

TANSLEY'S PSYCHOANALYTIC NETWORK: AN EPISODE OUT OF THE EARLY HISTORY OF PSYCHOANALYSIS IN ENGLAND

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There is no need to dwell on the difference between life

in Cambridge and the lives of our ancestors a few thousand years ago.

(Ramsey 1925, p. 320)

Introduction

In a wide-ranging and well-informed article, Hinshelwood (1995, pp. 135-51) distinguished seven points of entry for psychoanalysis into British cultural life in the period 1895 to roughly 1925: the Society for Psychical Research; Havelock Ellis and the development of sexology; psychological developments within psychiatry, associated with W.H.R. Rivers, Bernard Hart, May Sinclair and the Brunswick Square Clinic; the Psycho-Medical Society and the formation of a Medical Section of the British Psychological Society during World War I; the 'literary' strand, dominated by the Bloomsbury Group's intense interest up until the early 1920s; progressive education, including Homer Lane and somewhat later A.S. Neill; discussion in philosophical circles, including the influence of Bergson's and Rivers's work. The path taken by the career of Sir Arthur Tansley (Figure 1, p. 202) - and, as we shall see, some of his colleagues - indicates that this classification scheme, while useful, might be insufficient. I

To the seven paths Hinshelwood lists, we need to add a road to the unconscious which will be neither medical nor psychological, neither philosophical nor literary in character. To map this road, since covered over and silted up, this study examines a number of eminent scientists, nearly all from

1. Tansley himself may have been aware of this important cultural distinction between the medical and the scientific worlds: in his Memoir concerning his contact with Freud, written in 1953 for the Sigmund Freud Archives, he self-consciously described himself as a 'non-medical biologist'.

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Cambridge, who, in the early 1920s, were drawn to psychoanalysis, forming themselves into two loose networks around the person of Tansley. Between them, these men - ranging from the philosopher Frank Ramsey to the polymath geophysicist Harold Jeffreys - underwent personal analyses, published papers in psychoanalytic journals, wrote popular treatises on psychoanalysis, corresponded with Freud, and were admitted to membership in the British Psycho-Analytical Society. What these men had perhaps most in common to bring them together was 'scientific curiosity'. Amongst them were those scientists of the first or second generation of scientific professionals - i.e. men (predominantly) who were employed full-time on account of their scientific skills - who regarded the new ideas and practices associated with psychoanalysis as a natural extension of the 'scientific attitude' of careful and empirical inquiry into the nature of human beings. Whether or not they were trained in biology, and regarded psychoanalysis as an extension or supplementary revolution to the Darwinian and other biological advances of the late nineteenth century, is secondary to their self-conception as scientists engaged in a new field of enquiry opened up by Freud. And they were the first to admit that this new field of enquiry required 'self-knowledge' - in timehonoured fashion, submitting themselves, first and foremost, to the tools of enquiry. (On the varieties of self-experimentation, see Schaffer 1992.)

Nor should we underestimate another factor which may have had some weight with this Cambridge group. When interviewed for the Columbia Oral History Archives in 1965, Edward Glover sketched a picture of Viennese psychoanalysis as consisting in a small group huddled around Freud, of American psychoanalysis as having a psychiatric background and of British psychoanalysis as having a cultural source:

This development [of psychoanalysis] differed in England from that in other countries... psychoanalysis in this country developed along cultural lines. It began about the middle of the First World War when for the first time Freudian ideas began to percolate here and a number of people having different backgrounds, psychiatric, philosophic, biological or merely cultural, began to take an interest in it.²

And he continued in similar vein:

About 1920 I should have said that in Oxford and Cambridge no young undergraduates who were really interested in cultural matters but [sic] would accept or consider Freud; it wasn't a technical psychiatric approach to analysis, it was a cultural approach.³

Something of this atmosphere can be seen in Roger Money-Kyrle's recollections of moving from Cambridge to analysis with Freud in 1922-1923 while he worked on a doctorate in philosophy with one of the founders of logical positivism, Moritz Schlick:

2. Edward Glover, interview by Bluma Swerdloff, 5 August 1965, London, Columbia University Oral History Project, p. 1.

3. Edward Glover, interview by Bluma Swerdloff, 5 August 1965, London, Columbia University Oral History Project, p. 4.

In Vienna, we met several people from Oxford and Cambridge, nearly all subsequently famous, who were more or less secretly in analysis. And I did not know till many years after that a half-uncle of my wife, a Fellow and Lecturer of Trinity, Cambridge, had spent one long summer vacation travelling Europe in analysis with James Glover, who was himself simultaneously in analysis with Abraham. Shades of the Peripatetic School of Athens in the third century BC! Incidentally, of course, I never mentioned psychoanalysis to Schlick till I left, and then discovered that he himself was extremely interested in, but never spoke of it. (Money-Kyrle 1979, p. 266)

What Money-Kyrle was describing is the atmosphere of the social forms and customary codes of psychoanalysis before it was institutionalized. Historians and analysts have had enough difficulty finding adequate models for characterizing the institutions of analysis. To take a well-known model, that of Otto Kernberg (1986, p. 807): are analytic training institutes most akin to art academies, technical trade schools, religious retreats or universities? In the period with which this study is primarily concerned, the 1920s, these amorphous and hybrid institutes were in the process of formation. So it is eminently plausible that the environment of psychoanalysis will have been ordered far more differently than Kernberg's model, designed for the end of the twentieth century, can possibly envision.

The Cambridge Circle

On his return from completing his analysis with Freud in Vienna in the summer of 1924, Arthur Tansley, aged 53, founder of British ecology, and recently resigned from the Lectureship in Botany at Cambridge which he had held from 1907-1923, moved back to the home he had left in Grantchester, near Cambridge. (For considerable detail on Tansley's life and career, and on his specific path from botany to psychoanalysis in the period from World War I on, see Cameron & Forrester 1999.) Tansley, ever his own organizer and energetic pursuer of his intellectual interests, formed new psychoanalytic circles - a Cambridge circle and a circle of field scientists. And it is these, rather than the more familiar organization of the British Psycho-Analytical Society that we will examine for new insights into the development of psychoanalysis in England. Prior to the period we will discuss, interest in psychoanalysis specifically in Cambridge was already represented by W.H.R. Rivers (see Barker 1991; Slobodin 1978; Stone 1985; Young 1995) and C.R.A. Thacker.⁴ Rivers even

4. Dr Cecil Robert Allen Thacker, born 18 June 1889, went up to Downing College, Cambridge in 1908, took Natural Sciences and specialized in physiology - a pupil of Sherrington's - in his final year (1912), completing his medical studies at St Bartholomew's Hospital; he received his M.D. in 1920. During the war he worked in military hospitals in Cambridge, and relinquished his commission in the R.A.M.C. to do work on the Special Medical Board for neurasthenia and shell-shock, under the Ministry of Pensions. He was elected Fellow of Sidney Sussex College, Cambridge, in June 1918, where he taught till 1926, becoming University Demonstrator in 1921. He was an Associate Member of the British Psycho-Analytical Society from 1920 to his death. Owing to ill health he resigned all his posts in December 1926, and went to the South of France, where he died in May 1929. We would like to thank the Master of Sidney Sussex College, Professor Gabriel Horn, and the Librarian of the College for providing this information on Thacker.

invited Freud to come to lecture there in 1920. Nothing came of that invitation, nor of a later one from an enthusiastic student in psychology, W.J.H. ('Sebastian') Sprott in 1922 (Freud & Jones 1993, Jones/Freud, 498, 22 August 1922; Freud/Jones, 500, 3 September 1922). Rivers, for his part, was notoriously ambivalent in his assessment of psychoanalysis; typical might have been the view recorded by Karin Stephen in a letter from 1921: 'Tremendous talk with Rivers about Psychoanalysis. He thinks it is very dangerous though a few people might come out of it all right!'⁵

Oxford and Cambridge were centres of the informal cultural interest in Freud that Glover, accurately in our view, recalled. Early in 1925, a psychoanalytic discussion group of which Tansley was a part was organized in Cambridge, described many years later by John Rickman:

Soon after the Malting House School was started [in 1924] there arose in the vicinity, but independently, a discussion society which made the experience of personal analysis its major qualification for membership. It was a small group composed of two members of the Royal Society, three others clearly heading in the same direction, one literary person occasionally came and there was one nondescript member, all were graduates of Cambridge. A topic was taken at each meeting, announced beforehand, memoranda were sometimes circulated; the theme was outlined before lunch, discussed casually in the after lunch walk, seriously tackled before, during and after tea and brought to a close usually before but sometimes after supper. (Rickman 1950, p. 281)

The nondescript person was Rickman himself; the 'literary person' was James Strachey - a joke the latter would have approved of. The other members were Harold Jeffreys, Lionel Penrose, Frank Ramsey and Arthur Tansley (Strachey & Strachey 1986, p. 210). Of these men, only Tansley was F.R.S. at the time, though Jeffreys was to be elected in 1925 - so it is possible there is one member missing from this list. The three destined-to-be F.R.S.s would indeed be distinguished, as a description of the lives and careers of the six group members will make clear.⁶

John Rickman

John Rickman (1891-1951) was educated by the Quakers and then at King's College, Cambridge, where he took a degree in Natural Sciences and then in medicine, completing the clinical training at St Thomas's Hospital in London in 1916. During World War I, as a pacifist he volunteered for the Friends' War Victims Relief Unit in Russia, where he met an American woman, Lydia Cooper Lewis, whom he married in Russia in 1918. On his return to England Rickman wrote a series of articles on his experiences when practising medicine amongst

5. Karin Stephen to Ernest Jones, 14 June 1921, Archives of the British Psycho-Analytical Society, KS/27.

6. Penrose and Jeffreys became F.R.S. Ramsey undoubtedly would have done. Neither Rickman nor Strachey did - they became psychoanalysts instead. Rickman's account gives the membership of the society as seven; we can only determine its membership as six.

the Russian peasants, which, characteristically, were published anonymously at the time. On his return to England, he worked as a psychiatrist at Fulbourn Hospital near Cambridge, and, encouraged by W.H.R. Rivers, an Associate Member of the British Psycho-Analytical Society from 1920 (Jones 1922), he decided to seek psychoanalysis with Freud in Vienna. He began analysis with Freud in April 1920 and, through the Freud-Jones correspondence, we can track the views of Ernest Jones, the self-appointed patron and manager of English analysis, and those of Freud, the actual analyst, on the young apprentice.

After six months in Vienna, on a trip back to London in October 1920, Rickman spent a fortnight discussing analytic questions with Jones, who formed a dislike for the 'aloof' young man. Freud jumped to Rickman's defence: 'I think your judgment is too severe. His peculiarities are not beyond the measure of any young man getting aware and not yet sure of his powers. He is not conceited and full of passion. I rather like him' (Freud & Jones, 1993, p. 393, 12 October 1920). As with Alix and James Strachey, both in analysis at this time with Freud, Rickman's analysis became a family affair: in the fortnight Rickman was discussing analysis with Jones in London, his wife conceived a child and herself entered analysis with Jones, starting (probably) on 4 October 1920 (Jones/Freud, 1 October 1920). Then, when her child was born in the summer of 1921, Rickman's mother took her place on Jones's couch (Jones/Freud, 30 November 1921). While Jones 'managed' the Rickman family, Freud was taking care of the London psychoanalytic enterprise:

[Rickman] is a nice, strong fellow, his narcissism, I expect, a phase on the track of libido from mother-wife to some new, sublimated object. His analysis is just now rather sterile. I make excellent progress with Alix Strachey and expect to succeed with James Strachey, both of whom may become highly valuable members of your staff. (Freud & Jones 1993, p. 443, 6 November 1921)

Rickman and the Stracheys returned to London in July 1922; all three became members of the British Society and, as had been planned by Freud and Jones, were put to work: the Stracheys in their labour of translation that would last a lifetime, Rickman as an indispensable administrator both for publications and in the running of the Society. It was Rickman who found the building and set up the legal framework for the Institute of Psycho-Analysis in 1926 (and he would perform similar tasks as President of the Society after World War II, in overhauling the rules of the Society and Institute and the acquisition of a new building, Mansfield House). In 1926 he also completed his M.D. thesis on the aetiology of prolapse of the uterus; his first psychoanalytic publication, apart from reviews and a piece on psychoanalysis and alcoholism for the *British Journal of Inebriety* in 1925, was on psychosexual aspects of prolapse of the uterus (Rickman 1926). A supremely hard-working and skilled editor, he acted

7. They were later revised and republished in a joint book with Geoffrey Gorer, the psychoanalytic culturalist (Rickman & Gorer 1949).

as Assistant Editor (1925-1934) and then Editor (1934-1948) of the *British Journal of Medical Psychology*, thus ensuring that one of the principal British psychiatric journals had a predominantly psychoanalytic, or psychoanalysisfriendly, orientation. (During the war, Jones and Flugel had reorganized the British Psychological Society, 'changing its nature', in Jones's words, 'from a small select body of professional psychologists to a large body of generally interested people and dividing the whole into several special sections', one of which was the Medical Section, from which the *British Journal* emerged as a separate publication (Jones 1956, p. 193).) As a stalwart of the British PsychoAnalytical Society from 1922, one of the founders of its press, always involved in the publications and the journal, Rickman became one of the specialist teachers in the psychiatric aspects of psychoanalysis and, in his systematic and exhaustive style, produced in 1928 two working books of exposition for students and psychoanalytic researchers: the *Index Psycho-analyticus*, a record of all psychoanalytical work from 1893-1926, and *The Development of the Psycho-Analytical Theory of Psychoses, 1893-1926*. The *Index Psychoanalyticus* and a later bibliography of Freud's writings that Rickman appended to his *A General Selection from the Works of Sigmund Freud* of 1937 formed the basis for the superb works of Freud scholarship we now value in Volume 24 of Strachey's *Standard Edition*.

Rickman's analysis with Freud did not satisfy him. In February 1924 he requested further analysis from Freud and apparently did journey to Vienna after the April 1924 Salzburg Congress - at which it was decided to hold the next International Congress in Cambridge (Cameron & Forrester 1999, p. 73).⁸ In July 1928 he contacted Freud again, requesting further analysis. This time it was impossible for Freud to take him on and, on Freud's recommendation,⁹ Rickman spent periods of time in Budapest in analysis with Ferenczi over the course of the next three years; and from 1934 on with Melanie Klein in London, continuing fitfully throughout the war. He became a firm supporter of her work, to the extent that she could, in a Memorandum submitted to the British Society in 1942, refer to him (along with Joan Riviere, Susan Isaacs and Donald Winnicott) as one of the four training analysts in unqualified agreement with her on matters of method and technique (Klein 1942, p. 199), i.e. as part of the core group of Kleinian senior analysts. He never became a notable training analyst but one of his analysands was indeed notable: W.R. Bion, who had an extensive analysis with Rickman in the 1930s and with whom Rickman wrote their extremely influential paper on groups for *The Lancet* during World War II.

Rickman became the backroom psychoanalytic organizer *par excellence*. On becoming Editor of the *British Journal of Medical Psychology* and entering

8. Freud to Rickman, 26 February 1924, Library of Congress Freud Archives, Falzeder transcription (English in the original).

9. Freud to Rickman, 30 July 1928, Library of Congress, Freud Archives, Falzeder transcription (English in the original).

analysis with Klein in 1934, he was increasingly active in making connections with organizations outside the small psychoanalytic community. From 1935 on he was a regular (and anonymous) leader-writer for *The Lancet* (he wrote 23 in total), and thereby influential on public opinion and much quoted in the national press on general medical topics (see Payne 1952). As war approached, he was active in the Peace Pledge Union, the Medical Peace Campaign and the Quaker Medical Society; he was also involved in the All London Aid Spain Council formed to bring food to the starving children of Spain (King 1989, p. 17). On the day Poland was invaded, he joined the Emergency Medical Service at Haymeads Emergency Hospital, Bishop's Stortford; Susan Isaacs and Melanie Klein joined him in the town a few days later. World War II was the perfect theatre for Rickman's gifts: starting with the Haymeads Memorandum, written three days after war was declared, he was incessantly organizing and communicating, finding new solutions and new strategies for the organization of psychiatric services in both military and civil life.¹⁰ Though ill from 1944 on, at the end of the war Rickman travelled to Germany as part of the German Personnel Research Branch, whose aim was to discover suitable people who had been opposed to the Nazis, and who would be able to aid in the 'rehabilitation' of Germany. Rickman visited those analysts who had remained in Germany and advised Ernest Jones and the IPA on which of them had 'survived' the war as adequate human beings and which had not. As Rickman put it: 'If one just man could save Sodom and Gomorrah, Berlin is trebly saved in Fraulein Drager, Steinberg, and Dr Kemper' (King 1989, p. 32).

President of the British Society, Editor of the *International Journal* - as Bion put it, 'I never heard of anyone who was able to persuade him to consider his own welfare' - his final paper for the Society before his sudden death in 1951 was titled 'Reflections on the functions and organization of a psycho-analytical society'. There could have been no better informed author.

Lionel Penrose

Lionel Penrose (1898-1972) may have been destined to be an F.R.S., but at the age of 30 nobody could have guessed from his initial distinctive gifts (a playful speculative bent and a dogged commitment to empirical exactitude) and his scientific education (in mathematics and logic, theoretical biology, psychology and medicine) in what field he would, in his own due time, make his

10. Starting with his move to Sheffield in January 1940, his other notable activities included the 'Wharnccliffe Experiment', in which Rickman initiated what was later to become the therapeutic community; the 'Northfield Experiments' (regarding neurosis as treatable by the leaderless groups), conducted when Rickman and Bion, both from early 1942 in the Army, were simultaneously posted to Northfield Hospital, Birmingham - experiments which were to become extremely influential after the war via the Tavistock Institute, an institution held very much at arm's length by the British Psycho-Analytical Society before the war, and with which Rickman helped to engineer a *rapprochement* in 1946. As part of the War Office Selection Boards system, Rickman played the part of the psychiatrist in their training film.

distinguished, indeed fundamental, contribution. Starting as a psychologist and logician who mutated into a psychoanalyst, he turned out to be the founder of modern human genetics.

Born in 1898 to an old and wealthy Quaker family, Penrose served from 1916 to the end of 1918 in the Friends' Ambulance Train of the British Red Cross in France.¹¹ In an unpublished memoir, probably written in the 1960s, he wrote:

I think that my interest in psychiatry began very suddenly when, during the First World War, one evening I heard a short lecture on Freud's theory of dreams, given by a lecturer at Manchester University.¹² The occasion was an informal one when there was a break in the routine on the Ambulance Train in Northern France on which I was then working. I was astonished to hear that some fairly reasonable explanation could be given of the apparently disordered sequence of ideas in the nocturnal theatre with an audience of one. And I decided then, if possible, to give up mathematics and to study something more exciting. When the war ended and I went to Cambridge, I tried to study in this new field but it had not penetrated into the University curriculum. The nearest possibility was psychology and this was linked to philosophy and mathematical logic in the cumbersome academic configuration known as the Moral Sciences Tripos.¹³

Coming up to St John's College, Cambridge, in January 1919, he recalled at some point in that year walking down a Cambridge street and running into John Rickman, whom he knew through the Quaker school, Leighton Park, they had both attended. Rickman had just started work as a psychiatrist at Fulbourn Hospital and was full of stories about 'the new psychology' and his plans to go to Vienna. Some five months after arriving in Cambridge, on Freud's birthday, 6 May 1919 (a date only an extremely thoroughly committed Freudian would choose), Penrose gave a talk to a society entitled 'Treatise concerning dreams and Prof. S. Freud's theories'¹⁴ - thoroughly well-informed about Freudian theory, to the extent of defending the thesis (not Freud's, but commonly understood by Rivers and others to be his) that all dreams are sexual. Going through the motions of a Cambridge degree - disappointed by the absence of Russell and the tedious lectures of Whitehead substituting for him (enlivened only by the presence of the one woman in the dozen or so students in the lecture-hall)¹⁵ -

11. For general accounts of Penrose's life and work, see the very well-researched and contextualized account by Kevles (1985), for whom Penrose is the central figure in the turn against eugenics within science; see also Barkan 1992, pp. 260-6; Harris 1973; Penrose, O. 1999. Kevles's account, to which we are greatly indebted for its later account of Penrose's life and work, is marred by a singular blind-spot concerning Penrose's interest in psychoanalysis.

12. Quite probably T.H. Pear, later Professor of Psychology at the University of Manchester, author of *Remembering and Forgetting* (1922), mentioned by Jones to Freud before World War I as a Manchester psychologist sympathetic to psychoanalysis, and author of 'The analysis of some personal dreams with reference to Freud's theory of dream interpretation' *Br. J. Psych. Vol. VI*, Part 3, 4 Feb. 1914.

13. Penrose Papers 20/2 'Memoirs - Lunacy', pp. 100/4-101/5.

14. Penrose Papers 21.

15. Penrose Papers 20/2 'Memoirs - Lunacy', p. 106/10.

Penrose did, however, meet and spend time with W.H.R. Rivers (recently returned to his Fellowship at Penrose's College, St John's), without ever becoming close to him;¹⁶ he attended the lectures he gave in 1920-1921 which became the posthumously published *Conflict and Dream* (see Slobodin 1978, p. 74). It should be said, however, that Penrose's memories of his undergraduate studies being entirely devoid of psychoanalysis were not completely accurate: on 4 June 1920, when sitting Part I Examinations, Question 8 read: 'In what precise sense, if at all, should reference to the Unconscious be admitted in psychological analysis?' And the next day, another examination proposed to him an essay on 'Instinct and the emotions'.¹⁷

Graduating in 1921, Penrose remained in Cambridge, attached in a vague way to the Psychological Laboratory. 18 As he himself recounted his career, when elected F. R.S. in 1953 (and, like all newly elected Fellows, having been asked to give a confidential account of his early development and career - a formality to which each new Fellow is asked to submit, knowing that the material culled will later be used for the extensive obituaries the Royal Society publishes):

After taking my degree at Cambridge I spent one year of psychological researches with F.C. Bartlett and then went off to continue postgraduate studies at Vienna. I conducted some experiments on memory & perception in Prof. Buhler's laboratory. What was for me much more important was the opportunity to meet S. Freud and other psychiatrists including Wagner-Jauregg and Paul Schilder. I was at that time writing a thesis on the psychology of mathematics but it was a failure.¹⁹

The chronology of these years is difficult to ascertain: Penrose informed Roazen that he first went to Freud, who referred him to Siegfried Bernfeld for analysis - Bernfeld spoke in German, Penrose in English and the analysis lasted about a year. He attended meetings of the Vienna Psycho-Analytical Society.²⁰ Penrose's notebooks for this period - from 1920 to 1925 - range widely, from

16. Penrose Papers 20/2 'Memoirs - Lunacy', p. 102/6.

17. Penrose Papers 22. The examination asked candidates to answer up to five questions; Penrose ticked four questions and placed a question mark against the question on the Unconscious.

18. It should be remembered that Cambridge did not introduce a formal postgraduate qualification (the Ph.D. degree) until 1919.

19. Penrose Papers 20/4. That this project was redolent of significant future research projects is clear to any reader of Penrose's notes from this era who remembers how Alan Turing hard-wired the first computer to represent specific arithmetical operations in base 32. A later reader of the notebooks, almost certainly Penrose himself, probably thought so as well: there are a number of comments in biro (certainly not invented in the 1920s...!) in the passages concerned with logiconeurology, including '(later 1969!)'.

20. When Paul Roazen interviewed Penrose in 1965, Penrose told him that his analyst was Bernfeld (personal communications to JF, Paul Roazen, e-mail, 5, 6 and 13 February 2000). He also informed Roazen that he bought from a bookseller the psychoanalytic library of Herbert Silberer, who had committed suicide on 12 January 1923, and whose widow had sold his library to the bookseller. Penrose's manuscripts mention Bernfeld by name on a number of occasions when discussing associations to dreams and chess problems.

neurology, logic, psychological experiments, the mathematics of growth, chess problems and psychoanalytic metapsychology. His heart may have been with psychoanalysis, but his inquiring mind was as ever in many other places.

Penrose appears to have been commuting between Vienna and Cambridge. In July 1923, he completed one 'dissertation' - possibly an unsuccessful Fellowship Dissertation - entitled 'The fundamental nature of neurotic reasoning'.²¹ Penrose was extremely prolific in producing such papers. In lighter mode, some time in 1923, Penrose collaborated with a friend he would see frequently in Belgrade, A.J.C. Brown, on a poem they called 'The Neurotic's Progress'. In December 1923 he wrote 'A formulation of the fallacy in neurotic reasoning',²² an attempt to show the logical structure of obsessional neurosis, based on Freud - teasing out the original error and mistaken beliefs constituting the neurosis. In August 1924, Penrose was attempting to put these papers together under the title 'Three papers on psycho-analysis and formal logic': (1) A formulation of the fallacy in neurotic reasoning; (2) The intellectual aspect of the psycho-analytic cure; (3) The elements of the psychology of mathematics.²³

As he noted in the introduction to these papers, the first two apply logic to psychoanalysis, whereas the last applies psychoanalysis to 'the study of formal logic and mathematics'. These were, he noted, written over a period of months and may show changes since 'during the whole time I was intensively studying the subject of analysis and improving my acquaintance with it'.²⁴ The major influences on him at this point are also clearer: the first paper was being entirely rewritten in the light of comments and criticisms by readers, 'who include Dr Ernest Jones, to whom my special thanks are due'.²⁵ The third paper, the heart of his 'failed' doctoral dissertation, arose when it:

was suggested to me by several people some two years ago that the psychology of mathematics was an interesting and practically unexplored field of research. I thought then that it would be quite impossible to make such a study, as the mathematical activity seemed to be carried on at such a deep layer of the mind and yet to be so abstract and mysterious that the mental processes concerned were quite invisible. Two factors have helped to throw light on these processes though. One is a study of the work of Wittgenstein who, carrying Russell's ideas about mathematics to their logical conclusion, produces a metaphysical view of mathematics and formal logic which reveals to some extent its psychological structure. The other factor which, in connection with the one I have mentioned, has revealed a sufficient amount concerning the workings of the mind of one who studies mathematical logic, is the fairly thoroughgoing psychoanalysis of myself, to which I have been sufficiently inquisitive to be subjected. The

21. Penrose Papers 24.

22. Penrose Papers 24.

23. Penrose Papers 26.

24. Penrose Papers 26, 1.

25. Penrose Papers 26, 2.

important point in the whole paper, to my mind, is just the explanation of how the obsessional interest in tautology develops ...²⁶

It is quite likely that Ramsey was the person who suggested this project to Penrose. Certainly the growing influence of Wittgenstein is visible in Penrose's notebooks at this time and Ramsey, as translator of the *Tractatus*, would be very closely associated with Wittgenstein at this time.²⁷ In another paper from April 1925, entitled 'On the applications of elementary logic to some problems in psychology',²⁸ Penrose described how his original project of constructing a mechanical or electrical machine that would do logical work in the same way a slide rule does arithmetic had grown less interesting to him since he found out that he was 'anticipated in some of these inventions by no less an authority upon logic than Wittgenstein himself. My reason for discontinuing these researches lay in the discovery that an excellent machine had already been prepared on similar lines for solving these very problems in formal logic. I refer to the invention of God, the human organism. And in this paper I propose to discuss the way in which this particular apparatus for solving logical problems works.'²⁹

In his own personal analysis, Penrose had discovered a decidedly unconventional method of work, and it was the fruits of this personal style of analysis that he reported on to the Cambridge psychoanalytic group at their first meeting in March 1925 in a paper entitled 'Psychoanalysis and chess'. At one point in his analysis he asked Bernfeld what could be the cause of his strong interest in the game of chess, to which Bernfeld had replied that 'he probably projected his infantile family conflicts onto the chessboard'.³⁰ Soon after this exchange, Penrose had a dream 'whose manifest content is the attitude of the dreamer towards the position of certain pieces upon a chessboard'.³¹ (See Figure 2, p. 202 and the dream text beneath it.) The position represented a dramatization or pictorialization of the dynamics of his family conflicts: on one side were the Black King (his father), the Black Queen (a governess who had taught him to read) and himself (a Pawn); on the other side were the White King (his grandfather, Lord Peckover), the White Queen (his mother) and a series of Pawns (representing his brothers) and Knights (representing himself and his brothers): 'the Black forces are composed of the unpunctual people [much

26. Penrose Papers 26 'Introductory note' to 'Three papers' 3-4.

27. On Penrose's death, David Garnett, writer and member of the Bloomsbury Group, wrote a letter to *The Times* with his memories of the distinguished scientist, whom he had first met in 1923, noting: 'At Cambridge he discovered that Frank Ramsey, then a fellow undergraduate, had the most remarkable mind in the university and he was profoundly influenced by him' (*The Times*, 22 May 1972). Thus Garnett placed the beginnings of the friendship between Ramsey and Penrose in the period 1920-1921, when they were both undergraduates.

28. Penrose Papers 27, dated 9 April 1925.

29. Penrose Papers 27 'On the application...' f. 1.

30. Penrose Papers 5/1 'Psycho-Analysis and chess', March 1925, 18 f., f. 2. 31. Penrose Papers 5/1 'Psycho-Analysis and chess', March 1925, 18 f., f. 1.

criticized by the grandfather]: agreeing with the rule that White moves first'. The grandfather also held power over the Black King through his wealth: 'in the action of the problem as in real life, the White Queen was to come from the White King and give "cheque" to the Black King'.³²

The most forcefully dynamic aspect of the dream's latent thoughts, however, attached to the thought the dreamer had that '*in two cases the nature of the mating move is actually known to me*'. This referred to his knowledge of the sexual relations between his father and mother, reaction against which was, in Penrose's eyes, the most powerful source of the dream. The 'mating position' in the dream depended upon the actions of the Black Pawn, himself. It was thus, he concluded, a dream of omnipotence, a dream that he could affect the 'mating' of the King and Queen, whereas in reality he had no such power. As he remarked, his youngest brother Bernard (born in 1903) was not yet born at the time of the events associated with the formation of the dream (though Roland, born 1900, was in the world).³³

After this thinly disguised account of his own analysis, Penrose passed on to a more general consideration of psychoanalysis and chess under a series of rubrics which he had outlined succinctly in a notebook:

1. A homosexual activity
2. A sadistic activity
3. Masochistic activity, & castration complex
4. An Anal Erotic Activity
5. Another sexual satisfaction given by chess play
6. Examples from Books & Papers
7. Chess as oral activity
8. A chess problem as a dream of family conflict

Penrose was not the first to consider chess from a psychoanalytical point of view. On 15 March 1922, a dentist by the name of Dr Fokschaner gave a paper 'On the game of chess' to the Vienna Psycho-Analytical Society, followed by a discussion led by Freud, Bernfeld, Federn, Kolnai and Schmiedeberg.³⁴ According to Willi Hoffer, Fokschaner drew a parallel between chess and obsessional neurosis and attempted to interpret symbolically the pieces and their movements on the chessboard; according to Kenneth Colby (a pioneer in the 1950s of artificial intelligence, computer chess and a computer program simulating psychotherapy), Bernfeld recalled how Fokschaner was heavily

32. Penrose Papers 5/1 'Psycho-Analysis and chess', March 1925, 18 f., f. 4.

33. The following may also be relevant: in his confidential autobiography for the Royal Society, he noted the date of marriage of his parents, his own date of birth and then gave a list of the birth of his siblings and himself as follows: 'Male stillbirth 1894/ Male Alexander, 1896, Fellow of King's, died 1950/Lionel/Roland, 1900, Art expert and critic/Bernard, 1903, Mariner, Lt. Commander in Navy'.

34. Int. J. Psch 3 (1922): 137.

criticized by Freud for his simplifications (Reider 1959, p. 323). How Bernfeld's response to Penrose's demand for a psychoanalytic understanding of his own interest in chess might have been informed by Freud's comments is a matter of speculation; the next contribution to consider chess in the light of psychoanalysis - always regarded as the first major contribution in this area, since Penrose never published his paper - was written by Ernest Jones in 1930 (Jones 1931 a).

Throughout Penrose's notebooks from this period are scattered chess diagrams and notes on problems. These would not be surprising, given his early interest in chess - he even had chess problems published when still a boy. His grandfather, Lord Peckover, was a keen chess-player and the game was one of the few less than serious activities allowed on the Sabbath. Yet it is highly probable that Penrose was doing more than explore the source of his own interest in chess through dream analysis; he was conducting a substantial portion of his analysis in the form of this curious method of dream and free association. The dream was only one in a series of notes on chess positions and problems which, as it were, were being formulated for their analytic utility. Thus in his notebooks,³⁵ there are somewhat different associations than those in his formally delivered paper, with the names of women currently preoccupying his erotic imagination. And there are a series of other chess problems to which Penrose adds comments like: '(I am composing the problem myself this time)/Dream problem in the making/ I first compose with bishop on g2 and then change it to e4'.³⁶

While in analysis in Vienna, Penrose's interests shifted to the abnormal mind - as he described in his notes for the use of the Royal Society:

My interests swung to the problems of the abnormal mind which seemed to offer so much opportunity for the enquirer. I determined to start afresh and to take a medical degree so that the field of abnormal psychology would be open to me. [Adds with asterisk at foot of page:] *I was much encouraged in this aim by my discussions with A.G. Tansley. So I returned to Cambridge and took the 1" and 2" M.B. examinations.*³⁷

The first paper Penrose published was entitled 'A note on the relation of rate of growth to structure in plants'. It appeared in the *New Phytologist* in late 1925, the journal Tansley had founded with his own money in 1902 and edited until 1931. It was a much truncated version of the paper Penrose delivered to the psychoanalytic group on 20 May 1925 (see below), which he also delivered to the British Psychological Society, Medical Section (of which Rickman was Secretary), on 27 May 1925. This paper, 'The relation of the pleasure-pain

35. And there are at least two notebooks in which are found associations and analysis of the chess dream discussed in his paper.

36. We do not have sufficient space here to devote to a detailed discussion of the development of Penrose's interest in psychoanalysis, his method of working with chess in his analysis and the main themes of his inner life, particularly on the question of career-choice, all of which we will discuss in a future publication.

37. Penrose Papers 20/4.



Figure 1. Sir Arthur Tansley with colleague A.S. Watt on an East Anglian heath in the summer of 1949: detail reproduced from L.J. Watson's painting 'The Naturalists' courtesy of the Department of Plant Sciences, University of Cambridge.

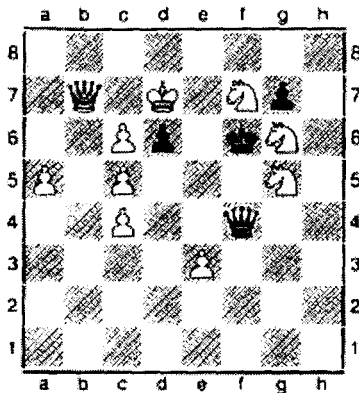


Figure 2. Penrose's Chess Dream

I see before me a certain chess position. It is a problem. White is to checkmate Black in two moves. The location of the pieces is not however quite settled, and I feel as though there may be a misprint in the case of the Black Queen. The White Queen ought to be 'pinning' her, I think, in order that the White Pieces should succeed. As it is, too many moves of Black are unprovided for. But in two cases the nature of the mating move is actually known to me. If the Black Pawn (on the square d6) takes the White Pawn (on c5) I know that the White Queen can then checkmate by moving down to the square b2. If the same Black Pawn simply chooses to move on to d5, the White Queen will mate somewhere else.



Figure 3. Dorothy Wrinch on the roof of the Meteorological Office, London, 1919; photo by Harold Jeffreys, courtesy of St John's College, Cambridge.



Figure 4. E. Pickworth Farrow in Breckland, ca 1916; photo by Harold Jeffreys and entitled 'An Overworked Bike', courtesy of St John's College, Cambridge.

principle of Freud to the question of growth', was far more concerned with the mathematics of growth than with Freud's metapsychology. Penrose was attempting to establish equations regulating the rate of growth in a variety of organisms, and drew on data from Quetelet's statistics of human weights and heights, rates of growth of leaves and plants, and, finally, the eugenicist Goddard's figures comparing the growth of normal and defective children - data supplied to Penrose by Rickman in a letter two days before he completed the paper.³⁸ The next paper Penrose published was 'Some experiments upon inhibition and suggestion', in 1926 in the *British Journal of Psychology*, of which Rickman was Assistant Editor. On 3 February 1926, Penrose gave a talk to the British Psycho-Analytical Society on 'Psychoanalytic notes on negation' (Bryan 1926b) and a revised version (Penrose 1927) appeared in the *International Journal*.

The immediate stimulus for this paper was the publication of Freud's remarkable four pages on 'Negation' in 1925 but, as we have seen, Penrose had been thinking about the relations between logic and psychology since he was an undergraduate imbued with Freud's theories. Just as Penrose had supplemented D'Arcy Thompson's account of the mathematics of plant growth, so he supplements Freud's characterization of the 'not' of negation as a sign of repression, its trademark. There is a comparable process of assertion as a sign of the unconscious, captured in the logical notation introduced by Russell and Whitehead, 's'. 'It is only when there is no repression in connection with the subject matter of a thought stated that neither assertion nor negation are necessary: all that need be expressed then is acquiescence, and this is done by affirmation' (Penrose 1927, p. 49).

The sole fruits of Penrose's attempt to develop a psychology of mathematics to be published found their way into this paper: 'The laws of formal logic are in fact symbols for the processes which allow or prevent thoughts entering the conscious mind in its normal state (i.e. preconscious mechanisms)' (Penrose 1927, p. 50). And he characterizes the essential abstinence of mathematical and logical thought from assertions of content (following Russell and Wittgenstein) as a loss of contact with the external world: 'it can equally well be used for building up a systematic paranoiac illusion as to help in the understanding of reality. And it is based solely upon the interaction of the two ideas, assertion and negation, which we have seen represent pleasure and pain'. Thus he is led to a disputed metapsychological point: 'I do not think Alexander is correct in stating that the laws of logic are copied from the laws of nature, and that they represent a fragment of introjected reality. It would seem to be more accurate to say that they are projections of the mechanisms by which the mental apparatus functions' (p. 51).

38. Rickman to Penrose, 18 May 1925: 'I enclose the book references that I spoke about yesterday, and a few silly little diagrams of the weights of lunatic children [drawn from Goddard's 1912 paper]', Penrose Papers 29.

The place of the erotic in reasoning, Penrose argues, is to be found in induction, not deduction - in risking *not* finding the world to be as thought assumes it to be: 'the Ego reasons deductively and the Id inductively... while negation represents a complete withdrawal from reality, assertion is indicative of the aggressive, sadistic attitude towards external objects. Both states show a disharmony between the conscious and the unconscious, whereas affirmation is the mark of harmony between these two' (p. 52).

After Cambridge, he moved to St Thomas's Hospital, London, for clinical work, taking up residence in Bloomsbury. Some sense of how Penrose initially conceived of combining objective scientific methods and psychoanalysis can be gained from his account of data-gathering at this time:

When attending hospital out-patients as a medical student I spent a little time enquiring into the dreams patients experienced under nitrous oxide when some minor operation was performed. The idea was to discover whether there was any regularity or uniformity to be found. I observed a frequent tendency to identify the whole body with the part which was operated upon. For example, a dream, during dental extraction, that the patient was being pulled out of a ditch ...(Penrose 1953, p. 79)

Penrose became an Associate Member of the British Society on 17 February 1926. When the Institute of Psycho-Analysis opened in 1926 - amidst rebuilding works, John Rickman ceremoniously listened to the first patient on Freud's birthday - Penrose was amongst the first to be appointed as a Clinical Assistant permitted to conduct analyses under supervision (Freud, A. 1928, p. 147); this entailed devoting five hours a week to the analysis of one patient. Again, it was Rickman who acted as go-between between Ernest Jones and Penrose in this appointment.³⁹ Penrose was supervised by Ernest Jones,⁴⁰ and it may well have been on these occasions that Jones communicated his view that Penrose had too many other interests besides psychoanalysis, a view with which Penrose agreed.

Penrose's initial experiences with his clinic patients were not entirely happy.⁴¹ His first, a newly qualified doctor from Ireland with a severe stammer, soon failed to turn up to his sessions. (Penrose noted after the first session: 'He is Protestant, but [sets] great store upon the Catholic 'confuc-fuc-fuconfessional' 4²) His second patient, a married woman, came to one session and then refused to come to any more, on the grounds that she had caught a cold in the draughty consultation room: 'If it is really necessary for me to lie for an hour in a complete draught at every treatment, I am afraid I shall be unable to attend again'.⁴³ And to the Treasurer of the Institute she reported: 'I mentioned the

39. Jones to Penrose, 18 October 1926, Penrose Papers 46.

40. Penrose to Jones, 30 December 1926, Penrose Papers 46.

41. Penrose interview with Paul Roazen, 1965, personal communication with Paul Roazen, 5,6 and 13 February 2000.

42. Penrose Papers 46.

43. Mrs R to Penrose, 2 February 1927, Penrose Papers 46.

discomfort of the draught to Dr Penrose, but he did not see his way to alter the conditions'.⁴⁴ Whether any other of Penrose's analyses were more successful we do not know.

He completed his medical degree in 1928 and then took a position at the City Mental Hospital in Cardiff (while retaining a London address), where he completed a thesis for the M.D. centred on one elderly man diagnosed with schizophrenia. He published a paper concerning this man in the *British Journal of Medical Psychology* in 1931 (Penrose 1931 a).

Penrose did not pretend to 'treat' this old man psychoanalytically. His paper portrayed it as a sufficient triumph that an 80-year-old man who had been in mental institutions for 50 years could, firstly, given his complex delusional system, be understood; and, secondly, that this successful act of comprehension demonstrated that the man's mental powers had in no sense degenerated. Penrose's paper analysed the complex delusional system, including its new language, calendar and system of measurements of time and distance. And, he noted, it was this delusional system that kept his mind alive, very much akin to a philosopher or abstract scientist.

When Penrose considered the causation of the patient's mental illness, he dismissed the factor of heredity, noting - and foreshadowing his later life-work - that, if insanity were a recessive Mendelian trait, 'we should expect to find the taint on both sides of the family' (Penrose 1931a, p. 28). (As we have seen, Penrose's analysis with Bernfeld had alerted him to his own preoccupation with the different legacies of the two sides of his own family.) Having shown, in classic psychoanalytic style, such impatience with the doctrines of heredity and degeneration, he located the precipitating cause of the illness in the patient's early sexual history: an affair with a previously childless landlady which led to a pregnancy and stillbirth, combined with an episode of gonorrhoea, seemed adequate in Penrose's eyes. While concluding that this was not a *sufficient* specific cause of the illness, and that a set of predisposing causes was necessary, he pointed out that such an aetiology was entirely analogous to the onset of infectious diseases. What is more, he modelled his account of the case on Freud's reading of Schreber, even to the point of seeking to locate the onset of the delusions in the repression of homosexuality and the patient's regression to masturbatory fantasy, in which anal and oral erotisms were dominant under a regime of primitive magical thinking.

Perhaps proud of his remarkable achievement in this limpid and sympathetic understanding of a man locked away in an asylum since 1875, long before psychoanalysis was dreamt of, Penrose sent this paper to Freud, who replied courteously (and in German).⁴⁵

44. Mrs R. to Treasurer, 5 February 1927, Penrose Papers 46.

45. Freud (1931): Postcard dated 18 May 1931 to 35 Lexden Road, Colchester, England. 'Dear Dr Penrose, Danke Ihnen sehr für den Sonderabdruck. Der Fall ist doch sehr lehrreich und es ist wieder einmal gut zu sehen wie eine Lebensgeschichte erst verständlich wird, wenn man das Sexualeben erforscht, Ihr ergebener Freud' - Sigmund Freud Archives B-17 Lionel Penrose.

Dear Dr Penrose,

Thank you for your offprint. The case is really quite educational and it is once again good to see how a life-history first becomes understandable once one investigates the sexual life.

With respects,
Freud

However, in early 1931 Penrose had published a paper which returned to one of his initial interests: the theoretical status of the biological principles underlying psychoanalysis, in particular, that of pleasure-pain. He was contributing to a debate sparked by the attempt of his own analyst Bernfeld together with the physiologist Sergei Feitelberg to integrate Freud's theory of instincts and particularly of the death instinct with the physical sciences, giving a physical interpretation of the notion of 'psychic energy' and attempting to equate the death instinct with the second law of thermodynamics (the tendency of entropy to increase) (Bernfeld & Feitelberg 1931). The other English commentator on their paper, Reginald Kapp (of whom more will be said below), was scathing in his dismissal of the elementary errors in physics, chemistry and biology committed by the Viennese Freudians (Kapp 1931). Penrose's verdict on their theory was equally negative: the *rapprochement* with physics was preposterous and their interpretation of biology was deeply flawed and, in addition, considerably different from Freud's, which had maintained a more appropriate tentativeness. Penrose's undoubted respect for Freud had not hindered him concluding that:

the attempt to shew that Freud's death instinct is the same as the second law of thermodynamics is rejected. It is concluded that if Fechner's principles of stability are made the basis of a theory of pleasure and pain the death instinct becomes of theoretical interest only. (Penrose 1931, p. 97)

Both Kapp's and Penrose's papers were much quoted in later psychoanalytic literature as sounding the death knell, firstly, for any *rapprochement* between the notion of energy as employed in metapsychology and that employed in thermodynamics, and, secondly, for the biological coherence of the death instinct.

On 21 October 1931, Penrose gave a further paper to the British Psycho-Analytical Society, entitled 'Recent psycho-analytical research in the psychoses', which consisted in 'a review of work done in the period 1926-1931' (Glover 1932) - quite clearly a supplement in the most literal sense to John Rickman's magisterial *The Development of the Psycho-Analytical Theory of Psychoses, 1893-1926* published in 1928. One has the sense that Penrose was still following in Rickman's footsteps. But it was to be his last psychoanalytic foray for many years.

Penrose family stories tell of a vague and tragic love affair sometime in the 1920s, which ended traumatically with the mental illness of the woman; Penrose

is said to have moved more towards a biological view of abnormality. Although the evidence for this is by no means clear,⁴⁶ it is certain that in the period 1928-1931, his personal and professional lives were transformed radically. In 1928 he married Margaret Leathes, the daughter of a distinguished physiologist; they were to have four children, the first born in 1929.⁴⁷ In 1931, Penrose took up a post that was to re-orient his scientific interests and produce the distinguished work on mental deficiency, hereditary factors in disease and mental illness and the study of human genetics for which he became famous. He was appointed Research Medical Officer, sponsored by the Pinsent-Darwin Trust, at the Royal Eastern Counties Institution at Colchester, a large asylum devoted to the mentally defective. Within months of his arrival he was publishing studies on the heredity of mental defects, in particular in 'mongolian imbeciles' - a term which his work would do much to make archaic. Employing Mendelian genetics, painstakingly gathering enormous amounts of information about the family backgrounds of the patients in the Institution, he constructed a vision of what a truly scientific account of the factors - hereditary, congenital, environmental - in mental deficiency, mongolism and rarer defects (such as phenylketonuria) could look like. Based in Colchester till 1939, when he became Director of Psychiatric Research in Ontario for the duration of World War II, he carved out a singular identity as a human geneticist, chromosomal researcher, statistician *extraordinaire* and severe (while never polemical) critic of the wild eugenics of the first third of the twentieth century.

Yet, even in this work, there are hints of his Freudian past. In his 1933 book *Mental Defect*, when discussing the supposedly abnormally strong sexual drives of the retarded, he argued:

It is a well-known psychological mechanism that hatred, which is repressed under normal circumstances, may become manifest in the presence of an object which is already discredited in some way... The greatest psychiatrist of modern times, Sigmund Freud, has pointed out very clearly that some of the most serious troubles affecting civilization come from man's imperfect mastery over his aggressive impulses against his neighbour.

46. See Kevles 1985, p. 154, 340 n. 20. As if anticipating such an account, the manuscript which Penrose entitled 'Memoirs 2 - Lunacy' addresses the place that lunacy had played in his life, starting with his experience at the Cardiff Hospital and immediately asserting, following Freud, the comprehensibility of the symptoms of mental illness, and then moving back in time to his own close relationships which had been marred by mental illness: the brother of a close friend who had a breakdown, the wife of the same close friend who turned out to be schizophrenic and, finally, the woman he befriended in Whitehead's lectures, Molly, who, in the period 1919-1920, was clearly an intimate friend and perhaps a lover, and with whom Penrose kept in contact, and who succumbed to mental illness many years later (no earlier than the late 1930s, it is reasonable to estimate). These distressing stories of lunacy fade into the shadows once the bright sunny figure of John Rickman enters into his story.

47. Two of whom (Oliver and Roger) became distinguished mathematicians, one of whom (Jonathan) was ten times British Chess Champion, and the fourth (Shirley Victoria) became a genetic paediatrician. Kevles's interviews of Margaret Penrose Newman (as she became after Penrose's death) indicate her attitude to psychoanalysis was distinctly sceptical of, if not hostile towards, what she regarded as the analysts' self-importance. (See Kevles 1985, esp. 340 note 19.)

An excuse for viewing mentally defective individuals with abhorrence is the idea that those at large enjoy themselves sexually in ways which are forbidden or difficult to accomplish in the higher strata of society. The association between the idea of the supposed fecundity of the feeble-minded and the need for their sterilization is apparently rational, but it may be emphasized by an unconscious desire to forbid these supposed sexual excesses. It has been pointed out that the advocates of sterilization never desire it to be applied to their own class, but always to some one else. (Penrose 1933, pp. 172-4; cited in Kevles 1985, p. 108, and Gosselin 1935)

But Penrose's activity was now lodged elsewhere, including developing (with J.C. Raven) a new set of psychological tests, eventually known as the Penrose-Raven Progressive Matrices Test, which were eventually made standard in the British Army during World War II by John Rickman (Penrose & Raven 1936; see King 1989, p. 24). In 1940, Penrose was corresponding with Rickman about another set of tests for schizophrenia that he was formulating.⁴⁸

When Penrose returned to Britain in 1945 he was elected Galton Professor of Eugenics at University College London, where he created the leading post-war centre for human genetics research. He never cared for the name 'eugenics' with its associations of racial purification and, until he succeeded in having the name changed to the Galton Professorship of Human Genetics in 1963, he used notepaper titled simply 'The Galton Laboratory, University College' (Harris 1973, pp. 537-8). Working on statistical studies of heredity, including sceptical studies of the efficacy of psychiatric treatments (e.g. shock therapy), on the biology of mental defect (the title of his 1949 book), he remained an Associate Member of the British Psycho-Analytical Society till his death in 1972, and accepted an invitation in 1947 to give a paper to the Society, sandwiched between Melanie Klein on schizoid mechanisms and Donald Winnicott on hate. His title was 'Psychoanalysis and experimental science' (Penrose 1953).

Voicing more clearly the same muted scepticism concerning the fundamental biological and physical principles of Freud's metapsychology that he had expressed in his 1931 paper, he nonetheless declared himself, once again, convinced of the fundamental revolutionary character of Freud's scientific work:

Freud's greatest contribution to psychological medicine has practically nothing to do with theories of mental energy, mental philosophy and metapsychology. It was a revolution in thought comparable in its effects to the discoveries of Darwin or Copernicus... There should be no need for me to discuss the nature of this nuclear discovery from which the recognition of repression, infantile sexuality, and the Oedipus complex followed. Suffice to say that it was the realization (1) that the phenomena of hypnosis, suggestion and so on were fundamentally sexual, in a word, that the patient obeys because he is in love with the doctor, and (2) that this neurotic love is itself a morbid symptom, which can be subjected to psychological analysis in terms of conditioned associations and ultimately resolved by a process of re-education. The analysis of transference is the key to Freud's contribution to therapeutics. It is this which differentiated his method from that of his teachers, Charcot and Bernheim, and which should differentiate present psychoanalytical from other forms of psychotherapy. It is

48. Penrose to Rickman, 11 February 1940, Penrose Papers 165/3.

perhaps unfortunate that the discipline was named psychoanalysis and not transference analysis. Much misunderstanding would thereby have been avoided. (Penrose 1953, p. 75)

Having delivered this scientific *credo*, he gave his audience a sense of how he had kept alive his connection to psychoanalysis while he became involved in biological research that was increasingly remote: 'For many years I have tried to keep a watch on the developments in fields related to psychoanalysis in order to see how new information would help to strengthen or modify the assumptions inherent in psychoanalytic theory. It is difficult to present these points in any very coherent order since the original compilation is of the nature of a mental scrapbook' (p. 75). Penrose then went on to survey a number of fields of investigation which touch on psychoanalysis, and to which psychoanalysts should either take note or contribute: the artificial cathartic cures (narcoanalysis, drug-induced convulsions); conditioning experiments in animals and humans; the relation of hormone chemistry and genetic variations to sexuality and sexual types, including the differential distribution of characteristics associated with males or females to the neuroses and psychoses; the experimental study of dreaming; the study of smell in relation to the unconscious; the part played by genetics in determining mental illness, including such factors as the choice of mate; the possibility that the superego may be localized in the prefrontal lobes. He concluded with a rousing call to analysts not to rest on the laurels of their founders: 'In science, we cannot go wrong so long as we stick to the facts' (p. 81). His final call was: 'I should like to see every patient given a battery of psychological and physical tests before beginning treatment, and perhaps afterwards as well - children not excepted. But then, I am not a practising psychoanalyst; if I were I might think differently' (p. 81). One imagines that Penrose knew just how far he had come since starting out as a practising analyst. Yet he was quite happy to let his name be used on an appeal for the new building acquired by Rickman for the British Psycho-Analytical Society and Institute in 1950, alongside those of Dr J.C. Flugel, Dr William Gillispie (its Chairman), Dr Ernest Jones, Sir Arthur Tansley and Professor F.R. Winton (Cameron & Forrester 1999, p. 76).

Penrose and Rickman also maintained their close friendship - in much the same style as in the 1920s perhaps. Penrose's career had been, as we have seen, very much in Rickman's footsteps up until the early 1930s. In the 1930s, following the Einstein-Freud published correspondence *Why War?*, Rickman, Penrose and others (including Adrian Stephen, Norman Glaister, John Bowlby, J.C. Flugel and Charles Madge) formed the Psychologists' Peace Society, which also published a Medical Peace Campaign Bulletin. (Penrose was instrumental in founding a similar post-war organization, the Medical Association for the Prevention of War, in 1951, at the time of the Korean War, to which he devoted much time and energy in the 1950s and 1960s.)⁴⁹ In 1946 Rickman offered to

49. Norman Macdonald, 'Lionel Penrose' *Medical Association for the Prevention of War Proceedings* Vol. 2, Part 5 (1972), pp. 117-22.

have Penrose elected a Fellow of the British Psychological Society.⁵⁰ Penrose read the proofs of the last paper Rickman wrote before his sudden death in July 1951.

In a footnote to this long and striking paper on the organization of psychoanalytical societies, Rickman's discussion of the work of committees referred to Penrose's observation that:

in respect to the Unwritten Agendas, that the total number of topics of discussion including the Written Agenda is two to the power n , where n is the number of Committee members who stay awake. The practical importance of this formulation is that it gives a hint, perhaps a strong hint, that the enlargement of a small committee even by one member produces a greater effect than arithmetical proportions would suggest. (Rickman 1951, p. 230, n. 4)

This seemingly whimsical note was closely related to the work Penrose was then engaged on, *On the Objective Study of Crowd Behaviour*, completed in May 1951. In the Introduction to his book, Penrose referred to Rickman's recent study, 'The factor of number in individual and group dynamics', published in 1950 (itself a part of a series of papers concerning number and groups⁵¹), which viewed Freud's Oedipus complex as having the characteristics it does have because it is a 'three-person relationship'. Hence Rickman introduced the notion of two- and three-body psychology - to be taken up by others, such as Michael Balint (1951). It was this notion of the importance of number that led Jacques Lacan to call Rickman 'one of the rare souls to have had a modicum of theoretical originality in analytic circles since Freud's death' (Lacan 1988, p. 11). While it was Bion, Rickman's other close collaborator in thinking about group dynamics, who was to have most influence, it is clear that Rickman and Penrose were thinking on very similar lines and sharing their ideas with one another. Penrose's limpid and original little book still incorporated psychoanalytic ideas: in its final few pages he cited Freud's account of the repressed homosexuality that lies behind paranoid delusions, indicating that it is this force 'which binds together members of the same sex into groups which follow a paranoid leader' (Penrose 1952, p. 62).

Penrose remained an Associate Member of the British Psycho-Analytical Society to his death. However, from 1960 onwards, a curious footnote was attached to his name in the roster - 'Not eligible for Associate Membership of the International Psycho-Analytical Association.'⁵² The reason for the sudden

50. Penrose Papers 165/3.

51. Collected together after his death in Rickman (1957), in particular 'Methodology and research in psychopathology' (1951) and 'Number and the human sciences' (1951). A roneoed version of 'Number and the human sciences' which Rickman had made up for use of the British Psycho-Analytical Society members is also in the Penrose Papers (54/3), though there are no annotations on it.

52. Pearl H.M. King, 'List of Members of the Component and Affiliate Societies of the International Psycho-Analytical Association 1959-1960' *Bul. /nt. Psychoanal. Assn.* 41 (1960): 215-273, this passage from p. 239.

appearance of this forbidding qualification would have amused John Rickman, while it also indicates the scarcely visible trace of the historical episode we are here recounting. In 1958, the then President of the IPA, Heinz Hartmann, announced that 'the relationship of the associate members of Component Societies to the International Association has never been clearly outlined. The practice has been inconsistent. For years, the associate members of some Component Societies have paid dues to the International and have been regarded as its members; in others, they are not charged dues to our Association by their Component Societies and not considered as its members. I shall submit to your vote a change of statutes that tends to regularize this situation.'⁵³ Under the guise of an item concerning the correct dues to be paid to retain membership in the organization, an item that would be sure to send a large proportion of any Committee to sleep, the IPA then proposed a new regulation governing the membership by Associates of the IPA - a proposal that was adopted and incorporated into its Statutes in 1959: 'The Association consists of ordinary and associate members. Its ordinary membership is composed of the honorary and ordinary members of the component societies, whose election is therefore decided by the conditions valid for the individual societies. Its associate membership consists of the associate members of the component societies, but only in so far as the standing of an associate member in an individual society implies graduation from a recognized psychoanalytic institute'. Penrose thus found himself in the company of a very few associate members similarly excluded, such as Dr Josephine Stross (Freud's doctor, along with Max Schur, in the last years of his life), Mr Prynce Hopkins of Santa Barbara, California (an Associate Member of the British Society on account of the fact that he had donated a substantial sum to found the Institute of Psycho-Analysis in 1926), and Dr F.R. Winton, physiologist, who had been elected an Associate Member in 1923 and would remain one until his death in 1985.

The condition that made these relics of an earlier idea of a psychoanalytic society appear so anomalous was the clause that 'the standing of an associate member in an individual society implies graduation from a recognized psychoanalytic institute.' Stross, Hopkins, Winton and Penrose had close links to the psychoanalytic movement, but they had never 'graduated'. From its early conception as a sign of *scientific* interest in and affiliation with psychoanalysis (the early Statutes of the IPA had only mentioned Associate Members in order to assert that 'Associate Members of the Branch Societies have the right to be present at the Scientific Meetings of the IPA'⁵⁴), Associate Membership gradually came to mean trained but still junior member of a professional

53. Ruth S. Eissler, M.D., General Secretary, '113th Bulletin of the International PsychoAnalytical Association - Report on the Twentieth International Psycho-Analytical Congress', *Bul. Int. Psychoanal. Assn.* 39 (1958): 276-96, this passage from p. 290.

54. 'Statutes of the International Psycho-Analytical Association', *Bul. Int. Psychoanal. Assn.* 9 (1928): 156-9, Statute 4, Branches and Membership.

organization.⁵⁵ It may be an index of Penrose's tenacious allegiance to his first intellectual love and original choice of career that he did not take this downgrading of his status to heart and resign his membership. If we are looking for signs of a residual ambivalence in relation to Penrose's long-standing affiliation to psychoanalytic organizations, we may find it in the list he drew up for the Royal Society, in his confidential autobiographical description, of societies of which he was a member:

Scientific or partly scientific societies to which I belong include, British Psychological Society, Biometric Society, Pathological Society, American Psychiatric Association, Royal Medical-Psychological Association, Harveian Society, Cambridge Philosophical Society.⁵⁶

In this document drawn up in 1953, he singularly failed to mention his longstanding membership of the British Psycho-Analytical Society. And, unlike over his debt to Tansley, he failed to add a note at the bottom of the page correcting his oversight.

Frank Ramsey

Frank Ramsey, for many a quasi-mythical figure in twentieth-century British philosophy, was born in 1903 and died in 1930, having already made major contributions to probability theory, economics and the foundations of mathematics (see Mellor 1995). Ramsey, like his patron Keynes, came from a Cambridge family - his father was Master of Magdalene College and his brother eventually in the 1960s became Archbishop of Canterbury. As a second-year undergraduate in 1922, having learnt to read German in ten days, he became the principal translator of Wittgenstein's *Tractatus Logico-Philosophicus*, and then wrote in *Mind* one of the most perspicacious expositions and criticisms of Wittgenstein's argument. By 1923, he was helping Bertrand Russell revise *Principia Mathematica* in the light of criticisms made of it and was a friend of Keynes's (whose *Treatise on Probability* he also criticized in print, in effect destroying its central arguments, in the eyes of many, including Keynes himself). Ramsey spent the summer of 1923 in Vienna, where he met Wittgenstein, then teaching at a village school, and became one of his few friends. In part, as Strachey reported to Alix, because of an unrequited passion for Mrs Geoffrey Pyke, wife of the founder of the Malting House School managed by Susan Isaacs from September 1923 on (see Rickman 1950, p. 270), he was considering psychoanalysis. As he put it to Wittgenstein, in a letter dated 12 November 1923:

Most of my energy has been absorbed since January by an unhappy passion for a married woman, which produced such psychological disorder that I nearly resorted to

55. It may be noted that no other major Psycho-Analytic Society affiliated to the IPA had Associate Members to whose name this asterisk was attached (save one member of the Italian Society and all the Associate Members of the Indian Psycho-Analytical Society). Why the British and Indian Societies should have been distinctive in this respect is not clear.

56. Penrose Papers 20/4.

psychoanalysis, and should probably have gone at Christmas to live in Vienna for nine months and be analysed, had not I suddenly got better a fortnight ago... (Wittgenstein 1973, pp. 81-2)

Some sense of Ramsey's broodings can be gained from a talk he gave on 26 January 1924 to the Apostles, the secret dining and debating society with close connections to Bloomsbury from the early years of the century on.⁵⁷ (He had been elected a member in 1921, probably around the time of Penrose's election. James Strachey had been a member since 1906, and he combined meetings of the two societies on his visits from London.) Originally entitled 'Psyché: or will it bear the light', its final title was 'An imaginary conversation with John Stuart Mill' (Ramsey 1924, pp. 302-12).

John Stuart Mill walks in the door while Ramsey is sitting beside the fire feeling bored, and opens the conversation: 'Have you never read my Autobiography?' Ramsey replies that he has done, remembering particularly his 'remarkable education' ; Mill replies that he specifically wants to talk about 'the account of my mental crisis... one to which all young men are liable, if they use in introspection analytic intellect' (p. 302). Quoting extensively from Mill's *Autobiography*, their discussion turns on the question whether Freud's discoveries represent a decisive advance - and thus a satisfactory account of and answer to mental depression - when compared with Mill's own associationist psychology ruled by the pleasure-pain principle.

Ramsey: But you know psychology has advanced since your day, yours is very out of date.

Mill: Has it? I don't think so. You have advanced in philosophy in a way that excites my profound admiration but in psychology hardly at all. Perhaps you are thinking of the followers of Freud, who seem to regard the analysis of the mind as a panacea.

Ramsey: Yes, I am thinking of them; you are probably put off by their absurd metaphysics, and forget that they are also scientists describing observed facts and inventing theories to fit them.

Mill: I doubt that; they alone observe these facts, and the different schools do not agree as to their nature, still less in their interpretations...

Ramsey: But of course [Freud] would dispute your psychology; he would say that the most important associations in determining your desires were those formed very early in life and no longer accessible to consciousness...

Mill: I don't agree... (pp. 306-7)

And thus the argument swings back and forward, until Ramsey 'concedes' with the very Cambridge phrase 'You may be right...' so as to move on to another topic: obliging Mill to recount the details of his depression and his mode of selfcure, only to play the omniscient analyst and demonstrate that Mill's own account of that cure and his new goals in life is inadequate. Mill rebuffs the suggestion and Ramsey concedes:

57. See Deacon 1985, esp. p. 92 with regard to the 1920s: 'psychology was the magic word at this time, though sociology was regarded with some contempt.' Ramsey's manuscript is dated 26 January 1924, which was a Saturday, the normal meeting day of the Apostles.

Ramsey: But I do heartily agree that we must not too frequently ask ourselves or discuss with one another whether we are happy, which perhaps is your chief contention.

Mill: Yes, that is what I came to tell you.

And walked out of the room. (p. 312)

Ramsey was clearly trying out an identification with another English philosopher who had been, like him, an extraordinarily precocious thinker, and who had also confronted a major mental collapse: Ramsey feared this was what awaited him and wondered if the major difference between 'then' and 'now' - Freud - could make a difference to their respective destinies. Central to Ramsey was the question of happiness.

By February 1924, Ramsey had decisively revised his proposal to Wittgenstein:

If I live in Vienna I can learn German, and come and see you often (unless you object) and *discuss my work with you*, which would be most helpful. Also *I have been very depressed* and done little work, and have *symptoms so closely resembling some of those described by Freud* that I shall probably *try to be psychoanalysed, for which Vienna would be very convenient*, and which would make me stay there the whole six months. *But I'm afraid you won't agree with this. Keynes still means to write to you; it really is a disease - his procrastination; but he doesn't (unlike me) take such disabilities so seriously as to go to Freud!* (Wittgenstein 1973, p. 84; letter dated 20 February 1924, original italics)

In March 1924 Ramsey left Cambridge for Vienna and, after a brief meeting with Freud, he began analysis with Theodor Reik. He spent much time with Wittgenstein's sister - also a friend of Freud's - Margarete Stonborough and her circle, seeing Wittgenstein himself only on four occasions (Appignanesi & Forrester 1992, p. 345). He probably spent time with Penrose, Rickman and Tansley.⁵⁸ Ramsey completed his analysis in the summer, visited Wittgenstein again, trying to persuade him to return to Cambridge, and himself returned in late October 1924, having been elected to a Fellowship at King's not yet having completed his undergraduate degree.⁵⁹

As soon as his analysis was over, the gossip networks connecting Vienna and Berlin informed Alix Strachey of the progress of the *Wunderkind* and she naturally passed the news on to James in London: '[Reik] was enthusiastic about Frank Ramsay's [*sic*] beautiful character, & seemed to think, analytically, that all was for the best' (Strachey & Strachey 1986, p. 86; letter dated 13 October 1924). A few weeks later, in early November, with Ramsey back in England,

58. When Penrose was interviewed by Paul Roazen in 1965 about his years in Vienna and his contact with psychoanalysis, he spontaneously informed Roazen, who knew nothing of Ramsey, that Ramsey had been in analysis with Theodor Reik. And, as we have seen, Penrose specifically mentioned Tansley as an important influence in his decision to seek medical training, clearly while they were both in analysis in Vienna in the first half of 1924.

59. For some of these details on Ramsey, see Monk 1990, esp. pp. 215 ff., and personal communications from Ramsey's sister, Margaret Paul. In 1926 he was appointed a Lecturer in Mathematics in the University.

James delivered a further progress report, this time from brother Lytton's visit to Cambridge:

Ramsey (who, before he went to Vienna, didn't know that he wanted to fuck Mrs Pyke) discovered there that he did, but thought himself cured of such wishes. On returning & meeting her, however, he was more bowled over than ever; but asked her to go to bed with him - which she declined. (Strachey & Strachey 1986, p. 107; letter dated 3 November 1924)

The London analytic gossip network was equally informative:

Glover said Ramsey (so the story went) is under the impression that he's completely analysed, & that Reik said - 'if you'd had unlimited time & money, we couldn't have gone deeper'. (Strachey & Strachey 1986, p. 108; letter dated 6 November 1924)⁶⁰

Within weeks of his return from Vienna, Ramsey was involved with Lettice Baker, a former Cambridge undergraduate a few years his senior who had returned in 1924 to work in the Psychological Laboratory. As Strachey reported to Alix:

Incidentally, he [Sprott] said that Ramsey *has* been cured. He's abandoned Mrs P[ylke]; has taken on a new lady with whom (though, before, the idea had filled him with repulsion) he proceeds to the furthest limits... Perhaps we'd better all go on to Reik. (Strachey & Strachey 1986, p. 157; letter dated 22 December 1924)

The couple married in September 1925.

Two months after his marriage, on 24 November, Ramsey again spoke to the Apostles, this time on 'Civilization and happiness'. It was only ten months since his debate with J.S. Mill, but the tone was significantly weightier, more sombre but less stricken - not quite the tone one would expect of the newly married 21-year-old which he in fact was:

I have only lately begun to feel that civilization is opposed to happiness; I feel it as a burden which I am forced to carry and cannot throw off, and I should be interested to discover whether we all suffer under it or whether I am merely objectifying the heaviness of my heart. (Ramsey 1925, p. 320)

In this inquiry, so akin to Freud's *Civilization and Its Discontents*, which had yet to be written, Ramsey was at his most Freudian: happiness comes from the satisfaction of instincts, but civilization, which induces the sublimation of those instincts, deprives us of happiness: 'I think that it is just because they are the products of sublimated and not of primitive instincts, that our pursuits so often seem not really worthwhile' (p. 321). The pursuit of truth does not bring

60. The tone of James's remarks implied a criticism of Reik, which explains why Alix in Berlin jumped to his defence: 'I think all that about Reik is nonsense.... He said to me that he'd done all he could to Frank [Ramsey] *in the short time* at his disposal - that the analysis had gone very well owing to Frank's crystal-clear mind & soul - was enthusiastic about him; and wound up by saying that there'd never been anything much wrong with him. All of which seems fairly reasonable' (Strachey & Strachey 1986, p. 112; letter dated 7/8 November 1924).

happiness, because we are not really interested in it for its own sake, but rather on account of the `diversion' of other instincts, such as infantile sexual curiosity and a desire to triumph over our parents. `And it is not the truth which will make us happy, but the satisfaction of those other repressed desires which our conscience will not allow us.' Then Ramsey hit a new note, a more personal note, clearly the fruit of his time spent in analysis:

In my own case I think that my interest in philosophy and all kinds of criticism, which is much greater than my interest in constructive thought, is derived from a fairly wellrepressed infantile rivalry with my father and my wish to kill him. This means that I can never get any great satisfaction from philosophizing, never anything like the pleasure I should have got from killing my father, which my conscience or rather my love for him forbade me to do when I was small. This has incidentally another unfortunate consequence, namely that my philosophical criticisms should always be regarded with suspicion, as I am probably identifying the man I am criticizing with my father, generally in his hostile aspect, so that I am biased against the philosopher who in my unconscious mind represents my father. I am also liable to identify someone like Wittgenstein with my beloved father and attach a most exaggerated importance to his every word. (pp. 321-2)⁶¹

At this point, Ramsey's argument switched to the position of women in civilization and to the feminist movement's influence on general happiness. Recognizing a historical inevitability in the success of women's demand for education and the same position as men, he could not but see this as both loss and gain:

Not merely is feminism bad for the race but it is unfortunate for the women also, who are forced away from the kind of life which they are fitted by nature to enjoy. But it seems to me bound to happen, and also to some extent excites my admiration. They are taking upon themselves the burden of civilization and turning from sexual to intellectual activities which though less satisfying seem to me more excellent. We have here again in opposition culture and happiness, and what I really feel about that business is that I should like myself to be happy and other people to be cultured. (pp. 323-4)

Ramsey might have become the philosopher of psychoanalysis he had enthusiastically promised to be - he turned his hand so quickly and profoundly to so many fields, and the implications of his work are still being pursued. He died, not yet 27, in January 1930.

Harold Jeffreys

Harold Jeffreys (1891-1989) was another brilliant young polymath, elected into a Fellowship at St John's College, Cambridge in 1914, which he held until his death, a tenure longer than the entire duration of the Soviet Union. (On Jeffreys,

61. As Penrose's unpublished papers and notebooks make clear, he also was extremely impressed by Wittgenstein at this time; but, with a different character and less sunnily murderous and idealizing relation to his father, the consequences were different, perhaps playing a part in his turn away from logic, philosophy and psychoanalysis.

see Cook 1990; Howie 1999.) His interest ranged from mathematics, via mathematical physics (he and his wife, Bertha Swirles, wrote *Methods of Mathematical Physics* in 1946, which went through seven editions to 1980 and is still in use for teaching), to geophysics, which established his world-wide reputation (his earthquake travel timetables, produced with K.E. Bullen in 1940 are still standard), theorist of solar system formation, probability theory, econometrics and botany - he taught alongside Tansley in the Geographical Tripos in the early 1920s. His book *Scientific Inference* (1931) is still a classic in the philosophy of science, and included a robust and original defence of the findings and methods of psychoanalysis, quoted at length by Jones with almost paternal pride in his review for the *International Journal* (Jones 1931b).

It is unclear when Jeffreys first became interested in psychoanalysis. He worked in the Cavendish Laboratory from 1915-1917, but then moved, at the suggestion of Professor Newall, to the Meteorological Office in London, to work on hydrodynamics for war-related work. He stayed there until 1922. During this period, he made the acquaintance of Dorothy Wrinch (1894-1976; Figure 3, p. 203) (see Abir-Am 1987; Abir-Am 1993) to whom James Strachey, when he finally met Jeffreys at the first meeting of the Cambridge Psychoanalytic Group, attributed Jeffreys's turn to psychoanalysis:

The $\Psi\alpha$ meeting itself was very gloomy. Mr Jeffreys, in whose rooms we met, turned out to be... Miss Wrinch's fiance. Do you remember? - a rather rugged figure, like a wire-haired terrier, whom one constantly met on the stairs. I was so aghast at the recollection that I was on the *point* of saying: 'I used to know your wife, Miss Wrinch', but luckily restrained myself. I suppose his jilting was what brought him into Jones's hands. He's incredibly dull-minded, and comes so much from the North as to be almost incomprehensible. (Strachey & Strachey 1986, p. 223)

There is no direct evidence other than Strachey's immediate presumption that Jeffreys and Wrinch were ever engaged. It is just as likely that the Stracheys saw Jeffreys and Wrinch as they were visiting Bertrand Russell in Gordon Square from 1916 on, when Russell held informal classes in logic there after his excommunication by Trinity College for his pacifist views. After Wrinch became a Wrangler (i.e. gaining a First in the Mathematical Tripos) at Cambridge in 1916, her interest in logic brought her to Bertrand Russell. Quickly becoming a friend, over the next few years she took on the role of managing key bits of his life. She introduced him to Dora Black, who would become Russell's wife in 1921; she brought him books and papers when he was imprisoned for his pacifist views; she found both German and English publishers for Wittgenstein's manuscript, which turned into the *Tractatus Logico-Philosophicus*; she put up Russell and Dora for some months in her cottage in Winchelsea, Kent, in the early autumn of 1921 when they returned from their visit to China with Dora some months pregnant (see Russell 1968, pp. 96, 100, 122, 141, 175; Monk 1996, pp. 525n, 561-2).

Lecturing at University College London in the period 1917-1920, Wrinch shuttled back and forth between UCL and Girton and, in 1919, began a collaborative research project in the mathematical and philosophical theory of probability with Jeffreys which would result in three substantial joint papers in those areas in the period 1919-1923. Their collaboration extended into other areas of physics: a joint response to Einstein's relativity theory (Wrinch & Jeffreys 1919; 1921a; 1921b; 1923a) and a joint paper concerning the possibility of using the seismographic records of the gigantic explosion at a chemical works on 21 September 1921 in the village of Oppau in Bavaria to produce a standard for establishing more accurate measurements of the travel times, position and size of earthquakes, from which more sophisticated models of the internal structure of the Earth could be developed (Wrinch & Jeffreys 1923c). Jeffreys, who had long been interested in articulating geophysics with theories of the origin of the solar system, would go on to make this one of his principal areas of expertise, particularly with the publication of his book *The Earth* in 1924. This book went through five influential editions. The final edition of 1970 remained adamantly opposed to the continental drift theory he had definitively refuted in 1924 (in Wegener's version), on the grounds of the impossibility, according to the accurate quantitative model he had put forward, of their being convection currents in the earth's mantle - an argument bypassed with the rise of plate tectonic theory in the 1960s.⁶²

Before Jeffreys established a major reputation in geophysics with this work, his collaboration with Dorothy Wrinch had come to an end, their ways parting with dramatic changes for both of them. Sizing up her academic situation, and 'after carefully investigating various alternatives' (as she put it in a semiautobiographical non-fiction *The Retreat from Parenthood* she pseudonymously published in 1930), in the summer of 1922 she quickly and quietly married John William Nicholson (1881-1955). He was another top Cambridge mathematician, who had taught her in 1913 and who had moved via London to Balliol College, Oxford, in 1921. The next few years were spent struggling to establish her career either in London or Oxford. This became increasingly fraught with the birth of her daughter in 1927 and her husband's breakdown in 1930. The breakdown resulted in the speedy termination of his contract at

62. See Stewart 1990, esp. p. 116 and pp. 34-5. To give some sense of the style and argument of Jeffreys's continued opposition, we quote the introduction to his six-volume *Collected Papers* (Jeffreys 1977, p. viii): 'Taylor's result on tidal friction in the Irish Sea is extended to other shallow seas. The damping of the 14-monthly variation of latitude, together with the fact that S pulses [one of the two principal reverberations of earthquakes through the mantle] are reasonably sharp at a distance of 80° , leads to a law of creep under which the creep under constant shear stress is like t^∞ ($0 < \infty < 1$). For a free vibration the decrease of amplitude in a period would vary as (period) $^\infty$; for a wave travelling a given distance, like (period) $^{-1 + \infty}$. The limiting case $\infty \rightarrow 0$ leads to Lomnitz's law, but the two data together indicate that ∞ is about $\frac{1}{4}$. S. Crampin and I have shown this to account for the Moon's rotation, the persistence of its ellipticities, and the undetectability of its free oscillations. ($\infty = 1$ is the elastiviscous law, and leads to hopeless contradictions)... [T]he law with $\infty < 1$ forbids convection and continental drift.'

Oxford, her legal separation from him (the marriage was dissolved in 1938) and his installation in a mental hospital as an alcoholic.⁶³

Meanwhile Jeffreys, discontented with the Met Office in early 1922, had resigned and returned full-time to Cambridge. But his academic future was by no means secure, and in both 1923 and 1924 he journeyed to North America, partly in response to overtures from Harvard and Yale. In the event, he decided to remain in Cambridge. Together with his earlier studies of fluid dynamics, dynamical meteorology and celestial mechanics, the success of *The Earth* secured him an F.R.S. in 1925, a University Lectureship in 1926, and a Readership in Geophysics in 1931. Whether he was upset, as Strachey immediately assumed, by Wrinch's quick marriage to an older Cambridge mathematician is unclear. It remained unclear to the woman he married in 1940, Bertha Swirles; in 1999 she expressed a distinctly agnostic position on Jeffreys's feelings about the termination of his collaboration with Wrinch: 'I don't know that Harold minded all that much; he was never really clear about that - as one wouldn't be'.⁶⁴ Certainly Wrinch had marked his life as he repeatedly emphasized the importance of the joint papers they wrote in 1919

63. After her separation from Nicholson, Wrinch attempted to leave Oxford. To further the work published as *The Retreat from Parenthood* (a vehicle for her Utopian hopes for a society that would free professional women from domestic and financial worries and allow them the opportunity to combine work and children), she applied for Rhodes and Rockefeller travelling fellowships and to the American Federation of University Women, but was rejected. With a small allowance from her affiliated Oxbridge colleges, she was able to fund nine months in Vienna with her 5-year-old daughter; there she began a research project applying mathematics to biology in the area of chromosome structure. In 1932, along with Joseph Needham, Joseph Woodger, C. H. Waddington and J.D. Bernal, Wrinch helped to found the Biotheoretical Gathering. In hopes of revolutionizing biology with philosophy and mathematics (as physics had been transformed through General Relativity), the members met on shared vacations to engage in informal interdisciplinary discussion. Working at the borders between physics, chemistry and biology, Wrinch was a Rockefeller Fellow 1935-1941 during which time she made a distinctive scientific contribution with her answer to the much debated 'secret of life': the 'cyclol theory', the first model of protein structure in the pioneering transdisciplinary field of molecular biology. Giving her a short-lived celebrity status in America, of the 'female Einstein' variety, her ambitious claims for her work (such as her assertion that it was corroborated by X-ray data) soured relations with British crystallographers, including her old friend, Bernal. In 1939, by then in America, she engaged in a public clash with rival protein theorist, Linus Pauling, which damaged her reputation and made her search for employment difficult. In 1941 she married Otto Charles Glaser (died 1951), Professor of Biology at Amherst College, Massachusetts and acted as a visiting research professor in three Connecticut Valley colleges. Her main work involved rehabilitating her cyclol theory. By the 1950s, when some experimental support emerged, molecular biology was concerned with nucleic acids rather than proteins as the 'secret of life'. Nevertheless, Wrinch upheld her theory against the double helix model of DNA until her death in 1976. Her daughter Pamela, who gained a Ph.D. in international relations from Yale, predeceased her in an accidental fire. Wrinch published consistently and widely, from *Nature* via the *Journal of Genetics* to the *Proceedings of the International Botanical Congress*: her books include *Fourier Transforms and Structure Factors* (1946), *Chemical Aspects of the Structure of Small Peptides* (1960) and *Chemical Aspects of Polypeptide Chain Structures* (1965).

64. Lady Jeffreys, interview with Laura Cameron, 7 January 1999, Cambridge. Lady Jeffreys recalled that she and Harold 'lost touch' with Wrinch: 'last time we saw her was in New York about 1951'.

1923.⁶⁵ Yet, mysteriously, when it came to publishing his *Collected Papers*, he omitted them, explaining that: 'These papers contain the first statements of several principles, but only summaries are given, since all the subject matter is further developed in my *Scientific Inference and Theory of Probability*' (Jeffreys 1977, Vol. 6, p. 251). For the historian and philosopher of science, this account is not only misleading but is untrue: David Howie's careful reconstruction of the Jeffreys-Fisher debate over the status of probability devotes more space to these joint papers with Wrinch than to any other of Jeffreys's writings. Which leads one to conclude that the place of Dorothy Wrinch in his life and work remains enigmatic.

Where did Jeffreys's interest in psychoanalysis belong, both conceptually and chronologically? In August 1918, Dorothy Wrinch was already corresponding with Russell about psychoanalysis: 'extremely interesting... but not quite scientific enough' (Abir-Am 1987, p. 246).⁶⁶ However, we do not know either whether she or anyone else was the specific source for his psychoanalytic interest, or whether she knew Jeffreys by the time that interest was aroused. Long-standing his interest did remain - it was Jeffreys who, in 1936, was instrumental in Freud being elected a Foreign Member of the Royal Society.⁶⁷ In replying to Freud's inquiry as to the identity of his English scientific patron, Jones gave a quick description:

Harold Jeffreys is a Cambridge professor - I think of geophysics.⁶⁸ He was in analysis with me for a couple of years, and later with Miss Sharpe. The remarkable feature of his case was a combination of the highest intellectual capacities with a very low degree of ordinary social capacities, such as *savoir-faire*, common-sense, etc. I think the success of the analysis was only moderate. He has an astounding facility for rapidly acquiring the profoundest knowledge of any subject, e.g. botany, higher mathematics, physics, etc., and has made himself world famous through his mathematical researches into physics and the structure and movements of the earth, on which he has written some heavy books. (Freud & Jones 1993, pp. 753-4; letter dated 6 July 1936)

It seems highly probable that the first of Jeffreys's analyses, that with Ernest Jones, took place while he was living part-time in London working at the Met

65. 'Harold always stressed the importance of those papers which were done while she was a fellow of Girton. First I saw of him was going up the stairs to her room I think...' - Lady Jeffreys, interview with Laura Cameron, 7 January 1999, Cambridge.

66. Russell's own relationship with psychoanalysis may of course have some bearing on those who came into his circle; clearly Wrinch and he discussed psychoanalysis; another of the young women who followed his lectures was Karin Stephen, who, following work on a dairy farm with her conscientious objector husband, Adrian, during World War I, became a psychoanalyst - having followed Jones's advice to train medically at the same time. Russell's path was to cross those of the psychoanalytic enthusiasts (e.g. A.S. Neill) many times in the next few years.

67. Tansley, with Bertrand Russell's and Julian Huxley's support, unsuccessfully attempted to get Jones elected to the Royal Society. Tansley to Ernest Jones, 12 April 1953, Archives of the British Psycho-Analytical Society, CTA/F02/03; Huxley to Tansley, 21 July 1953, Tansley Archive, Dept. of Plant Sciences, Cambridge.

68. Jeffreys became a reader in geophysics in 1931; in 1946 he was elected Plumian Professor of Astronomy and Experimental Philosophy; he retired in 1958, at the statutory retirement age of 67.

Office from 1917-1922. Not only can we be sure that he had already had analysis by late 1924, when the Cambridge psychoanalytic group was forming, but a letter from Ernest Jones, dated 21 August 1923, displays that strange quality of impersonal and guarded informality so often found in letters between analyst and patient, and thus makes it probable that the analysis had ended by that date:

Dear Mr Jeffreys,

How quickly time goes! So you are not greatly impressed by the Transatlantic atmosphere. I was shocked to hear of your liking Toronto, though I have often heard the same from other people, who have not had the misfortune to live there. Many thanks for your fierce essay, with which I intend to have a serious wrestle.⁶⁹

A year later, when Jeffreys may have been still undecided about taking up a post at Harvard, he had also made contact with leading psychoanalysts on the East Coast, Smith Ely Jelliffe and William Alanson White, joint editors of the *Psychoanalytic Review*, in which they published Jeffreys's forty-page analysis of Ibsen's *Peer Gynt* (which dealt with Ibsen's use of alliteration and the urinary complex amongst other themes) in October 1924. Jeffreys tried to arrange to visit Jelliffe and White en route from Canada to New York in late August.⁷⁰ (Strangely enough, when the group of psychoanalytic 'insiders' met in Cambridge in early 1925, the geophysicist Jeffreys was the only one to have published a substantial paper in a psychoanalytic journal.⁷¹) While it is probable that Jeffreys had analysis with Jones well before 1924, the dates of his analysis with Ella Freeman Sharpe, the doyenne of English analysts in the first half of the twentieth century, are less certain. The analysis left a trace in a paper of hers published in 1930:

I have registered during one week a number of things which, had I personally known more about them, would have enabled me to reach more quickly the unconscious themes that were being given to me in a representative way. In one analysis I needed an intimate knowledge of *Peer Gynt*, and a swift recognition of the roles that Asa, Ingrid and Solveig were playing at that moment in terms of the patient's own identifications. In another an immediate recall of a Dutch picture would have given me the link I needed between an actual scene and an unconscious phantasy. The knowledge of the exact duties of a trustee; the differences between two ways of calculating commission on sales; a knowledge of the differences between two makes of motor cars; the appearance of a cider-press and the way it works; the precise meaning of football terms; an understanding of the processes of etching - all these would have enabled me to grasp more quickly than I did the unconscious significances that were being represented. (Sharpe 1930, p. 255)

69. Ernest Jones to Mr Harold Jeffreys, 21 August 1923, Harold Jeffreys Papers.

70. Smith Ely Jelliffe to Harold Jeffreys, 25 August 1924, Harold Jeffreys Papers. Jelliffe gives Jeffreys's address as 436 Gilman Street, Ottawa.

71. James Strachey's first psychoanalytic publication under his own name (as opposed to translation) was a critical review of two psychoanalytic studies in German of August Strindberg, in the *International Journal* for 1923. Rickman's psychoanalytic publications in 1924 also comprised a series of reviews.

That *Peer Gynt* was a leitmotiv in Jeffreys's relation to psychoanalysis is clear from the fact that he published two papers on the play, the one noted above in 1924, and a further short note in the journal *Scandinavica* (1964).

Jeffreys's public connection with analysis was to be restricted to his philosophical defences of it and a number of published papers: one, in 1924, on Ibsen's *Peer Gynt* and another remarkable paper on 'The unconscious significance of numbers', published in 1936.⁷² Some sense of the argument of this paper, in which he enquired into the superstitions and significance given to certain numbers, particularly odd ones - 3 (representing the male genitals), 5 (whose Pythagorean magical status is due to the pentacle representing the 'phantasy of the father's penis preventing the angry child's return to the mother'), can be gained from the following hypothesis he put forward:

The notion of a 'prime' number, one possessing no factor other than unity and itself, is clearly a development of that of an odd number, which merely does not possess the factor 2. Thus a prime (other than 2), while carrying the attribute of maleness [the '1'] like other odd numbers, is specially resistant to separation into parts. The theory of primes is therefore essentially a play mechanism designed to provide a defence against the fear of destruction by tearing apart into pieces of comparable size, and avoids consideration of the danger popularly associated with odd numbers, that they may lose a unit and become even... To sum up, numbers in language, folklore and superstition appear to carry affects derived from pre-genital situations, mainly oral and urethral. The interest in odd numbers, and especially in primes, is originally phallic, while even numbers and especially those with a large number of factors are associated with ambivalent attitudes to the mother. (Jeffreys 1936, p. 223)

Jeffreys considered even numbers to be less freighted with fantasy. Thinking about numbers in a particular way is sexualized, and Jeffreys went on to speculate that opposition to a decimal system of weights and measures is affected by the fear of danger associated with too much preoccupation with the number 10, the base of the decimal system which is intimately associated with the ten fingers of the hands; referring to Penrose's case of schizophrenia (Penrose 1931 a) with its cosmos of language and system of measurement, he noted that 'extreme decimalization was associated with habitual masturbation'.

Both Jeffreys and Penrose were preoccupied with the psychology of mathematics, and may well have exchanged psychoanalytic views. Whereas Penrose had given a paper based on the analysis of his relationship to chess, Jeffreys revealed in his paper some of the elements in his personal relations with numbers:

In my own analysis the number 4 appeared with strong female attributes. This was traced partly to the four teats of the cow, and partly to the diamond on the label of one of Bass's beers often seen in childhood. This diamond was thought of as two equilateral triangles

72. While undoubtedly eccentric, the paper was recently used for a further psychoanalytic exploration by another mathematician (Hide 1984). Jeffreys published a further paper linking the problem of inference with psychoanalysis by using material from Melanie Klein's book on the psychoanalysis of children to show how children and scientists use the same - reliable - principles of inference. See Harold Jeffreys, 'The problem of inference' *Mind*, Vol. XLV, N.S., No. 179 (1936):324-33.

on a common base, and represented the breasts. Seven was thought of as 4 + 3, representing a hermaphrodite figure with the external organs of both sexes. It had associations with irresistibility and perfection; we have seen above that these are just what are found in the folklore of the number. (Jeffreys 1936, p. 222)

It is possible that Tansley was another catalyst for Jeffreys's curiosity about psychoanalysis. Certainly Jeffreys's botanical interests had early on brought him into contact with Tansley. His three papers on plant ecology, two of which were local studies of the area of the North-East of England (Jeffreys 1916, 1917) where he grew up, the third being an investigation of the comparative viability of certain grasses in Durham and Suffolk (Jeffreys 1918; to which Jeffreys added a note in 1970), all date from the period 1916-1918 and were published in Tansley's *Journal of Ecology*. Their theoretical framework is indebted to Tansley and to Farrow, a student of Tansley's and a close friend of Jeffreys whom we shall discuss below - he footnotes both men extensively. It is thus possible that it was Tansley who introduced Jeffreys to psychoanalysis, as well as serving as his professional patron in ecology.

In 1974, the 80-year-old Jeffreys, by then knighted, opened a short note concerning R.A. Fisher and inverse probability - a topic on which he and the mathematical geneticist Fisher had clashed formidably at the Royal Society in 1932-1934 - with a statement of his origins:

I think that I should begin with my first use of probability theory, which was in a paper with Dorothy Wrinch in the *Phil. Mag.* for 1919. My interest started through Dr E.P. Farrow, a plant ecologist, who introduced me to Karl Pearson's *Grammar of Science*, still the best general work on scientific method... (Jeffreys 1974, p. 1)

Farrow also taught Jeffreys how to ride a bike. He may even have been a further catalyst for Jeffreys's interest in psychoanalysis, as we will see. Certainly this friendship with Farrow lasted longer than that with Wrinch - Lady Jeffreys remembered Farrow as 'an odd fish - rather!⁷³ And the debt to Pearson's positivist primer is clear throughout Jeffreys's writing on science, particularly in his *Scientific Inference* of 1931.

We thus have a fragmentary picture of Jeffreys at the time of his involvement with the other Cambridge psychoanalytic insiders: socially ill at ease, restlessly polymathic while yet to find, from amongst the many scientific fields he became interested in, the lines of enquiry which would allow him to make his major contributions.

The last member of the Cambridge psychoanalytic group had found his vocation, but only very recently. The connection of James Strachey (1887-1967) with psychoanalysis has been so widely discussed that we will not offer more than a few words here.⁷⁴ A core member of the Bloomsbury Group, he had been

73. Lady Jeffreys, interview with Laura Cameron, 7 January 1999, Cambridge.

74. The best source for Strachey's life is the fine edition of his letters with *Alix Strachey* edited and annotated by Perry Meisel and Walter Kendrick as *Bloomsbury/Freud. The Letters of James and Alix Strachey 1924-1925*. Together with the very full and useful annotations provided by the Editors, the letters provided us with the starting-point for this paper in Strachey's accounts of the psychoanalytic group's meetings.

in analysis with Freud from October 1920 to July 1922, who had quickly set him to work on his life-project of translating Freud into English. By 1924 he was a practising analyst and full Member of the British Psycho-Analytical Society.

The first meeting of this quintessentially Cambridge group was held on 2 March 1925. Its agenda included Jeffreys discussing 'Psychoanalysis and the death duties' and Penrose on 'Psychoanalysis and chess'. Strachey reported:

I was crushed by the unaccustomed intellectual level - especially of Ramsey. And it was rather like the third Act of Siegfried to hear the tone that he adopted about poor old Dr [G.E.] Moore. He seemed on the whole to accept $\Psi\alpha$, but thought the theory very muddled. The theoretical work of the Prof.'s which he most admired was *Das Ich and das Es*. He is thinking of devoting himself to laying down the foundations of Psychology. All I can say is that if he does we shan't understand 'em. He seems quite to contemplate, in his curious naif way, playing the Newton to Freud's Copernicus. (Strachey & Strachey 1986, p. 223; letter dated 2 March 1925)

Another meeting two months later gives a further sense of the enthusiasms and wide differences of viewpoint in this curious Cambridge group:

Tea at the Tansleys... Dinner at the Union with Lionel, Rickman, & Tansley; and afterwards Lionel's paper on the Biological implications of the Pleasure Pain principle, with all his measurements of leaves & of the growth of the human body. It was rather amusing, because Frank began very hubristically, declaring that Lionel's mathematical formulae were dotty; but eventually it turned out that on the contrary Frank had made an absurd mistake in his calculations. He was thereafter so much upset at the revelation that he declared he was greatly impressed by the paper & its statistics & curves (though personally I think it's all my eye). (Strachey & Strachey 1986, p. 270; 20 May 1925)⁷⁵

75. In 1950, writing his obituary of Susan Isaacs, Rickman recalled another meeting of the group, which must have taken place in the autumn of 1925: 'To one of these rigorous and rather select gatherings Susan Isaacs was invited. Let us see her as she came across the lawn on a bright autumn morning as the little company assembled - a sturdy figure in tweeds, a robust Lancashire girl; there was that in the vigour of her gait which put aside the fact that she was forty; she had a pale complexion, a chubby face with a mass of fair hair and bright hazel eyes... Seen across the lawn, brisking a little after the walk across the fields to the house where the meetings took place she was full of gaiety and sparkle - here was an occasion she was going to rise to, in fact she rose to the occasion in every discussion. That day's discussion was memorable. She gave an account of the new educational ideas emerging from the work at the [Maltings House] school. She spoke of the insight into the development of the ego there displayed and the very important part played by freedom for the children to speak of, to think of and to experience as part of their daily life the erotically tinged excitements and interests in their excretory and on occasion genital functions; the role of aggression in the social relations of the children was given a greater place than would have been usual among psychoanalysts at that time. Someone referred to the view that boys and girls in other, warmer climates, proved to be ineducable (in the scholastic sense) after puberty and that this was perhaps due to the fullness of their sexual freedom, so the question arose whether the freedom in this school might not have some of the quality of a 'pre-genital brothel' and so hinder the development of the cultural gains which are bought at the cost of erotic deprivation. Where would the energy for the sublimation necessary for cultural achievement come from if erotic satisfactions were not denied? Viewing this question a quarter of a century later the matter seems to have its chief interest in the theoretical formulations of that period (though the point is not finally settled yet), but to Susan Isaacs that question 'with its half truth', stuck, barbed like a fishhook, in her memory. She never accepted another invitation' (Rickman 1950, p. 281). The member of the group most likely to have asked the question about the sources for the energy of sublimation, given the date (autumn of 1925) and the style of direct question is most probably Ramsey - but this is only speculation.

This group bears the hallmark of Tansley's gift for organizing informal research groups. Tansley was by far and away the eldest and most senior of the members - their ages were 53, 37, 33, 33, 26 and 22. He had much scientific organizing already under his belt; its ethos may not have been unlike the Cambridge Ecology Club he convened in 1921. The psychoanalysis group had two striking features: firstly, the criterion that all members should have experienced a personal analysis; secondly, the fact that Rickman was the sole member with a medical degree or any extra-analytic interest in the 'profession' of psychopathology.⁷⁶ The traditional discipline best represented in the group was mathematics - Ramsey, Jeffreys and Penrose were all fine and creative mathematicians. A physicist, a philosopher of mathematics, a philosopher-psychologist, a botanist, a translator cum practising analyst and a 'nondescript' (a psychiatrist working at this time at the out-patient clinic at St Thomas's Hospital in London) do not make a conventional cohort of analytically oriented researchers.

The second interesting feature of the group besides their personal experience of analysis was their location in Cambridge, a small university town with a strong tradition in the developing sciences. This location would necessarily give the analytic orientation of the group a non-medical slant, since at that time clinical training in medicine did not exist in the University. Rickman, for instance, had completed his medical training at St Thomas's Hospital Medical School, where Penrose, his 'shadow', was soon to go. And if we look to other parts of Tansley's network, we see more ways in which the 'Cambridge' psychoanalytic group functioned outside medicine and outside the professions we usually associate with psychoanalysis.

The Field Scientists

According to Joseph Needham, the Cambridge biochemist and sinologist, it was Tansley who sparked interest in psychoanalysis amongst students by mentioning Freud in his lectures.⁷⁷ Godwin, one of those students, affords us a

76. Penrose was just beginning his medical training. A curious seeming contradiction in the historical evidence may be interpreted in this light as well. In 1925, Ramsey married Lettice Baker, a physiologist and psychologist who worked in the Cambridge Psychological Laboratory from 1924 on. Recalling her marriage some 50 years later, she made the following statement: 'All the philosophy and ethics and logic went in one ear and out at the other, I think. That was far above my head. It wasn't my subject, really, at all; and he wasn't a psychologist. So we didn't discuss psychology' (Mellor 1995). This memory is on the face of it an odd one, given that James Strachey reported Frank Ramsey as ready to devote himself to the foundations of 'psychology' in March 1925. The solution to this disparity of evidence may be found in the vast distance, amounting to sharing nothing in common, between 'psychology' as practised in the Cambridge Psychological Laboratory and 'ΨA' as discussed and practised by the Cambridge Group. Ramsey may indeed have had no interest in 'psychology' as it was to be found in the Psychological Laboratory, and this statement may have held true for all members of the Cambridge ΨA group, including the psychiatrist John Rickman.

77. Personal communication, Edward Timms to Cameron, 17 March 1997; referring to Timms's interview with Needham about interest in Freud in the 1920s in Cambridge. Cited in Cameron and Forrester 1999, p. 74.

glimpse of life in the Botany School at a time when dream analysis and vegetational analysis might work hand in hand. Tansley's botanical protege in Cambridge, Harry Godwin (1901-1985) was a Cambridge undergraduate (obtaining first classes in both parts of the Natural Sciences Tripos) who became his colleague and close friend and, upon Tansley's death, the heir to his intellectual copyrights and composer of his Biographical Memoir for the Royal Society. In addition to pioneering quaternary research in Britain with his wife, Margaret (who took up pollen analysis at Tansley's suggestion), Godwin (F.R.S. 1945) was, like Tansley, a leader in ecological thought, an editor of the *Journal of Ecology* (1948-1956), editor of the *New Phytologist* (jointly 1931-1961), a president of the British Ecological Society (1942-1943) and a founder member of the Nature Conservancy (West 1988). From 1948 to 1966, Godwin directed the Cambridge Botany School's new Sub-Department of Quaternary Research. Connected to Clare College as an undergraduate (1919-1922), research fellow (1925-1934) and official fellow (1934-1968), Godwin (Kt. 1970) lived in Cambridge until his death in 1985. In 1995 the Godwin Institute for Quaternary Research at Cambridge University was established in honour of his achievements.

When Tansley established the Cambridge Ecology Club in 1921, the club's first expedition, with Godwin in attendance, was a bicycle trip to the nearby National Trust nature reserve, Wicken Fen. Although Godwin recalled that he saw little of Tansley in the next few years, 'Tansley was very helpful in discussion of ecological problems and helped me directly in initiating field records and experimentation at Wicken Fen. These activities accompanied my main line of plant physiological research and for at least one day in the week allowed me to exchange the confines of the laboratory for the open air and direct contact with the world of free living plants' (Godwin 1985, p. 146). Tansley's and Godwin's shared interests in the ecology of Wicken Fen led to a joint publication in 1927 (as part of *The Natural History of Wicken Fen*) and laid the basis for Godwin's career as an ecologist and botanist. In this work, they identified deflected successions due to human activity, *plagioseres*, that deviated from the normal course of vegetational succession. Observing that there was no fundamental difference between normal vegetation and that disturbed by people, their conclusions on vegetational disturbance had interesting resonances with Tansley's understanding of the human mind: 'The great importance of Freud's work on dreams lies in the fact that in it he was able to demonstrate for the first time the principles and mechanisms he had discovered in neurotic minds at work also in a perfectly normal activity of the healthy mind. This led the way to psychological constructions of universal validity' (Tansley 1939-41, p. 260; see also Cameron 1999, pp. 17-8).

Godwin recalled in his 1985 memoir, *Cambridge and Clare*, that in terms of his relationship with Tansley, 'our area of contact was widened by the fact that as an undergraduate I had read those works of Freud available in English'. He was a member of the Clare 'Dilettanti' Club where on one occasion:

Ffrangcon Roberts, medical fellow of the College, outlined for us the principles of psychoanalysis, a subject unfamiliar but attracting attention as part of the widespread occurrence of psychic disorders in the extreme stresses of trench warfare. We were not then aware of the involvement of the Cambridge psychologist W.H.R. Rivers in the later publicized breakdown of Siegfried Sassoon, one of the greatest of Clare's authors, whose *Memoirs of an Infantry Officer* was a deserved classic of the First World War. (Godwin 1985, p. 30)

Although Godwin never attended Rivers's lectures himself, for some time a close friend in Clare, E.G. Chambers, had attended the courses for Part II of the Tripos in Psychology (then part of the Moral Sciences Tripos), where he had exercised his facility for swift longhand recording of lectures. Thus in the late evenings the two of us had reconstructed, reread and discussed the current views of such considerable scholars as W.H.R. Rivers. We not unnaturally had tried our hand at dream analysis and collected our own instances of *The Psychopathology of Everyday Life*.⁷⁸ Dream analysis, as a matter of course, intersected Godwin's youthful anxieties as he embarked on original research:

When in 1922 I turned from reading for the Tripos to commencing research, I found the mental break an extremely significant one. No doubt it remains so for all students, placed as they are so that the whole of their future may now be determined by choice of aim and adviser. It is a change just as fundamental as the earlier step from schoolboy to undergraduate, a sudden acceleration of growing-up and a new access to adult independence...

The choice of my prospective field of research offered some difficulty. On the one hand I was deeply attracted to ecology and especially to the meeting-ground of that subject with plant physiology, and on the other I recognized in Mr Blackman a scientist of world authority, training by whom would offer experience of quantitative and experimental methods less available if one began ecological research directly, even under Mr Tansley, eminent as he also was. The difficulty of choice was reflected in the anxiety of the dreams I had, one of which, as I vividly remember, involved a theatre stage where one of the leading actors was an impressive negro. At this period I had been much interested in Freudian dream-analysis and so spent considerable time trying to understand my dream: its main significance emerged as I saw that the dream symbolism had the negro for Blackman, and thereafter it became evident that my worry was over the dilemma of choice. In the event I was accepted by Mr Blackman and shortly afterwards I learned that my anxieties of choice had been pointless, since Tansley had in any event arranged to go to work with Freud in Vienna and could not have undertaken to supervise me. (Godwin 1985, pp. 55-6)⁷⁹

Godwin commenced studies under Blackman, Tansley's brother-in-law, and, in 1926, he was awarded his Ph.D. for work on the mechanism of starvation in leaves. Although Tansley's presence in Cambridge would be intermittent during

78. In editing Rivers's posthumous book *Conflict and Dream*, G. Elliot Smith employed Chambers's lecture notes in the reconstruction of Rivers's early 1920s lectures. See his comments in Rivers 1923, vii.

79. The fact that, as we document in our "'A nice type of the English scientist': Tansley and Freud" (Cameron and Forrester 1999), the course of Tansley's own botanical career was determined by a dream and its interpretation has an interesting resonance with this story told by Godwin, his protege and executor.

the 1920s, he and Godwin kept in contact because of shared enthusiasms in both psychoanalysis and vegetational succession. In 1926-1927, Godwin returned the 'unconscious' favour to Tansley, by encouraging him to allow his name to be put forward for the Chair in Botany at Oxford - and thus return to the 'mother subject' (Godwin's phrase) - to which Tansley was appointed in 1927 (Cameron & Forrester 1999, p. 74).

In some sense, Godwin's ready way with psychoanalytic talk was an offshoot of his profound allegiance to Tansley. Two other figures owed the beginnings of their life-long psychoanalytic loyalties to Tansley.

E. Pickworth Farrow

The first was his student before the World War I, E. Pickworth Farrow (1891-1956; see Figure 4, p. 203), who gained a Diploma in Agriculture at Trinity College, Cambridge, in 1912 and had his dissertation 'On the ecology of the vegetation of the Breckland Heath' approved in 1915. His interest in psychology was aroused in 1912 when Tansley brought the proofs of Hart's *The Psychology of Insanity* into his Cambridge botany class (Farrow 1942, p. 1). Hart's account of Jung's experimental investigations of the reaction times of associations, particularly when conducted on himself, persuaded him that there was something in psychoanalysis. He read *The Interpretation of Dreams* as soon as the translation was published in 1913. In the Great War, he volunteered for Army service, but in 1917 returned after a breakdown. Farrow, singled out by Tansley for being the first to call general attention to the significance of the biotic factor on British vegetation (Tansley 1939, pp. 129, 136-9, 500, drawing attention to the impact of rabbit attacks on heathland), kept in touch with his teacher, who later put him in contact with Freud.

By 1922, Farrow was working in the family engineering firm; his interest in psychoanalysis was rekindled in 1922 by a dream provoked by business worries, and, early in 1923, he 'decided with some reluctance (or resistance) to be analyzed' (Farrow 1942, p. 9). His first analysis lasted three months, when he moved on, at his analyst's suggestion, to another, with whom he had two-hour sessions five days a week for three and a half months. When Ernest Jones wrote to Freud in late 1924 in one of his more inflexible and narrow-minded moods ('A Mr Farrow, with whom you are in correspondence, has sent me a rambling auto-biographical article. From the content of it I should suspect him of suffering from *dementia praecox*, though Tansley who knows him does not think so. I am trying to get the article re-written in a form possible of presentation' (Freud & Jones 1993, 7 November 1924; see Farrow 1925a), Freud replied, defending the eccentric and maverick Farrow - and implicitly endorsing Tansley's judgement over Jones's:

You must not take Mr Farrow for a fool. I know him through Tansley and from a personal conversation. He is an odd man, but a very able, 'shrewd'⁸⁰ one, who had no luck with two analysts and has since then undertaken a self-analysis and is coming up with quite

80. In English in the original.

serious findings. To be sure, he is a bit of a grumbler, but both analysts (near you) really made technical mistakes with him. (Freud & Jones 1993, p. 562, 16 November 1924)

Tansley helped Farrow rewrite his piece into a form acceptable for publication in the *International Journal*. Clearly what had unsettled Jones was not simply the fact that Farrow had criticized the methods of his two analysts (both members of his recently formed British Psycho-Analytical Society) and was attempting to enlist Freud as a patron of self-analysis. What made him think of a psychotic diagnosis was Farrow's insistence on the terrifying reality of the castration threats in childhood that his self-analysis had reconstructed: a scene from his early childhood in which an adult female cousin brought a scissors closer and closer to his body while a collaborator held a cooking bowl to catch the penis as it was lopped off (Farrow 1925a, p. 45). The scene as described (in the version published, which Tansley revised (Freud & Jones 1993, 24 November 1924)) has a feeling of 'hyper-reality'. Farrow continued to find sympathy and support in Freud, who wrote a preface to another paper of his - concerning a memory recovered from his seventh month - published in the *International Zeitschrift fur Psychoanalyse* in 1926 (see also Farrow 1925b), in which, as if to challenge Jones's vision of what counts as authorized psychoanalysis, he commended Farrow's efforts to employ 'the procedure of self-analysis which I myself employed in the past for the analysis of my own dreams. His findings deserve attention precisely on account of the peculiar character of his personality and of his technique' (Freud 1926, p. 280). With Tansley and, as a result, Freud as patrons, Farrow was invited to speak to the British Psycho-Analytical Society in November 1927 on 'Conventional methods of scientific research in relation to psychoanalysis' (Bryan 1928, p. 276).

Farrow's long-term study on the Breckland Heath, conducted first under Tansley's supervision at Cambridge, utilized pioneering fixed-point photography and experimental methods in the field. Farrow analysed rabbit dung in the laboratory and used rabbit-proof cages and fences in the field to test the hypothesis that the degeneration of calluna (heather)-heath to grass-heath could be attributed chiefly to rabbit attack. In his *Plant Life on East Anglian Heaths, being Observational and Experimental Studies of the Vegetation of Brecklands*, a revised collection of several articles that had appeared from 1915-1924 in the *Journal of Ecology*, Farrow stressed the urgent need for such experiment so that the new science of ecology could provide something more than superficial and uncertain conclusions. Similar concerns had led him into psychoanalysis: as he recalled, he was 'used to trying to solve certain kinds of problems in the external world, but here were some very interesting problems inside his own mind urgently calling for solution' (Farrow 1942, p. 5).

Just how strongly resonant were the problems of outer and inner worlds (Farrow's 'real' world of heather and voracious rabbits and his equally 'real' world of small boys and scissors) is an interesting consideration. Does it take a

man who has been 'really' threatened with castration in his childhood to discover the causal agency of rabbits' teeth in the ecology of grasses? Or vice versa? Does it take a man who has recognized the causal agency of rabbits' teeth to discover that castration threats might exist in reality? Twenty-three years after writing his *Plant Life on East Anglian Heaths*, Farrow noted (1948, p. xxx) the successful planting of forests due to a management regime which heeded his discovery that areas bereft of vegetation 'will grow and develop adult trees quite satisfactorily, provided the young seedlings are protected from certain extremely detrimental influences to which they are particularly susceptible'; he was struck by the 'curious coincidence that, in an apparently quite different form of biological research, he should have been caused to investigate in some detail detrimental influences upon young humans which similarly prevent them from growing into healthy adults'. For Farrow, the highly dynamic mind of the child was to be regarded as a 'rapidly growing tree'. An adult who hits or frightens a child 'risks pushing the young tree's roots unnaturally far down into the ground, or lopping off various branches as it were, and may easily do permanent damage which will prevent it from ever being able to grow into a satisfactory adult tree unless the damage is eventually removed and repaired by analysis' (Farrow 1948, p. 145).

Somewhat curiously, the rabbits did not figure as malevolent destroyers in Farrow's botanical work. Rather, Farrow's detailed investigations and diagrams revealed a phenomenon of vegetational degeneration that he praised rather more than he lamented: he found 'an extensive and beautiful generalized degeneration and zonation round the rabbit burrows' (Farrow 1925c, p. 104), 'a very striking and beautiful example of a dynamic biotic succession' (p. 41). As Darwin identified with the worms he studied (Phillips 1999, p. 44), so perhaps Farrow identified with the rabbits, in both ecological and psychological fields. Small wonder then that Farrow's two analysts were uncooperative: Farrow had castrated them of their analytic powers. That is, he created a castrating transference relation with them: either him or them (the two of them). And he (the rabbit) won - he transformed the British landscape (of psychoanalysis) from a forest (of analysts) to grassland (no analysts - self-analysis). We must, however, be very cautious in advancing such an interpretation: after all, Farrow was upset with the analysts for not partaking in his democratic ideal of scientific enquiry, as he wanted to share power, not simply remove it.

After the publication in 1925 of his *Plant Life on East Anglian Heaths*, Farrow (1925c) became less involved in ecology. Indeed, apart from a short comment for the *Journal of Ecology*, 'Notes on vegetation on Cavenham Heath, Breckland' in 1941, he seemingly dropped out of professional ecology altogether. In part he was devoting much of his energy to the family business in Spalding of E.W. Farrow and Sons Ltd. which sold surplus army supplies after World War I and later became an engineering firm dedicated to crop irrigation. He was a member of the Spalding Club and a very active member of the Spalding Gentlemen's Society (since 1913) which he addressed on 'Recent discoveries concerning the human mind' in 1927, the same year he spoke to the

British Psycho-Analytical Society. For many years he was the secretary of the Spalding Chiming Clock and Carillon Committee, one of the prime movers in an attempt to restore the Corn Exchange's carillon. His gentleman farmer and industrialist's lifestyle did not prevent him from publishing another series of psychoanalytic papers and, in 1942, *A Practical Method of Self-Analysis*, which was translated into Spanish, then German and French,⁸¹ and was republished twice in Great Britain (cf. Farrow 1948) with an American edition appearing in 1945 as *Psycho-Analyse Yourself*. This book, with a Foreword by Freud,⁸² consisted mainly of his papers published in the 1920s stitched together with the help of 'his old friend, Professor Tansley' (Farrow 1942, p. xiii), who contributed an Introduction to the third edition of 1948. But the analytic experiences on which it was based were maintained by Farrow over two decades: 'This book includes the results of more than 2,800 hours' research work spread over a period of 18 years, and the production of more than 12, 000,000 words of free association' (p. ix). In his revised and extended third edition, Farrow included excerpts from appreciative readers reporting unearthed repressions and new feelings of ease; he also quoted an undated letter sent by Tansley from Vienna (and almost certainly written in 1924) while the book was still in draft form:

I have received Chapter 3 and have discussed it with Professor Freud. We were both greatly interested. I am particularly impressed by the vigour and success with which you bring out the correct technique of free association. I have never seen it so well and so vigorously done and when I drew Freud's special attention to this part of your work he replied, '*Ja, tadellos*', meaning, of course, 'Yes, beyond reproach' or 'Impeccable'; and this is very high praise from him. (Farrow 1948, p. xx)

Farrow asserted that his *Practical Method* - noting down whatever comes into the conscious mind on fine writing paper (in five one-hour sessions per worrying matter) - had several advantages over work with an analyst: it was quick, comparatively inexpensive, it allowed shy people to keep thoughts to themselves, with the added benefit that the analyst could not be accused of reading the results into the mind of the patient. Above all, in proving the superfluity of the transference, the method checked the power of the (castrating) analysts who, in his experience, interfered too much and could not be induced to share in his spirit of experimental and democratic scientific enquiry. Farrow wrote: 'Being trained in what is probably a good school of scientific thought and

81. Anon. 'Obituary [E.P. Farrow]', *Lincolnshire Free Press*, 5 June 1956.

82. In the Preface to his 1942 book, Farrow translated the German original of Freud's Preface to his 1926 paper, published in translation in the *Internationale Zeitschrift*. By translating the simple 'Die Verfasser' as 'The author of this book' Farrow implied that Freud had seen the book. We cannot confirm that Freud did not approve its use - the book may have been completed and sent to Freud in, say, 1938-39. In addition, Strachey comments that in Farrow's book the Preface was printed 'with a statement that it was included by his [Freud's] permission. (This was, of course, some years after Freud's death)' (SE 20, p. 280 n.1). We have not been able to find such a statement in Farrow's book.

method, he would have respected the analyst more if he had permitted him to experiment and for being an experimentalist like himself' (Farrow 1942, pp. 201). Farrow also attempted to verify his findings concerning the reality of castration threats by interviewing the residents of Spalding: 'Careful observation and inquiries, extending over many years, in a typical English town have convinced the author that, on the average, not more than two, or at the most three, boys out of every ten escape some threat of this nature between the ages of 2 and 6 years' (p. 120).

Farrow's self-analysis brought up vivid memories of specific incidents of chastisement, allowing him to discover the lasting effects 'caused by physical blows and slaps which may be light in themselves but heavy to the child, in early infancy' (p. 93). After a session in 1924, Farrow traced the cause of his unconscious fear of the 'feminine body' to an incident that occurred some time between the age of 11 and 14 months. Hungry and having recently been weaned, he attempted to grasp the breast of a woman visitor to the house. She rebuffed him, he persisted and she eventually hit him: 'the feeling of injury, caused by a big strong woman to a defenceless infant, was enhanced no doubt because at the time she was regarded by the child as a substitute for his mother' (pp. 76-7). Going back even further to the age of 6 months, Farrow remembered his father pulled him away from his mother's breast when he hadn't finished suckling and thinking something like: 'I'll teach this big object to move me away from my mother (or food). I'll kick him very hard and perhaps he will not do it again' (p. 81). He discussed the incident with Tansley, who suggested to Farrow the term 'instinctive foresight' to describe the kind of thought the 6-month-old baby had had. Ten pages later, having just discussed how he had ventured too close to his puppy, Sally, while she was finishing off a nice bone, so that she expressed enormous 'anger and energy' in the 'most terrible and vicious doggy language', he comes back to Tansley:

While the author is extremely grateful to Professor Tansley's conception of 'instinctive foresight' as it seems to him to offer some reasonable explanation of his definite extremely early thoughts yet, nevertheless, on his thinking the matter over for several months, he rather doubts whether the foresight is really 'instinctive' in itself, although the power of the foresight is instinctive and inherited and foresight undoubtedly exists at a much earlier age in the human mind than is generally realized... On this view the foresight would arise owing to a strong instinctive and inherited tendency towards inductive reasoning on the part of the minds of some of the higher vertebrates rather than to its being instinctive in itself. (pp. 91-2)

A loner, practising on himself, Farrow was not averse to biting the hand that fed him; and, as we will see, not even the hand of Freud escaped Farrow's treatment.

In Farrow's last published article in the *International Journal of PsychoAnalysis*, 'On the view that repressed fear of severance of the genitalia is solely caused by external reality and is not inherited' (1945), Farrow dissected a letter that Freud sent him (18 July 1939) just two months before Freud's death (Farrow 1945, p. 162):

The manner I have come to look at castration-fear is the following:

- (1) I am sure it is inherited as well as individually acquired.
- (2) There is a well-known verse of Goethe giving expression to such a constellation:
What thou hast inherited from thy father thou must acquire anew in order to possess.
- (3) The intensity and pathogenic importance of the fear are based on the phylogenetic origin.
- (4) Few boys, if any, will escape the opportunities by which the inherited tendency is aroused, awakened.
- (5) In some cases the recent individual instigations may appear to have done the work themselves,
- (6) in other cases, where the threats have been too mild, insufficient, (7) the hereditary part will be called up to complete the effect.
- (8) The threat for itself could never be efficient if another condition be not fulfilled.
- (9) The boy must have had the visual experience of a female genital either before or after he is threatened.
- (10) It is necessary that these two factors combine.

Having numbered Freud's sentences, Farrow then considers - and, for the most part, attacks - each in turn. After commenting that 'It is important that the parents of a small boy should know that two women may, in certain circumstances, form a sort of `pack' in making extremely severe genitalseverance threats against him - far more severe than either of them would make alone...' (Farrow 1945, p. 163), Farrow enlists the help of his old friend Tansley:

With regard to point (1), Professor Tansley pointed out to the writer that it was difficult to see how a fear, as such, could possibly be inherited, and that the view that it could be seemed to him to be certainly founded upon confused thought. He said that what could be inherited was surely the tendency or disposition to feel a fear under the action of an appropriate stimulus. Perhaps what Professor Freud really meant when he said that the fear was inherited was that all males inherit such a mental disposition as to make the arrival of the fear inevitable under stimuli which they always, in practice, receive, or which they can rarely or never escape. A strong tendency towards a primitive, or at least very early, feeling of omnipotence does, on the other hand, appear to be definitely inherited by the individual. This primitive feeling is, however, utterly opposed to any fear of loss of the genitalia. (Farrow 1945, p. 163)

When Farrow and Freud met is unclear but, in the third edition of Farrow's *Practical Method*, there is the following passage:

The author well remembers Prof. Freud saying to him, when he had expressed doubt regarding the importance of the Oedipus complex, 'Ah! At one time you doubted the great importance of the castration complex. Eventually you will realize in the same way the great importance of the Oedipus complex.' (Farrow 1948, p. 166)

After a long illness, Farrow died in Spalding in 1956, a victim of Parkinson's disease.

C. C. Fagg

The other field scientist from Tansley's circle to acquire a life-long absorption with psychoanalysis was C.C. Fagg (1883-1965). Born in 1883, Fagg was forced, because of family poverty, to work in a shop from the age of 14. Seized with the desire for self-improvement, he took evening classes and was eventually admitted to the Civil Service in his early 20s, working as a Customs and Excise Officer until his retirement in 1941 at the age of 58. From 1906 to his death in 1965, he was a vigorous member, in fact, the most stalwart member, of the Croydon Natural History and Scientific Society (originally, in 1870, the Croydon Microscopical Club). He founded its Sociology Section in 1912, alongside a new Regional Survey Committee to help execute a survey of the Croydon area along the lines of other ongoing surveys inspired by the geographer-sociologist Professor Patrick Geddes (elected an honorary member of the Society in May 1922). Tansley, one of Fagg's many university extension course lecturers, was extremely supportive of the regional survey movement in its formative years and came to speak to the Society in 1912 regarding aspects of vegetational survey. The Sociological Section, which was initially planned 'so as to forward the department of the survey dealing with mankind',⁸³ discussing census methods and ways to investigate human geography, came to be wholly devoted to the study of psychoanalysis after 1921 when the Section added 'Psychological' to its title. Tansley (who accepted honorary membership in the Society in 1936) was key to this transition. In publicizing the new direction of the Section in the Society's monthly circular of September 1921, Fagg wrote, under the heading *The New Psychology*, that 'A recently published book under the above title, by A.G. Tansley F.R.S., affords an admirable introduction to the subject. The author, it will be remembered, addressed this Society on another subject a few years ago.'⁸⁴ In an obituary for Tansley published in the Society Proceedings, Fagg recalled that:

[he] first met Sir Arthur (then Mr A.G. Tansley) at the inaugural meeting of the British Ecological Society in 1911 [sic 1913]. He then carried an addition to the draft rules admitting societies to corporate membership and our Society became the first corporate member. Thus we have a complete set of the *British Journal of Ecology*, a valuable possession. Sir Arthur twice visited our Society to give lectures on practical aspects of ecological fieldwork. His lecture on 'Practical study of vegetation in the field' on November 19th, 1912 was published in our Transactions for that year. A rival interest to botany for Sir Arthur was Freudian psychology. His book, *The New Psychology and Its Relation to Life* (1920) soon became a best seller and was the text book for our Psychological Section in 1923 [sic 1922].⁸⁵

83. *Proceedings and Transactions of the Croydon Natural History and Scientific Society*, Volume VII (1910-1914) p. cxviii.

84. *Proceedings and Transactions of the Croydon Natural History and Scientific Society*, Volume IX (1919-1925), Croydon Natural History and Scientific Society No. 19 (new series), September 1921.

85. *Proceedings and Transactions of the Croydon Natural History and Scientific Society*, Volume XII (1948-1958), p. 177.

Ernest Jones gave a lecture to the General Meeting of the Society on 16 May 1922, entitled 'Psycho-analysis' - over 400 people attended.⁸⁶ In response to this great interest, the subject was selected for that year's university extension course and Fagg was asked to give a library talk entitled 'A simple introduction to psychoanalysis', at Thornton Heath Library. At the peak of its popularity in 1923, the monthly meetings of the Sociological and Psychological Section became fortnightly meetings with additional meetings held in private members' homes. In 1925, Fagg could report:

Nine meetings were held during the year, all of which were devoted to the discussion of some aspect of psychoanalysis. The interest of members of the section has remained keen throughout. During the early part of the year a course of three lectures by Mr Fagg were read, and discussed preparatory to their being delivered at a week-end school arranged under the auspices of the Adult Education Committee. Largely as a result of the interest aroused by these lectures many important psychoanalytic works have been added to the Public Library, which is now probably better equipped with works on the subject than any similar library. Mr Fagg has accepted an invitation from the Chief Librarian to prepare a reading list on psychoanalysis, which will be available as a guide to students early in the New Year.⁸⁷

This Section was exclusively devoted to the study of psychoanalysis until the early 1930s when the society experienced an 'unusual drop in membership' (26 resignations in two years). Undeterred, Fagg lectured on 'The interpretation of dreams' at a general meeting in 1932 and on 'Some thoughts on evolution' in 1933 which likely made use of material from his paper 'Psychosynthesis, or evolution in the light of Freudian psychology' delivered to the British Psychological Society the same year. The Croydon Psychological Section was well past its heyday by this time, consisting of only three members, including its leader, Fagg.⁸⁸

Fagg's scientific interests were never restricted to purely psychoanalytic ones. In 1909 he joined the Geologists' Association and was elected a Fellow of the Geological Society in 1918. From 1912, he also worked with the Surrey Photographic Survey (Taylor 1995, pp. 55-63), instigated at the Croydon Society the project of a survey of Croydon (which eventually came to fruition in the Society's major published achievement, the *Atlas of Croydon* of 1936) and, in 1915, founded the Regional Survey Section of the South-Eastern Union of Scientific Societies to bring together various sections, such as botany, geology and photography. He was co-author, with Geoffrey Hutchings, of the standard 1930 *Introduction to Regional Surveying*.⁸⁹ He acted as the Section's Honorary

86. Minutes of meeting on 17 February 1958, signed 17 March 1958, Psychology and Social Science Section, Archives of Croydon Natural History and Scientific Society.

87. *Proceedings and Transactions of the Croydon Natural History and Scientific Society*, Volume X (1925-1935), p. 5.

88. *Proceedings and Transactions of the Croydon Natural History and Scientific Society*, Volume X (1925-1935), p. 210.

89. See Matless 1999 for a critical examination of Fagg's psychoecological regional survey.

Secretary from 1915-1923, remaining a dominant influence throughout the 1920s, at the same time being very active in the Croydon Natural History Society, as President in 1921-1922 and 1937-1938, and as Editor of its *Proceedings and Transactions* from 1922-1930, again in 1940, and then from 1942-1948. Fagg was also a founder member of the Field Studies Council, formed in 1943 with Tansley as President, becoming the first warden at Juniper Hall from 1945-1951, where he proposed that a whole range of research activities, from geology to sociology (but with no mention of psychoanalysis), should be conducted.⁹⁰

In 1959, Fagg gave a talk to the Society (attended by four people), on the topic of 'My contact with Freud and English analysts'. The relevant portion of the Minutes reads as follows:

He traced his interest in medical psychology from a very early age. He studied (?) many books on the subject and later contacted Dr Ernest Jones and Prof. A.G. Tansley F.R.S., whose book *The New Psychology* was 'a best-seller' in 1920. This book gave Mr Fagg a feeling of emancipation so he took up psychoanalysis in earnest. In 1921-1922 Prof. A.G. Tansley spent nine months with Prof. Freud and Freud advised Tansley to take a patient to study and treat on his return to England. This Tansley did but when he went to Cambridge he asked Mr Fagg to cover for this patient. Mr Fagg did so and finally cured him of hand-washing. He became a cook and ? much later returned to teaching. During the treatment Mr Fagg contacted Prof. Freud and explained to him the method he had followed. Prof. Freud was very interested and many letters passed between them - many of them were on view. He asked Mr Fagg to prepare a paper on his method for international publication but subsequent difficulties arose and the paper did not go to press. Mr Fagg explained the fundamental differences between Freud and himself and went over the cases he successfully dealt with over the years in his own home. In 1921-1922 when Mr Fagg was president of our society, this section was formed.⁹¹

As a minute of a small society's meeting (with an acting secretary for that evening), we should not place too much trust in the accuracy of this account. The dates may well be inaccurately recorded or remembered wrongly by Fagg: certainly the dates for Tansley's analysis with Freud are inaccurate. We do, however, have some lateral corroboration in the form of Freud's six letters to Fagg, over the period 1923 to 1933, the period to which we now return.

Given what Fagg says in his recollections, it is quite possible that he was one of the correspondents who responded to Tansley's *The New Psychology* of 1920 with questions that went beyond Tansley's expertise. By January 1923, Fagg felt sufficiently clarified and informed to give his Presidential Address to the Croydon Natural History and Scientific Society on the topic 'The significance of Freudian psychology for the evolutionary theory'. In September 1923, he sent

90. C.C. Fagg, Memorandum, 28 April 1945, Archive of the Croydon Natural History and Scientific Society.

91. Ms. Minute of meeting held 14 December 1959, Croydon Natural History and Scientific Society: Psychology and Social Science Section, Minutes of Meetings 22 January 1951-16 April 1962. Our transcription, in places uncertain.

this paper to Freud, who responded warmly, both 'pleased' and 'surprised to get so clever a presentation of Psycho-Analysis from so remote a corner,'⁹² and requested permission to have it translated for publication in the *Internationale Zeitschrift für Psychoanalyse*, which Fagg eagerly gave. However, Freud's colleagues in Vienna were less than happy with those parts of Fagg's doctrinal presentation in which he used Adlerian rather than Freudian concepts, so asked Fagg to alter his text. We have no record of Fagg's reply, and the translation of his text did not appear.⁹³

In 1928, Fagg renewed his correspondence with Freud, prompted by the publication in the April number of the *International Journal of Psycho-Analysis* of the English translation of Freud's paper 'Fetishism', with its interpretation of the fetish object as representing the maternal phallus. Fagg clearly agreed with this, but wished to add a component concerning anal gratification, which Freud did not support. He did, however, compliment Fagg in a backhanded way:

All your remarks on [the cloaca] theory and the relation of the cloaca to the female genitals are excellent. I may add that they are universally adopted and belong to the common stock of our knowledge. It is highly to your credit that you have found them out for yourself.⁹⁴

Fagg was clearly being informed that he was an amateur - or, at the very least, handicapped by his lack of German - and was rediscovering the wheel (otherwise known as independent scientific confirmation). Nonetheless, in a postscript, Freud reassured Fagg about the correctness of his unconventional analytic technique:

Your following the obsessional patient into his privacy was by no means a sin against the orthodox technique. If we do not always the same lack of time is our only motive. In a similar case I had the patient watched by a person of my confidence with the same good results.⁹⁵

It is probable that the patient referred to in Freud's next letter, written four weeks, later, is this same obsessional:

92. Sigmund Freud to C.C. Fagg, 25 October 1923, Freud Museum, London.

93. In 1933, Fagg did comment on Freud's recommendation that he alter the 1923 paper as follows: 'Freud would claim that the will to power is a function of libido, defined as the mental aspect of the sexual instinct. He wished me to substitute 'narcissistic ego-libido' for the 'omnipotence impulse,' but I could not see my way to do so without either extending the conception of libido or sacrificing something that is, in my view, of vital importance in the interpretation of the phenomena of organic evolution. The sublimation of repressed id impulses may in some cases be regarded as the *reclamation* of psychic energy for purposes outside and transcending sexuality, even in the extended Freudian sense. In man such purposes may not always conform to contemporary views as to what is socially useful or even socially tolerable. Those individuals who wish to pursue them must either gain and uphold oligarchical power, conceal their activities from democracy or perish at its hands' (Fagg 1933, p. 136).

94. Sigmund Freud to C.C. Fagg, 28 June 1928, Freud Museum, London. 95. Sigmund Freud to C.C. Fagg, 28 June 1928, Freud Museum, London.

If your patient has succeeded in finding new outlets of social use for his pathological trends I think this effect should be considered as a recovery from his neurotic condition. We have no right to claim for more ⁹⁶

And Fagg added a handwritten note at the bottom of Freud's letter clarifying what they had been discussing:

A symptom of the obsessional patient referred to in the first paragraph was skoptophilia directed towards full-breasted women. Towards the end of my treatment of the patient this was displaced to watching birds in my garden.

Whether Fagg had informed Freud about - or Freud already knew - the double meaning of the English word 'tit' - or 'bird' for that matter - is not clear.

What is most striking about Freud's interchanges with Fagg is how supportive he is of Fagg's work with patients, while being authoritatively carping about theoretical questions. In 1932, Fagg sent Freud draft chapters of a book he was preparing - he would spend the rest of his life preparing it - and Freud replied with some comments, one of them critical:

Your description of consciousness does not conform to our conception. The 'conscious' cannot be called a region or system. To be conscious is a quality or a condition superadded to being psychic, while psychic processes in themselves are all unconscious in the descriptive sense, and are to be classified into preconscious and truly unconscious. The system Bw-W is a special organ and the place where consciousness is generated. But your diagram is all right.⁹⁷

Such an assured and non-negotiable tone about the theoretical terms crucial to Freud's psychoanalytic system had appeared once before in his letters to Fagg, in December 1923:

I have to complain that you misrepresent my definition of the Unconscious which in reality is absolutely identical with yours and with Tansley's. I would like to compliment you on your comparison of this primary unconscious with the magma of the earth's interior and would fain appropriate it. There is a misunderstanding; the Oedipus complex and others annexed to it are the beginning of the *repressed* matter, not of the *unconscious* one. In my judgment, what is repressed is unconscious, but not all the unconscious belongs to the repressed. You will find this view clearly explained in my last pamphlet *Das Ich and das Es* (not yet translated). But former utterances evidently meant the same. It is Tansley who is responsible for this mistake and I am sorry you followed him rather than my own sayings. But perhaps you do not read German easily and were dependent on translation.⁹⁸

Here, in 1923, it is Tansley who is blamed for Fagg's error, as if Fagg were part of the Tansley 'school' of dynamic psychology. By 1932, Fagg is seen to be

96. Sigmund Freud to C.C. Fagg, 25 July 1928, Freud Museum, London.

97. Sigmund Freud to C.C. Fagg, 30 November 1932, Freud Museum, London.

98. Sigmund Freud to C.C. Fagg, 13 December 1923, Freud Museum, London. It is possible that Fagg's analogy of the magma stimulated Freud to his well-known examples of plausible and implausible scientific hypotheses - the interior of the earth consisting of soda-water, or of jam (Freud, 'Dreams and occultism', *New Introductory Lectures* (1933 [1932]), SE XXII 32).

responsible for his own errors - by this time Tansley's relations with psychoanalysis were more distant.

Freud's final letter to Fagg, dated 3 December 1933, was a response to Fagg's offprint of his lecture to the Medical Section of the British Psychological Society, printed in the *British Journal of Medical Psychology*, at that time very much dominated by psychoanalysts - Jones and Rickman were both very active in running it. Fagg's paper, 'Psychosynthesis, or evolution in the light of Freudian psychology', was a speculative essay, somewhat in the spirit of Freud's *Beyond the Pleasure Principle*, which joined considerations of the properties of protozoa, metazoa and finally humans as the most successful of the metazoa - although, as Fagg whimsically confessed, 'it may be that other creatures inhabiting the earth or sea hold potentialities far transcending those of the human species - I have a great regard for the common or garden slug in this connection' (Fagg 1933, p. 123). Drawing upon the influential work of Geddes and Thompson, *The Evolution of Sex*, whose vocabulary of anabolism (acquisition of energy) and catabolism (expenditure) could replace the language of eros and the death drive, Fagg saw the principal process at work in life as being the input and output of energy of the organism. The opposition and tension between these two processes formed a duality that ruled the entire biological realm, from the life-choices made by the amoeba in the course of global history, via the contrast between anabolic plants and catabolic animals to the last great problem facing the human species, the transition from the 'state of solitariness to that of the social organism' - after which Fagg interpolated the emphatic comment that 'there is no going back' (Fagg 1933, p. 120). Passing via Mendelism and his assumption that the only way forward to the higher social organism was the emergence of a few specimens of a higher species that he, following the eugenic vision of George Bernard Shaw, called 'superman', Fagg used his dualistic schema to divide the human race into two categories, the Inspectors and the Bohemians.⁹⁹ Fagg's psychoanalytic scheme underlying these two types emphasized the crucial importance of urethral erotism:

My view is that most so-called genital erotism has a urethral basis, and for object, so far as the male is concerned, it is often, and particularly in fetishism, unconsciously directed towards an anus, not necessarily male. I find this view thrust upon me with the persistence of a paranoid delusion. For this reason I have regarded it with suspicion, but I still adhere to it. (Fagg, 1933, p. 127)

Equating - roughly - anal sexuality with anabolism and urethral sexuality with catabolism, Fagg envisaged the emergence of a higher type, synthesizing the virtues of anabolism and catabolism, anal and urethral erotisms, the Inspector and the Bohemian into the *Vir supremus*.

99. Fagg corresponded with George Bernard Shaw in 1907-1908, and a poem in his hand begins: 'Shaw is a something that shall be surpassed' - Fagg Papers, Archives of the Croydon Natural History and Scientific Society. He also attended Fabian socialist meetings before the Great War.

Freud's letter of thanks for this paper is pithy and ironic:

I am glad hearing of you after so long an interval. I got the reprints. That which brings me together with Darwin did not teach me many news, but I liked it so much. The other is full of suggestions for serious thought. I am sorry I do not believe in *Vir supremus*, in any case I am sure not to meet him myself.¹⁰⁰

Late 1933 was not a moment when Freud would be particularly welcoming of a proposal to bind psychoanalysis into a speculative bio-cultural project aiming at the production of a supreme being, a superman. The tone of humorous scepticism he adopted is often found when he encountered millenarian hopes; for instance, when the BBC was declaring in September 1939, a few days before he died, that this would be the last war, he was asked if he believed that and responded: 'Anyhow it is my last war' (Jones 1957, p. 262).

Fagg's involvement with the psychoanalytic movement is intrinsically interesting for the light it throws on how an enthusiastic 'amateur' could develop a life-long commitment to Freud's ideas, publish papers developing them, and could, without having any training of which we are aware, establish a psychoanalytic practice of some kind without encountering hostility from powers that be (such as Ernest Jones and the British Psycho-Analytical Society), and with continued encouragement from Freud himself. Fagg's relationship with Tansley - which probably developed from Fagg approaching Tansley following the publication of *The New Psychology* - not only gave him an entree to Freud, but gave him at least one patient, when Tansley's own switching of professional direction required him to move on from his 'experimental' psychoanalytic practice. As Fagg's obituary notice put it, 'Fagg however became and remained a loyal but critical disciple. In his time he corresponded with the master and indeed, in a small way, practised psychoanalysis'¹⁰¹ His writings are interspersed with knowing asides derived from his own psychoanalytic practice, for instance: 'Whoever has analysed obsessional patients must have been struck at times by the way in which their unconscious conflicts reflect the actual conflicts that have taken place between their parents. This may be adequately explained by infantile identification with both parents. On the other hand it may be due in part to gametic amphimixis...' (Fagg 1933, p. 135).

Fagg was particularly active in the area of 'depth psychology' during the 1950s: he gave talks to the Psychology and Sociology Section on Darwin's psychoneurosis (1954), 'The death wish and dreams' (1954), 'The choice of a mate' (1955), 'Dreams and the occult' (1958) and 'A case of hysterical blindness' (1960).¹⁰² A further unpublished Minute of the Croydon Society's

100. Sigmund Freud to C.C. Fagg, 3 December 1933, Freud Museum, London.

101. Anon. (1964) 'Obituary Memoir. Christopher Charles Fagg, F.G.S. (Past-President)', *Proceedings and Transactions of the Croydon Natural History and Scientific Society* 13(4): 1934.

102. Paul Sowan, 'Check list of documents in Archives of Croydon Natural History and Scientific Society', 24 July 1998.

Proceedings, this time from a talk given in 1961 concerning 'a case of childlessness', throws light on Fagg's style of therapy, as he practised it at some point between the 1920s and 1960:

During 8 years of his chairmanship Mr Fagg observed that he felt a little conscious of dragging in psychopathology which he had studied from an early age. Forty years ago he studied Freud's Psycho-Analysis which proved a great revelation to him. Freud also sent him patients. One case of Analysis he studied concerned Tom and Joyce. After 4 years of marriage, they were worried about infertility. They did not speak of this to each other but to Mr Fagg and his wife. Tom shewed mild obsessional symptoms (repeated formulae to lay ghosts) - fetishism traceable to interest in elder sister. Masturbation hadn't been conquered which caused mental worry and fears. He was extremely sensitive, hated to hurt others - inferiority complex. Both had advice - too much inhibition on part of one or both - hymen intact - what could be done? Tom confided that he had not given complete satisfaction. Joyce had had nightmare - house in flames and phobia. House - body. Flames - male organ. Hidden wish - had she been married to others she would have had children - she really wished to find house in flames. She had nervous indigestion and was hysterical - next day flames replaced by fear of crossing the road and being run over - lasted a few days. Freud states this to be sexual intercourse. Little later Joyce needed an extra shelf - Tom showed disinclination to oblige - saying he had no tools - tools masculine - timber feminine. Mr Fagg gave instructions and helped.

Tom and Joyce later came to supper and asked if reaction was repressed sadism - in the dream - Tom had knocked driver of car down. His case seemed to be oversublimation of masculine sadism (fear of hurting Joyce) and she had revealed repressed masochism. Later everything well - child born. Mr Fagg ended by saying that a proportion of barren marriages usually are due to psychological causes, but by Tom and Joyce allowing the facts to emerge, they had learnt more of themselves and about life. 103

When Fagg read the first two volumes of Ernest Jones's biography of Freud, he sent him copies of his correspondence with Freud, together with two chapters of the manuscript he had been 'writing and scrapping for the past 50 years'. 104 The book was to be titled 'Sublimation or Perversion', and included material that he had sent to Freud in the 1920s and early 1930s which had received Freud's approval. Fagg voiced his own reluctance to publish - after all some of this material had been seen by Freud 30 years previously, who had already noted how he hoped 'to see the book finished one day not too far away'. 105 Fagg told Jones how he was half-hearted about publishing the book in part because 'I was obliged to use a pseudonym as an additional safeguard of the identity of 'patients'. In theory I believe that the conquest of narcissistic exhibitionism is personally important but I have not succeeded in conquering it.' 106 This somewhat contorted argument is clarified when one notes that the pseudonym he had chosen for the author of the book was Christopher Kent - an allusion

103. Minute, Psychological and Social Sciences Section, Croydon Natural History and Scientific Society, Monday 20 February 1961 at 7:30 p.m., Archives of the Society.

104. C.C. Fagg to Ernest Jones, 27 September 1956, LoC Sigmund Freud Archives.

105. Sigmund Freud to C.C. Fagg, 30 November 1932, Freud Museum, London.

106. C.C. Fagg to Ernest Jones, 27 September 1956, LoC Sigmund Freud Archives.

back to his 1930s preoccupation with the psychobiological theory of the superman. This strange pseudonym may throw some light on the identity of the unknown 'Mr Kent' who visited Freud in London shortly after he arrived from Vienna, on 18 July 1938 (Molnar 1992, p. 243).

It is clear from his writings that Fagg's psychoanalytic imagination was fired by fetishism; his clinical correspondence with Freud centred on the theory of fetishism and its relations to obsessional neurosis. His evocation of the aesthetics of the shoe fetish is, indeed, *a tour de force* and like no other in the psychoanalytic literature:

A feminine shoe with a high Louis heel several times repeats the graceful double curve that was styled by Hogarth 'the line of beauty'. I have already mentioned that this curve held for Charles Darwin who gained inspiration from 'the charm of the contours of the dry valleys of the Chalk country at Downe'. It seems to hold a very special place in Nature and in the human mind not only in relation to aesthetics. In bodily functioning it is the wave-like curve of the peristaltic motion of the intestine which propels its contents. It is the pleasing curve of ripples on the surface of a pond or waves in the open sea, of serpentine propulsion, of a cornfield in the wind and many other natural phenomena. In mathematics and physics it is the graphic representation of *simple harmonic motion* propagating in space, as in the wave theory of the propagation of light. In its static condition, if one can speak of motion as static, it is represented by a circle enclosing a cross, the sign of the Rosicrucian Fraternity and, as in the hot cross bun, the symbol of fertility.¹⁰⁷

This brief passage captures something of his style of amateur polymath, bringing together biology, sexuality and aesthetics in a grandiose speculative theory. Almost certainly, like so many other enthusiasts for psychoanalysis, Fagg was using his own personal experience and sexuality as raw material for his writing. Yet for Fagg, as for Tansley, it was personal experience fused into a far larger biological vision - Fagg's admiration for Darwin was as great if not greater than his admiration for Freud; yet it was more the Spencerian Darwin of evolutionary metaphysics than the Darwin that contemporary mathematical biologists such as Ronald Fisher or Lionel Penrose would recognize. For Fagg, sexual selection was possibly a greater discovery of Darwin's than natural selection. And his account of human sexuality, while replete with the clinical investigation of anal, urethral and oral sexuality in Freudian style, would also include the function of sexual selection interwoven with the Oedipus complex. What is of interest for us is to see how an amateur scientist, bulwark of the local scientific institutions of his day, infused both his personal utopias for a planned society which transcended the narrowness of the Inspector's world - surely one he as a Customs Officer knew well - and the sexual peccadilloes of the private world of the genteel residents of Croydon into a lifetime of work enthusiastically devoted to science - a social, psychoanalytic and biological science.

107. Kent, Christopher [C.C. Fagg] 1956 (?) Manuscript, Chap. XIV 'Fetishism', ff. 8-9.

Reginald Kapp

And he was not the only one of his type. In 1955, Professor Reginald Kapp, Member of the Institute of Electrical Engineers, was elected an Honorary Member of the Croydon Society. Kapp (1885-1966), born in 1885, followed his father's profession by becoming an electrical engineer. In the 1920s he played a significant part in the development of the British National Grid, and became Professor of Electrical Engineering at University College London, retiring in 1950. His career had a strangely parallel path to that of Fagg in his enthusiasm for psychoanalysis in the 1920s. James Strachey reported the occasion when this enthusiasm was publicly displayed to the British Psycho-Analytical Society in June 1925, within a few months of the formation of the Cambridge Psychoanalytic Group:

Yesterday there was a meeting of the Psych-An Society; and Rickman induced Frank [Ramsey] to come to it. By a most curious chance it was by far the best meeting there's ever been: a paper was read by a curious friend of Rickman's - not a member - called Kapp (one sometimes sees him at the 1917 Club). He's ultimately some kind of foreigner (Jew) and by profession an optical engineer, I believe. But he evidently has immense knowledge of 'Pa of the most modern type, and produced a very clearly thought out & difficult paper on - heaven help me if I know what - something to do with Repression. My poor brain failed entirely. But Jones & the Glovers put up a very interesting discussion, which was just the right blend of the theoretical & clinical. Frank was enthusiastic about it & said he thought it was the best discussion he'd ever heard at any such society & that not a single foolish remark had been made all the evening. (Strachey & Strachey 1986, pp. 288-9; 18 June 1925)

In October 1925, Kapp was elected an Associate Member of the Society (Bryan 1926a). His paper was published in the *International Journal of PsychoAnalysis* in 1926, under the title 'Sensation and narcissism'. And Tansley in 1925, possibly as a result of hearing Kapp's paper, noted in an abstract of a paper published in German which he wrote for the *International Journal* that: 'The use of tools, i.e. the projection of bodily organs on to the outer world (Kapp), is the result and not the cause of the evolution of man, these means of culture corresponding to the return of the repressed, not as bodily organs, but in the form of artificial tools' (Tansley 1927). Perhaps Tansley recognized a kindred soul in Kapp - a committed, working scientist, whose latest scientific enthusiasm was the inner world of psychoanalysis. And, as we have seen, Kapp was called upon - or volunteered himself - in 1931 to publish a caustic criticism of the metapsychological speculations of Bernfeld and Feitelberg alongside another of Tansley's scientific colleagues, Lionel Penrose.

Kapp gave another paper to the Society on 2 November 1927, entitled 'Exognosis', whose argument was that direct knowledge can only be had of occurrences within one's own body - the deduction that sense data are due to external objects constitutes a psychological act to which he gave the name 'exognosis' (Bryan 1928). In January 1930, Kapp again spoke, this time on 'Non-clinical case-material', arguing that 'the full possibilities of

psychoanalytical work outside therapeutic practice have not yet been exploited... It is suggested that the time has come when the method employed by the research worker should depart more radically from that used by the therapist' (A. Freud 1930). Kapp did not maintain his institutional commitment to psychoanalysis; in October 1934, he resigned from the British Society. But he maintained his intellectual interest, as is indicated by his long friendship with John Wisdom, a philosopher at Cambridge who maintained a vigorous interest in psychoanalysis from the early 1930s to his death in 1993, since in 1954 he was on the Editorial Board of the British Society for the Philosophy of Science and, in that capacity, still vigorously considering the foundations of Freud's theory of the unconscious (Wisdom 1984 p. 316, n. 1). And Fagg and Kapp, these two unlikely members of the British psychoanalytic world of the 1920s and 1930s, ended up as Honorary Members of the Croydon Natural History and Scientific Society; they died within a year of each other, Fagg in 1965, Kapp in 1966.

English Psychoanalysis in the Mid-1920s

In June 1925 Tansley reviewed the Stracheys' translation of Freud's case histories for *The Nation & The Athenaeum*.¹⁰⁸ His introductory comments on the value of case histories revealed a subtle appreciation of the genre which was surely a product of his own experience in analysis: 'Case histories are of special value to students because every psychoanalysis is, or should be, an organic whole, a definite development of the patient's mind in relation to the analyst' (Tansley 1925a, p. 348). A wide-ranging controversy, whose echoes were transmitted by Ernest Jones to Freud in Vienna, erupted in response to his review. A Miss E.C. Allmond emphatically protested at Tansley's tone, which implied that Freud's contribution to science was 'epoch-making', comparable to those of Copernicus and Einstein (Allmond 1925a).

Despite the fact that Melanie Klein's first London lectures, at which Rickman, Strachey and many others were present, were then taking place in Gordon Square, Tansley's response came from Sweden (Grosskurth 1986, p. 137-8). He was attending the second post-war International Phytogeographic Excursion (Cooper 1957); his reply to criticism thus indicates the exquisite balance he attempted to maintain between his botanical and psychoanalytic commitments. Tansley's reply surmised that the neglect of Freud by psychologists and others

108. Given the non-medical and non-psychological character of the Cambridge group of experienced analytical men of whom Tansley was one, his remark at the end of his review of the Hogarth Press's translation indicates the tensions between the various groups in 1920s England: '...according to the publishers' announcement, "the sale of this book is strictly limited to members of the medical profession, psychoanalysts, scholars, and to such adults who may have a definite position in the field of psychological or social research" - a somewhat vague public, whose limits would not be easy to determine!' (Tansley 1925a, p. 348). No doubt Tansley himself wondered if, under these restrictions, he would qualify as a legitimate buyer of the book.

that Allmond had cited as evidence of the worthlessness of his theories was not only due to their wise decision to remain silent on a subject with which they were imperfectly acquainted, but also that Freud's teaching:

is undoubtedly very astonishing, and his theories certainly give a first impression of being bizarre and grotesque to an extreme degree - and this apart from the disgust and general repugnance they arouse in many people. Psychoanalysis is altogether disconcerting, and most men dislike being disconcerted. Darwin touched human pride and sensitiveness on a sore point with his conclusions on human ancestry. He was bitterly attacked. Freud probes far more deeply and painfully, and is even more bitterly attacked. (Tansley 1925b, p. 567)

Tansley also introduced a theme that would develop in the weeks ahead: 'I have devoted what intelligence and critical judgment I may possess to a first-hand as well as to a literary study of Freudian analysis, and, in common with many other students of science who have done the same thing, I see no alternative to accepting the essential body of Freud's doctrines as working hypotheses, without the help of which we cannot at present form a picture of many fundamental phenomena of the human mind, untouched or at most quite inadequately treated by exponents of psychology who neglect Freud' (Tansley 1925b). The key word - 'first-hand' - introduced the question: what counts as a qualification to talk with authority about Freud?

One correspondent, Bryan Donkin, backing up the laudatory references to A. Wohlgenuth's *Critical Examination of Psycho-Analysis*, published in 1923, focused his criticism on the 'eccentric view that nobody has a right to criticize psychoanalysis at all unless they have previously practised it' (Donkin 1925). Miss Allmond amplified her criticism by asserting that psychoanalytic doctrines are entirely a 'subjective reaction' and the phenomena they describe are the result of suggestion by analysts or parents. In her view, no controlled experiments had refuted this charge of 'subjectivity' (Allmond 1925b).

The following week brought an attempt to mediate between proponents and critics by one 'Siela', a pseudonym chosen by John Maynard Keynes ('Siela' [John Maynard Keynes] 1925; see Winslow 1986, 1990). Keynes subtly contrasted Freud's overwhelming persuasiveness with the very small number of instances of inductive proof on offer, ending his letter by referring to Freud, approvingly, as 'a sort of devil'. A more representative considered view of the critics was that of P. McBride, who, after several readings of *The Interpretation of Dreams*, was obliged to conclude that Freud's theories rested on 'no evidence whatever, excepting the too vivid imagination of the author' (McBride 1925). Tansley's rejoinder - a 'brilliant letter'¹⁰⁹ as Jones called it in reporting on the controversy to Freud, Abraham and the other members of the Secret Committee

109. Ernest Jones, Circular letter, 19 November 1925, Library of Congress. The system of circular letters (*Rundbriefe*) by the six members of the Secret Committee (Abraham, Eitingon, Sachs, Rank, Jones, Ferenczi) was initiated by Freud in September 1920 (see Grosskurth 1991, pp. 96 ff.).

- pointed to 'the sympathy and approval' of 'one group of outside observers' who 'find the doctrines [of psychoanalysis] in harmony with their independent observation of human life' (Tansley 1925c). In Tansley's letter, in a number of respects a response to Keynes, the latter's 'intuitions' had become 'independent observations', since Tansley's fundamental riposte to Freud's critics was that of the independent observer who had, unlike the critics or outside observers, at 'first-hand' looked through the 'new instrument for investigating the human mind' at the disposal of psychoanalysts, asking rhetorically: 'What would Sir Bryan Donkin say to a disbeliever in the results of histology who refused to look through a microscope?' Once again, Tansley's personal account of his own path from initial scepticism served as argument by example:

I confess that at first I was sceptical of very many of the Freudian theses, and even now there are interpretations which strike me as far-fetched. But I have become very chary of downright unbelief, for in so many cases I have been forced by accumulating evidence to accept interpretations which at first I rejected as nonsensical. Freud generally turns out to be right. (Tansley 1925c)

To the 'pseudo-scientific bombast' of Freud's critics concerning scientific method, Tansley replied, clearly with his own non-experimental decades of botanical research in mind: 'We all know that controlled experiment is by far the most satisfactory method of establishing any scientific conclusion. But the method of controlled experiment is simply not available in many spheres of scientific investigation, and no one denies them the name of science or refuses to give credence to results based on converging lines of evidence' (Tansley 1925c). Tansley refused to defend psychoanalysis on any other grounds than those of scientific solidity and the indubitable experiences of the unprejudiced, enquiring mind. (Not so James Strachey, whose 1924 review of Wohlgenuth's book had opened with languid Bloomsbury flair: 'This volume is chiefly remarkable for its dust-cover, and we therefore propose in this instance to review the dust-cover instead of the book which it contains' (Strachey 1924, p. 222).)

A week later, Jones reported to Freud: 'Tansley, who is the last man to welcome polemics, has nevertheless been drawn into a controversy in a weekly magazine where he had been extensively attacked' (Freud & Jones 1993, p. 581, 19 September 1925; see also Jones 1957, p. 113). Jones was not quite accurate here: Tansley had spent much of his botanical career in fundamental disagreement with the established methods and direction of research, and had also been involved in sustained controversy. But he was not a man who was used to polemics in the literary and weekly press. His appearance there on this occasion may have, by accident, led to his re-appearing in fictional form in a novel published the following year. In the same number of *The Nation & The Athenaeum*, adjacent to Tansley's lengthiest letter so that her name and 'Freudian psycho-analysis' were placed in dangerous and immediate proximity, Virginia Woolf conducted one of her delightful debates: 'while I would cheerfully

become Shakespeare's cat, Scott's pig, or Keats's canary, if by so doing I could share the society of those great men, I would not cross the road (reasons of curiosity apart) to dine with Wordsworth, Byron, or Dickens' (Woolf 1925). In *To the Lighthouse*, with its anti-psychoanalytic animus, Woolf's Mr Tansley figures as a repressed and self-absorbed Oxbridge type.

Concluding Remarks

We do not have sufficient evidence to be certain that Tansley was the central figure in these two networks - the Cambridge network (Jeffreys, Rickman, Ramsey *et al.*) and the field scientists' network (Godwin, Fagg and Farrow) - but he was certainly both of these groups' senior scientific presence. It should be borne in mind, in addition, that we came upon these groups as part of a larger study of Tansley's relations with psychoanalysis (Cameron & Forrester 1999; Forrester & Cameron 1999). Yet Tansley's later decision not to devote himself full-time to psychoanalysis is both representative and emblematic of the paths taken by these informal colleagues. Just as the existence of these psychoanalytic enthusiasts of the 1920s tells us about a moment in the history of psychoanalysis and the more general organization of 'scientific' enquiry in Britain in the early part of this century, so do Tansley's and these others' later destinies highlight an important fact about that history: that this particular moment and opening up of psychoanalytic enthusiasm would be closed down, would no longer be possible. The events that led to this 'closure' are well documented, but a sense of the consequences - what may have been lost - has not been recounted in this detail.

Fagg certainly felt that Tansley's path was truly emblematic for a whole generation. In 1926, he drew up *Psychoanalysis: A Select Reading List* for Croydon Public Libraries in which he described Tansley's *New Psychology* as follows:

The book has a special interest in itself as marking a phase in the transition of the author, who is a well known biologist and ecologist, into the psychoanalytic camp, and it probably owes its popularity in part to this fact. In other words the author is symptomatic of our times and by becoming eloquently articulate he has struck a sympathetic chord in thousands of thoughtful readers. (Fagg 1926, p. 10)

And Ernest Jones, as we (Cameron & Forrester 1999) have indicated in "A nice type of the English scientist", welcomed Tansley into the small British PsychoAnalytical Society with open arms - his Cambridge connections, his scientific standing and his F. R.S. counted for much. Jones was certainly more welcoming of Tansley than he was of the Stracheys. Tansley's analysis with Freud and Freud's evident approval of him made his election as a Full Member of the Society a straightforward matter. Yet in the very circular letter in which Jones reported this election to the other European leaders of psychoanalysis was another item of news which would make psychoanalysis in the future less welcoming to the Tansleys of this world:

At our annual [London] business meeting last week, the same officers were elected. Mr Tansley was promoted to be a full member, the name of one associate member who had recently written an unsatisfactory book was omitted and two new associate members [Inman and Kapp] were elected. Following the instructions of the Congress, we elected an Education Committee of five, consisting of the four directors of the Institute (Bryan, Glover, Rickman and myself) with the addition of FlUgel."¹⁰

A resolution passed at the Bad Homburg Congress of 1925 - a congress which Jones and other English analysts had originally planned would take place in Cambridge under Tansley's aegis - required each national society to set up an Education and Training Committee. Combined with Max Eitingon's proposals for a formal system of psychoanalytic training, also submitted to the Bad Homburg Congress, this system of Education Committees spelt the beginning of a new development in the national psychoanalytic societies. To put it crudely, they ceased to be primarily 'scientific colleges' and became 'training institutes'; they ceased to be relatively informal meetings of equals devoted to psychoanalytic ideas and became more strictly hierarchical institutions devoted to training professional psychoanalysts. As Pearl King has put it, the founding of the Education Committees marked the beginning of the end of the era of the gentleman (and gentlewoman) psychoanalytic scholar (King & Holder 1992, p. 154).

Symbolically, then, Tansley became a full Member of the British Society the same day as the international psychoanalytic movement changed direction, away from distinguished *savants* like him, towards a more restrictive conception of the responsibilities of organized psychoanalysis. From the 1920s on, there would be fewer Tansleys and Stracheys who could become practising analysts via Freud's, or anyone else's, couch, without the 'appropriate' qualifications. Tansley was welcomed into the analytic institutions just when they were becoming more vigilant about both external threats, whether vitriolic critiques or undisciplined and semi-ignorant popularity, and internal threats (who is permitted to be an analyst?). Hence, a month later, Jones is writing another circular letter, again mentioning Tansley, but in the context of a different but related debate - that on lay analysis:

In regard to lay analysts, no one except Professor has supported them more consistently than I have. From the beginning no discrimination has been made in our Society between lay and medical analysts and at present nearly 40% of our members are laymen. Among these are several of the most competent such as Flugel, Tansley, the Stracheys and Mrs Riviere.¹¹¹

While it is true that Jones never objected to lay analysts, he was also amongst the keenest of the close Freudian followers to secure the respectability and acceptability of psychoanalysis and its practitioners, especially amongst the medical profession. He was to spend many hours in the late 1920s battling

110. Ernest Jones, circular letter, 15 October 1925, Library of Congress.

111. Ernest Jones, circular letter, 19 November 1925, Library of Congress.

against the conservative scepticism of the British Medical Association so as to win their approval - in the event, highly distant and qualified - for analysts trained by the Education and Training Committee under the aegis of the British Society. What is curious is how easily, once again, Tansley is made to fit into one of his schemes - alongside those other distinguished graduates of the Cambridge-and-Berggasse-19 ethos.¹¹² Within 14 months of this letter being posted, Tansley had finally decided against becoming a full-time analyst and accepted the Chair in Botany at Oxford.

Tansley continued to work on his psychology papers, exchanged letters with Freud and considered writing another book on the subject of psychoanalysis. At Magdalen College where he resided from Monday to Friday, returning to Grantchester on weekends, Tansley participated in a Philosophy Club which included physicists and chemists and Fellows engaged in philosophical pursuits such as Thomas Dewar Weldon and John Frederick (Jack) Wolfenden (who as Lord Wolfenden produced the *Wolfenden Report* in the 1950s). Prefacing one of his own talks to the Club, 'The temporal genetic series as a means of approach to philosophy', with a few autobiographical notes, Tansley admitted that he didn't know if he was 'regarded primarily as a biologist or as a psychologist by my fellow members of this club.'¹¹³ Tansley then proceeded to elaborate his scheme for approaching the gap between psychoanalysis and biology and, in doing so, he offered a description of 'mind' strongly analogous to his 'ecosystem', a concept he would introduce into botanical literature three years later (Cameron forthcoming). Another talk given to the group, 'On criticisms of Freudian theory', stressed the important role open-minded scientists, such as himself, had to play in advancing psychoanalytic knowledge. And it was Tansley who wrote Freud's obituary for the Royal Society (Cameron & Forrester 1999)

So what audit can be drawn up of the relations of these disparate and creative individuals to psychoanalysis? Penrose's biological interests took him a long way from psychoanalysis, but he never renounced his conviction of the importance of Freud's discoveries. Ramsey's energetic and roving intelligence might have brought him back to psychoanalysis if he had lived, but there were fewer Ramseys or Stracheys amongst later psychoanalysts. Farrow was, obviously, by nature a loner, and pursued his serious commitment to psychoanalysis in his own way. Fagg, the most unlikely of all of Tansley's psychoanalytic colleagues, remained committed to Freud, to his ideas, to his methods, and practised some form of analysis well into the 1950s - but with

112. On Alix Strachey and Joan Riviere, see Appignanesi & Forrester (1992) pp. 352-71. While not a graduate of Cambridge, Joan Riviere spent much time there with her uncle, the classicist, A.W. Verrall, whose lectures James Strachey singled out as the only ones worth attending in Cambridge, and who, before World War I, when Joan would often visit, played a major part in the activities of the Society for Psychical Research, which introduced many pre-war students to Freud's writings for the first time.

113. A.G. Tansley, 'Autobiographical introduction', 5 May 1932, probably delivered to the Magdalen Philosophy Club, Tansley Archive, Dept. of Plant Sciences, Cambridge.

absolutely no contact with either the British Psycho-Analytical Society or the International Psycho-analytic Association, keeping his work in 'psychology' restricted to the local audience of the Psychology Section of the Croydon Natural History and Scientific Society.

It is certain that, having followed the threads of Tansley's network, we need to broaden somewhat our conception of the different strands that made up that early history. Beyond the medical, the psychological, the literary, there are the Jeffreys, the Tansleys and the Faggs - scientists whose predisposition towards psychoanalysis stemmed in large part from their having, effortlessly and without ambiguity, subscribed to what Freud would call the 'scientific *Weltanschauung*' in his *Introductory Lecture* of 1932. But as the definition of 'official' psychoanalysis came into focus in the mid-1920s, with the rules governing training, qualification and the overall aim of psychoanalytic institutions, such larger than life natives of the 'scientific' world would find it less easy to include psychoanalysis in their public preoccupations - although many of them would, naturally, continue to end up on the couch. Did the rise of 'official' psychoanalysis put an end to such promiscuous pursuing of knowledge? Did its rise exclude the youthful and restless, fine and inquiring minds, so prominent in Tansley's network, so central to the ethos of Cambridge, from the circles of psychoanalysis?

The professionalization of psychoanalysis may well have had this effect; but in mitigation of the implication that the process of professionalization deprived psychoanalysis of exactly the openness that its technique seems to imply, one might, with some justice, point to developments wholly outside psychoanalysis which required scientists to specialize, display the necessary credentials, communicate more exclusively with immediate colleagues, buckle down to the demands of big and corporate science. (For a discussion of such changes due to the professionalization of British plant ecology, see for instance Lowe 1976.) In other words, it may not have been only psychoanalysis that lost its mercurial Ramseys and its polymathic Jeffreys. After all, Freud himself shared their vision of science as open to all who had the wit and graft - hence his resolute defence of the Farrows of this world. Resonating with this individualism, Cambridge of the 1920s balanced an unselfconscious elitism with democratic, universalist cravings - one can think of no more representative examples of these twin values than Penrose and Ramsey, nor could one imagine any finer advertisements for these values. Nor should the defender of professionalization and the necessary 'maturing' of psychoanalysis neglect to point to the local and unrepeatable circumstances of this episode - to the opening up of English society in general following World War I, with a chafing tolerance of new ways and paths, its new-found but possibly short-lived awareness of the pounding blood of fragile youth and the skull beneath the skin. Lenity towards eccentricity will always benefit the Freudians. The paradox of this episode is, thus, that professionalization may have been intended to create Freudians, but its effect may also have been to unmake them - or at least make them disappear from

view, obscuring a network of analytic practitioners which became 'secret' or 'private'. This may appear an improbable conclusion but the discovery of the Tansley network makes it less improbable than it used to be.

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ABSTRACT

The paper traces the psychoanalytic networks of the English botanist, A.G. Tansley, a patient of Freud's (1922-1924), whose detour from ecology to psychoanalysis staked out a path which became emblematic for his generation. Tansley acted as the hinge between two networks of men dedicated to the study of psychoanalysis: a Cambridge psychoanalytic discussion group consisting of Tansley, John Rickman, Lionel Penrose, Frank Ramsey, Harold Jeffreys and James Strachey; and a network of field scientists which included Harry Godwin, E. Pickworth Farrow and C.C. Fagg. Drawing on unpublished letters written by Freud and on unpublished manuscripts, the authors detail the varied life paths of these psychoanalytic allies, focusing primarily on the 1920s when psychoanalysis in England was open to committed scientific enthusiasts, before the development of training requirements narrowed down what counted as a psychoanalytic community.