew's sister, seems to have been the only incident to inspire acute terror during this phase, and it marked the end of manifestations for the time being. Dr. A.R.G. Owen, who had been called in, expressed the view that the happenings had been genuine, and that Matthew was the most likely source on the grounds of age, although there was no other factor to indicate this especially (L25).

After this Matthew took Common Entrance examinations and went to public school. In 1968 the family moved to beautiful house built and extended between 1550 and 1730, where there were no ostensibly paranormal incidents for some time, at any rate no extended ones. Matthew gives the impression that he thought there was a long and entirely blank gap so far as the paranormal was concerned. There do seem, however, to have been some stirrings during this time. In his third book he says that the family had become accustomed to 'odd happenings' ever since they had moved to this house (S11). Also late in 1968 there was a 'craze' for seances at Matthew's school, and phenomena seem to have been more prolific when he was a participant (L26). However the seances were abandoned when, because as quite often happens in schools, all concerned became scared.

Disturbances started up again in the home in July 1970 and gradually increased in intensity. There were footsteps and raps, opening of cupboard doors, boots were thrown about, cushions began to take on a life of their own, and so forth. This time the manifestations clearly centred around Matthew, but he says that his parents for a considerable time simply declined to believe him. He felt safe only at his boarding school. By Easter 1971 the phenomena had become so violent that one night Matthew refused to sleep in his own room 'until something was done about it' (L35). The dinning room looked next day 'as though a bomb had hit it' (L36).

In the summer term of 1971 the disturbances finally followed Matthew to his school, where there was genuine concern that the upheavals in the dormitory could endanger pupils' O-level studies. The headmaster, matron, fellow students, as well as Dr. Owen (who was again consulted, this time by correspondence) were convinced of the genuineness of the happenings, and it was with some difficulty that the headmaster was persuaded to keep Matthew at the school. Levitating beds, flying objects, the formation of pools of water, hot spots on walls, upturned book cases and the appearance of knives of unknown origin, were among the episodes of havoc that occurred.

In May 1971, during a week-end at home, Matthew had his first experience of 'hearing' a spirit voice — one 'Henrietta Webbe' — and in June 1971 he first found himself writing automatically in a handwriting quite different from his own whilst trying to prepare a pre-O-level essay. In the following months he increasingly engaged in automatic writing, which purported to come from numerous departed persons, and concerning the origin of which Matthew himself is admirably cautious. There is some suggestion of paranormal content, and indications that Matthew was also beginning to acquire some other psychic accomplishments at this stage. In my view it is of special significance that by means of deliberately developing his automatic writing Matthew seemed

to be able to attenuate and eventually abolish the poltergeist manifestations.

In June 1971 there was the first communication by way of automatic writing from 'Robert Webbe', who was to become a frequent and garrulous communicator over the next years, and who as late as May 1977 'compelled' Matthew to communicate with him (S120). In July 1971 names began to appear on the walls of Matthew's room; virtually all the panels and much of the ceiling of which are covered with names and dates. Matthew and his family estimate that the bulk of these appeared in 1971, and that this activity has now virtually ceased.

On 2 November 1971, while Matthew was home for half term, his mother suggested he should try his hand at automatic drawing, calling for inspiration on the spirit of Sir Alfred Munnings, since it was believed that Matthew's limited skills as an artist would make sure he could not himself draw a good horse. He did produce a horse of apparently no great artistic merit, but still beyond his own abilities. This started a new phase; Matthew's automatic drawing activities in the style characteristic of large numbers of deceased artists, among them Dürer, Picasso, Beardsley, Keble-Martin, Rowlandson, Leonardo, Beatrix Potter, Goya and Klee. The drawings certainly look very accomplished, and were done at some speed. These continued until 1975 and have since ceased.

Later in November 1971 Matthew had his first actual 'visual' encounter with 'Robert Webbe'. During the winter of 1969/70 Matthew had been engaged on an O-level history project on the Webbe family from contemporary sources held in the Cambridgeshire Public Records Office. He selected the Webbes from the names predominant in the documents because the name 'John Webbe 1731' was scratched into a brick of the house, and it was known that a Webbe family had once occupied it. The Webbes turned out to be reasonably, but not by any means amply, documented in local records. In this school record project Matthew had described how he would imagine a typical 18th century gentleman to look, and this was strikingly different from the apparition he experienced in 1971: this convinced Matthew that what he had seen was no mere hallucination.

From the summer of 1971 poltergeist phenomena disappeared, and Matthew engaged more and more in automatic drawing and writing. Early in 1973 he came to the attention of Mr. Colin Smythe and Mr. Peter Bander, directors of the publishing firm Colin Smythe Ltd. Messrs. Bander and Smythe at that time contacted me with the suggestion that I should take charge of Matthew, investigate and help him to write a book about his experiences. However nothing came of these plans. In fact my copy of *The Link* kindly presented to me by Matthew years later, is inscribed 'The book you nearly wrote with me'. Matthew started a career as a writer, and he visited a large number of countries

and institutions at somewhat breathtaking speed to demonstrate some of his phenomena.

In January 1974, following Uri Geller's demonstrations of metal bending, Matthew followed suit and bent numerous metallic objects in front of large numbers of witnesses. He came to dislike this activity and soon discontinued its practice. He has visited Canada, the United States, Sweden, Japan, New Zealand, Germany and the Netherlands. Matthew is now active mainly as a healer (see Postscript).

#### PERSONAL REFLECTIONS BY AND ABOUT MATTHEW

Matthew's earliest memory (A2/1) is at the age of 18 months sitting in a high chair at the side of some french windows (which details apparently fix his age), 'refusing to eat chicken because it was cruel to eat birds'. He did not, until his poltergeist episodes, feel himself to be different from other children. Mrs. Manning had an extremely severe electric shock while she was carrying Matthew three weeks before his birth (T1, A2/1). She recalled an occasion when, at the age of 7 or 8, his life was endangered as, during a seaside holiday he was cut off by the tide. Matthew himself remembers this incident as terrifying especially because he found himself surrounded by fishes which frightened him. He has had recurring nightmares about being cut off by water with fishes swimming in it ever since (T1).

Matthew also recalled during the interview in November 1978 nightmares he had had at 10 or 11, when a boy died at his prep school. His parents could not remember his ever telling them this before. Matthew said:

I'd suddenly wake up in the middle of the night and I'd suddenly see somewhere probably within three feet of me, though it's difficult to gauge the distance in the dark, I'd see a disembodied face somewhere close to me and I knew that I was awake because if I close my eyes the face disappears but I get a kind of direct communication between whatever it is straight into my head, and I can communicate with it. Some of the faces I recognise and some of them I don't.

The passage is interesting for a number of reasons. It shows Matthew's sophistication and detachment vis-à-vis his experiences, it is a clear description of experience, and it indicates that these visions have continued since, and the original link with a death might also be of importance.

Mrs. Manning thought of the near-drowning episode as having left a

particularly deep impression. Mr. Manning recalled another incident which he felt to have been of special significance, and I particularly appreciate his candour in this matter; which of us, who are parents, cannot think of things we would rather we had not done? In the incident in question, Andrew did something which, as Mr. Manning said,

incurred my extreme displeasure and I belted him for all I was worth. I think I really did lose my temper and perhaps lost control of him and myself as well, but Andrew was remarkably resilient and it went over him, and he came up smiling and he'd beaten me at the end of it. But Matthew nevertheless I remember was extremely distressed as if it had all happened to him and he'd experienced it personally, and I have never seen anybody so distressed as Matthew at the fact that his brother was given a hiding.

The reason for citing this is that Mr. Manning strongly felt that the incident may have been important, and we know so little of these matters we cannot afford to ignore his feelings. Moreover, Matthew himself had totally forgotten it, and since we were able to narrow down the time to when Matthew was 10 or 11, this seems somewhat unusual. At first what precisely had been Andrew's misdeed seemed forgotten, but Mr. Manning finally recalled that it involved something being thrown down the stairs, probably a tray, with such violence that it smashed a plate glass window. Matthew, when he recalled the forgotten incident, said that he thought Andrew had slipped on the tray. 'I remember the window being broken but not him doing it'. Mrs. Manning contributed that she thought it was just stupidity, but Mr. Manning was certain it was deliberate. Now according to Matthew and his parents quite a substantial number of incidents of poltergeisterei in fact centered around things being thrown about on the staircase of the house near Cambridge, which was all the noisier since during much of the time there was no carpet down, and indeed the first 'Webbe' apparition took place on the staircase. The original incident involving Andrew happened in the earlier home of the Manning family in Shelford.

Matthew was due to take examinations at the time of both poltergeist outbreaks. He himself now regards this as irrelevant, as he was not 'bothered one way or another' although he claimed that 'compared with other people at school you [his parents] were far less interested in what I was doing at school than most parents' (T1). He thinks that at some unconscious level he was eager to disrupt his parents' structured existence 'without being for it' (T1 and A1/1). In particular, Matthew

and his parents agree that there could have been a strong element of defiance of Matthew against his father, whose special favourite tankard was the first object to be displaced. It might also be of interest that 'Webbe' disputed the Mannings' ownership of the house.

Matthew is generally held not to have any talent for drawing in his normal state, nor did he ever show any promise at school in this respect. He never took art lessons since these clashed with Latin. He does however feel himself to be 'an artistic person by nature' (T1). He liked pottery and hated games.

His mother says that as a young child he did a great deal of reading, far more than his contemporaries. He also day dreamed a lot— the headmistress of the school he attended at 5 said: 'Matthew always had his head in the clouds, he's miles away' (T1). The only religious feelings he can recall were 'anti-religion', and he never had any religious faith. The family are not religious except in a very generally conventional way: Matthew considers having been confirmed 'a waste of time' (T1).

In 1977, while in the Himalayas he had an experience which deeply impressed him whilst looking at the mountains and watching the sunrise. He

realised how completely unimportant I was physically, how transient human life is... I just felt a tremendous feeling of harmony and unity. And I just... felt some Presence while I was up there that told me what I should do and what I shouldn't do. In fact, I should do what I wanted to do and not what other people told me to do (A1/1).

Table 1.1 provides a sketchy chronological record pieced together from Matthew's published books and tape recordings of his and his parents' statements.

#### ARRANGEMENTS FOR THE INVESTIGATION

Matthew wrote to me on 1 March 1978 suggesting that I should arrange for him to be investigated, and we met for the first time in person and discussed what form the experiments should take. Matthew was keen to try and replicate some of the biological experiments which he had done in the United States: he felt that he was at this stage more likely to succeed in influencing organisms than inanimate matter. However he agreed to try whatever I could set up. I explained that my own major interest was in an attempt to replicate the ostensible occulting of an infra-red beam obtained with the physical medium Rudi Schneider in the 1930s (Osty 1931, 1932; Hope 1933; Gregory 1983).

Date	Event	Source
Pre-natal . . . . .	Mother's electric shock during pregnancy. . . . .	T1
17 August 1955 . . . . .	MM's birth. . . . .	Pers comm.
Summer 1961/62 . . . . .	At 7 or 8, MM cut off by tide. . . . .	T1
1965 or 1966 . . . . .	Age 10 or 11 boy died prep school, nightmares. . . . .	T1
1966/7 . . . . .	Age 11 or 12 father belted Andrew. . . . .	T1
18 February 1967. . . . .	Onset phenomena; A.R.G. Owen called in; date not quite certain, MM 11, Rosalind 8, Andrew 6. . . . .	L15
Easter 1967 . . . . .	Rubber episode ends this bout of phenomena. . . . .	L24
1968 . . . . .	MM takes Common Entrance. . . . .	
	Family move home. MM to public school. . . . .	L26
Late 1969 . . . . .	Craze for seances at school. . . . .	L26
July 1970 . . . . .	Onset recurrence of home phenomena. . . . .	L28
Christmas 1970 . . . . .	Increase home phenomena. . . . .	L33
Winter/early Spring 1970 . . . . .	MM compiling Webbe family project for O-level project. . . . .	L83
Easter 1971 . . . . .	Violent phenomena; MM's bed moves, room 'as if bomb hit it'. . . . .	L34/36
Summer term 1971 . . . . .	Disturbances start at school; violent disruptions. . . . .	L41
May 1971 . . . . .	Weekend at home: first communication 'spirit' entities, 'Henrietta Webbe' voice. . . . .	L59
June 1971 . . . . .	Automatic handwriting, different from own; virtual fading of poltergeist phenomena. . . . .	L62
June 1971 . . . . .	Ostensible successful ESP. . . . .	L66
June/July 1971 . . . . .	Takes O-levels . . . . .	
June 1971 . . . . .	Automatic writing characteristic of 'Robert Webbe'. . . . .	S16
31 July 1971 . . . . .	Names on wall begin. . . . .	S20
2 November 1971. . . . .	Half term; automatic drawing suggested by mother. . . . .	L92
November 1971. . . . .	Vision of 'Webbe' on stairs. . . . .	S11
Some months after November 1971 . . . . .	MM discovers old school project with different Description of 18th C. gentleman. . . . .	S15
1972 . . . . .	Various phenomena, including apports, automatic drawing, communications. . . . .	e.g. L102
Beginning months 1973 . . . . .	Contact Bander and Smythe. . . . .	
June 1973 . . . . .	Projected outline of MM's first book to AG. . . . .	Corr. 1.6.73 3.6.73
January 1974 . . . . .	MM bends metal following Uri Geller. . . . .	L141
Summer 1974 . . . . .	MM in Toronto. . . . .	M1
April 1975 . . . . .	MM in Netherlands. . . . .	M88
Summer 1975 . . . . .	MM in U.S. . . . .	M96
Autumn 1974 (?) . . . . .	MM in Freiburg. . . . .	M16
January 1976 . . . . .	MM in Sweden. . . . .	M127
June 1976 . . . . .	MM in Japan. . . . .	M145
15 May 1977 . . . . .	MM 'compelled' to communicate once more with 'Webbe' after completion of archival research. . . . .	S120
1977 . . . . .	Himalayas experience. . . . .	A1/1
1 March 1978 . . . . .	MM writes to AG. . . . .	Corr. 24.7.78

TABLE 1.1

Rough chronology. T1 = taped interview 12.11.1978; L = Manning (1974); M = Manning (1977); S = Manning (1978) followed by page number. A1/1 = first side of first tape during investigation. (See first paragraph in this section.)



In fact I had just had an instrument constructed for me by Mr. C. Brookes-Smith in the hope that a former poltergeist focus or physical medium would come forward to be examined. Matthew was also keen to devise some experiment to test the ability which he felt he had acquired recently, namely to draw pictures which were unambiguously relevant to or illustrative of a poem as a paranormal target.

While the planning of these experiments was in progress, an incident occurred which should perhaps be mentioned. Mary Rose Barrington and I, while spending an afternoon and evening together preparing amongst other things a sheep-goat questionnaire to be administered to participants in the Manning investigation, heard four or five exceedingly loud but dull thuds or thumpings for which in the event no normal cause could be found. It did not in the first instance occur to me that they might be anything other than normal builders' noises, since I did not even know that the floor above us was uninhabited. Neither of us had ever heard any alleged paranormal noises before nor, to the best of our knowledge, have either of us been subject to any hallucinations, collective or otherwise. Mary Rose Barrington immediately investigated all conceivable possible normal sources of these noises, that sounded as if a muffled battering ram were hammering against the walls of the house immediately adjacent to where we were sitting.

As was subsequently ascertained, at the very time when we 'heard' the hammering, a client of Mary Rose Barrington's was longing to ring her up about a thorny problem, but did not dare to do so on a Saturday evening. (Miss Barrington is a solicitor, and her client was in the painful position of not being able to gain possession of her own house.) Now this client, Mrs. 'M', had been involved many years previously in a set of ostensibly paranormal experiences where she reported loud hammering noises emanating from the ceiling (Barrington (1965)). At the time of the noises Matthew was in fact in the middle of delivering a lecture (A1/1).

Mary Rose Barrington had a further subsequent experience conceivably connected with Matthew when she together with her mother and Mr. Jack May heard a loud explosive sound. Everyone immediately supposed it to be the explosion of a light bulb in a lamp standing on a sideboard. At the foot of this lamp stood a tray of mung beans 'treated' by Matthew in the course of the experiments described below. However, neither this nor any other bulb or TV tube was found to be damaged, nor was there any other plausible explanation for the sound. Prior to this incident Mary Rose Barrington had been talking about psychical matters.

Full accounts are too long to be appropriate here, and details of the incidents and of the sheep-goat questionnaire are deposited in the archives of the SPR. It does however seem worth mentioning these

incidents since there is some reason to suppose that there might conceivably be a connection between Matthew and ostensibly paranormal events reported by other persons after they have established contact with him.<sup>1</sup> In an obscure field it seems wise not to ignore obscure data that have a possible connection, the more so since the relation between psychical phenomena and meaningful coincidences or synchronicities remains to be explored both theoretically and empirically.

The investigation was arranged for the period 24 July to 4 August 1978. Professor A.J. Ellison offered us his cooperation and that of his technical staff, as well as his hospitality at the Bio-Electricity Laboratory in the Department of Electrical and Electronic Engineering at City University, London. Experiments took place there as arranged, with the exception of two sessions in the Department of Physics at Birkbeck College, University of London, arranged by Professor J.B. Hasted.

In addition to my own projected infra-red experiments which have been mentioned, the following were invited to plan experiments:

Miss M.R. Barrington, Professor W.B. Brown, Professor A.J. Ellison, Dr. I Grattan-Guinness and Professor Hasted. Dr. Grattan-Guinness was unable to be present but offered to try some experiments from a distance (see p. 322). Professor Hasted was in the country only during the latter part of the period; as mentioned, he conducted his experiments at Birkbeck College.

Mr. David Chapman, a Research Assistant in the Department of Electrical and Electronic Engineering, was present throughout and participated in virtually all experimental sessions. He was in charge of instruments and recording apparatus, which included a chart recorder for use in the infra-red experiments, audio recording which was running throughout all sessions at City University, and video recording from time to time. (Original recordings are in the archives of the SPR.) Mr. Chapman was assisted by Mr. F. Sullivan.

I asked Matthew to bring a friend both to keep him company and to act as his witness during experiments. Matthew suggested several names among whom I chose Dr. Brian Inglis, who was unfortunately only able to participate occasionally. His place in this respect was taken for much of the time by Miss Ruth West, who also helped me in the, at times formidable, task of organising, convening and chairing the many meetings before, during and after the investigation. She also gave invaluable clerical and other practical assistance.

The experiments were, as will be seen from the individual accounts, of very different types and durations. Provisional time-tables were drawn up with the clear understanding that flexibility would be needed to meet unexpected contingencies. Obviously some element of compro-

<sup>1</sup> Matthew tells me that people have often reported this to him; see for example *Here's Health*, December 1981.

mise between rigid adherence to schedules and spontaneous activity had to be faced, somewhat after the manner of a free activity day in a fairly informal school; the visit to St. Bartholomew's Hospital for Matthew's EEG had to be a fixture, as had his visits to Birkbeck for John Hasted's *muco* experiments.

Dr. Peter Fenwick, Consultant Neurophysiologist at St. Thomas's Hospital, kindly examined Matthew's EEG taken at Barts by Miss Marion Smith, and found that the EEG revealed a few paroxysmal bursts and central theta normally found only in somewhat younger persons, depending however on the normative group studied. Matthew's left hemisphere showed a greater activity than the right while he was attempting 'psychic' tasks which, in a right-handed subject, is unusual (Dr. Fenwick, personal communication, 9.6.1982).

Professor Brown's haemolysis experiments in which I acted as second experimenter took longer overall than anticipated but, once the design was agreed, time for all the sessions had to be found and each session had to be continuous once the collection of the blood had taken place. An account of the haemolysis experiments was not available by the time set for collating this volume. These experiments were attempts to confirm the findings of Braud (1979) using an improved methodology. No significant results were obtained. The bean germination experiments could be done at any time once the beans were available. The poetry experiments could be initiated by Matthew at odd moments whenever he chose, and he was also free to try the pendulum and the RNG experiments on demand. Infra-red sessions were provisionally time-tabled (see below p. 316).

In addition to the experiments planned, Matthew at one point felt he would like to try 'sending' drawings to us. This, being a spontaneous activity, was not planned and designed beforehand and, although there seemed to be at times some subjectively quite impressive similarities and relationships between sender's and percipient's drawings, subsequent scrutiny, including independent appraisal by the ESP Committee of the SPR, suggested that nothing could be claimed on the strength of these.

The sheep-goat questionnaire (administered during a light-hearted social gathering at my home on the first evening of the investigation) might be regarded as an exercise in group interaction rather than as one conveying definitive information, especially since it is by no means clear that answers represented settled convictions rather than responses to a particular social situation. It was clear that most of us with the possible exception of David Chapman were willing to countenance a certain varying spectrum of paranormal phenomena, and expressed reasonable optimism about the forthcoming experiments. It is hardly surprising that Matthew himself showed a greater readiness to accept paranormal explanations than any of the experimenters and observers,

all of whom gave at least one thoroughly 'goat' style response.

A further experimental week was organised 12 to 16 February 1979, during which further infra-red, haemolysis and several further experiments were conducted. No positive results of any kind were obtained: it cannot be claimed that conditions, either physical or psychological, were similar. Matthew was unwell, and it was the middle of term as well as winter. Rather few of us could attend consistently.

#### COMMENTS ON THE INVESTIGATION

A number of general issues arise out of an investigation of this type, relating to the organising of experiments in this manner, others to the question of authenticity, *modus operandi* and meaning of results.

##### *Block type of investigation*

There are advantages in organising a number of experiments over a concentrated block of time, involving several experimenters and different activities, and there are also drawbacks. The block format is more suitable for the investigation of a star subject with limited time, especially one living at a distance, than for the experimental investigation of a trait or ability in the general population.

It is also useful to have a number of diverse participants present, often at the same time, in order to witness each other's observations and to make suggestions and corrections, and to complement each other's repertoires of competence. It may be easier, indeed this may be the only way, to arrange for cooperation between workers from different distant locations and fields of expertise.

On the other hand, assembling a team of participants with widely different backgrounds and commitments may mean considerable disparity of approach, attitude and time required and devoted to evaluation of data and so forth, and there is unlikely to be enough time to put into effect important suggestions for improvements. Also the administrative workload is very substantial.

These different rather practical considerations need to be balanced against each other in the circumstances of any given set of experiments.

The questions arising out of this kind of experimental organisation are of course by no means purely administrative, since the spacing and timing of experiments and the presence and absence of different participants may well have characteristic consequences on the results obtained.

For subject and other participants alike the setting aside of a short but quite substantial block of time such as a fortnight may highlight an investigation as a special event, and generate a sociable group mood of

fellow feeling and hopeful expectancy widely believed to be psi conducive, especially in the case of physical phenomena. The team located in this case in a laboratory, replaces the home seance circle or the sitter group.

The audio record certainly bears witness to a buoyant and active group mood at times when some ostensibly paranormal results were being registered. By the same token, however, the greatest care and caution must be exercised subsequently when assessing results. The block type of investigation is more likely to provide a *prima facie* case for selecting future areas for experimenting in depth than hard and definitive evidence. The very flexibility and informality may militate against a rigorous methodology in terms of experimenter initiated trials and controls for some of the experiments.

#### *Laboratory setting*

Although intangible and yet possibly vital factors such as group mood are hard or impossible to control, at least the laboratory setting and some form of continuous instrumental monitoring makes it possible to arrange for a high-spirited mood not to interfere too much with the collection of hard data. The permanent auditory record, for example, means that it is possible subsequently to check at least what was vocalised at any given time. Despite gaps and shortcomings in the recording and collection of data which will become apparent to readers, a good deal was learned about the conduct of future experiments, and it is of course also valuable to capture the subject's and other participants' reactions at the time.

One of the most important features to emerge that cannot be stressed enough is that it is essential to have as complete and continuous a set of hard and preferably automatically synchronised automatic records as possible. Subsequent evaluation will depend entirely on the quality of these records.

#### SOME PSYCHOLOGICAL ASPECTS

A number of questions naturally present themselves about Matthew Manning himself. On any interpretation he is a most unusual young man, and I personally was convinced of his sincerity. There are obvious problems about discussing in print a named individual and his psychology. I will therefore confine myself to questions which I hope will be relevant, relatively inoffensive, and lead to parallels and clues in the case of other subjects and future investigations.

#### *Problems about using biographical data as explanations*

There is inevitably an element of *post hoc* reasoning about any attempt

to unearth biographical factors so as to 'explain' a person's subsequent development. On the other hand, almost all of us tend to look at our own and other people's past to try to understand the present, and most psychologists, whatever their allegiance, will at least consider past events and experiences when trying to understand a person's current or eventual state. It would certainly be unwise to neglect such potential sources of information in a field as obscure as psychical research, if only to rule out certain factors as individually critical.

In asking how significant such events might be, we are facing the problems of all attempts to be 'scientific' about human understanding. Different individuals react differently to ostensibly similar events. For one thing, similar events have different meanings for different persons—it is precisely this factor of meaning which makes it so difficult, if not intrinsically impossible, for psychology ever to become wholly a normal scientific subject.

In psychical research, especially when we are concerned with a specific star subject, this difficulty shows itself in a fairly acute manner. There is the well-known gulf between experimentally and physiologically oriented psychologists on the one hand, and those concerned with understanding individuals on the other; normally the problem is dealt with by ambiguous and ambivalent co-existence. This is not the place for a discussion of this issue; it is however essential to make some reference to it because what counts and what is picked out as relevant, what is selected as a possible set of explanatory constructs, depends upon one's basic assumptions; and if one is discussing why one person should be different from others, one is immediately faced with the need to make reference to some explanatory framework; and a plurality of frameworks may be applicable to discuss diverse characteristics. It is pertinent here to point out that one of the best recent general books on psychology was written by one of our most eminent psychical researchers and is named 'Psychological Sciences' (Beloff (1973)).

#### *Limited psychodynamic approach*

In considering biographical particulars in the present context, then, apart from listing events and vicissitudes for future comparison with other psychics and control individuals and groups, the most sensible procedure would seem to me to be to adopt a non-doctrinaire and tentative limited psychodynamic approach, with the obvious restrictions entailed, and without any suggestion that this is the only possible method. This does not prevent supplementation by a more learning theory oriented analysis: any behaviour pattern may presumably be affected by reinforcement. My reflections are based upon what Matthew himself has said and written, and is not therefore, I hope, too impertinent or absurd.

In Matthew's own view, the purpose of the early poltergeist disturbances was principally to disrupt his parents', especially his father's, ordered existence 'without being for it' (T1).

My father is an extremely structured person. He does everything according to a plan, and to routine. Everything is time-tabled...if anything upsets his time-table he gets into a flat spin, he's completely inflexible whereas I do things completely impulsively and I hate any kind of inflexibility of time-table...I think it was a means of disrupting his structured existence without being given the blame for it directly (A1/1).

It must be said that there is nothing immediately striking if one considers Table 1.1. The pre-natal electric shock may perhaps be of importance; there are quite frequent accounts of shocks and injuries and close encounters with death in the early histories of psychics and mediums. But neither the cutting off by the tide and horror of fishes, nor the episode of the beating administered to his brother, nor nightmares in response to the death of a fellow pupil in themselves seem particularly unusual. It might be thought significant that Matthew had actually forgotten the beating of Andrew and also that he had apparently never mentioned his fellow pupil's death to his parents, until the interview in November 1978.

The very first manifestations during the poltergeist time were the moving of the father's prize tankard, and putting a bunch of flowers in front of his mother's place at table. 'Webbe' disputed the family's, presumably the father's, right to the house, and certainly to his own bed.

The early relatively sedate disturbances soon got out of hand after the manner of temper tantrums and grew more and more chaotic and frightening, and Matthew himself became frightened and took refuge in his parents' bedroom. I think it is quite apparent from reading Matthew's books that his father was more under attack during outbreaks of poltergeisterei than anyone else in the house.

The onset of both bouts was loosely linked with public examinations. Matthew does not consider that he was 'all that bothered' about these, but he clearly did resent what he felt to be his parents' relative lack of interest in his school work, a view which seemed to surprise them (T1). It should perhaps be mentioned here that whereas Matthew dislikes and rebels against time-tables which impose any restraints upon himself, he also intensely resents being kept waiting or in any sense inconvenienced by a failure of others to keep to a time-table. As I see him, he likes to be free himself whilst expecting others to be at his instant disposal. Most of us keep this perfectly natural set of desires slightly more in check than he does — or did.

Probably the fairest way of dealing with Matthew's own comments on this passage is to quote his own words, in a letter to me of 15.2.1982:

Re timetables! I have refrained from altering observations on myself in this text even if I don't particularly agree with them. However, you may have misunderstood my reaction to timetables, or else I am older and wiser since we spoke about them! I don't entirely agree with your comment that I like to be free whilst expecting others to be at my instant disposal. That's not really true and makes me sound like a megalomaniac. I'll say no more!

If the earlier poltergeist phenomena are viewed as transcending the bounds of normal behavioural effectiveness (and Dr. A.R.G. Owen is an impressive and expert witness, quite apart from other considerations), then these phenomena might be regarded as expressing externalised aggression and self-assertion in a particularly stringently dissociated form.

There is nothing unexpected about the element of dissociation, in the sense of Matthew's not wishing to be held responsible for the havoc he created. It was a voice in the Himalayas that *told him* to do what he wanted to do, and not what others expected of him. But there is of course something thoroughly heterodox about the working hypothesis that Matthew's energy and aggression dissociated themselves from his body! From a strict classical psychoanalytic point of view even to entertain such a possibility might be thought of as countenancing 'omnipotence fantasies'. In Freud's cosmology it is precisely our subordination to the 'reality principle' that forces upon us the dilemma of acceptance, resignation, limitation and frustration on the one hand, or delusion and mental disturbance on the other. Now the psychic and, for that matter, the psychical researcher who is in principle willing to allow genuine and especially physical phenomena, are persons who have implicitly at least refused to submit to this Freudian dilemma: acceptance of conventional limitations or mental illness. It will be remembered how violently Freud himself reacted when he felt threatened by what he considered the 'tide of occult mud', the very ocean of irrationality that he felt to be represented by Jung's querying of the pan-sexual theory, accompanied by ostensible poltergeist phenomena (Jung (1963)). Here too, paralleling Matthew, there was a direct challenge to a father figure (Freud (1909)).

It is impossible in the present state of knowledge to provide an adequate discussion of this issue, but it is in my view relevant to mention these considerations. It is a far cry from 'omnipotence' or even 'omnipotence fantasies' to the relatively slight practical deviance from normality represented by the throwing about of everyday objects by unknown



means. Just exactly what are the limits of physical reality and potential, or of the human condition, are not *in themselves* psychiatric issues; but this is apt to be obscured by the fact that the psychiatrist is called upon to provide the rest of society with practical guidance as to what constitutes pathological perception and experience, and morbid or deviant action. It is here that a psychic like Matthew, or an open-minded psychical researcher, or for that matter any pioneer who rightly or wrongly challenges belief in existing limitations is bound to present a difficult and painful challenge to the world at large: established authorities, renunciations made and safeties taken for granted, all these are thrown open to question; the result is bound to be anxiety and hostility. These are important aspects of the psychology and sociology of innovation.

The crux of the question of the genuineness of psychical phenomena is: has there been, or has there not been, a transgression, or transcendence, of conventional limitations by, or in connection with, the psychic? If not, the question is one of the psychology of error, deception and self-deception, of ordinary deviance; but if so, if there has been exosomatic action, then the issues are rather different: are the psychic's psychology and development manifestly different from those of the rest of mankind?

#### *Problems of normality and paranormality*

If it felt that Matthew's effects are basically genuine, and we ask 'is there something strikingly different about Matthew from other people?', I think the initial answer must be 'no'. Development and motivation seem to follow on the face of it quite normal patterns: the only really unusual characteristic is the emergence and subsequent transformation of the phenomena themselves.

This transformation however is itself worth noting. It looks as if Matthew managed somehow to control the increasingly disruptive physical manifestations by the practice of automatic drawing. Now as Matthew himself shrewdly as well as honestly says, these drawings, however accomplished or attractive, were not really creative so far as he was concerned; they are in the fullest sense of the word derivative. Nevertheless, the activity of abandoning conscious control whilst engaging in the act of drawing did produce the most remarkable artefacts and this did, on the face of it, abolish destructive and chaotic physical manifestations. It seems to me that this is almost like witnessing something like the process of 'sublimation' in action, a particularly spectacular and successful application of the therapeutic exercise of 'active imagination'.

How are we to account for the astonishing and instantly recognisable family resemblance of these brilliant pieces to the work of so many

artists? Matthew does not on the whole believe that they are the work of deceased persons any more than would most parapsychologists, and he is of course not unique in accomplishing in a dissociated state work vastly superior to that which he is able to produce normally. It might be interesting in a future collection of clinical data to adopt the working hypothesis that automatic performances are a defence against physical or physiological disruption, usually hysterical or psychosomatic, i.e. *endo*-somatic, but exceptionally paranormal or *exo*-somatic; it might further be asked whether the greater the superiority of the automatic performance compared to normal accomplishments, the more often are there accounts of spontaneous psychokinesis.

As has been mentioned, Matthew has not continued with the automatic drawings, and when he approached me he was becoming more interested in biological than in physical effects. This was the reason for planning the seed germination, haemolysis and *mucor* experiments.

He described his attempts to influence Mary Rose Barrington's beans as follows:

... I'm imagining that I am expanding my consciousness right out round me through out of the walls, through the building and out across London. And I just imagine that I keep going round the globe till I start to go right into space. And at some point something clicks and I just feel then that I am just a part or in harmony with it, and that there is then some energy which is then coming through me which goes through my hands. Which is why I use my hands for this. And when I've got that feeling, I then put my hands over the seeds to make them grow faster, I keep an image in my mind of the seeds sprouting very quickly. And I imagine that they're in a very humid atmosphere with everything they need, with water and soil and so on. But when I try and retard them, then I go through the same mental process but I imagine they've been thrown into the desert, or put on a rock or something, where they can't grow and there's no water and no sunshine (A1/1).

Positive results were, as will be seen, obtained in this experiment, but it is clear that such imagining is for him at most a necessary and certainly not a sufficient condition, and may remain without effect, as in the case of the haemolysis experiments, where he imagined:

I've surrounded the entire test tube with some kind of white light that comes through my hands with that white light surrounding every single cell. The...cells won't break open...(A1/2)

Matthew's attitude to the pendulum and random number generator experiments was explicitly hostile. He disliked the heavy machinery surrounding the pendulum and the fact that the bob to be moved was in fact a light Christmas bauble made no difference to his subjective feeling. In the case of the RNG experiment, this was basically an 'out of the body' or clairvoyance test to 'see' random numbers, and not an attempt to generate a physical effect. It may of course be the case that the instrument's physical malfunctioning, i.e. continuing to produce the number 8, after the same manner as it had done for Ingo Swann, was an accident. Matthew himself thought he had probably made it go wrong: 'It's also one of the experiments I didn't want to do. That's not the first time it's happened. Tart's machine produced 8's' (A1/2). It might also be akin to the curious observation made by Bierman, DeDiana and Houtkooper (1976) in connection with Matthew, where in a random number generator they failed to obtain the effect they sought, but their program got stuck in a loop 'with a probability that such a loop would turn up in this condition  $p = .028$ '. Houtkooper and DeDiana (1977) is also of interest in connection with Matthew. Palmer, Tart and Redington (1979) also reported a tendency for a random event generator repeatedly to generate a particular target number after Matthew started to concentrate on it while planned analyses were non-significant.

There can be no doubt that in Matthew's case at least, the ideas and quite conscious fantasies accompanying attempts to produce 'physical' effects were aggressive, competitive and destructive, whereas those accompanying more 'mental' or 'healing' modes of action were on the whole peaceable, expansive, relaxed and benevolent. This reflection is not intended to constitute a value judgment about these modes: both are integral to living.

Although Matthew is plainly an unusual and exceptionally determined person as well as a highly intelligent one, I can see no reason to describe him as particularly saintly or inordinately altruistic or considerate of others. He does not consider that he is unique in possessing gifts in which he believes, but is of the opinion that others could do much the same if they admitted their own potential in this respect. He professes no idea of their nature or origin. Matthew can be very difficult indeed, as well as exceedingly charming.

As it now appears to me, the investigations of which this is an account, may be regarded as a series of experiments, concerning which views will no doubt differ, and also as encounters with an unusual and talented person. This dual perspective is as it should be, and is characteristic of a field in which we are concerned to investigate *objectively* characteristics of human beings, an enterprise in which the *subjective* experience of all concerned is likely to be of crucial importance.

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## 2

## BEAN GROWTH PROMOTION PILOT EXPERIMENT

MARY ROSE BARRINGTON

## THE EXPERIMENT

Trays of Mung beans were prepared for submission to Matthew Manning, who was to treat half the population, the other half being set aside as controls.

As originally prepared, four beans were placed in each compartment of ice-cube containers, the compartment having been lined with two layers of white blotting paper. It was intended that one out of each pair of compartments (the one to be selected by a random method) should be treated, and 40 pairs were prepared.

This did not suit Matthew's style of treatment, so the assistant experimenters (M.R. Barrington being absent from the proceedings) set aside one of the seven boxes, and selected three boxes for treatment as a whole.

One of these pairs of boxes was discarded by M.R. Barrington, as the boxes were not identical, and the first pair, each containing 48 beans, were handed over to K.L. Fudge, a biology teacher, for nurture and observation. The results are set out below in Table 2.1.

Experimental Sample (Promoted): 48 Beans		Day	Control Sample: 48 Beans	
Emergence of Radicles	Plumials		Emergence of Radicles	Plumials
20	0	1	23	0
12	0	2	3	0
5	1	3	2	3
2	17	4	1	11
2	7	5	3	4
1	5	6	0	1
0	4	7	0	2
<hr/> 42	<hr/> 34		<hr/> 32	<hr/> 21

TABLE 2.1

Taking the appearance/non-appearance of radicles and plumials within 7 days as an index of success/failure, 2 x 2 tables may be set out as follows in Table 2.2.

RADICLES:	<u>Succeeded</u>	<u>Failed</u>	<u>Total</u>	
Promoted	42	6	48	Allowing for Yate's correction Chi-square = 6.26 (1d.f.) P less than .02
Control	32	16	48	
PLUMIALS:				
Promoted	34	14	48	As above, Chi-square = 6.13 (1d.f.) P less than .02
Control	21	27	48	

TABLE 2.2

As an afterthought, Matthew was asked to treat a further sample of beans (in a small sealed container) for retardation, and these were grown under the same conditions as before. The retarded beans did slightly worse than the promoted, but better than the controls, in neither case to any significant degree.

Experimental Sample (Retarded) : 48 beans. Total of Radicles: 39  
Plumials: 27

The second pair of boxes will now be treated to see if the same differential effect can be obtained.

ATTEMPTED FOLLOW UP

Regrettably it has not been possible to carry out a satisfactory replication of this experiment.

When the first experiment was carried out, Matthew operated on the beans on 24 July 1978, and they were given over to K. Fudge for cultivation on the following day.

As soon as it was known that the first experiment had produced promising results, Mr. Fudge was given the remaining beans; however he was not able to attend to them until the last week in August, some four weeks later.

The results were decisively non-significant, though not without curiosity. In the case of both radicles and plumials, and both in the promoted and the control sample, the number that succeeded amounted to 28. The oddity of this coincidence in numbers may be judged by showing in brackets the result of the first experiment (see Table 2.3).

RADICLES:	Promoted Beans	28	Succeeded	(42)
	Control Beans	28	..	(32)
PLUMIALS	Promoted Beans	28	..	(34)
	Control Beans	28	..	(21)

TABLE 2.3

Despite the encouraging statistics the first results may of course have been due to chance; if however they were indeed due to successful treatment then it is evident that the results will not be repeated unless the beans can be cultivated soon after treatment. Unfortunately it has not been possible to arrange for this to be done, the three persons concerned living at some distance from one another and having other commitments.

The first results suggest that it would be well worth while for a full-time researcher with the required botanical knowledge to carry out some extended tests using Mung beans.

3

PENDULUM EXPERIMENT

A. J. ELLISON

On an earlier occasion an apparatus had been installed in the laboratory for certain tests of PK. The equipment consisted of a long pendulum installed in a transparent plastic tube. A very light bob (a Christmas tree bauble) was supported on a very flexible fine glass thread (this

material absorbing very little moisture so having a consistent stiffness and length with temperature and humidity changes). The pendulum base was mounted on a heavy wooden table itself on a concrete slab in the building. Transparent screens prevented voluntary or involuntary mechanical interference with any of this equipment.

In one version of the experiment the aim was to cause the pendulum to move from rest by PK. As the pendulum was approximately 1 metre long and very light, the force required to produce a small movement was very small. In another version of the experiment the aim was to change the motion of the pendulum from approximately circular paths to appreciably elliptical ones by sideways PK force at one place on the orbit. If this occurred, the pendulum was arranged gently to brush microswitches and trigger an electrical indication of the change. Two microswitches, adjustable in radial and angular positions, were arranged at positions 90° apart. Continuous chart recordings were made of the state (open or closed) of the microswitches.

If the pendulum had been approached by the subject (Matthew) and he had attempted to use PK on it to cause motion, and if motion had then resulted, it might have been suggested that the cause was building vibration, due perhaps to other equipment or to road traffic. The equipment was therefore kept switched on for some days and the absence of any movement observed. Movement starting coincident with PK intention of the subject would then be much less likely to be attributed to coincident normal causes of any kind.

Matthew stated when he inspected this equipment that he did not consider it likely that he could affect it. He was kind enough to try on several occasions but no positive results were achieved.

## 4

## RANDOM EVENT GENERATOR EXPERIMENT

A. J. ELLISON

On an earlier occasion equipment had been built primarily for tests of pure clairvoyance related to out-of-the-body experiences. The equipment consisted of a box carrying electrical circuitry so arranged that depression of a button at the front produced in a window at the back a display consisting of a 3-digit random number. Each digit was produced from the common arrangement of seven straight bars illuminated selectively to form the numbers. The original aim was to ask volunteer subjects, who were able to have an OBE on request when in hypnotic trance, to report the number in the window, invisible at the back to the subject in the normal state, to the experimenter and to the independent

witness(es). The experimenter would then set up the digits reported by the subject on dials at the front of the box. The button would then be depressed for another random number, the first one being lost, but the circuitry kept a record of rights and wrongs, i.e. agreements or disagreements between the numbers at the back and those set up at the front. At the end of a series of trials a dial at the back could be set to show the number of agreements in the window at the back, either for all three digits together or for each separate digit.

It is perhaps instructive to describe earlier uses of this equipment before the present series of experiments. The first OBE subject was unable under the conditions described to have the OBE. The second subject had a satisfactory OBE and on the first and pilot occasion, reported the numbers to the experimenter (myself) who looked to see what they were. There was almost complete agreement between the statements of the subject in trance at the front of the box and the numbers at the back. With this auspicious start, the first experiment was begun, the number at the back not being observed normally at any time. The subject then expressed difficulty in seeing the numbers in the window and the run had to be abandoned for practice elsewhere with numbers of a similar size.

Some time later the well-known American psychic Ingo Swann briefly visited the laboratory socially and, *inter alia*, this equipment was described to him. He at once asked to do a run. The box had not been in use for some months but was taken out, switched on, and appeared, on the basis of one or two rapid tests, to be working correctly. Ingo Swann stated that the OBE was not necessary for him to succeed in an experiment of this kind and he simply sat at one end of the table with the experimenter (again myself), stating what he considered the numbers to be. At the end of the run of about 20 trials the display indicated an agreement of 8 of the 3-digit random numbers with his statements. This was received by the experimenter with some surprise but arrangements were tentatively made to do similar experiments across the Atlantic, using the telephone. The following morning the experimenter did a run of his own and also achieved an indication of 8 agreements. There was clearly something wrong. A fault in the microcircuit contacts causing the illumination of all seven LCD bars would cause an indication of 8. The microcircuit contacts had no obvious fault but were nonetheless carefully cleaned. The apparatus was then found to have returned to normal, the experimenter scoring agreement between front and back numbers only when he observed the numbers at the back first.

The present subject (Matthew Manning) kindly agreed to do several runs in a way similar to that used by Ingo Swann. The equipment was switched on and achieved a steady temperature well before the first experiment. Both experimenters (myself and D. Chapman) carefully



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checked it for accuracy, with success, all indications being correct. The Subject then did a run of 20 trials. The box indicated 8 successes. With some disquiet the experimenters themselves did another test run and found that they also scored 8. Careful cleaning of the microcircuit contacts again restored the functioning to normal.

Careful checks were made exactly as described above and the Subject did another run on the following day. Again he achieved an indication of 8 and again cleaning of the contacts was necessary.

These salutary experiments are clearly of little use as indicating pure clairvoyance to a sceptical observer. The experienced parapsychologist, however, would observe that similar results are not infrequent. It may be hypothesised that the unconscious machinery of the gifted psychic found it easier to bring about faults in the contacts by PK than to discover the numbers by clairvoyance. Sadly this could not be proved at the time and that experiment had to be left in this unsatisfactory state.

5

POETRY EXPERIMENTS

ANITA GREGORY

ORIGINS

In the course of preliminary discussions between Matthew Manning and myself, Anita Gregory, Matthew expressed the wish that a means might be found of testing an ability he felt he had recently developed of drawing a picture that aptly illustrated a poem he had to sense by psychic means. I suggested to Matthew experiments on the lines of Besterman (1933) and Dingwall (1924) with Ossowiecki, who had produced drawings that bore a striking similarity to the contents of carefully sealed prepared envelopes.

It was explicitly intended to use these experiments as occasional tests as and when Matthew felt like it, so that he would decide whether and when trials were made. It was also anticipated that there would be gaps between other experiments, and the poetry experiments were partly devised to bridge these gaps by means of prepared material.

METHOD OF PREPARATION

The preparation was devised and carried out by myself and no one other than I knew the details until after the opening of the first package.

20 brief extracts from poems, most of them two to six lines, were typed on slips of paper. They were chosen because they seemed to

evoke a clear mental image. Each extract was allotted a number from (1) to (20) which was typed on the slips. The slips of paper were folded in two, wrapped in aluminium foil and punctured in four places through the slips and foil, so that light could be seen through the holes. The punctured foil sachets were inserted into a first stout manilla envelope ca. 19.2cm x 12.5cm. The flap was stuck down, I signed my initials across the closed flap and body of the envelope, and, while the ink was still wet, sealed sellotape across my initials, flap and envelope. This was inserted into a second larger manilla envelope, ca. 25.2cm x 17.5cm. This envelope was again stuck down at the flap and I initialled both the flap just stuck down as well as the opposite end, sellotaping across as before. A blank piece of paper ca. 21 cm x 14.5cm was taped onto the front of the outer envelope. The packages were thoroughly shuffled and were then labelled on the outer blank paper E1 to E20. I was unable to tell which package E1 to E20 contained which slip with verse (1) to (20).

#### PROCEDURE

I carried the 20 packages with me in a brief case. Out of these ten packages altogether drawn out of the case at random by myself were used on different days.

On the first occasion that a package was handed to Matthew he was told that it contained a verse. He was asked to draw on the blank piece of paper stuck onto the envelope. On this and later occasions he then drew and wrote whatever came into his mind. He certainly liked the experiment and initially expressed confidence in his ability to achieve results.

Immediately after each test I opened the successive envelopes, checked that the punctures were intact, invited any one present to do likewise, and compared poem with drawing. The opened and used packages complete with drawing and verse were then returned to the briefcase.

#### RESULTS

Ivor and Enid Grattan-Guinness were asked some days later on 12. 8. 1978 to blind-match the drawings with the snatches of verse, giving first, second and third choice. Similarities between drawings and poems were negligible. They subsequently applied a procedure devised by I. Grattan-Guinness (see note below). Again, results were negligible.

One drawing, package E5 containing verse (19), awarded the highest marks by I. and E. Grattan-Guinness, may be used to illustrate the

difficulty in making qualitative judgments of this type. The drawing, the first one Matthew made, was a crude sketch of a knight in armour on a winged horse with a castle and pine trees and a bird flying overhead. Matthew wrote above the drawing: 'Knight in armour. Breughel-like image of dogs and a castle — passing through trees. Reminds me of Dürer drawing of knight and devil. Horse has wings. Blindman.' The verse in the package, by Keith Douglas, was

Now on my dial of glass appears  
The soldier who is going to die

Matthew was far from delighted with this as a 'hit', but he, as well as others present, thought it reasonably relevant. In naming the apocalyptic Dürer etching, Matthew had omitted the third partner, 'death', and certainly the mood of the drawing and that described by Matthew were quite apposite to the Douglas poem. Matthew commented that a knight was a soldier and the mood was of doom and death.

The attempt took place in the laboratory while we were waiting to do a set of haemolysis experiments, delayed because of adjustments to the equipment requested by Matthew. He made several subsequent attempts at other poetry packages on that and subsequent occasions, which both he and I agreed showed no relation either of us could imagine between verse and drawing, although there were quite frequent allusions and associations that subjectively felt relevant.

#### DISCUSSION

It is plain that no positive results can be claimed. The whole endeavour highlights once again the problem of quantitative assessment of basically qualitative material. A better technique for blind-matching might have been by, for example, presenting judges with several independently selected extracts of verse in addition to the critical one, and asking them to rank these for goodness of fit. In my view there is also room for I. Grattan-Guinness' suggested *post hoc* grading, especially if a sophisticated design were devised by which the judges did not know whether they were assessing the drawing by the Subject under test, or some control drawing. Alternatively the Subject could have been presented with several extracts and asked for *his* preference.

In assessing the method as a whole, a number of considerations arise which may have a bearing on future experiments of this type.

1. The packaging was most effective. The puncturing of the metallic wrapper and slips after the two outer envelopes were removed remained intact for well over three years and survived posting by first and second class mail of control packages (19.1.1982). The packages can be

recommended for GESP tests.

2. The format of the experiment was attractive to the Subject despite the fact that it did not ensure success.

3. As has been pointed out, a more refined set of techniques for judging should be devised.

4. The somewhat ambiguous part played by these experiments in the setting of the investigation as a whole should be pointed out. Whereas they were regarded as important in their own right and had required a good deal of preparation, and precautions were incorporated in such a way as to safeguard spontaneity at the time of test administration, the packages were *in effect* often used as fillers and side occupations while we were waiting for other and perhaps by psychological implication 'more important' things to happen. Had time been specifically set aside this would almost certainly have made a difference to the group mood and expectations surrounding these experiments and consequently – possibly – the results. Again, quite different results might have been obtained under Ganzfeld conditions.

5. The 'psychometry' mode was chosen deliberately for safety, ease of administration and spontaneity. However, it could well be that in order to test Matthew's ability to match drawings and verse, a more plainly 'telepathic' mode of experimenting might have been more appropriate. The principle of collaborating with a Subject by attempting to incorporate a spontaneously experienced phenomenon into a more systematic set of tests should be further explored.

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#### A NOTE ON THE METHODOLOGY OF BLIND MATCHING

by IVOR GRATTAN-GUINNESS

The usual procedure in blind matching is to place the sensitive's product in choice order (up to 3rd, usually) with the given data. When faced with matching Matthew's 10 drawings (some with annotations) against the 10 extracts from poems, I felt overwhelmed by the 100 possibilities, especially as *any* aspect of correlation was allowable. I also noted that each order is internal to the extract, and gives no indication of the

*quality* of the correspondences which led to the choice of ranking that was made.

In this matching experiment I followed the alternative of assessing the quality of *each* extract with *each* drawing, without having to have the other extracts to compete for ranking with this or any other drawing. Three categories of correspondence were used: 'good', 'fair' and 'marginal'. Assignments to categories had to be made qualitatively, but it turned out not to be difficult to do so. I revised some of my judgements afterwards. I allowed for no, or more than one, extract, to be placed against each category.

My wife and I made the correspondences independently, and then did a joint assessment without looking at our previous individual judgements. The three tables of results were pooled, and a cumulative quantitative analogue produced from the measure good = 5, fair = 3 and marginal = 1 for any judgement which we made of the correct extract for each picture. While there are obviously still considerable difficulties in interpreting the results we felt that a procedure based on grading, rather than the more normal one using ranking, was easier to produce and led to more satisfactory judgements.

## 6

## INFRA-RED EXPERIMENTS

ANITA GREGORY AND KATHLEEN WILSON

## INTRODUCTION

The first researchers to use infra-red radiation as a detector of psychokinetic activity were Eugène Osty, Director of the Institut Métapsychique in Paris, and his son Marcel, an engineer (Osty and Osty (1933), (1932)). As it happened, their discovery was a by-product of quite another endeavour. Eugène Osty had originally been concerned with the perennial problem of controlling a physical medium during trance, particularly in the dark. While ostensibly physical paranormal phenomena were occurring, the traditional way to achieve this was by means of controlling the medium either manually or, more elaborately, by means of electrical signalling devices of various sorts. As usual, each method has its advantages and drawbacks. Among the difficulties that may be urged against immobilising a medium are that this does not exclude trickery by some other participant, that watching either the medium or a control panel deflects the attention of some of the most critical people present, and that there is felt to be something inelegant and clumsy, as well as less than humane, about physically restraining a person. Osty suggested that instead of (or in addition to) immobilising the medium, arrange-

ments should be made to monitor automatically and instrumentally the objects to be moved, and he devised an infra-red burglar alarm style device for thus guarding the objects to be moved paranormally by the physical medium Rudi Schneider: any normal encroachment on the areas to be guarded would immediately be detected, signalled and automatically recorded by cameras. When the device was first put into operation, signals were obtained suggesting that security had been breached and that something tangible had indeed penetrated the infra-red network. However, the target objects were left undisturbed, and the monitoring cameras, using magnesium flashes which had been triggered off by the infra-red alarm system, showed no sign of any intruder. After initially assuming that the repeated triggering of the alarm system was due to malfunctioning of the apparatus, it was noted that in fact the interruptions of the beam were correlated with attempts by the medium's control personality 'Olga' to move the target objects: 'Olga' (or Rudi speaking as 'Olga') would announce that now 'she' would lift up a handkerchief, and at this point the alarm would be set off. There ensued a good deal of experimentation, and the results with the same medium were replicated in England, both under the auspices of Harry Price (1933) (although the apparatus was not under his control) as well as by Hope *et al.* (1932) and again by Schwaiger in Vienna (1935-36). Osty delivered the Myers Memorial Lecture on the subject of these experiments in which he discussed his view that the medium exteriorised in trance some form of force or matter, invisible in white light, but which could be detected by means of infra-red radiation of suitable wavelength.

Parapsychological interest in the field of PK in the intervening years apart from metal bending in the wake of Uri Geller, has focussed more on the meaningful or intentional influencing of physically random events than on attempts to detect and evaluate directly the exertion of paranormal physical influence. However, in view of the outstanding quality of the evidence we have for the Schneider phenomena, especially the IR effects, it seemed important to attempt to replicate the observations. Not only would it be of considerable interest to obtain evidence corroborating the hypothesis of some tenuous and transitory form or phase of matter, such a method might also be expected to yield results in an area of investigation from which promising experimental subjects, i.e. physical mediums, have all but disappeared, at least in Europe and in the U.S.: the results with Rudi were obtained towards the very end of his mediumship when even fewer, if any, phenomena were still being observed. The working hypothesis therefore naturally presents itself that effects in the infra-red might be vestigial manifestations found in conjunction with persons who had earlier given evidence of physical paranormality, such as former poltergeist children.

When Matthew Manning approached Anita Gregory as described in the

introduction, this appeared to be a promising opportunity to test out the equipment previously prepared by Mr. Colin Brookes-Smith. It was decided to carry out pilot experiments to see if Matthew would affect the infra-red apparatus in a manner similar to that of Rudi Schneider, thus providing a *prima facie* corroboration for the working hypothesis that former PK agents may still produce vestigial paranormal influence in the form of ostensible partial occulting of infra-red radiation and, in the event of positive effects being obtained, to note some of the characteristics of the phenomena for future experimentation. Moreover, results indicative of a vestigial form of matter might also have a bearing on the fundamental theoretical problems trenchantly outlined by Braude (1981).

#### APPARATUS

The equipment for detecting occultation of the infra-red beam (see Figure 1) consisted of a mounting board with an infra-red source at one end and a photocell at the other. The photocell output voltage was amplified and biased in a separate amplifier unit having an output of direct voltage of 1 under zero occultation conditions and 0v under total occultation conditions. The output voltage was displayed on two independent analogue voltmeters, and on a digital voltmeter (Gould-Advance model Alpha iii); it was also recorded on chart paper using a Watanabe Linear-corder Model WTR 281.

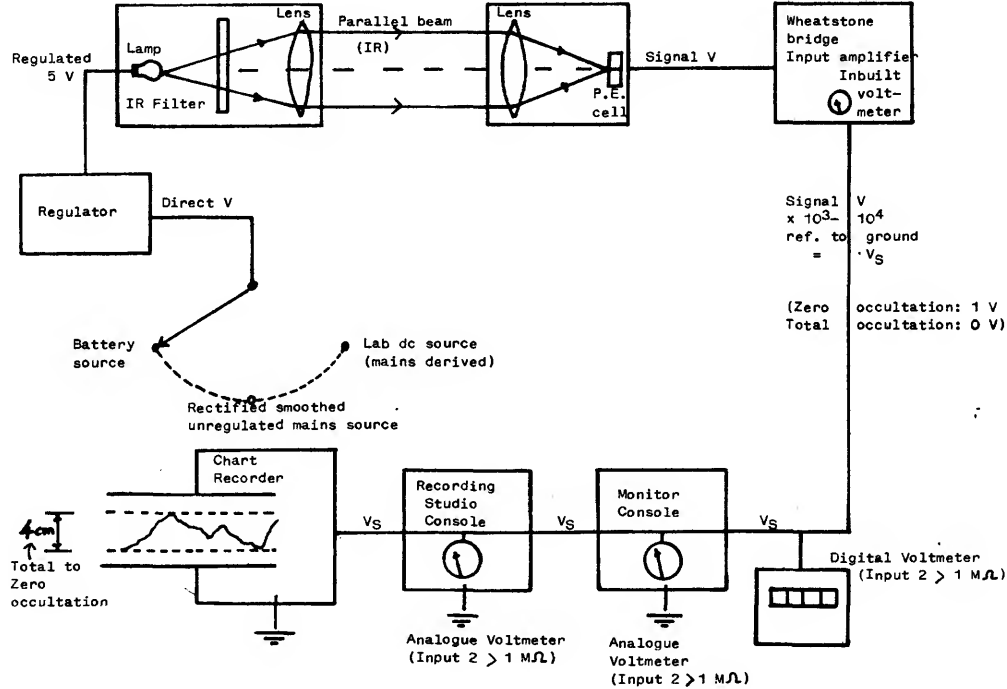
The IR light source was housed in a plywood box with plywood feet extending from each side so that it could be firmly screwed down. The light source was a 6 volt MES bulb focussed on a 4" lens resulting in a parallel beam.

The IR filter transmitted light  $\lambda = 0.9-2.5\mu\text{m}$  measured on a CARY 14 spectrometer.

Power for the 6v bulb was derived from three different sources during these experiments. These were:

- (a) A bank of alkaline accumulators followed by a conventional solid state voltage stabilizer; the regulated output voltage was set at 5v.
- (b) An unregulated, rectified and smoothed direct voltage derived from the 240 volt mains supply via a transformer, followed by the same stabilizer as in (a) above.
- (c) Laboratory twin stabilized d.c. supply unit (Advance model PP3) and DS 5/05 power unit adjusted to provide a direct voltage output of 5v.

The photocell unit was contained in a plywood box similar to the lamp unit. A 4" diameter lens was fitted at the end facing the lamp unit and a silicon solid state light sensitive cell was mounted on an adjustable stand located in the lens focus. Output connections were provided



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Figure 1. Schematic diagram of equipment.



by a pair of 4mm sockets in parallel with a 3.5mm co-axial plug and socket connection. The plywood case again had projecting feet on each side so that when it is correctly aligned to the light beam it can be firmly screwed down.

The amplifier unit had a plywood case. Amplifier panel and power supply were in separate compartments, and there was also a small compartment for a 6ft mains flex and its 13 A plug.

The amplifier panel was 7½" square. On it were mounted the panel meter, all the essential controls and the inlet and output sockets. The input circuit was essentially a d.c. Wheatstone bridge having 10,000ohm resistance arms, and there were coarse and fine controls to obtain null balance. The bridge output was connected to a two stage 741 operational amplifier with variable gain control, the overall voltage gain being between 1,000 and 10,000. The d.c. output signal was at the 1v level.

The experiments were conducted in the Bio-Electricity Laboratory at the City University, use being made of the installed monitoring and recording facilities.

The infra-red source and sensor were mounted on a rigid board, usually placed on a trolley located in the experimental area close to, and connected by an umbilical to the amplifier unit situated at the monitoring console. The amplifier unit output was plugged into the appropriate data lines at the console which provided an analogue electronic voltmeter display (3v full-scale deflection), having an output impedance of about 1MΩ and in addition a digital voltmeter display. This latter 3½ digit instrument, set to show 1.000v at zero occultation was sensitive enough to display the noise in the system (0.01% per digit). Since the noise was typically between 1% and 2%, the digital reading was continually varying between about 0.980 and 1.020; this appeared to attract the Subject's interest and he used the instrument for feedback during most of the experiments.

Data lines from monitoring console transmitted the amplifier unit output to the recording studio on the mezzanine floor immediately above the experimental area. Here the data were displayed on an analogue voltmeter similar to that on the monitoring console and monitored by technician staff; the voltage was also recorded on the Watanabe chart recorder. The chart record showed the amplifier unit output on a scale providing 4cm deflection for total occultation, together with timing pulses at 1 minute intervals. Additionally event marker pulses were recorded by operating a press button in the experimental area.

Audio recording was in use throughout the experiments. Two microphones were active, one situated centrally overhead in the experimental area, and the other, for use by the experimenters, at the monitoring console. Synchronisation of the chart and audio records was effected

by manually marking the time chart and audibly recording it on the tape. Further details are given in the text. The recorder used was a Phillips model 4407 stereo. Some of the sessions was recorded on an Akai video recorder Model VT 1100.

BRIEF DESCRIPTION OF EVENTS AND RECORDS

*Timing*

A provisional timetable was drawn up and circulated to participants. It was explicitly envisaged that arrangements would be changed, since the two weeks were regarded as a period of concentrated exploration, rather than providing definitive results. Table 6.1 shows departure from the original tentative timetable.

IR Experiments Projected	IR Experiments Conducted
a.m. Tuesday 25 July	p.m. Tuesday 25 July
a.m./p.m. Wednesday 26 July	p.m. Wednesday 26 July
a.m. Friday 28 July	noon/p.m Friday 28 July
'any time' Monday 31 July	p.m. Monday 31 July
(if promising)	p.m. Wednesday 2 August (impromptu trial at Barts)

TABLE 6.1

The timing was partly dictated by the needs of other experiments and partly by Matthew's decisions to turn his attention to the infra-red experiments, with the exception of Wednesday 2 August. (The circumstances surrounding these 'decisions' will be more fully discussed below.) The fact that the infra-red equipment was set up most of the time and that the experimental area was so organised that attention could be switched from one experiment to another at will facilitated maximum flexibility. As will be discussed, such flexibility combines advantages and drawbacks.

The experiments will be described in terms of the days on which attempts were made by Matthew to influence the infra-red.

*Some comments on apparatus and terminology*

A few words of description in non-technical terms are necessary here to clarify what follows. Phenomenologically, that is from the point of view of the Subject as well as non-technical participants, three items of equipment were of significance: the infra-red (called IR) equipment, the digital voltmeter (DVM) and the chart recorder.

The IR equipment was composed of two wooden boxes mounted on a wooden board about three feet apart: if the IR was 'on', that is, if the box containing the IR source projected its beam on to the cell, the

space between the two boxes had to be seen to be perfectly vacant. Any intervening object would immediately affect the 'beam' and, depending on its size and density, would result in a recording of a partial or total 'occultation'. Objects would occasionally be deliberately put in the beam for test purposes to see if the instrument was working, and this was noted. It was the Subject's task somehow psychically to infiltrate or invade the space between source and cell, so as to cause a partial 'occultation' of the beam (Figure 2). (The word 'occultation' will be used in the descriptive sections for simplicity, without pre-empting the interpretation of the causal agency of deflections registered and recorded.)

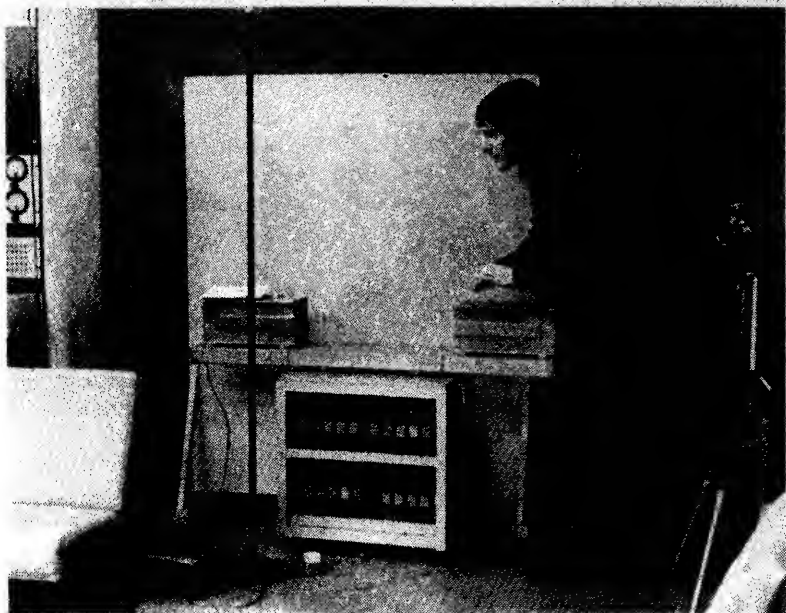


Figure 2. Matthew Manning attempting to influence infra-red equipment. He is holding hands over source box and looking at digital voltmeter (not in picture) for feedback.

The digital voltmeter registered the state of the IR beam, flickering permanently around the 1000 mark when the beam was 'on' and there was nothing in between the boxes, meaning 'zero occultation', 'nothing obstructing the beam' or, of course, 'all the beam is getting through to the cell'. If anything was put in the way of the beam, the reading went down, say to 750, which would mean a 25% occultation. Matthew's

chanting 'down, down!' reflected his attempts to reduce the voltage so as to show a figure lower than 1000. This meter was installed by D. Chapman in response to Matthew's request for feedback, so that he could see whether he had indeed affected the IR.

The permanent record corresponding to the flickering voltmeter (though with far from perfect sensitivity) was made by the large chart pen recorder on the mezzanine floor. (Unfortunately the pen registering occultation ran along a base line when there was no occultation but went *up* in response to *lowering* of the voltage or increased occultation. Thus (geometrically) 'up' on the permanent chart record corresponds to (arithmetic) 'down' in the readings of the voltmeter. This was perfectly clear to participants, but presents problems in interpreting the audio record, where 'up' and 'down' are not always qualified.)\*

As will be seen, during three of the experimental sessions prolonged irregular deflections of the pen were recorded on the chart, indicating sudden lowering of the voltage input designed to measure amount of occultation. The colloquial term 'bout' is used deliberately to indicate sets of irregular deflections such as are depicted in Figure 3 (discussed below). The reason for the use of this term is that there is a certain arbitrariness in the sub-division of any given group of irregular deflections. It is impossible precisely to pinpoint in time when any given bout starts and stops: there is often a gradual smooth rise in the trace which would have been reflected in a gradual lowering of read-outs of the voltmeter, and return to baseline after a set of peaks is often not to a placid horizontal line at zero (or 1000mV). Furthermore, the clumps of irregular deflections found from about the middle of the chart for Friday 28 July onwards are often not divisible into separate incidents without doing violence to the data.

Surnames only (except for Matthew) are normally used in what follows to save space. Initials were considered too confusing in view of the number of participants.

#### *Tuesday 25 July*

It had been originally planned to place some object in the path of the beam itself to see if Matthew's attempts to influence the object would affect the beam. Osty, Price, and Hope and Rayleigh had used an object such as a handkerchief for this purpose. Brookes-Smith, in his manual for the use of the infra-red apparatus, had suggested a strain gauge, but in the event we used small pots of cress, as Matthew declared that he had no interest whatever in physical objects, but was keen to attempt to influence organic systems, since he believed he had been so successful in this in the United States fairly recently.

The small pots of cress were placed in the path of the beam and suitable instrumental adjustments made. Matthew attempted to 'make

\*See footnote on page 348.

them grow faster'. He placed his hands on either side of the beam and attempted to 'promote growth'. The path of the beam was protected by tapes stretching from source to detector box to prevent his accidentally occulting the beam with his fingers. At no point was any occultation observed while he was trying to influence the plants. However, he said that he had a feeling of coldness on his hands, 'like ether'.

There is ample evidence in the discussion recorded on the audio tape, that there was an 'upward' drift in one of the recording pens (corresponding to a gradual lowering of the voltage). Such a trend would normally be interpreted as indicating a lowering of temperature of the apparatus which would, if anything, have been expected to rise the longer it was switched on. However, this drift was subsequently (next day) attributed by Chapman to the running down of the battery, since he found that the battery, supposed to have been fully charged but not by himself, was running down.

Matthew suggested at this time that he would like to attempt haemolysis experiments in the infra-red beam which were subsequently put into effect. It should be stated at this point that at no time was there any irregularity in the infra-red record in response to Matthew's attempts to influence the samples of blood. No attempt was made to examine the cress itself, which would have been quite impracticable.

#### *Wednesday 26 July*

In the morning Ellison and Chapman attended to the instrumentation in an attempt to take care of the drift of the pen recording the infra-red noted the previous day. Matthew did try, at Ellison's request, to influence the IR, but without effect. After the battery was found to be apparently responsible for the drift, the apparatus was put on stabilised mains during lunchtime, and after a drift due to the normal heating up period, a stable, horizontal trace was obtained.

In the afternoon, Matthew was being kept waiting for a haemolysis experiment because Brown was making adjustments to his equipment. Ellison went to try and find him. Meanwhile, the first poetry experiments were being tried out by Gregory at Matthew's request (see page 309).

After one conceivably relevant correspondence, four more poems were used, but the drawings and captions made by Matthew bore no relationship to the enclosed verse that he or Gregory could discern. Mary Rose Barrington, in an attempt to encourage him, tried to find some barely conceivable relationships between drawing and lines which seemed farfetched in the extreme to the others present. Matthew clearly found this exasperating and the more irritated he grew, the more she endeavoured to provide barely imaginable correspondences of meaning. Gregory was far from delighted by this well meaning but in

her view ill-judged attempt to jolly Matthew along. The audio recording bears witness to an atmosphere in the laboratory tense with irritation, although only Matthew, characteristically enough, expressed his vexation in so many words. Gregory then went to collect Ellison and Brown.

During this time, after some further verbal sparring between Matthew and Barrington, he got up and put his hand in the beam to see if the equipment was working. This incident is recorded on the chart and was marked as normal occultation. He was at that point under observation by Chapman and Barrington, who noted that he was not in contact with the equipment. Within a minute, Chapman pointed out that there were irregularities in the trace (Figure 3). As can be seen, the trace begins to rise, gradually at first, and soon, severe irregularities in the trace manifested for the first time. During this time, Matthew was holding his hand well above the photocell of the IR, attempting to lower the voltage on the DVM.

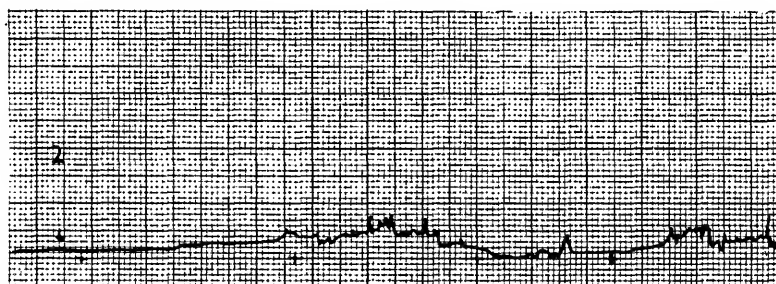


Figure reduced from copy of the original for purposes of illustration

Figure 3. Beginning of the first set of irregular deflections, afternoon 26 July 1978, chart 1. Chart speed 50mm per minute. Approximate period covered by entire illustration is 2¼ mins.

Neither from the audio record, nor from anybody's recollection, does it appear that Matthew consciously and deliberately *initiated* whatever it was that caused this irregularity in the trace. He himself is heard to describe the events as a good instance of a 'spontaneous' influence. He expressed his agitation, which he attributed at least in part to still being kept waiting. At some point which is not entirely clear, Ellison, Brown and Gregory returned to the lab., the two latter proceeding immediately to the preparation for the next haemolysis experiment. Chapman expressed puzzlement at the irregularities and Barrington called for witnesses. Matthew is heard to observe, somewhat bitterly, that 'nobody else is interested in these experiments'. The end of this bout of irregular deflections is unfortunately not accompanied by audio record because the tape ran out.

When the tape recorder comes on again, there is conversation where both Barrington and Chapman testify that Matthew was not touching anything while irregularities were occurring.

The fourth bout began at approximately 16:42. Participants called out the numbers on the digital voltmeter which so far as can be ascertained on this and subsequent occasions, corresponded with reasonable accuracy in time and magnitude to the deflections shown on the chart record. The analogue voltmeter also corresponded to both other indicators. After this, Matthew was asked to go away and Ellison and Gregory simulated the movements Matthew had made over the apparatus to test whether deflections such as those observed could be caused by normal factors such as shadow, proximity, etc. However, their movements did not have the slightest effect on any of the indicators, i.e. digital voltmeter, analogue voltmeter or chart recorder. At this point video recording equipment was introduced, operated by Chapman. Matthew returned, and after another two bouts, the tracing returned to normal.

Matthew had by this time taken to addressing himself to the digital voltmeter, 'willing' it to go down. The chart recorder was only set to reflect a lowering in voltage, i.e. occultation of the beam, and not an increase in voltage. In view of Matthew's preoccupation with the voltmeter, and to see if it was the voltage rather than the IR beam that was being affected, it was decided to ask him to 'will' the digital voltmeter to go in the opposite direction, i.e. 'up'. Since this could not be reflected on the chart, systematic reading out aloud on the audio recording was substituted. According to the audio record, the readings on the digital voltmeter rose for a period of approximately five minutes, the highest voltage read-out being 1032. This, as Chapman observed at the time, was only one tenth of the value obtained in the opposite direction, and it could be deemed to be within the noise level in the system. (On subsequent occasions, Matthew showed a strong preference for 'making it go down' rather than up. Later attempts were made to re-adjust the baseline but these turned out to be un-satisfactory.) At 17:00 Matthew said, 'It'll be upset for at least an hour' and

I just felt that I was controlling it and that it would do whatever I told it to do. That's why when it was going down I could make it go lower and when I decided to turn around and make it go up I just told it to go up.

Ellison: 'You just told it as though it was an animal doing what you said?'

Matthew: Yes. But I really believed myself at that moment that it was going to do exactly what I told it to. Just as now I believe

that it will take a long time to settle down again. Something has got into that which it will hold for a long time. (A3/1)

However, there was only one further bout lasting about 2 minutes after which the machine settled down to normal and the chart was signed at about 17:09 and Matthew left the laboratory soon after.

*Thursday 27 July*

No infra-red experiments were envisaged for this day and from the audio record it would appear that although the IR equipment was running, the chart recorder was not switched on. However, some impromptu experiments were carried out by Ellison, encouraging Matthew to affect the beam. It would seem that some instability in the voltmeter was detected. However, since Chapman repeatedly stated that he was not satisfied that the instrument was as yet stable, and since there is no chart to provide a permanent record of instrumental deflection, the results dictated onto the audio record should, in our view, be disregarded. It is, however, worth noting that according to the audio record, attempts to influence the beam by means of a magnet and by means of a hair dryer failed to affect it in any significant manner.

*Friday 28 July*

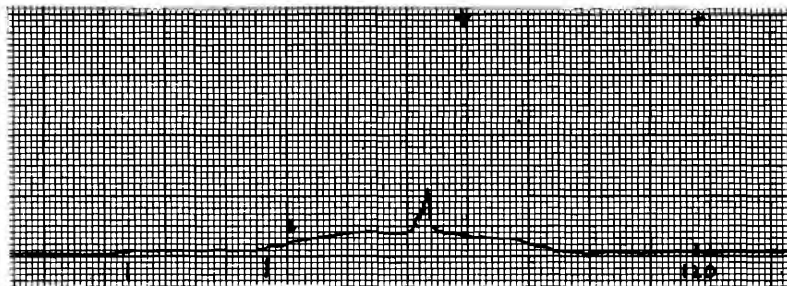
Infra-red experiments were expected to take place sometime during this day, although no exact timing was determined. It had been arranged to start with haemolysis sessions and on the previous evening Gregory had arranged with Dr. and Mrs. Grattan-Guinness, who rang her up from Devon, that they would be attempting to 'send' some telepathic messages starting precisely at noon, to be determined by the BBC time signal. I. Grattan-Guinness had originally been asked to be a member of the investigating team, but he was unable to attend in person, since his holiday had been previously arranged. Immediately when Matthew arrived at the laboratory on Friday morning, he refused to have anything to do with the Grattan-Guinness experiment on the grounds that if they couldn't be bothered to be present, he couldn't be bothered to do experiments with them.

A chart is available for the whole day from 10:53 to 16:00. After the trace was stable, the whole infra-red equipment was moved so as to make it more accessible to the video camera. The transportation of the instrument is recorded on the chart trace by very small irregularities in the baseline. The trace then immediately settled down to complete stability, from 11:15 onwards, and a haemolysis experiment was undertaken. At 11:54 a video recording was started. Although Matthew's refusal to collaborate with the Grattan-Guinness experiment was briefly referred to in connection with the conduct of other experiments, no



further attempt was made to persuade Matthew to change his mind.

At approximately 11:58 (as subsequently estimated by reference to the speaking clock) the chart trace leaves the baseline and described a shape never previously nor subsequently observed (Figure 4).



*Figure reduced from copy of the original for purposes of illustration*

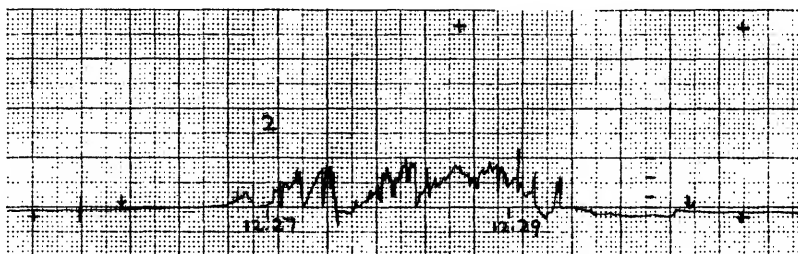
Figure 4. 'Grattan-Guinness peak', 28 July, Chart speed 25mm per minute. Approximate time covered by entire illustration is 11:57 to 12:02.

Sullivan was observing the chart at the time and called out to Chapman who immediately alerted the other participants. From the audio record it would appear that Matthew was not in the experimental area at the time, since a haemolysis control experiment was in progress. This was noted by Gregory, Chapman and Matthew himself. When attempts were made to establish the exact time, since this was about noon, the conversation turned naturally enough to the Grattan-Guinnesses, who were presumably sitting somewhere in England sending unrequited messages! However, haemolysis was resumed and while Gregory's voice can be heard counting down the seconds, Matthew's voice is heard whispering 'down, down' a number of times. It seems clear from the audio record that Matthew had switched interest from haemolysis to infra-red at this stage. At this point the chart baseline is nearly stable, though not perfectly straight.

Between 12:09 and 12:10 Matthew asks 'is Ivor Grattan Guinness known as a great psychic?' with the clear implication that paranormality might be imputed elsewhere, and he goes on to speculate that he might have unconsciously picked up the Grattan-Guinness messages and translated them into activity in the infra-red. From this point on the chart trace shows a fairly steady rise, whilst Matthew is addressing the voltmeter, adjuring it to go down. Between 12:13½ to 12:15, there is a characteristic jagged bout after which irregularities continue at a much lower level until about 12:17 at which point the trace settles down again.

At about this time, Mr. Roger Chapman, Senior Lecturer in Electronic Engineering, a member of Professor Ellison's staff, arrived in the laboratory. There are introductions and the apparatus was explained and demonstrated.

At about 12:25 the trace begins to rise again and there is a characteristic jagged bout (Figure 5 and Table 6.2), Matthew being closely observed and verbally described at the same time.



*Figure reduced from copy of the original for purposes of illustration*

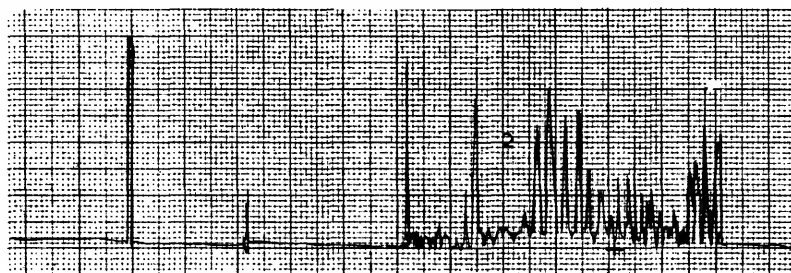
Figure 5. Irregular deflections 28 July, chart 2. Chart speed 25mm per minute. Approximate time covered by entire illustration is 12:25 to 12:30½. Table 6.2 refers to coincident events from 12:27 to 12:29 marked in illustration.

The video apparatus is on during part of this incident showing Matthew passing his hand back and forth over the box. As described by Ellison the trace drifts back towards normal baseline and Roger Chapman mimics Mathew's movements. This also is recorded on video tape. Unlike Matthew, Roger Chapman repeatedly touched the box which manifests itself in vertical cross-hatching deflections, apart from which the trace is horizontal and at zero. There followed some attempts at normal simulation of the observed traces.

Between approximately 12:34 and 12:35 Sullivan physically shook the base on which the IR instrument rested without obtaining more than a thickening of the horizontal trace. Gregory occulted the beam normally by first placing her hand in the path of the beam and then by rapidly flicking her fingers into it. Brown inserted a thin box into the beam (Figure 6). The traces of these normal occultations look entirely different from the irregular ones under investigation.

About two minutes later Matthew attempted to influence the beam from a distance of approximately four metres from the apparatus. About a half minute later, the trace leaves the baseline rising to a peak, although its height can not be exactly determined at any one moment because at that point the motor was accidentally switched off, the chart stopped moving and the pen recorder went up and down on the same

*Figure reduced from copy of the original for purposes of illustration*



*Full hand  
occultation*

*Rapid flicking of  
fingers in the beam*

*Lowering of thin  
box in the beam*

Figure 6. Normal occultations, 28 July, chart 2. Approximate time covered by entire illustration is 12:35 to 12:41½.

spot for at least a minute. It then resumed its jagged path while Matthew was attempting to influence the beam from a distance of about three metres. There is a sudden recovery to zero and Gregory's voice is heard saying 'Matthew is now relaxing'. The trace begins to climb again soon after, while participants are making an attempt to persuade Matthew to relax. He explains that he 'cannot stop thinking about it' and expresses the opinion that the instrument will be disturbed for some time. At this point, about 12:50, Brown and Gregory took Matthew out of the laboratory for a walk around the square, leaving the electrical engineers and technicians, Ellison, David Chapman, Roger Chapman, and Sullivan, to discuss the behaviour of the equipment. Hardly had Matthew *left* the laboratory than the pen resumed its aberrant course. It did not settle down until six minutes later. The engineers switched off the IR source, re-set the baseline to midpoint, and switched it on again. Matthew made no further attempts to influence the IR that afternoon. He felt it unlikely that any more would happen as he 'felt drained'. The chart record was completely straight from 13:06 to 16:00, and is signed by participants.

#### *Monday 31 July*

The chart for this morning is completely level from 10:38 to 13:00, indicating that there was no occultation or fluctuation in the voltage beyond noise level throughout this period. The power source is the mains. During this time there is general discussion (including a discussion of the 'Grattan-Guinness peak') and a haemolysis experiment. Shortly after midday, Matthew says 'I had to stop myself from doing it [the IR] this morning... I want to wait for this afternoon'.

The chart for the afternoon is in many ways the most problematic

1 Notional clock time and recording monitors	2 Combined Audio/ chart time	3 DVM chart readings (millivolts)	4 Transcription of audio record
12h27'00"  Audio & Chart	87'26.0"	850-925	Gregory: 'Absolutely clearly -no possibility of any deception-Matthew's got his sleeves rolled up'. Brown: 'And he's not in contact with the apparatus in any way'. Gregory: 'Seven, seven, yes, 790'. Ellison: '78, 78, can you see that Roger? Do you want to come and have another look?' R. Chapman: 'Oh it's much more interesting watching it....' Ellison: 'We're all watching that space-there's nothing going in the beam. Just waving his hands about over the top'.
	87'30.8"	800-960	
	87'35.6"	750-870	
	87'40.4"	745-950	
	87'45.2"	780-875	
12h27'24"  Audio & Chart	87'50.0"	725-950	Gregory: 'Seven'  Matthew: 'I've been down to six something'. Ellison: 'Yes you did-it's back up now, 89, yes, it's going down again'.  Gregory: '8, yes'.  Ellison: '89, 88, 86, 5, 3, 84, 82, 80'.
	87'54.8"	720-975	
	87'59.6"	975-900	
	88'04.4"	900-975	
	88'09.2"	925-950	
12h27'48"  Audio & Chart	88'14.0"	860-925	Gregory: 'I am watching the space the whole time...'  Ruth West: 'That's seven, six, up to six'. Ellison: 'Yes' Gregory: 'Nothing at all can be seen between the
	88'18.8"	825-925	
	88'23.6"	730-890	
	88'28.4"	740-875	
	88'33.2"	675-820	
12h28'12"  Audio, Video & Chart	88'38.0"	720-945	cell and the receiver'.  Ellison: 'Make sure you get the picture of both things, you can see the meter going down as he goes like this around it. That's what we call PK that does this'.  Gregory: '...751, 752.....'
	88'42.8"	740-900	
	88'47.6"	800-875	
	88'52.4"	770-825	
	88'57.2"	725-775	
12h28'36"  Audio, Video & Chart	89'02.0"	725-800	...absolutely marvellous'.  Ellison: '72, yes, 73'  (Matthew makes sound like 'phew') Gregory: 'Arthur, I would just quite like to go out a bit -could you and Ruth.....'
	89'06.8"	780-850	
	89'11.6"	750-850	
	89'16.4"	715-780	
	89'21.2"	725-860	
12h29'00"	89'26.0"		...Mary Rose would hang us otherwise if we don't.....

TABLE 6.2

*Legend for Table 6.2*

This diagram shows in tabular form a two minute period of the events described from 12:27 to 12:29 on 28 July (chart 2)\*. The chart paper is divided into 1 cm squares which are further subdivided by five vertical fine lines and ten horizontal fine lines, such that time can be read on the horizontal and voltage on the vertical axis (see page 318). This chart was run at a speed of 25mm per minute.

Column 1 provides notional clock time; this was calculated by taking initial clock time written on the chart and measuring along the horizontal axis and converting centimeters into times. The word 'notional' is used because of possible variations in chart speed. There is, however, reasonable coincidence with frequent verbal read-outs on the audio record, and occasional timed events on the chart itself. This column also shows monitoring devices used during any given 24 second interval. A 24 second interval was chosen because 1cm represented one 24 second interval at the chart speed on this occasion.

Column 2 combines audio and chart time. The matching of the audio tape time to chart time was done by listening to the entire audio tape for the period covering the chart time and choosing certain clear words which were to be timed to provide an approximate fit with the chart. The first clear word uttered near the beginning of the audio tape covering the chart time was assigned a time of zero, and each chosen word was timed in relation to this first word using a Casio PQ-7 stopwatch. A suitable point on the chart was selected where a total occultation of the beam was effected by Gregory, accompanied by the words 'I'm putting my hand in the beam now', 'now' coinciding with a reading of zero on the voltmeter. The audio tape was then started, taking the first clear word as  $t=0$ ; the intervening pre-selected words being timed in relation to  $t_0$ , so that an actual time could be established for the word 'now'. It was thus possible to fit dialogue to chart by making a rough equation between Casio time and the chart time as determined by chart speed. Because the chart and audio recorder did not move at precisely identical speeds, only approximate corresponding times are available, but the coincidence between audio and chart record is reasonable. Each 24 second interval is divided into five fine vertical lines, which thus represent time markers for intervals of 4.8 seconds, shown in this column.

Column 3 shows the extreme voltmeter readings recorded on the chart in the interval represented by the chart times in column 2, beginning with the time immediately above left of the mV readings and ending immediately before the next chart time, left below of the mV readings; e.g. between chart time 87'26.0" (inclusive) and 87'30.8" (exclusive) the lowest reading reached was 850, the highest 925. (It will be remembered that the lower figure measures the higher degree of occultation if this is what is being measured.) Millivolts are determined by reference to the fine horizontal lines. Here the precision is limited by, among other things, the thickness of the pen trace and the responsiveness of the chart recorder to voltage fluctuations, which is not necessarily identical with that of the digital voltmeter.

Column 4 is a transcription of the audio record for the two minutes covered by the events described from 12:27 to 12:29. In this column the dialogue is given as corresponding to the times calculated for column 2.

\*see Table 6.3

chart for a number of reasons. Once irregularities appear on the chart recorder, they hardly cease at all for the entire period from 16:07 to 17:50. Also, timing becomes virtually impossible because at one point the tape runs out and there is no clear indication of the time at which it comes on again. There were so many changes in the apparatus that it is difficult to be certain whether a stable baseline was obtained at any time within this period.

There were two visitors in the laboratory, to be called Professor D.G. and Dr. J.B.. Matthew clearly set himself to 'hex' or disrupt the apparatus (psychically), ignoring all pleas from participants to let go and relax.

At approximately 1:40 and 1:43, there are two departures from the baseline on the chart, which is now set at midpoint, to about 900. Although the audio tape was not running at the time that they occur, subsequent dialogue between Ellison and Matthew suggests that they probably coincide with Matthew's entry into the laboratory. The horizontal trace which continues to show a very slight drift was re-set at intervals.

Brian Inglis arrived at about 15:00. At about 15:04 Chapman noted that irregularities were beginning to take place and Matthew said 'If it's started, then we should start'. There was a bout of irregularities starting at approximately 15:07 stopping abruptly about a minute later, after Matthew had accidentally hit the box. The trace is approximately horizontal, apart from a slight drift, until 15:27. During this time Matthew was attempting to influence the beam without any apparent success. He relaxed, and the slight upward drift continued.

Between about 15:27 and 15:29 Chapman returned the chart recorder to the original configuration.

At about 15:32 D.G. arrived, immediately after Brown and Gregory had left in search of some other apparatus. Ellison explained the apparatus to him and was called out of the laboratory by his secretary.

While Inglis and D.G. were discussing other matters, Matthew drew their attention to the fact that 'it's going down again' and at about 16:07, there is a burst of irregular activity for about half a minute, peaking at about 800mV. At about 16:08, the trace rises again and does not really settle down until 17:50. No discrete bouts can be discerned, nor can any relationship be claimed between psychological or behavioural variables and the vagaries of the chart trace, without the most specious pleading. On three, possibly four occasions, the trace passes the 500mV mark, corresponding to an occultation of just over 50%.

Brown and Gregory returned about 16:13. Just before 16:20 Chapman switched off and on again the 5 volt supply to the lamp to clear any possible switch contact fault. The trace continues its irregular course. At about 16:22½, Chapman changed from the rectified mains (25 volts)

to a battery (25v) supply to regulator. The trace continues irregular. Just before 16:24, there is another change of battery, the trace is normal for about 22 seconds and then becomes jagged once more. There is yet another change just after 16:31 when it was decided to change back to the mains but with an alternative voltage regulator. Chapman challenged Matthew 'now muck that one up', and the trace remains stable for three minutes. However, after some adjustments by Chapman, so marked on the chart, and while Matthew is trying to rise to the challenge, the trace rises once more, to resume its jagged irregularities.

At about 16:55 J.B. arrived. All the while the trace continued its irregular course. Matthew resisted all attempts to get him to calm down and (naturally enough) J.B. expressed his assumption that it was simply a question of discovering the fault in the equipment. He suggested CO<sub>2</sub>, carbon dioxide as a possible cause, and the trace reflects the introduction of CO<sub>2</sub> into the beam by means of a fire extinguisher by one of the participants. The resulting occultation is far more violent than anything observed previously or subsequently, and it is hard to see what such gross interference with the apparatus could have shown, since J.B. was presumably referring to exhalation of breath. J.B.'s alternative suggestion for explaining the aberrant behaviour of the apparatus was variations in the power supply; he also suggested electrostatics as a possible cause. Before leaving he recommended for future reference that the amplifier be rebuilt and that low noise cable be used, as well as batteries sealed in metal boxes without leads or switch contacts.

At about 17:50 Chapman had completed re-setting with a new power supply and the trace returns to normal, despite several attempts by Matthew to influence the trace. Matthew pointed out that he was now exhausted.

At about 18:09 the power supply was changed back to the original mains and the trace continues perfectly steady until 18:20 when the chart ends.

There is some dialogue at the end of the audio tape for this period in which Ellison considers J.B.'s suspicions of the power supply. After further discussions, the power supply seems ruled out as a source of trouble and the only remaining possibility described by Ellison is some loose contact that cured itself.

### *Tuesday 1 August*

The infra-red equipment was set up all day from 10:25 to 17:16. Matthew was at Birkbeck College and only Chapman and Sullivan were in the laboratory. The trace is horizontal throughout, apart from a very slight, flat upward deflection to 995mV during the warming up period, labelled 'shadow'. No sign of voltage fluctuations or loose contacts were registered.

*Wednesday 2 August*

The IR equipment was set again all day and there is a chart from 10:40 to 16:53. The trace is virtually horizontal throughout; there are however, some very small occasional irregularities which it is impossible to time with any accuracy since chart speed, which is not explicitly recorded, was varied. In addition, a fault had developed in the timing mechanism of the chart recorder as reflected both in the pen trace indicating minutes, and also in recorded speech.

Mr. I Bloomfield visited the laboratory in the morning about 11:30 to 12:00 and Matthew attempted to influence the beam for him. There are very slight irregularities in the trace not noticed at the time, but there are similar irregularities after everybody had left the lab. In any case, this chart shows a tendency for small blips to occur lasting a minute immediately following switchings and re-settings. The overall impression however, of the trace for the entire day, is flat and horizontal.

In the afternoon Matthew went to St. Bartholomew's Hospital accompanied by Ellison, Gregory, Chapman and Brown to have his electroencephalogram recorded by Miss Marion Smith. Matthew made his distaste for the entire proceedings more than plain. In an attempt to distract him, he was given four of the poetry envelopes. Gregory wrote notes, using her watch (which was between one and two minutes fast as subsequently checked) to give approximate timings. At approximately 15:18 she suggested that Matthew might try influencing the infra-red in the laboratory from a distance of approximately three-quarters of a mile. Again, at about 15:20½ she asked Matthew to have a try at influencing the IR, using the words 'come on Matthew, that'll be a world first'; and he made the attempt. About a minute later a telephone call was made to Sullivan, asking him to check that the equipment was running and to leave the laboratory, locking it behind him.

From 13:10 the trace is absolutely flat, until a few perturbations appear at times subsequently calculated by Chapman (audio tape) as occurring at 15:16 and 15:22½ respectively. The first of these irregularities is a very slight but noticeable protracted wobble, lasting about a minute. The second is a very slight step in the trace, such as might occur when the apparatus is re-set. In our view these irregularities on balance may not be interpreted as being significant.

The trace thereafter continues entirely flat until approximately 16:53.

#### DISCUSSION OF FINDINGS

It should be remembered in discussing findings that these experiments were part of a block investigation, and were designed in the first instance, to see whether the results obtained with Rudi could be replicated with



another Subject in whose case physical phenomena had been claimed in his earlier years. On the face of it similar tracings were obtained. However, the interpretation of these traces is subject to a number of qualifications to be discussed.

### Source material

The main records available were the chart tracings, and the audio tapes available for almost the entire period of investigation. Although video equipment was used from time to time and certainly corroborates the audio record, e.g. it shows Roger Chapman trying his hand at influencing the equipment, the recordings are too poor in quality and too few in quantity for more to be claimed for them than occasional illustrations and corroboration that no one was near the instrument while strong deflections were occurring.

Table 6.3 shows the following chart records which are available:

Chart	Date	Approximate length of chart
1	Wednesday, 26 July	1hr. 7min.
2	Friday, 28 July	5hrs. 7min.
3	Monday, 31 July (morning)	2hrs. 22min.
4	Monday, 31 July (afternoon)	5hrs. 10min.
5	Tuesday, 1 August	6hrs. 51min.
6	Wednesday, 2 August	6hrs. 13min.
7	30-31 Oct., 1-3 Nov.	33hrs. 58min.
8	16 November	4hrs. 30min.
9	3 January 1979	1hr.
10	12-16 February 1979	

TABLE 6.3

Irregularities of the trace are unambiguously recorded only on three days on charts 1, 2 and 4. It would be too expensive to reproduce these here.

It is however possible to present rough arithmetical profiles of some aspects of charts 1, 2 and 4, (see Tables 6.4, 6.5 and 6.6).

### Explanations of Tables 6.4, 6.5 and 6.6

For reasons which will become apparent in the discussion these are unsuitable for assessment of statistical significance. They may, however, be useful for descriptive purposes, and give an idea of the durations of time during which the trace did, and when it did not, depart from its normal baseline. It also provides some idea of the extent of irregularity, that is, degree of occultation (or lowering of voltage).

The first row down (a) in each case indicates the lengths of time in minutes and decimal fractions of a minute for which the trace remains at the baseline (i.e. no occultation).

The second row down (b) indicates the lengths of time in minutes and decimal fractions of a minute during which significant occultation is observed (as indicated by a decrease in voltage below 975 mV). These two lines must be read in conjunction as a vertical zig-zag sequence, and represent successive times, e.g. chart 1, beginning: 1.64 (below 975mV), 0.12 (above 975mV), 0.02 (below 975mV), 0.04 (above 975mV) etc. Asterisks (\*) indicates that normal causes for deflection are known for the amount of time indicated. These asterisks occur in the (a) column if such normal deflection occurred during periods of quiescence, e.g. test moving of apparatus; they are put in the (b) column when normally caused occultations were made during bouts, e.g. to test that the IR was working properly. Occasional comments without asterisks describe salient events.

The third row down (c) indicates maxima of occultation (or minimal voltage measured in millivolts) reached during any particular period immediately to the left of the voltage figure. Thus, on chart 1, during the first 1.64 minutes the lowest voltage reached was 795mV; during the next brief departure from the baseline for 0.02 minutes the greatest departure was 970mV and so on.

In all cases we have concentrated on the portion of the chart where disturbances below 975mV occur, which we have called the 'bout period', defined as beginning at the point at which the trace leaves the baseline for the first time, and ending at the point when irregularities cease. Departure from the baseline to an extent greater than 975mV was selected as amounting to an 'appreciable irregularity'; as the normal fluctuation (see apparatus section) of the DVM was approximately 980-1020mV and the first fine line parallel to the base on the chart record indicates a level of 975mV. In the case of the first two charts, the total bout period is described arithmetically. Chart 4, as depicted in Table 6.6, is only described to that point on the chart after which such measurement has become impossible, because for the chart speed used (25mm per minute) the trace returns to zero so frequently that analysis is impossible without gross distortion. Also, so many tests were subsequently carried out, notably flooding the instrument with CO<sub>2</sub> by means of a fire extinguisher, that any analysis would be futile.

Throughout chart 1 there were prolonged periods of time, lasting several minutes, when the trace returns to base. Towards the end of chart 2 there is a decrease in the length of these periods of quiescence. In chart 4 this breakdown of quiescent periods occurs quite early and it can be seen clearly from the figures that there is rapid oscillation between baseline and departure from baseline.

It can also be seen from the three charts that the maximal occultation (lowering of voltage) reached increased somewhat throughout the three days, being: chart 1: 650 (once); chart 2: 630 (once); and chart 4: 450 (twice).

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*London Experiments with Matthew Manning*

a Chronological duration in minutes at 1000 ± 25	b Chronological duration in minutes below 975 mV	c Minimal mV (Maximal occultation)
	1.64 (Figure 3)	795
0.12		
	0.02	970
0.04		
	0.06	965
0.04		
	0.02	970
0.02		
	1.46	790
0.02		
	0.44	845
1.84		
	0.78	850
0.88		
	1.16	780
0.88		
	0.38	960
0.06		
	0.12	950
0.06		
	0.04	945
13.20 (*4.67 adjusting equipment)		
	2.14	695
7.38 (Ellison and Gregory try)		
	0.18	770
0.44		
	0.38	910
0.02		
	0.14	925
0.02		
	0.28	900
0.02		
	1.6	720
0.02		
	1.34	650
8.1 (Matthew tries to raise DVM)		
	1.44	810
TOTAL = 33.16	TOTAL = 13.62	

TOTAL BOUT PERIOD = 46.78

TABLE 6.4 Chart 1

a	b	c
Chronological duration in minutes at 1000 ± 25	Chronological duration in minutes below 975mV	Minimal mV (Maximal occultation)
	1.76 (Matthew not in lab; Figure 4)	720
11.60		
	0.8	965
0.12		
	3.48	640
0.04		
	0.4	915
6.48 (*.2 test occultation)		
	0.04	965
0.12		
	0.04	960
2.32		
	1.8 (Table 6.2 & Fig 5)	720
0.16 (Table 6.2 & Fig. 5)		
	1.6 (Table 6.2 & Fig 5)	630
0.04 (Table 6.2 & Fig. 5)		
	0.36	765
0.72		
	0.12	960
12.92 (*2.68 test occultation)		
	0.28	965
0.12		
	0.32	685
0.68		
	0.4	750
0.4		
	0.16	760
0.12		
	1.28 Matthew at 3 metre distance	750
0.8		
	0.12	960
0.16		
	1.56	790
0.04		
	0.12	925
.08		
	4.36 (*1.28 test) Matthew out of lab.	675
.08 Matthew out of lab.		
	2.08 Matthew out of lab.	650
TOTAL = 37.72	TOTAL = 20.08	
TOTAL BOUT PERIOD = 57.80		

TABLE 6.5 Chart 2

a	b	c	a	b	c
Chronological Duration in Minutes at 1000 ± 25	Chronological Duration in Minutes below 975mV	Minimal mV (Maximal occultation)	con't	con't	con't
	7.8	740	0.004		
0.04				0.06	875
	1.84	450	0.004		
5.32 (*.04)				0.02	875
	11.76	860	0.008		
18.4 (*1.04)				0.05	875
	0.16	945	0.01		
22.6 (*2.2)				0.02	865
	0.64	835	0.19		
0.6				0.008	915
	0.88	750	0.008		
0.04				0.08	730
	0.44	660	0.02		
0.04				0.05	820
	1.48	710	0.03		
0.04				0.08	800
	0.48	660	0.02		
0.04				0.02	700
	0.2	700	0.02		
0.04				0.02	850
	0.52	660	0.01		
0.04				0.22	750
	1.8	635	0.02		
0.16				0.008	965
	1.08	650	0.02		
0.04				0.01	965
Alt. ch. spd. .12/1.61(=1.73)			0.02		
0.02				0.1	965
	2.54(*.06)	560	0.07		
.12(*.04)				0.48	770
	0.06	800	0.01		
.004				0.02	925
	2.79(*.05)	600	0.008		
0.01			Alt. ch. spd. .79/4(=.83)		750
	1.3(*0.1)	560			
0.36			TOTAL =	TOTAL =	
	0.22	735	48.466	39.806	
0.08			TOTAL BOUT PERIOD		
	0.01	930	COVERED = 88.272		

TABLE 6.6 Chart 4

*Collation of sources*

Transcriptions were made of all available audio recordings irrespective of whether there was a chart available for that day or not. Although some rough notes were available, the final transcription (118,000 words) used for interpretative purposes was the responsibility of Kathy Wilson, who was familiar with the voices of the participants but had not been a member of the original team, to avoid certain artefacts of suggestion which had been shown to be invidious. In the case of ambiguity, she omitted the words or groups of words in question. Because of the informality of the arrangements, a verbatim record of the proceedings is clearly desirable if not essential, and there is good reason to suppose recording did not unduly inhibit participants from expressing themselves. From the transcript it is possible to reconstruct, with reasonable accuracy, what was done and when. Since the infra-red experiments, although roughly timetabled, were Subject initiated, it was clearly important to know whether irregularities are found only when the Subject attempted to influence the apparatus, or whether attempts were similarly made on days when the chart record indicates no irregularity. We also wish to stress that the auditory record is vital, since it quite often showed up serious discrepancies between the recollection of some participants and what in fact happened.

For those charts where irregularities occur, every effort was made to obtain as close a correspondence as is possible between chart and audio record. Indeed, this was one of the major points in having an audio record. For example, when an attempt was made to see if Matthew could raise rather than lower the voltage this could not be registered on the chart because of the way the baseline was set; but readings from the digital voltmeter were deliberately and collectively dictated into the microphone for the purpose of subsequent correlating with the chart record. The method described in the legend to Table 6.2 was applied to charts 1, 2 and 4.

Precision of coincidence in time is limited for the following reasons: only relatively approximate times were entered in the chart itself and frequently not at the very beginning of irregularities; audio tape is elastic, and repeated playing reached different numbers on the tape counter on different occasions; on several of the charts the time marker is defective, particularly charts 4 and 6; on two of the days the audio tape ran out for short durations, the precise length of which is not known; in chart 1, not only does the tape run out at one point, but there is no definite time marker; in chart 4, the tape runs out after the time marker and only very approximate timing is possible when the tape resumes. In addition to unavoidable changing-over of tapes, there were also some minor accidents, such as accidentally pressing of a

switch turning off the motor of the chart recorder.

### Possible interpretations

It will clearly be seen that in view of this critique of the instrumental arrangements available, any attempt at precise co-ordination of chart and audio record would be spurious and misleading. There is, however, one observation that can be made, and which could only be made because so complete a transcript is available. This observation is to the effect that over and over again Matthew noticed an instability in a downward direction on the part of the digital voltmeter and only *then* decided to start to try and influence the *intra-red*. Here the audio record is supported by the long gentle upward slope preceding jagged irregularities (see Figure 7) which clearly reflects a gradual lowering of the voltage in the first instance. Fixing a beginning for 'bout periods' is therefore to some extent arbitrary. As has been mentioned, on one occasion he actually says 'It's started, so we should start'.

We propose to call this the 'bandwagon effect'. This term is not intended to pre-empt interpretations of either normality or paranormality, but it certainly affects the interpretation of the phenomenon studied. What it unfortunately invalidates is the negative success of other participants (such as Ellison, Gregory and Roger Chapman) to influence the equipment, whereas Matthew ostensibly did influence it. At the time, and even in retrospect, it seemed as though he *could*, and we *could not*, affect the instrument; however, scrutiny of the audio record in conjunction with the pen tracings shows that there is ample auditory evidence that Matthew tended to start from a base of instability, whereas the experimenters tried their luck when the instrument was stable.

It is however, certain that imitating Matthew's movements to the best of our ability did not create any disturbance in the record; therefore it is very unlikely that either shadows, breathing (CO<sub>2</sub>) or electrostatics were responsible for the irregularities. It must also be pointed out that Matthew frequently exerted himself from quite a distance. On one occasion (chart 2) he was not even in the laboratory (so far as we can tell from the audio record) when deflections were first noticed (see page 323 and Figure 4) and on the same day they persisted when he had definitely left the laboratory.

While there is still a 'bout' structure, i.e. grouping in time (chart 1 and first part of chart 2) of deflections, whereas the initial slope is gentle, the end tends to be most abrupt (Figure 7). Timing is not sufficiently precise to allow us to distinguish between whether Matthew relaxed *before* or *after* the cessation of a bout of irregular deflections. He may well have seen that the digital voltmeter reverted to flickering around 1000 and relaxed his considerable muscular effort in consequence, rather than that he stopped exerting himself whereupon irregularities

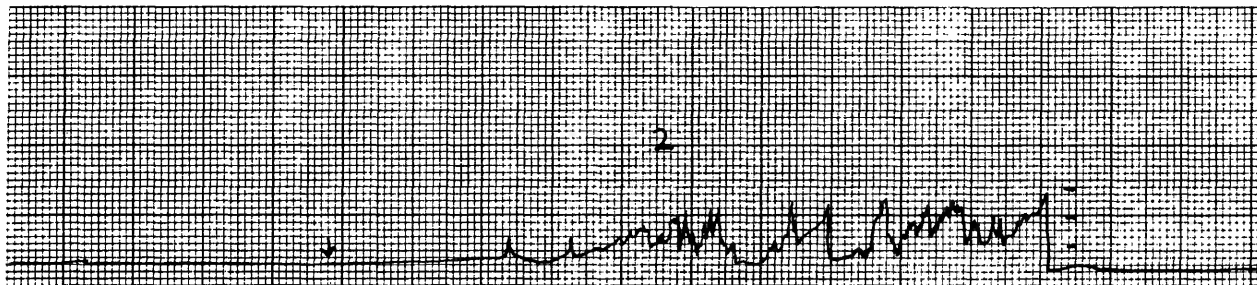


Figure 7. Irregular deflections, 26 July 1978, chart 1. Chart speed 50mm per minute. Approximate period covered by entire illustration is 3¼ minutes. Note gentle slope suggesting 'bandwagon effect' and abrupt drop at end.  
*Figure reduced from copy of the original for purposes of illustration*



Figure 8. Part of Osty's Figure no. 35, referring to sitting of 26 May 1931. Trace ostensibly similar to tracings obtained with Matthew; note however absence of gradual initial slope.



ceased. He might easily have been unaware of such 'bandwagoning'. It has already been described how Matthew was unable or unwilling to relax efforts while irregularities were in progress, which certainly suggests that he went along with an irregularity rather than that he in some sense voluntarily caused it.

It must be emphasised that we have no reason to doubt Matthew's complete honesty; he was by no means the only one present to confront possible bandwagoning and psychic causation. Moreover, in order to simulate the trace normally it was found necessary either to wiggle one's finger in the beam in a very conspicuous manner, or to use implements such as steaming kettles. Such activities were entirely ruled out by the conditions of observation during the experiments, by the audio records, and the video records even allowing for their poor quality, and these possibilities of imitating the trace normally were only discovered later as a result of a good deal of experimenting with the equipment. In any case, Matthew was virtually never unobserved, nor did he ever make the slightest attempt to evade observation, and while irregularities were on, one person was always delegated (usually Ruth West) to watch the space between source and cell.

One of the problems for the interpretation of this set of chart records and conceivably future ones also is that, to begin with, irregular deflections cluster in groups separated by regular long stretches of quiescence. However, from the last part of the second and throughout the fourth chart, this clustering in time is lost and ever more continuous irregularities are recorded, increasing only slightly in amplitude or degree of occultation. However, the total proportion of activity over quiescence increases, as may be seen from Tables 6.4, 6.5 and 6.6. It might possibly be that an epidemiological or 'seizure' model might be found to be useful interpreting data of this sort.

In view of clear evidence for the bandwagon effect, the absence of clearly defined trial and control periods and the problems of precise timing, any detailed correspondence between possible psychological states as expressed in the audio record and irregularities of the chart trace is out of the question. In any case, the cluster or bout structure of the chart records disappears, and it is clearly impermissible to pick out what seem subjectively meaningful episodes from the transcript, divide the instrumental record accordingly, and argue back and forth towards some *ad hoc* psychological concordance, let alone base statistical calculations on such manipulations!

It is however appropriate to discuss whether the irregularities could have been due to normal causes and if so which. If such causes are not deemed probable sources of the irregularities observed, it should be considered what might be possible paranormal modes of psychological influence.

*Possible normal causes of irregularities*

As has been mentioned, a good deal of trial and error was necessary to find out how to simulate the trace normally; and such methods would have been impossible under the conditions of the experiment. Neither vibrations, shaking of the apparatus, introducing solid particles (such as cleaning powder and cigarette ash) nor dangling threads or strips of tissue into the intervening space produced any effect remotely resembling the curves shown. Deflections of comparable size though of different shape were obtained by breathing into the beam, and these were due to misting over of the lens. On several days after the experiments (31 October, 1 November, 2 November and 3 November 1978) D. Chapman ran tests and found the equipment stable and responsive to test occultations in the normal manner. Despite the fact that the lab was in normal use (on one occasion by eight students) and normal lighting and equipment were running, no irregularities were noted.

Fluctuations in electricity supply were among the favourite 'normal' explanations. (We have not counted the drifts that might be attributable to running down of batteries as paranormal, since Chapman was not able to guarantee that power packs had been perfectly charged when he received them. However, some sort of paranormal drain on the power supply on the lines of the Rosenheim case (Bender 1974) cannot in principle be ruled out.) Although ideally there should be constant monitoring of mains or better still all power supplied by fully charged batteries, the mains seems an improbable explanation for a number of reasons: irregularities also occurred while the power was supplied by batteries; the supply was a stabilised mains in a department of electronic engineering, and furthermore, there were long periods of perfect stability before and during the investigation, and an indefinite period afterwards including the brief and unsuccessful February trials.

Instability of the bulb would affect the chart record and D. Chapman experimentally produced such fluctuations by loosening the bulb. However, the trace looks different from those under investigation, and it is difficult to see how quiescence would be spontaneously restored once the bulb had become unstable.

The possibility of a loose contact somewhere in the circuit is more difficult to exclude, but is not easy to reconcile with the long periods of stability before, during and after test periods, and we are assured by Ellison that from his point of view as an electronics engineer the effects observed do not resemble what he would expect to see if there had been a loose contact somewhere. Moreover, the seizure-like character and its abrupt cessation at the end of chart 4 would also seem to argue against such an interpretation. Quiescence of trace was only restored after switching back to the original power supply.

In addition to possible electrical faults, a possible normal cause to be considered is carbon dioxide. (It can hardly be claimed that discharging a fire extinguisher, without warning, into the beam constitutes some adequate test; obviously if the IR was working, and if the extinguisher contained CO<sub>2</sub>, it was bound to work.) Subsequent attempts at deeply breathing into the beam, taking care to avoid misting over the lens, produced no noticeable deflections; it therefore seems exceedingly unlikely that CO<sub>2</sub> from participants' breathing affected the IR. CO<sub>2</sub> is also an unlikely explanation of the irregularities found for a number of other reasons, principally the stability of the trace before, and especially after bouts of irregularity, the failure of others simulating Matthew's activities to reproduce his traces, the quiescence of the trace when all participants were in the lab and active, and occasions while students were in the lab all day, as well as during the entire February experiments.

Mist on the bulb resulting from direct breathing on it must be ruled out: not only is the trace quite different in shape from what we observed, but for one thing, frequently no one was near the apparatus when irregularities happened, and attempts by others to simulate Matthew's movements during periods of trace quiescence produced nothing whatever.

Table 6.7 summarises possible normal causes of malfunction other than complex experimenter fraud. Perhaps the best defence against this undisprovable supposition might be that, had we gone to this enormous amount of trouble, we might be credited with producing something rather more impressive!

### *Paranormal hypotheses*

Although as will be plain, no categorical claim to paranormal activity can be made, a sufficiently good *prima facie* case has been established to make it worth while discussing the hypothesis of paranormality. To this day Ellison and Roger Chapman express the view that they have not seen apparatus behaving like this before and can find no normal engineering explanation. David Chapman is more guarded. While, like the rest of us he would have preferred more time with the equipment beforehand, he has given no alternative explanation other than possibly a loosening of the bulb, which we cannot accept as plausible.

On the supposition that what is recorded does not reflect one of the artefacts mentioned or some other not envisaged, the question still arises whether it was the infra-red beam, the electronics of the apparatus, the power supply, or the monitoring instruments that were affected, normally or paranormally.

The only one of these that can be, if not ruled out, at least rendered extremely unlikely, is that it was one or perhaps all of the monitoring instruments that were affected since all were wired in parallel and,

Possible normal causes of disturbance suggested	Considerations against treating factors in left hand column as normal explanations
Mains fluctuations	Stabilised mains; persistence of irregularity after change to battery; days without disturbance before, during and since; escalation of disturbances.
CO <sub>2</sub> on infra-red	Days without disturbance with numerous persons present before, during and since; escalation and total cessation of disturbances; dissimilarity of trace; onset and incidence when no participant near IR. Failure to replicate by breathing without misting.
Contact faults in circuit before infra-red apparatus	Days without disturbance before, during and since; sudden and permanent cessation of disturbance.
Contact faults in circuit after infra-red apparatus	Days without disturbance before, during and since; sudden and permanent cessation of disturbance; unilateral direction of disturbance as per DVM readings.
Infra-red bulb	No evidence that this was loose at the time; different shape of trace; days without disturbance before, during and since.
Mechanical intrusion into infra-red beam	Carefully watched and monitored, including video; totally different trace, for most such intrusions.
Mechanical vibrations	Carefully watched and monitored; totally different trace.
Faulty chart recorder	Correspondence with other monitors; stability before, during and since; only suggestion of malfunction if timing taken into account.
Faulty DVM	Stability before, during and since; correspondence with other monitors; could have no causal efficacy on chart record.

TABLE 6.7

Considerations against treating various normal causes as explanations of disturbances observed.

so far as may be ascertained, all registered similar irregular deflections. The voltmeters could not have causally affected the chart recorder. One would have to hypothesise an identical tripartite paranormal force acting simultaneously on all three. Nothing in the proceedings suggests such fine and planned control.

Although on the face of it occultations of the infra-red were obtained and the infra-red apparatus responded appropriately whenever tested, it cannot, in the absence of monitors and switching arrangements in different parts of the circuit, be claimed with complete certainty that it was in fact some interference with the beam that was being measured.

It was thought at first that action on the infra-red beam was virtually ruled out by the fact that Matthew had apparently been able on at least one occasion, and on demand, to *raise* the voltage above the 1000mV limit. It will be recalled that, since this trial could not be reflected on the chart, participants dictated the DVM readings into the tape recorder. However, whereas the noise level of the instrument was ca  $\pm 20$ mV the readings as recorded never exceeded 1032. Since this constitutes less than 10% of characteristic disturbances in the opposite direction (i.e. lowering of the voltage) obtained during periods of disturbance (see columns of minima in Tables 6.4, 6.5 and 6.6), this small upward fluctuation cannot be regarded as significant.

It should also be stressed that the DVM is not on record on a single occasion as having fluctuated spontaneously in an upward direction to any substantial extent (i.e. one approaching the values corresponding to downward irregularities) such as 1300 or 1500mV. This would, without any doubt, have been spotted by participants. The unilaterally downwards direction of significant irregularity tends to suggest interference with the infra-red. A fault in the DVM could not have affected the analogue voltmeter, which similarly was never observed to exceed 1000.

Normal electrical faults in the circuit *after* the source would have been expected to fluctuate equally in both directions; this should have shown up in DVM readings, which it did not. Hence on a paranormal interpretation, if the *modus operandi* was not on the IR itself, it would have to have been somehow psychologically guided in a downward direction in accordance with Matthew's subjective expectations and those of most participants, especially Gregory's. It would have conflicted with those of Ellison, who was fairly firmly convinced that some paranormal electrical effect rather than any interference with the infra-red was involved.

Thus, if the effects obtained were paranormal (and it must be remembered that 'paranormal' is a residual category), three possible *loci* of paranormal influence might be isolated: power supply, electronic circuit or infra-red beam. It is of course not necessary to suppose that interference between source and cell would necessarily be due to an

'interloping substance' as hypothesised by Osty. As stated at the outset of this paper, the term occultation was used neutrally simply to indicate that ostensibly less light was recorded as falling on the cell than 1000mV. Mr. Roger Chapman has pointed out that, for example, the beam might also have been deflected away from the detector. None of these rival interpretations can at this stage be conclusively excluded. However, the downwards direction favours interference with the infra-red. For the time being the hypothesis of some paranormal intervention between source and cell, suggested by Osty in the case of Rudi, is also available here. However, in the present case, the hypothesis is less strongly supported because, unlike Osty, we did not have available multiple beams which the Subject affected on demand, and on the face of it Matthew at no time achieved anything like the control demonstrated by 'Olga', Rudi's secondary personality.

On the other hand, it must be conceded that in Osty, Hope and Rayleigh's day an audio record of the completeness of ours was simply not technically available; therefore, any 'bandwagon effect' would almost certainly have escaped detection. It seems important to consider the paranormal hypothesis that the psychic (Rudi) was operating on slight fluctuations in noise levels of the instrument, which would reach him and others by normal sensory routes such as the slightly louder scratching of the pen of a recorder. On the other hand, the gentle upward trace at onset of irregularities observed with Matthew is missing in Rudi's case (Figure 8), which would seem to counterindicate 'bandwagoning'. There can also be no doubt of the numerous experimenter initiated trials in Rudi's case, particularly with Osty. It is of course quite possible that the same phenomenon is simply not captured in the in the two cases.

#### *Some psychological comments*

At no time was Matthew in trance, nor did he display any altered state of consciousness beyond that which anybody displays who is concentrating on a task. This is in sharp contrast with Rudi, who was always in trance when phenomena were reported. It may or may not be psychologically significant that Matthew increasingly displayed aggressivity towards the instrument. It might well therefore be that, on a paranormal interpretation, the irregularities could be described as due to some temporary 'haunting' of the apparatus or power supply, rather than to the extrusion of some sort of psychic matter. On the other hand, the systematic downward nature of the voltage tends to support this latter possibility.

Taken altogether, the 'deviant charts', charts 1, 2 and 4 suggest activity periodic at first and then more and more disorganised and extensive. The overall impression is that of having created and captured a limited

poltergeist outbreak in a laboratory. (It will be remembered that Matthew's early poltergeist phenomena started as limited sporadic episodes and ended as a chaotic shambles.)

Matthew frequently or even usually thought or felt he had control over the disturbances but the record does not support this, quite apart from the 'bandwagoning' effect. Occasionally he tried and obtained nothing; he could not stop irregularities once they were under way; and he predicted irregularity when this did not occur.

On a paranormal interpretation it looks more as though in some way he (or the group) created instability in the system under investigation, at whatever level, and when this instability as fed back by the DVM reached a certain level, Matthew either enhanced this, or 'jumped on the bandwagon' as the irregularity increased in any case. Voluntary conscious action must be ruled out: perhaps some autonomic analogue reaching a crescendo over a period of days, or some isolated fluctuations or firings eventually coalescing into a quasi-seizure would be better models.

It is by no means wholly clear exactly who, on a paranormal interpretation, was responsible for the effects. Certainly all participants and Matthew thought of him as responsible for irregularities and indeed if he in some way introduced noise into the physical system which escalated over a period and then vanished, this would be an appropriate supposition. However, neither Matthew himself nor any other participant had to be present in the laboratory during irregularities: the presence of neither Barrington, Brown, Ellison, Gregory, Inglis nor R. West was essential. David Chapman and Sullivan were in the laboratory most of the time in any case. Chapman was the firmest 'goat' in the team, and Sullivan was only marginally involved and was often busy with other matters. Malfunction continued at peak intensity in the presence of J.B. who was not only a total skeptic but openly contemptuous and hostile. The 'Grattan-Guinness peak' presents another enigma, occurring as it does in Matthew's absence and while all those present are quietly otherwise occupied. On a paranormal interpretation either Matthew exerted PK on the instrument which became partially independent of him, or else the effect was a group phenomenon, with Matthew as psychological focus of the group.

It seems fitting to conclude this discussion of findings by referring to the dual aspect that we as experimenters are likely to present to our Subjects at the actual time of experimenting: we appear to be and indeed are enthusiastic, encouraging, positive, extravert and generally 'sheepish'; when - and if - we finally publish we are critical, skeptical, dispassionate, doubting goats. From the point of view of the Subject this may simply look like bad faith; yet we know it is not. This unavoidable duality needs to be faced much more explicitly in parapsych-

ological experimentation, and allowance be made in briefing and debriefing of subjects.

### *Concluding observations*

The present investigation, although exploratory in nature, was nevertheless a fairly ambitious and time-consuming enterprise, and was moreover, explicitly a mixture between a seance and an experimental set-up. The standard antiseptic type of write-up is therefore not appropriate, and readers might feel that it reads more like a sad chronicle of errors and deficiencies. However, we believe that only by making these shortcomings explicit is it possible to assess the evidence one way or another and, more importantly, to design future experiments in a field which has remained controversial, despite a hundred years of dedicated experimentation. However, we are of the opinion that the *prima facie* evidence is extremely good and that in an established field resources, human and financial, would automatically be forthcoming to clarify the questions raised. It can be seen that virtually all the ambiguities we have listed could easily be remedied by an injection of resources relatively small in comparison with the huge sums habitually lavished on scientific projects.

### FUTURE EXPERIMENTATION

It seems possible in the light of the Manning investigation to suggest how to set about designing experiments concerning the nature of these ostensible infra-red phenomena. In order to obtain adequate normal control baselines it is in our view essential to instal, test and run equipment for substantial periods of time before experiments are conducted at all. It is often claimed, especially in the case of spontaneous phenomena, that apparatus is liable to malfunction in the presence of psychics; as D. Chapman observed, seeing that instruments are usually (under such circumstances) used for the first time under strange conditions, it would be a miracle if they did *not* malfunction. This is of course a problem for all investigation of spontaneous cases and investigating physical phenomena in the laboratory almost unavoidably presents some of the difficulties as those encountered in spontaneous cases. At any rate, the instrumental problems can be overcome by an insistence that satisfactory apparatus must be installed and working prior to the introduction of subjects, and continuous records kept of normal functioning.

Automatic monitors and control switches should be introduced at each stage of the circuit, so that it is possible to ascertain at which point in the circuit the paranormal influence, if any, is exerted. This would make it relatively easy to establish whether the phenomena is



one involving power supply, electronic circuitry or the infra-red beam itself. Monitoring devices should be digital and automatically recorded on multi-track audio tape for easy computer analysis, and if desired computer graphic reproduction and print-out.

Ideally from a methodological point of view, one should of course have a protocol incorporating pre-determined random trial and control periods; it might be possible by suitable psychological devices such as incorporating periods of activity and non-activity in a game to make a psychic Subject willing and/or able to comply with such experimental conditions. However, we are by no means certain that this is psychologically at all promising, especially if the 'bandwagon effect' plays a substantial role, or if the psychic creates some sort of noise in a physical system partially independent of him/herself. If, on the paranormal hypothesis, the Subject somehow either capitalises on normal fluctuations or initiates minor paranormal fluctuations, when he is in some (unknown) state fit to do so, the Subject is far more likely going to feel the need to initiate periods of activity him or herself. For all we know the Subject may have subtly to combine some inner physiological or psychological fluctuations with some instrumental fluctuation in order so to capitalise in a paranormal manner. An alternative to trial and control periods would be the provision of independent, duplicate apparatus, such that one is the experimental, the other the control apparatus. If an effect is captured on one of the instruments it would then be possible not only to establish paranormality beyond reasonable doubt by suitable manipulation, but to begin on the investigation of some of the basic psychological variables which has barely started.

Once one has installed satisfactory duplicate equipment on a relatively permanent basis, attention can be devoted to the selection and preparation of suitable subjects. One possible group is that of former poltergeist children or alleged physical mediums; another would be children or others who had given indications of being 'metal benders'. It might also be useful to attempt to test individuals who felt themselves to be notably destructive, unsuccessful or clumsy with machinery. Ideally of course one would attempt to train individuals as PK subjects, although for the time being this remains a somewhat futuristic project. Intermediate between the naturally occurring special Subject and the trained one might be the joint efforts of a sitter group. An imaginary 'Philip' on the lines of Owen (1976) might well influence apparatus installed on the lines that we have advocated; we are convinced however that, for the physical effectiveness of such an artefactual person to carry scientific conviction, it would have to influence stable, reliable equipment.

During all attempts at experimentation, conversation should be monitored by means of audio recording, if only to provide clues for the interpretation of unexpected effects, and video apparatus of adequate

quality is desirable for security reasons. Audio, video and other monitoring channels should all be linked to a common time channel so that events can subsequently be synchronised automatically with one another and with independent clock time.

Over a hundred years of experimenting with the physical phenomena suggests that to *demonstrate* them *ad hoc* to the satisfaction of many observers, even competent and critical ones, is not too difficult, but that to *capture* them in a permanently satisfactory manner for those not personally involved as participants, observers or experimenters is quite another matter, and has not so far been achieved. We believe that the instrumental and technical means for such capturing of these phenomena have only fairly recently become available, and that we have suggested the conditions necessary for such capture. In any other context such requirements would appear essential, indeed minimal. If they were met the nature of some of the most puzzling and important phenomena of psychical research could at long last be elucidated.

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The illustrations (Figure 3-7) in this paper are artist's copies of the original charts; the original background being too faint to reproduce photographically. The graph paper used in the background of the copies is slightly different in that the fine lines are at millimeter square intervals both horizontally and vertically whereas on the original chart paper the vertical lines occur at 2mm intervals and the horizontal lines at 1mm intervals. The illustrations are also reduced so as to fit on to the pages of the *Proceedings*. Legends and descriptions in the paper refer to the original charts, photocopies of which are deposited in the SPR.

EXPERIMENTS ON POSSIBLE PSYCHIC EFFECTS  
ON THE GROWTH RATE OF MOULDS

J.B. HASTED

The mould *Mucor hiemalis* was chosen for growth rate experiments because of its many convenient features (Ingold (1973)). Mutant strains occur only very infrequently. A typical strain was kindly provided by Dr Brian Plunkett of Birkbeck College, and was checked for purity both during and after the twenty week period of experimentation. It was grown on nutrient jelly in 8.5 cm diameter Petri dishes, each of which was inoculated in the centre by placing on it a 7 mm diameter disc of *Mucor* and jelly aseptically cut by means of a cork-borer and transferred from a single previous specimen. The *Mucor* specimens were allowed to grow in the laboratory for about 24 hours, when two orthogonal diameters  $d'_1$  and  $d''_1$  were measured. Subsequent measurements  $d'_2$ ,  $d''_2$  and  $d'_3$ ,  $d''_3$  were made at approximately 24 hour intervals, so that the growth rate measurements extended over 48 hours; the period was not extended further because of the proximity of the outer wall of the Petri dish. All experiments were conducted double blind, the measurement of the growth rate being made by myself without knowledge of whether the mould had been exposed or not.

Batches of eight were made up and the dishes kept under a glass dome, or during transport in a polythene bag. Four, randomly selected, by blind choice of a technician, were kept as controls and the other four exposed to the subject, Matthew Manning, for a period of approximately five minutes. The temperature difference between the control and exposure laboratories was always within  $\pm 2^\circ$ . The instructions given to the subject were simply to 'interact' with the moulds, without any preconception as to whether accelerated or retarded growth rate could be expected. He was permitted to touch the Petri dishes, but not to remove the lids. During this period the control *mucors* were kept in another room in the same building, its location being unknown to the subject; in the first runs even the existence of controls was unknown to the subject. Immediately after exposure, the moulds were placed with the control moulds and returned to their original numerical order. The five minute separation, involving a small temperature change, would not affect the growth appreciably.

Eight separate experiments, numbers M1 to M8, were carried out. A further series of ten separate experiments, letters MA to MK, were carried out after an interval of three months. It was usual to conduct either two or three experiments in a day, and at least one week separated

Experiment	$\Delta_{21}$	$\Delta_{21}^c$	$\delta_{21}$	$\Delta_{32}$	$\Delta_{32}^c$	$\delta_{32}$	$\delta_{31}$
<b>c=A to J</b>							
A	2.57	2.59	-0.02	-	-	-	
B	2.08	2.16	-0.08	2.31	2.30	+0.01	-0.07
C	1.98	1.94	+0.04	2.39	2.38	+0.01	+0.05
D	1.99	1.99	0.00	2.39	2.38	+0.01	+0.01
E	2.24	2.18	+0.06	2.43	2.46	-0.03	+0.03
F	2.26	2.19	+0.07	2.44	2.44	0.00	+0.07
G	2.24	2.25	-0.01	2.43	2.43	0.00	-0.01
H	2.46	2.42	+0.04	2.53	2.59	-0.06	-0.02
J	2.44	2.41	+0.03	2.54	2.56	-0.02	+0.01
$\sigma_c$			0.044			0.024	0.041
<b>m=1 to 8</b>							
M1	2.20	2.17	+0.03	1.89	2.48	-0.59	-0.56
M2	2.10	2.10	0.00	2.14	2.15	-0.01	-0.01
M3	2.16	2.10	+0.06	2.12	2.19	-0.07	-0.01
M4	2.10	2.10	0.00	1.99	2.04	-0.05	-0.05
M5	2.11	2.08	+0.03	2.07	2.02	+0.05	+0.08
M6	2.28	2.29	-0.01	2.04	2.04	0.00	-0.01
M7	2.38	2.39	-0.01	2.06	2.07	-0.01	-0.02
M8	2.32	2.36	-0.04	2.08	2.09	-0.01	-0.05
$\sigma_m$			0.029			0.193	0.186
<b>n=A to K</b>							
MA	1.94	1.93	+0.01	2.40	2.42	-0.02	-0.01
MB	1.88	1.90	-0.02	2.45	2.44	+0.01	-0.01
MC	2.22	2.26	-0.04	1.86	1.82	+0.04	0.00
MD	2.20	2.18	+0.02	1.88	1.92	-0.04	-0.02
ME	2.18	2.15	+0.03	2.35	2.32	+0.03	+0.06
MF	2.19	2.18	+0.01	2.28	2.27	+0.01	+0.02
MG	2.08	2.21	-0.13	1.81	1.79	+0.02	-0.11
MH	2.15	2.13	+0.02	1.78	1.84	-0.06	-0.04
MJ	1.93	1.92	+0.01	1.81	1.83	-0.02	-0.01
MK	1.88	1.91	-0.03	1.81	1.86	-0.05	-0.08
$\sigma_n$			0.045			0.033	0.046
$\sigma_{m+n}$			0.040			0.137	0.132

TABLE 7.1

Twenty-four hour growths  $\Delta$ , differences  $\delta$  and standard deviations  $\sigma$ .  
All measurements in cm.

successive exposure days. Between the two series, a complete set of nine 'control blank exposures', denoted A-J, were performed with identical protocol except that the subject was absent from the College and was not informed of the proceedings. The measured growths in this case represent the normal difference between growth rates of similar batches in the absence of exposure to the subject. These differences include both biological variations and errors of measurement.

In each series, arithmetical means  $\bar{d}_i = \frac{1}{4} \sum_1^4 d_i$ , where  $d_i = \frac{1}{2}(d_i' + d_i'')$  of the four pairs of diameters (in cm.) of exposed *mucors* measured after twenty-four hour period  $i$  were compared with the arithmetical means of the four pairs of diameters of control *mucors* also measured after twenty-four hour period  $i$  ( $i$  and  $j$  are running integers). The difference  $\Delta_{ij} = \bar{d}_i - \bar{d}_j$  represent growths for the appropriate twenty-four hours.

The average growth of the exposed batch between days  $i$  and  $j$  is then  $\Delta_{ij} = \bar{d}_i - \bar{d}_j$  and the average growth for the control *mucors* is  $\Delta_{ij}^c = \bar{d}_i^c - \bar{d}_j^c$ . There is only one exposure to the subject, made at the end of twenty-four hour period  $i$ . The growth differential between exposed and control batches between days  $i$  and  $j$  is then  $\delta_{ij} = \Delta_{ij} - \Delta_{ij}^c$ .

All  $\Delta$  and  $\delta$  are tabulated in Table 7.1. The scatter in  $\Delta$  values is not considered to be significant, because there were differences in temperature, light and humidity from trial to trial, and also some small variations in the twenty-four hour periods between measurements, due to pressure of administrative duties on the experimenter. But these should not affect the differences between exposures and control batches, since individual diameters were measured consecutively, controls and exposures together, the whole operation taking less than five minutes.

An inspection of the values of  $\delta_{21}$  and  $\delta_{32}$  readily shows that the consistency of the growth rates in the absence of exposure is good; most of the exposed specimens also grow at consistent rates, showing small values of  $\delta_{21}$  and  $\delta_{32}$ . But in the cases M1, and possibly MG, exposure has seriously affected the growth rates.

For interest, the differences between the exposed standard deviations such as  $\sigma_m$ , and the control standard deviation  $\sigma_c$  were analysed using Fisher's 2-tailed F-test. The parameters

$$F = \left( \frac{\sigma_{m,n}}{\sigma_c} \right)^2 \left( \frac{1-1/N_c}{1-1/N_{m,n}} \right) \text{ are given in Table 7.2.}$$

Experiment	Periods		
	21	32	31
m	0.44 n.s	68.7 $P \leq 1.3 \times 10^{-5}$	21.1 $P \leq 6.7 \times 10^{-4}$
n	1.02 n.s	2.25 n.s	1.26 n.s
m + n	0.77 n.s	31.9 $P \leq 5.3 \times 10^{-5}$	9.79 $P \leq 0.0028$

TABLE 7.2

On this basis we might claim that the entire experiment was significant at  $P < 0.003$ , and that various parts of it were of greater significance.

However, this significance clearly rests upon the single experiment M1, actually the first to be conducted, although the experimenter had already many hours of routine experience with identical experiments using other subjects. The question therefore arises, could some error have been made with this batch? As soon as the double-blind was broken, and it became clear that there was a significant effect, possible sources of error were searched for and not found. A later search revealed nothing, although an arithmetical error was actually found in a subsequent batch previously believed to show significance.

The exposure seems to have affected three of the four *mucors* in the batch M1 (and in the case of batch MG, where the significance is less, all four of the *mucors* in the batch). Moreover, the growths have been radially symmetrical; the standard deviation for randomly chosen orthogonal measurements on 8 *mucors* in a batch was 0.08 cm, and the accuracy of measurement by ruler, (from underneath the transparent dish, so as to avoid parallax) was deemed to be  $\pm 0.05$  cm. However, it is possibly significant that there is for the M1 case a conspicuously large growth increment  $\Delta_{32}^c$  for the controls, as well as a small decrement  $\Delta_{32}$  for the exposures.

We may claim that whilst a consistent effect on growth rate has not been produced by the subject, there has been one exceptional exposure after which an extremely unlikely retardation of growth rate occurred. We are unable to fault the experimentation for this exposure, but are of course aware that it would be unwise to claim the capture of an effect on the basis of a single anomalous batch.

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## 8

## THE SUBJECT'S REPORT

## MATTHEW MANNING

It was against a background of unfounded claims by detractors that I asked for a series of tests to be organised by the Society for Psychological Research; and, I wanted to produce further evidence that would defeat my critics.<sup>1</sup>

I was interested also in replicating some of the successful tests in which I had been involved with Dr. William Braud in San Antonio, Texas (Braud (1979)). These tests in particular had the added attraction of obviating the need for a magician to be present as none of them could be reproduced by simple conjuring tricks. Even so, I wanted to have a conjuror present either watching the experiments, or approving their controls, and I made this clear to Anita Gregory when the plans for the tests were first being laid. Although at first my idea seemed attractive, it was rejected. Anita wrote (1978):

I have had second thoughts about a magician. I can quite see why you should feel like that; it is wounding to be attacked and goaded the way you no doubt have been and unfortunately are likely to be. However,

(a) A magician of your choice would never silence your critics: look what happened over Uri Geller when an eminent magician *did* vouch for him! The magician would simply be discredited by those who didn't like his conclusions.

(b) A magician likely to be hostile at all costs would spoil the sessions and disrupt the very atmosphere we are all trying to create.

(c) A magician's word *as such* is not necessarily more acceptable to, say, academic people in any case. After all, his business is to entertain and to sell himself and to make fools of people in an amusing way. He is as John Taylor has so often insisted, 'a professional deceiver';

(d) The kind of experiments we are planning are not, I would hope, such as could be at all easily counterfeited by a magician; so it could be totally useless!

If, as my detractors would have the public believe, I am helpless in a

<sup>1</sup>Matthew's somewhat edited contribution was written in the summer of 1979, a few months after the final February 1979 experiments.

controlled experiment, it is odd that I should even volunteer to participate in tests with researchers. I have a quiet confidence, some might say arrogance, about my performance under controlled conditions. This confidence is based firmly upon the knowledge that whenever I have engaged in tests there have always been some positive results, even if every test is not successful. I therefore start each of my series of tests with a psychological advantage because I have produced phenomena under controlled conditions in Canada, the United States, Germany, Sweden, Holland, and other countries.

There are, I have learned, disadvantages in working with researchers from different countries. The greatest drawback is that there is limited value to the work that can be conducted in a relatively short period of time if I am working abroad. Short periods invariably provide some initial insight into those experiments which suggest promising results but there is never time to do the necessary follow-up work, either because time or funds run out. After three or four weeks' work one is left with statistics and evidence that ESP or PK phenomena have occurred but nobody is any closer to understanding *how* or *why* it works. I have now decided to cease 'laboratory-hopping' because I have concluded that it is unproductive to both myself and the researchers. Thus I will now work only with organisations who are in sympathy with my desire to conduct experiments that have some useful application and benefit to humanity, where possible. Thus many tests have interesting implications for healing and medicine.

Another sad aspect of laboratory-hopping is that so few researchers seem to share their findings with colleagues elsewhere, except in the form of published reports. Often, I feel, that which is not published or written about is as interesting, if not more so, than the subject matter of the final report. I know it is difficult to get papers published but I find it deplorable that certain so-called scientists can engage a subject in a series of tests and then fail to write anything at all about them. This is another reason for my decision to work only with a few organisations, as so often in the past I feel I have been used by researchers who have not even the courtesy to explain to me why they decided not to write up a series of experiments. This selfish attitude benefits neither the subject nor psychical research as a whole.

I feel also that it is of great importance that a good psychological 'gestalt' exists between the subject and the researchers before any long-term study is undertaken. Of course, it is more difficult to establish a feeling of rapport with scientists during a short period of testing. I am sure that the feeling of rapport that developed during the S.P.R. investigation contributed to the success of the work. I never felt as if I was being used or manipulated, and neither did I feel that I was there to prove myself. This was a pleasant contrast to experiences that I have



had in the past with some researchers who perhaps lacked the experience that the S.P.R. team possessed.

Only once did I feel any sense of friction between myself and the researchers, although there may have been one or two occasions when I felt frustrated at the speed at which the tests were progressing. One such instance is reported by Mary Rose Barrington in connection with my first occultation of the infra-red beam. However, that was boredom rather than friction. Also, during the first session of the haemolysis test with William Byers Brown, I felt that he was concerned that he was having a 'negative experimenter effect' on me because the results were not as good as either of us would have liked. In trying to camouflage a non-existent apprehension he was actually succeeding in producing the atmosphere from which he was trying to escape!

I also feel that far from being suppressed, psychic ability should be developed. So often one hears of people, especially children, who hear voices or see non-existent figures, who pick up the thoughts of others or have premonitions of forthcoming events, and who are unaware that they are having a psychic experience. Instead they are scolded for telling lies, or laughed at for being stupid. If those people understood what was happening to them I am sure they would feel happier and relieved to know that they are not mad, unbalanced, or imagining things.

Although it is not widely known, virtually all my automatic writing and drawing was produced during a four year period from 1971 to 1975. In retrospect I feel that these were transitory phenomena, leading to other abilities which were perhaps of greater use. I learnt from the writings and drawings first of all how to bring spontaneous poltergeist phenomena under some form of control; later I feel that they taught me how to still myself and my mind, so that I could release all thoughts, apprehensions, fears etc. This was most important, especially now when I am healing. However, I became disinclined to pursue them too much because I found that after engaging in automatic writing or drawing for any period of time up to forty-five minutes I experienced considerable difficulty in producing the normal motor movements enabling me to sign my own name myself, for example. It was as if some circuit in my brain had been switched off so that I was unable to form words on paper by myself without difficulty for several minutes.

During the experiments in August 1978, I learnt a considerable amount about myself and my abilities through the frequent questioning of the researchers. Until then I suppose I had never really thought much about *how* I produced psychokinetic phenomena. It simply had not occurred to me to reflect with detachment about what I was doing. Perhaps I was frightened that by doing so it would inhibit my PK effects. Of course, even now neither I nor anyone else is any wiser about why or how I produce an occultation of an infra-red beam or a

promotion of growth in bean seeds when others are unable to do so. Although I have two distinctly different procedures for producing PK phenomena there are certain principles which apply to both methods:

1. It is essential that I *desire* that an event occur. If the task is something useless I will lose interest in it and thus fail to be mentally engaged by it.
2. I must *believe* that the event can reasonably take place. If I have no belief my mind works against it.
3. I must *expect* the event to occur. This is rather a subtle law because the first two are simple and passive whilst this third one introduces some dynamics. It is possible to both desire an event, and believe that it can occur, but still not EXPECT it to take place.

These three mental attitudes become easier to attain with practice and experience. Because I know that I have in the past been successful in my attempts to influence mentally a variety of physical and biological targets, I always expect that something will happen when I start to desire that an effect occur. Obviously I have few, if any, barriers of belief, and my degree of desire is related to the purpose and potential application of the test. I also think that to a certain degree one has to learn to recapture an essence of child-like simplicity and naivety, to disregard what one has been taught is real or unreal. Everything is real when you think it. Thoughts are energy and energy influences that which is material. You create your own reality. It's really that simple! We lose so much of our innate potential through formal education which strives to expand other areas of the mind.

I use two very different techniques for producing psychokinetic phenomena that, for me, fall into two very different categories. The first technique I use when I'm engaged in what might be considered the more dramatic visible effects, such as the occultation of the infra-red beam or the influence of electrical apparatus generally. I have less control over the phenomena once I have initiated it and usually I find difficulty in then being able to stop it. The phenomena brought about by this first method are by nature more spontaneous, and to my mind, of little practical application. They seem to be closely linked to physical exertion or restlessness and/or psychological irritation or friction.

Historically, many PK phenomena, whether premeditated or spontaneous, have occurred in situations of psychological stress or strain. This is my experience also; there have been many instances of spontaneous PK effects taking place when I have been irritated or stressed. I feel too that I am made up of several contradictory traits, the friction of which occasionally produces or contributes to physical phenomena around me.

When I first succeeded in influencing the infra-red beam equipment at

City University, it began in an almost spontaneous way without any real concentration or effort from me. At the time I was feeling very irritated and bored; I am a fairly kinetic person by nature - I will pace a room or clap my hands together when I feel restless. On this occasion I was stalking around the infra-red equipment, looking at it and wondering just how easy it would be to disturb it. As I remember, I then thought to myself that there was no point in even attempting to influence the apparatus as there was nobody there to take any interest in it. I started clapping my hands together out of frustration. Almost immediately David Chapman, who had been occasionally observing the monitoring devices, called out and asked what I was doing. The pen recorder linked to the infra-red beam equipment was indicating that the beam was being partially occulted.<sup>2</sup> Of course, my interest was immediately piqued and I started to make a deliberate attempt to consciously upset the apparatus by making exaggerated movements of my hands and arms within the general vicinity of the apparatus.

Whenever such a phenomenon occurs, it generates great excitement. I think that I feed on this energy which is conducive to the development of PK effects. The best PK phenomena that I have produced, either spontaneously or consciously, have been in the presence of sympathetic researchers who are relaxed and jovial. I believe that the researcher contributes to the results of any parapsychological experiment at some level.

Whenever I produce, say, an occultation of the infra-red beam, I imagine that I am involved in a game with the researchers, but a game at which I have the advantages. I know that I am producing a PK effect. I also know from experience just how the researcher will react. He searches for every conceivable, and sometimes inconceivable, logical explanation! The game is to baffle the opponent (researcher), in the full knowledge that I am producing a paranormal effect for which, try as he might, will not be explicable.

Charles Tart and John Palmer, in reporting a series of tests in which I participated at the University of California (Tart and Palmer (1979)) made a very perceptive observation: 'If our hunches about the perversity of Mr. Manning's psi are correct, the experimenter is involved in a game of trying to outwit this perversity, a game at which he is at a distinct disadvantage.'

The best method of confusing and baffling the opposition is to repeatedly ask for a normal explanation of the phenomena. I would, for example, repeatedly ask David Chapman and Arthur Ellison for their explanation of the infra-red beam occultation. Arthur learnt to

<sup>2</sup>Matthew's recollection here is at fault; he was indeed bored and irritated and restless. The actual event is described in the IR section on page 320, as supported by the audio record. A. Gregory.

ignore me but David always rose to the bait. I felt that he was growing irritated at his inability to find a feasible explanation, as well as my persistent questioning. Later he admitted this was so.

During the later sessions when I consciously occulted the infra-red beam, I would stand in the vicinity of the apparatus and clap my hands together. Sometimes I would merely clench my fists and tense my muscles, before jumping up and down, making large circular movements with my arms. By the end of the first week of tests my muscles were so strained from this activity that I had difficulty even scratching my back! I had no conscious thoughts in particular during these exertions but merely a great determination that I was to be successful. Interestingly, the greater my distance from the equipment, the greater my exertions had to become in order to generate the phenomena. I do not feel that my physical movements alone were responsible for what happened, especially as others imitating my movements were quite unable to produce the same effect. I have noted in the past that PK phenomena often occur whilst I am in a state between intense concentration and distraction. Perhaps the physical activity provided the necessary degree of distraction whilst mentally I concentrated and focused my attention on the infra-red equipment. I think an interesting study could be made to correlate physical fitness with PK phenomena.

This technique of producing PK effects will not work when used for tests which I consider have a positive application, for example the promotion of seed growth, the influence on cancer cells or blood. For such 'positive' phenomena I use a second method, which will not disturb electrical apparatus or generate the 'negative' effects. This, incidentally, is one of the reasons I feel that the results I have achieved in influencing biological systems are not the result of my disturbing through PK the electrical monitoring equipment, as some have claimed, but an actual effect directly upon the biological target.

Relaxing with my eyes closed, I calm myself, and expel all thoughts from my conscious mind until there is a state of blankness. Sometimes the image of a still mill pool is very effective in this respect. I then imagine that I am slowly expanding my consciousness through 360 degrees, rather like the ripples made by a pebble tossed into the mill pond water. These waves of consciousness float away from me, through the walls of the room, and out all around me. Then I imagine that I start to follow a stream of water flowing from a spring bubbling from a hill. It becomes a river and I flow with the water which eventually opens out into a wide estuary. I flow out to the sea, continually expanding until I see nothing but sky and water. I become a part of the water, a part of the sky. On the horizon, climbing out of the sea is a vast snow-capped mountain, reaching up to touch the sky. I ascend, very smoothly and gently, the mountain. Both the mountain and sea

give me a sense of peace and strength. I am reminded that I am a part of Nature. At the summit of the mountain I reach up to the sky. I leap. Slowly I am drawn out into space and I watch as the Earth sails away from me. After a few moments it hangs like a beautiful blue ball in the black space. I flow. I am in harmony. I reach a point where I feel that I am a part of everything around me. I am a part of the Universe. The Universe is a part of me. There are no barriers in this state. I am. At that point I feel as if I am a channel. I feel energy flowing through me; physically I sense warmth and tingling in my hands and forearms. It seems that this visualisation, which is very powerful for me, is somehow a symbolic expression of the start and ultimate living solution.

When I reach this point which I call 'oneness', I visualise a brilliant light flowing through me to permeate that which I am healing or influencing. I visualise the cells in the test tube or flask surrounded with this light. Mentally I talk to the cells. If they are blood cells being stressed I give them assurance that the light and energy will protect them; if they are cancer cells I explain to them that their useful purpose on this level of reality has ended and that they must progress. I never wish that anything should die, not even cancer cells. I always use positive thinking. I have a belief, derived partly from my psychic experiences and which some will call a superstition, that everything I think comes back to me ultimately. I am sure this is a universal law. Again, you create your own reality. Everything you think, everything you do, somehow, some time, comes back to you.

I am sure that many of the researchers with whom I have worked are unaware of this principle. Recently I listed every *different* test in which I had participated in a two year period between 1977 and 1979. There were a total of 32 experiments, of which 17 could be judged successful. However, that was not the most interesting fact. I was interested to note that there were certain researchers with whom I consistently produced successful results; there were a number of researchers with whom I consistently failed to produce anything at all. Sometimes I would successfully participate in a test with a particular scientist. Later, during an attempt at replication by another researcher, success eluded me. Perhaps the most interesting category of experimenters were those who engaged me in a test with a specific task. Although the desired result did not occur, another PK effect did occur, often on the other side of the room. Are those researchers unconsciously frightened of witnessing a PK event, or of being involved in a successful test, but insufficiently strong to prevent *some* effect occurring? It is evident that the researcher plays a vital role in the ultimate result of the experiment. He is a part of it just as much as the subject, which is obviously why hostile researchers and scientists fail to produce results. It is time for the scientist to look at himself and to break down the barriers created

by his role. Of course, this has devastating implications. At present I can be watched, observed, viewed, probed at a distance, by a detached and uninvolved scientist. He is able to make an objective analysis. This objectivity must necessarily be destroyed if *he* is also part of the experiment. He must analyse himself and there is the quandary. It is no longer a simple exercise of studying someone else, it is a subjective experience.

Coincidences have always intrigued and, latterly, amused me. The occasions upon which electrical apparatus designed to test me has malfunctioned are too numerous to recall. It is almost as if there is a cosmic joker whose sole job it is to incapacitate researchers' machinery. I can hear him saying in my more lucid moments, 'Why do you use all these gadgets? What use are they? You will never measure a non-physical energy with physical apparatus. Look, I can break it just like this. You don't need it!' And as his laughter dies away I am reminded of the words of Lyall Watson (1976): 'Looking for physical explanations of the mind is like attacking a piano with a sledgehammer to get at the concerto imprisoned inside. It is a lunatic endeavour.' Sadly, I suspect this is true. We know no more now about how these phenomena occur, we know no more about their mechanics, than we did one hundred years ago - in spite of our beautiful machines. Something is happening, but is there any benefit in collecting piles of statistics and statements from witnesses? I think the key lies not in studying a few gifted subjects, but in studying the scientists too. Is it so important to find out how and why? I am more interested in using what I have to benefit humanity in a practical way. If I can reach more people through my work with scientists I shall continue that work. I believe that many of the results of tests in which I have participated have a message. At some level we are all part of one another, linked through our unconscious minds. We are all part of every living organism, no matter how small. We are cogs in a cosmic system. If I can make people aware of these interconnections, of the fact that we have our roots in Nature, I believe there would be a greater respect for one another and the environment.

And so I would like to thank all those who contributed or helped in the tests. Each person plays his part, whether he is my critic or my supporter. Without my detractors these experiments would not, I think, have been conducted. I believe that eventually the explanations offered by my detractors will become more implausible than a paranormal explanation.

For those who understand, no explanation is necessary.

For those who do not understand, no explanation will suffice.

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## 9

## COMMENTS

## BRIAN INGLIS

In his 'Introduction to the Earl of Dunraven's record of experiences with D.D. Home', published in the *Proceedings* of the S.P.R. in 1924, Sir Oliver Lodge surveyed the problems which confront psychical researchers who are investigating physical phenomena, with a view to ascertaining their laws. 'For laws, of course, they must have', he argued; 'though inasmuch as the activity of live things is involved, those laws are bound to be complicated by the general difficulties inseparable from the activities of life'. It ought to be assumed, he felt,

that all well-ascertained laws are rigorously obeyed, whether by animate or inanimate nature. But it is a familiar fact that live things introduce a supplementary element, a guiding or controlling element, an infusion of will and or intelligence, something not suspending but supplementing the ordinary processes of nature.

It appeared to me, when the idea of trials with Matthew Manning was first mooted, that they would serve two purposes. They would show what, if anything, he could do in his capacity as a psychic. But they would also provide experience in the related field of learning how to allow for, and if possible to harness, the supplementary 'guiding or controlling element'. Assuming that what a psychic can do is influenced by a variety of subjective considerations, how can tests be so organised as to bring the best out of the psychic, in the sense of enabling him to demonstrate his capabilities in ways which will carry conviction to the investigators and later, to anybody who reads their report of the inquiry?

I have to admit that in my capacity as occasional onlooker at the City University laboratory, I was chiefly concerned with the problems presented by Manning's personality and attitude. He has no reason to respect the S.P.R. His recollection of the way he was treated by some

members of the S.P.R. who came to investigate the poltergeist activity which he describes in *The Link* are bitter; and, even allowing for exaggerations, justifiably so. His experiences with psychical researchers in general, too, have not encouraged him to feel well-disposed to them. Researchers are apt to forget that the psychic is not a human guinea-pig, resigned to being put through a succession of tedious tests. He may be - and in Manning's case, he is - somebody who has a clear idea of what he can do, and the circumstances in which he can do it. Boredom or irritation may wreck the proceedings.

Alternatively they may conceivably influence them in unexpected ways. Manning's 'psi' output, if the term is permissible, appeared on at least one occasion to be triggered off by the mention of the name of one of the conjurors who make a sleazy living out of doing imitations of psychics, and accusing them of fraud. The implications for research into poltergeist phenomena are obvious. But as a rule, Manning's results appear to be related, to some extent, to the degree of rapport which is established between him and the investigator(s). And this, it appears to me, represents one of the most serious problems confronting psychical research.

Partly it is simply a matter of investigator and subject 'getting on' together; but I doubt whether in Manning's case this is so much a matter of trust and liking, in the conventional sense, as of some intuitive feeling that here is somebody he can work with. But this inevitably brings up the question how far the psychic and the investigator are involved in a co-production. If they are, of course, it hammers yet another nail into the coffin of 'replication', as it would be absurd to expect Manning to demonstrate the same results in tests irrespective of the circumstances in which he is tested.

At this stage, more research is needed into experimenter effect along the lines exemplified by Fisk and West. Orthodox science is also occasionally being confronted with the evidence of experimenter effect: either directly, as in Rosenthal's 1963 report, or indirectly, as in Neal Miller's account of the results of his research into control of the autonomic nervous system with biofeedback, which were repeated until they appeared 'robust', but suddenly could be repeated no longer. Here, then, may be the way to slit open orthodoxy's soft underbelly.

Subjective aspects of parapsychology are now attracting increasing attention; but how best to understand them, and perhaps bring them under greater control, remains a task for the future. For the present, one lesson from these tests with Manning is that it is no longer sensible for the Society's investigators to conduct their task, as some have done earlier, in the frame of mind of an inquisitor. To treat psychic subjects as partners - though I suspect that, by disarming suspicion it could also make deception easier to detect. But the Society's investigators should be sheep - as in this series they have done their best to be - rather than goats.



10  
POSTSCRIPT

ANITA GREGORY

Matthew Manning has recently marketed two cassettes called 'Fighting Back' (1982) which provide some further information about his recent development and thought processes. The recordings are concerned with his approach to self-healing - the 'fighting' is against illness - and purport to teach people how to defeat sickness and improve their quality of life.

At the start of the first tape he says that, as a healer, he has treated hundreds of people from all walks of life and with a wide range of problems 'which, as a rule, orthodox medicine has been unable to help'. Even if he were to practise for 24 hours a day, he continues, he would not be able to treat everyone wanting his help:

Obviously, then, I've had to devise a method for selecting those people with whom I do work. If I have to choose between an elderly person with arthritic joints and a hearing loss, or a young child with cancer, my choice will almost certainly be the same as yours. I think it's fairer to work with a child that has a whole life in front of it than with an older person who is suffering from what one might call fair wear and tear.

The gentle sagacity of this passage betrays perhaps even more clearly than anything that follows the all of 26 years of its author. The effect is slightly modified by the ensuing statement: 'I often tell those that I am working with that what I am doing for them they could just as easily do for themselves'.

Usually, he says, people do not believe him: they expect to have things done to them rather than to do things for themselves. A little later he says he remembers being told on a radio programme 'that it was cruel to suggest to people that they could actually do something for themselves. I realised then the effect that the National Health Service had on many people'.

The element of cruelty suggested by the interviewer, one would have supposed, did not consist in encouraging people to make an effort but in raising false hopes. Matthew Manning of course does not believe his implicit promises to be false, on the contrary he considers that unfaltering faith in the possibility of self-healing is essential to eventual success. At the very end of the second side he explains his rejection of the word 'try': 'I feel that the word has somehow negative connotations in it....

...so long as you say you're going to try something you are allowing yourself the option of not succeeding...'. He concludes: 'Remember with the self-healing programme, that even if you don't succeed what is worse than that is not making the attempt to succeed in the first place'.

A lot of what follows on the cassettes is interesting and potentially perhaps even helpful: relaxation through a few breathing exercises, rather rough and ready examination of one's own motives, a bit of active imagination, auto-suggestion and positive thinking. Some of it, especially delivered as it is under conditions of hypnotic and hypnoidal suggestion, should be queried rather more critically, for instance the suggestion that past mistakes were ever only in one's own mind.

If one listens carefully, there are actually few if any concrete claims being made. Is there anyone today who would disagree about the hazards of 'stress' and the likely beneficial effects of counteracting it? Or who would doubt that a great deal of illness has a psychosomatic component? The question arises, just what is Matthew Manning's own exact contribution to medicine and healing?

If it is really the case that, as he now says, he does nothing more for people than they can do for themselves, he has presumably no special *cachet* as a psychic in promoting health: he has adopted the roles of teacher, counsellor and therapist, and is in competition with countless other practitioners, orthodox and alternative, physiological, psychological and spiritual, all of them purporting to help suffering humanity, and many of whom have years of training, professional expertise, experience and tradition at their disposal. His methods and techniques (if indeed they are specifically his which at least most of them do not seem to me to be) presumably will be subject to the same criteria of efficacy as anyone else's, and to establish their validity and reliability he would have to persuade clinical, psychological and social researchers to conduct relevant tests. This should not in any direct way necessitate his own cooperation over and above formulating what, in his view, people should do to get better.

From the point of view of the psychical researcher, on the other hand, the question arises what, if any, link does there exist between Matthew Manning's ostensible success as an experimental Subject (if that is a legitimate interpretation of some of the results) and his claims as a healer?

Claims to healing themselves need to be sub-divided into possible claims (on the face of it disowned on these cassettes) to direct paranormal influence over people's bodies, and claim to authoritative pronouncements on how to achieve health.

The link that I feel he would probably make is *via* his own mental imagery in the process of exerting influence on physical and biological targets. And yet it cannot be said that the record would support this:

he does have these images, but they do not clearly correlate with the effects associated with him; certainly there cannot be any question of having established a causal link. The problem of the role of the unswerving, contrary-to-fact symbolic imagination in parapsychology and, for that matter, in self and health management, is far too important to be left suspended between being dogmatically asserted and stonily ignored.

Matthew Manning does not claim any paranormal element in his cures, nor is evidence offered to support any contention that there has ever been any. Nevertheless, it seems to me just about conceivable and worth exploring that if he (or any other healer at least of his style) is therapeutically effective over and above normal suggestion and chance factors, this could be due to a two-stage process: that he could have a disruptive effect on an organism, probing its weaknesses and disabilities, after the manner of his ostensible PK effectiveness on the infra-red and random event generator equipment, and that, by then suggesting soothing as well as likely positive images, the patient is occasionally able to cure himself having been (paranormally?) shaken out of prior pathological adjustment.

The entire subject is one of the greatest possible human interest, and Matthew Manning must be congratulated for struggling so valiantly in so difficult and age-old a battle. The functions of belief, faith, imaging and right thinking in correct living have, so far as we can tell, occupied our race from time immemorial. It is precisely because these issues are of such vital and perennial human importance that, I believe, psychical researchers have always been particularly cautious about endorsing any claims that might conceivably suggest support of future cults and cult persons. So long as researchers are clear about exactly what they have and have not established, and so long as Subjects are willing to go on recorded tape as candidly as Matthew Manning, the danger is at least containable.

#### REFERENCES

- Fighting Back, A Guide to Self-Healing*, © and ℙ 1982, Matthew Manning Cassettes Ltd., Catalogue Number MMC3, music played and composed on side 4 by Robert John Godfrey, © and ℙ Enid Songs.

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