

Could psychoanalysis be a science?

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Could psychoanalysis be a science? There are three ways of reading this question, which will structure our discussion:

1. Is psychoanalysis the kind of investigation or activity that could, logically speaking, be ‘scientific’? If we can defend a positive answer here, then it makes sense to ask:
2. Is psychoanalysis, in the form in which it has traditionally been practiced, and continues to be practiced, a science? If there are good reasons to doubt its credentials, then we might ask:
3. Is psychoanalysis able to become a science? This is a question about what is needed for the necessary transformation.

I shall argue that psychoanalysis can be a science (§1), but that the historical debate raised important challenges to its methodology, viz. confirmation bias (§2.1), suggestion (§2.2), and unsupported causal inference (§2.3). I argue that recent developments (§3.1-2) meet these challenges, and conclude with some reflections on the interdisciplinary nature of psychoanalysis (§3.3).

1. Could psychoanalysis (logically) be a science?

This is a question about what psychoanalysis is and what counts as science, a question about our concepts of ‘psychoanalysis’ and ‘science’. Psychoanalysis involves both the clinical encounter and the production of psychoanalytic theory. It is a mistake to restrict the meaning of ‘psychoanalysis’ to the interaction between analyst and analysand (§1.3), though this remains central to psychoanalysis. Psychoanalytic theory is a theory of the nature and functioning of the human mind, especially in relation to motives. Much of its evidential base rests in the clinical data – neurotic symptoms, dreams, present thoughts and emotions – but psychoanalysis has always gone beyond clinical data to appeal to data from other fields of enquiry (§3.3).

Psychoanalysts have been active in producing some of this data, e.g. in child development or psychiatry, and in integrating the results into new psychoanalytic theory. It is the generation of psychoanalytic knowledge that is of central interest here, and so our immediate question is ‘Are the form of psychoanalytic knowledge and the method of its generation of a kind that could (logically) qualify as scientific?’.

On the meaning of ‘science’, I shall proceed pragmatically. I assume we have a rough conception that enables us to identify paradigmatic examples, and I shall argue by comparing psychoanalysis with ‘established’ sciences, especially social psychology (§1.2) and the social sciences more broadly (§2.2).

1.1 But first, could psychoanalysis be a natural science? The most plausible defence of an affirmative answer rests on Freud’s ‘economic’ model of human psychology. The aim of his *Project for a Scientific Psychology* (1895) was ‘to discover what form the theory of psychical functioning will take if a quantitative line of approach, a kind of economics of nervous force, is introduced into it’ (296). Freud intended ‘to furnish a psychology that shall be a natural science’ seeking to understand human psychology in terms of a ‘conception of neuronal excitation as quantity in a state of flow’ (296), governed by biological principles of homeostasis. The model he applied, popular at the time, was that of the reflex; energy in requires energy out, to prevent energy from building up dangerously within the system, which Freud argued is experienced as

pain. And so neurones and the nervous system as a whole have a tendency to divest themselves of energy.

If the economic model were the core of psychoanalysis, there would be reason to consider it a natural science. But there would also be good reason to reject it. First, many of the claims of the economic model have been superseded by neuroscience. The nervous system does not operate on a reflex model, and does not tend to divest itself of energy. Neurones generate their own energy metabolically, rather than receiving it from outside stimulation, which therefore modulates, rather than creates, nervous system activity. Sensory surfaces are not conductors, but transducers, of external energy, converting it into electrochemical impulses of negligible energy but with varying frequency – and so the nervous system cannot be swamped by energy from outside, nor can energy be trapped in it. The energy within the system is not conducted – it is not a quantity in a state of flow – but is transmitted by propagation. Finally, the quantity of energy involved bears no correlation to the psychological state of the person; the nervous system uses information, not energy, to structure its activities (see Hobson 1988, esp. pp.284-6; Holt 1965). Second, and perhaps most central to our enquiry, given this last claim, the clinical methods of psychoanalytic enquiry are inappropriate for generating knowledge of neural functioning.

Fortunately, psychoanalysis survives the demise of the economic theory. Freud repeatedly drew upon the economic model in his later psychological theorizing, e.g. he modelled psychic conflict as involving forces, understood associative links in content as involving energy pathways, talked of psychological ideas and experiences as cathecting neural networks, analysed psychic phenomena such as condensation and displacement in terms of transpositions of energy. This all needs to be reformulated just in terms of psychological content and processes. Freud sometimes approaches clinical questions in economic terms, e.g. narcissism (1914a), mourning (1917), and masochism (1924), and to the extent that psychoanalytic theories of these phenomena rest implicitly or explicitly on a mistaken conception of human beings as closed systems of fixed amounts of undissipated libido, the theories must be rethought. The theory of instincts, ‘our mythology’ as Freud put it (1933, p.94), must be translated into psychological terms, abandoned, or radically amended in the light of recent biological and neuropsychological investigations. All this can be or has been done.

These remarks leave open the possibility that psychoanalysis abuts neuroscience as a discipline, and there may be fruitful exchange (it may even be that neuroscience formulates a workable version of an ‘energy’ concept). But psychoanalysis does not qualify as a natural science in its own right. This does not rule out the possibility that it may qualify as a social science, and it is to this question we now turn.

1.2 According to the ‘hermeneutic interpretation’ of psychoanalysis, psychoanalysis is, at its heart, a process of self-reflection, self-interpretation and self-formation. This process can be transformative by opening up new ways of understanding ourselves and the meanings of our behaviour, providing insights into previously unrecognized motives. The theoretical framework of psychoanalysis must therefore be an interpretation of this process in general terms (Habermas 1972). The roots of this view lie in a grand tradition, developed most famously by Dilthey (but also Brentano and Husserl), that holds that understanding the behaviour of human beings, in particular the meaning of such behaviour, is fundamentally distinct from scientific enquiry. On certain stronger interpretations of this view, no enquiries into this subject and so no social ‘sciences’ – which include branches of psychology on any reasonable interpretation (social, educational, and personality psychology at least) – are sciences. Weaker interpretations accept the social sciences as such, but distinguish them from the natural sciences. On this view,

psychoanalysis cannot be a natural science, but it could be a social science. But some hold the view that while there can be genuine social sciences, psychoanalysis is not among them, because of the individual nature of its subject matter as described.

There are a number of grounds on which we can contest this conclusion. First, some of the reasons put forward for thinking that neither the social sciences in general, nor psychoanalysis in particular, qualify as sciences rest on a narrow understanding of science, e.g. that science employs strict, universal laws or that all sciences are reducible to physics. While these are interesting philosophical debates, they fail to demarcate between recognised sciences and recognised non-sciences in the present. There are clearly branches of *natural* science, let alone social science, which no one knows how to reduce to physics and which do not employ strict, universal laws, but whose explanations of phenomena recognise the considerable role of historical contingency, e.g. palaeontology and evolutionary biology. (This is not to say that there are *no* covering laws in these disciplines, but that scientific research and explanation carries on well enough without them in many typical cases.)

Second, a considerable portion of the strength of the original argument for the hermeneutic interpretation rested on a distinction between motives and causes (e.g. Ricoeur 1970, pp.358-63, who approvingly cites Toulmin 1948, Flew 1949, and Peters 1950; Taylor 1964): If motives are not causes, then explanations in terms of motives, such as those given in psychoanalysis, are not causal explanations; as scientific explanations are causal, psychoanalysis is not a science. Not only is the explanatory form different, to think of motives as causes is to reify them, and so to postulate metaphysically dubious entities. And the relation of causes to effects is always underwritten by (deterministic) laws, but there are no laws of this kind in psychology.

Two points challenge the inference. First, it is generally agreed that the original debate of the 1950s and 60s was confused and worked with restricted conceptions of ‘cause’ and ‘causal explanation’ which have since been superseded. It is now clear that there is conceptual room for a range of causalist views that avoid the original objections (see O’Connor & Sandis 2010, Part II).¹ And the way the debate has developed does not support the original claims about explanations in terms of motives. In large part as a response to Davidson’s classic (1963) paper, the debate has focused on whether *reasons* are causes, or again, whether explanations of action in terms of reasons are causal explanations. But not all we wish to understand about an action is the reason for which the agent acted (there is also timing, manner, symbolic meaning, etc.), not all behaviour is action, and not all motives are rational – by which I mean not just that people act irrationally because they take factors to be reasons when they are not; not all motives involve the apprehension of reasons. We cannot reduce explaining the meaning of human behaviour to an account of the reasons for which people act. To slam the door in anger does not typically involve taking anything as a reason for slamming the door (including the satisfaction one may gain from doing so), but my anger motivates my slamming the door, and saying that I was angry makes my slamming the door intelligible. Or, as a different example of meaning without rationality (though an example of imagination, not action), consider the expression of desires in dreams. Sticking uncontroversially to ‘transparent’ dreams, such as dreaming of drinking when thirsty, the desire makes sense of and motivates the dream, but provides no reason for it (though it would

¹ For example, causalists need not maintain that motives are themselves *causes* of actions. They could instead hold that it is agents, not their beliefs, desires, or other psychological states, that bring about actions. They may claim that an agent acted as she did in virtue of certain motives, with this statement being understood as a causal explanation. Causal explanation, in turn, can be understood broadly as providing relevant information about the causal history of an occurrence, rather than citing the cause specifically, in a form amenable to universal law.

rationalize the waking action of getting a drink). Goldie (2000, p.127ff.) provides a more complex example that combines emotional expression with imagination: scratching the eyes out of a photo of one's rival in love. Now, unconscious motives – of the sort discussed by psychoanalysis, but also those identified by social psychology, discussed below – do not always work through the provision of reasons for acting as the agent does. Even if we grant the view that *reasons* are not causes (or that reason-citing explanations are not causal explanations), this does not help the hermeneutic interpretation, since it does not support the claim that explanations citing motives are not causal explanations, at least when the motives do not give the agent's reasons.²

Second, the original argument entails that no branch of psychology seeking to investigate motives – or more generally, the meaning of human behaviour – can qualify as a science. In response, we can argue that social psychology is rightly recognised as a science, given its methodology, the growth of agreed results, and its integration with other branches of psychology. We should not make its status as a science dependent upon the resolution of the philosophical debate over the precise relation between motives and causes. Scientific work with motives is, therefore, possible. That psychoanalysis offers explanations of behaviour in terms of motives cannot, therefore, establish that it cannot be a science.

It is an important (and well-established) finding of social psychology that our behaviour can be shaped by motives and meanings we are unaware of, and this can be investigated by recognised scientific methods. For example, subjects were asked to memorize word-pairs, and later given a word association task. Those who memorized 'ocean-moon', when asked to name a detergent, were significantly more likely to say 'Tide' (Nisbett & Wilson 1977, p.243), but did not cite the relevant word-pair as motivating their answer. Likewise, many subliminal perception experiments work through the semantic content of the priming stimulus. Of course, the stimulus provides no *reason* for the response, but as already noted, we cannot restrict the project of self-understanding to rational explanation. Nevertheless, the hermeneut may object that this is the wrong sort of 'meaning', the right sort having to do with 'making sense' of our behaviour.³ This form of investigation doesn't do that, even if it explains it in some other way. It is therefore irrelevant to our discussion of whether psychoanalysis, which does attempt to make sense of our behaviour, can qualify as scientific.

² For example, see Dancy (2000), pp. 167 and 173-4, where he indicates that his book-length argument against considering reasons to be causes does nothing to rule out non-rational psychological explanations being causal. This includes not only non-rational motives, but also cases in which the motive cited, e.g. an emotional disposition or character trait, explains why the agent acted on the reasons they did.

³ We can identify four types of 'meaning' that may be relevant to explaining human behaviour. First, our psychology cannot be understood independently of intentionality and intentional content. Second, this content is, to a substantial degree, structured by language and the semantic meanings it makes available. Third, our mental states, with their intentional content and phenomenal character, are subjectively meaningful in a variety of ways. For example, they are cognitively meaningful as the means through which we conceptualize, think of and understand the world, and they are conatively meaningful both as preferences and evaluations directed towards the world, and as states towards which we take up (further) evaluative attitudes. Finally, we are rational creatures, and the (cognitive, conative, evaluative and semantic) meanings we make are governed by and answerable to rational discourse. The objection being considered is that the explanation appeals only to semantic meaning (type 2), but it is subjective meaning (type 3) that is relevant to the hermeneutic view.

It is true that social psychology does not (often) concern itself with what the behaviour and motive *mean to the individual*, while this is precisely what psychoanalysis does concern itself with (§1.3). But the line of argument just given is mistaken. First, we make sense – give the meaning – of behaviour in ways that are broader than accounting for the meaning of the behaviour to the individual; second, the influences on our behaviour investigated by social psychology interact with the subjective meanings focused upon by the hermeneutic interpretation. The example just given provides a case in point. The semantic content of the stimulus is what makes sense of the response – the sense lies in the connection in content. The words mean something to the subject, and this meaning is part of the explanation offered on the basis of scientific enquiry; the same results cannot be expected with subjects who do not speak English or have not encountered a detergent named ‘Tide’. (That the stimulus operates non-consciously is, of course, no objection. The hermeneutic interpretation of psychoanalysis must accept that the project of self-understanding allows both non-rational and also non-conscious motivation.⁴)

Another example illustrates the close connection between social psychological explanations and the meaning of behaviour to the individual. Consider Maier’s (1931) classic experiment on problem solving. Two cords were hung from the ceiling, placed so that subjects could not, while holding one cord, reach the other. The room contained a variety of objects, e.g. poles, clamps, extension cords. Individual subjects were asked to tie the two cords together. Subjects easily discovered three solutions, after each of which they were asked to find a further solution. Many struggled at this point for several minutes, when Maier then casually starting one cord swinging. Within a minute, subjects tied a weight to one cord, swung it like a pendulum, and thus brought it close enough to the second cord to tie the two together. Maier asked the subjects how they had come across the idea of the pendulum. None of them reported his action, but said it just occurred to them; the influence, therefore, was subliminal. Does Maier’s action of swinging a cord provide an explanation of the subjects’ doing so (less than a minute later) which ‘makes sense’ of their behaviour or not? I would argue it does. If the subjects had reported that they had seen Maier swing the cord, and this suggested the solution to them, and they had then enacted this solution, the explanation would, of course, make sense in the right way. That we are more influenced by situational factors than we can normally consciously report is an established finding of social psychology (Nisbett & Ross 1980). Yet many of these factors, if noted and considered consciously, can operate as reasons, interacting with our standing motives in rational and consciously recognisable ways.

Furthermore, the results of social psychological experiments can question the influence of the reasons we believe we act upon, and so present a challenge to explanations that *would* make sense if they were true. For example, two groups of students were asked to predict how much shock they would be willing to experience in an experiment on the effects of intense electric shock. One group were reassured that the shocks would cause no permanent damage, the other group was not. When asked if the reassurance had made a difference to their predictions, the first group replied that it had increased the level of shock predicted, i.e. the reassurance acted as one factor (among others) they had considered in predicting how much shock they were willing to take. The second group were asked, counterfactually, if such reassurance would have made a difference, and they reported that it would. However, there was no overall difference between the levels of shock predicted in the two groups (Nisbett & Wilson 1977, p. 246). So the reassurance appears not to make the difference the students claim it did. Understanding their

⁴ Whether or not the subject is conscious of the factors cited by the explanation undoubtedly alters the precise nature of the story to be told, as well as how we respond to and evaluate the behaviour, e.g. in terms of whether we hold someone responsible for what they do. But this does not imply that explanations citing non-conscious factors do not ‘make sense’ of the behaviour.

predictions as a form of action, we can say that the students' actions were not influenced by a factor that they took themselves to be acting upon.

Given these considerations, we cannot rule out the investigations of social psychology as irrelevant to our attempts to make sense of ourselves, nor can we declare that the project of investigating the meaning of human behaviour cannot qualify as a science.

1.3 There is yet a sense of 'self-understanding', central to the psychoanalytic project, that contrasts with the type of investigation of motives described above. So even if the argument successfully defends the possibility of a science of motives, it does not establish whether psychoanalysis could qualify. This search for self-understanding is what cannot become a science, argues the hermeneut. This statement contains some truth: understanding individuals (oneself or others) is not a science, not least because every individual is unique in the contents and connections of their mental states. Researchers cannot make judgments evidenced in the usual experimental way regarding why an individual acts as they do on any given occasion, nor are laboratory control studies much use to someone trying to understand an individual in this way (such studies may suggest hypotheses, but cannot establish which is correct). But we would be wrong to think either that individual self-interpretation is all there is to psychoanalysis, or that nothing scientific can be done which can helpfully inform self-interpretation.

Psychoanalysis is not only about understanding individuals, with all the idiosyncratic contingencies of their mental content and history. It is also the construction of a general psychological theory, based on information about the *generic* processes and obstacles present in self-interpretation. Claims about transference, the many patterns of psychic defence, resistance, the understanding of dreams, unconscious emotions, the symbolic content of motives, and the influence of the past are part of this psychological theory. This theoretical apparatus, and the concepts it employs, can be used to generate explanations and predictions regarding human behaviour. Clinical practice requires such a more general theoretical backdrop, and so any psychoanalytic approach to understanding ourselves must take some stance on these theoretical claims. These general claims (especially those concerning the clinical process) are often *assumed* by the hermeneutic interpretation – but they have been disputed by other psychological theories. Now, first, whether this psychodynamic picture of the mind is true or not makes a difference to how we should approach and understand the task of self-interpretation. For example, Timothy Wilson (2002), while acknowledging the immense contribution of Freud to psychology, argues that the results of social psychology indicate that we should turn *outward*, to our manifest behaviour and situational influences, in order to understand ourselves, not inward, to our dispositions and their symbolic content. Second, general psychoanalytic claims can be supported or revised on the basis of extra-clinical results (e.g. Westen 1998, 1999; Solms & Turnbull 2002; Holt 2002; Masling & Bornstein 1983-1998) and the statistical analysis of clinical data (both across individuals and using 'within-subject' designs, e.g. Dahl 1972; Luborsky 1977; Gill 1982; Gill & Hoffman 1982; Luborsky 2001; Kächele et al 2008); and there is a place for scientifically respectable case studies, even ones that involve no quantitative methods (Kazdin 1981).

We should conclude that even if the enterprise of understanding an individual's motives, and the meaning of their behaviour to them, is not a science, psychoanalysis is not logically barred from being a science. It is committed to producing a general theory of psychic functioning, especially regarding motives, and this general theory informs the process of self-understanding emphasised by the hermeneutic interpretation. Having argued that psychoanalysis can be a science, we may now ask if it is one.

2. Could psychoanalysis (as it is) be a science?

If psychoanalysis could logically be a science, does it instantiate one? The issue here is whether the form of psychoanalytic knowledge and the method of its generation qualify as scientific. Psychoanalysis presents us with a general theory of motivation, character, interpersonal relations and (aspects of) mental functioning, comparable to other such theories in empirical psychology. The form of this knowledge is scientific, so I shall focus on the question of method. A method can fail to be scientific if it cannot deliver the objectivity and rigour of science, or if it appeals to an evidential base that systematically fails to justify the inferences that are drawn from it. I shall therefore discuss three specific objections of this sort.

For the purposes of this section, I shall assume that the traditional methodology of psychoanalysis, viz. inferring its theoretical claims from clinical data presented as case studies, is its only method. This assumption is *narrow and out-of-date* in ways that will become apparent in §3, but it is useful for understanding the debate as it has been conducted.

2.1 Popper argued that scientific claims must be falsifiable – very roughly, they can (in principle) be tested against experience in such a way that it is possible for undermining evidence to come to light – and science proceeds by attempting to refute hypotheses. Those claims not refuted, despite vigorous testing, are (provisionally) held to be true. Popper (1963, pp.34-8; 1983, Ch. 2; see also 1995, pp. 87-9) argued that psychoanalytic claims are immunized against such testing, and so psychoanalysis is not a science.⁵

Popper begins by arguing that psychoanalysis proceeds by seeking out verifications of its theories, rather than falsifications (1995, p.87; see also 1963, p.38 ftnt.). Clinical data are ‘interpreted in accordance with established psychoanalytic theory’, but genuine evidential support only comes from testing claims in ways that may demonstrate the claims are false. In effect, Popper diagnoses a type of confirmation bias, which involves the selective gathering, weighing or interpretation of evidence that supports one’s existing beliefs or favoured hypothesis while neglecting or discounting evidence that tells against one’s view.⁶ Judged by standard psychoanalytic case study reports and key theoretical texts, many psychoanalysts have undoubtedly manifested confirmation bias in their handling and reporting of data.

Popper then argues that the logic of psychoanalysis is such that it cannot correct its confirmation bias, for two reasons. First, analysts’ theories may influence the clinical data in such a way as to produce confirming evidence. Disconfirming evidence, therefore, doesn’t emerge. We shall return to this issue of suggestion below. Second, psychoanalysis cannot countenance refuting evidence: not only do psychoanalytic concepts such as ‘ambivalence’ make it ‘difficult, if not impossible’ to agree on when a psychoanalytic interpretation has been falsified, but the theory attempts to explain ‘practically everything’ about human behaviour (1963, p.34), allowing no

⁵ Popper’s view of science has fallen into disfavour. It is generally agreed that scientific theories (as opposed to specific hypotheses) are not refuted by single observations or sets of observations, but rather, as Lakatos (1978) argued, through a widening gap between the facts that need to be explained and the explanations offered by the theory, a gap that brings the research programme of the theory to a halt. Making adjustments to a theory and to the ‘auxiliary hypotheses’ that link the theory to specific empirical predictions, in order to preserve the truth of the theory, is part of normal scientific procedure. However, Popper’s objection to psychoanalysis, as I interpret it, can stand on its own.

⁶ See Nickerson (1998) for a review.

conceivable event to refute it: ‘we can say, prior to any observation, that every conceivable observation will be interpretable in the light of psychoanalytic theory’ (1995, p.87).

Popper provides very little argument or evidence for this second claim, and it is unpersuasive. Freud claimed that ‘the theory of psychoanalysis is an attempt to account for two striking and unexpected facts of observation which emerge whenever an attempt is made to trace the symptoms of a neurotic back to their sources in his past life: the facts of transference and of resistance’ (1914b, p.16). If there were no evidence of either resistance or transference, the most important indicators of dynamic unconscious processes at work in the clinical setting, psychoanalysis would probably be placed beyond rescue. It is not *inconceivable* that there should be no such evidence (though we may have to suppose much about human behaviour to be different). For example, as evidence against resistance, we could observe that people are equally disposed to consciously recall and recognise motives that caused them psychic pain as those that did not, that situations that arouse feelings of helplessness or anxiety do not tend to increase rates of forgetting or other symptoms (see §3.1). Or again, against transference, we might observe that in their relation to their analyst, subjects remain calm, objective, and friendly over a span of months or years. Of course, highlighting psychoanalysts’ *refusal* to countenance evidence against the theory shows only the presence of confirmation bias, not the impossibility of its correction. Such refusal is not universal: Grünbaum (1984, pp.104-26) argues that there is significant evidence showing not only that central psychoanalytic claims are falsifiable, but that Freud (at least) gave up a good number as false on good grounds.

Rejecting Popper’s claim does not show, however, that psychoanalysis, as traditionally practiced, can correct for confirmation bias. The charge is, therefore, important and remains unanswered, and I will return to it in §3.2.

2.2 The problem of suggestion has been a perennial challenge to the scientific rigour of psychoanalysis and the evidential soundness of its claims. The challenge is that some of the mental states of the analysand as evidenced in the clinical data are, in fact, brought about through the ‘suggestive’ influence of the analyst. The clinical data is therefore not good evidence of the true structure and contents of the analysand’s mind, and so not a secure basis for theorizing about the human mind. This was certainly the thought that Fliess presented to Freud (Freud 1954, p.334-7).

Hypnotic suggestion operates by explicit and forceful communication intended to alter a patient’s mind by bypassing their conscious awareness of the idea communicated. Suggestion in psychoanalysis, if present, is unintended, subtle, and unconscious. It is best understood as a variety of ‘experimenter expectancy effect’. Experimenter effects are influences that the experimenter has on the subjects of an experiment which are not the effects under investigation.⁷ It has been shown that a variety of factors, from the experimenter’s age to their need for social approval, can make a difference to how subjects respond on experiments (Rosenthal & Rosnow 2009, p.327). Most relevant to the issue of suggestion, however, are expectancy effects: what the experimenter expects to happen, his or her favoured hypothesis, subtly affects how subjects respond in favour of that expectation. The effects can be found in everyday life as well, e.g. in education:

⁷ The leading researcher on experimenter effects for the last 50 years has been Robert Rosenthal, and his early work from the 1960s has not been surpassed or overthrown (see Rosenthal & Rubín 1978, Rosenthal 1997, Rosenthal 2000), such that Oxford University Press reissued his three major works in 2009 (Rosenthal & Rosnow 2009).

All of the teachers of an elementary school were told that a newly devised computer program was able to predict the intellectual development potential of children in their classroom. At the very beginning of the school year, a handful of children's names were selected completely at random and given to their teachers, who were told that those children would bloom intellectually in the academic year just begun. At the end of the school year those children whose names had been placed arbitrarily on the list of bloomers did, in fact, show greater intellectual gains than did the children in the control group. (Rosenthal 2000, p.294)

The theoretically-based expectations of the analyst thus might alter the clinical data so as to create 'evidence' that supports those very expectations. The most prominent vehicles for suggestion are the analyst's interpretations and the reinforcing of certain types of patient communication (through vocalizations, displays of interest etc.), with the authority of the analyst facilitating the patient's susceptibility to comply with the analyst's expectations (Fisher & Greenberg 1977, p.363; Erwin 1996, p.96).

It is generally recognised that the theories and values of the researcher are more likely to affect data in the social sciences, in part due to the nature of the interaction between researcher and subjects involving the need to interpret the meaning behaviour has for the subject. But there are two reasons to think psychoanalysis faces a distinct challenge. First, experimenters produce greater expectancy effects when they appear professional, business-like but friendly, competent, and expressive in communication, and work in a 'personal' space (Rosenthal 1976, Ch. 15). On these criteria, psychoanalysts look set to produce the greatest expectancy effects of all, even setting aside traditional concerns about positive transference. Second, more generally, psychoanalysis does not avail itself of several established means for reducing biases, of which experimenter expectancy effects are one form. In a recent *Handbook of Applied Social Research Methods*, the researcher is encouraged to

- a. engage in intensive, long-term participant involvement, to enable more complete data and time to formulate alternative hypotheses;
- b. collect 'rich', i.e. detailed and varied, data;
- c. secure 'respondent validation', systematically soliciting feedback from participants on the conclusions drawn;
- d. search for discrepant evidence;
- e. 'triangulate' conclusions, e.g. by drawing on diverse methods of collecting data, by looking at different sources of data, by reviewing findings using different researchers, and by interpreting the data in the light of different theories;
- f. employ 'quasi-statistics', i.e. using simple numerical results that can be easily extracted from the qualitative data;⁸
- g. use comparisons with other groups (the role usually played by control groups in quantitative studies) or other studies (Maxwell 2009, pp.244-5).⁹

⁸ The term 'quasi-statistics' comes from Becker 1970. Qualitative data often has an implicit quantitative element, e.g. in claims that something is 'frequent' or 'typical'. Employing quasi-statistics seeks to make such quantitative elements more explicit, and enables researchers to assess, e.g. the number of discrepant cases, the percentage of sources they come from, and the amount of evidence relevant to a particular claim.

⁹ Similar points are made in other handbooks on method, e.g. Patton 2000, Ch. 9 – from which I have taken the helpful expansion on different types of triangulation. Patton also comments on the importance of training, which I discuss in §3.2.

Psychoanalysis clearly fares well in relation to (a)-(c) – analyses last years, the data is highly detailed and covers many topics, and the analysand is constantly supplying feedback on the hypotheses presented by the analyst. (d) relates to confirmation bias, already remarked upon as a challenge.¹⁰ The last three recommendations develop that challenge to the traditional psychoanalytic case study methodology. Some triangulation is achieved by the diversity of individuals and their concerns, but there is no variety of method in gathering the evidence, and rarely are a variety of analysts with different theoretical perspectives asked to review the conclusions inferred from the data. The data is not recorded or presented in ways that facilitate statistical analyses. And comparisons of the kind that could prove corrective, e.g. with interpretations of similar phenomena in other psychoanalytic or non-psychoanalytic schools, are rarely drawn (see §3.2). Here, then, is an argument for thinking that psychoanalysis is not, as traditionally practiced, a science, because its method does not reduce bias sufficiently.

However, another means by which bias is overcome lies in corroboration. It is often not possible to tell, from the results of a single researcher, the degree to which expectancy effect has influenced the result. Replication by different experimenters (presumably with different expectations) is necessary. Rosenthal notes, ‘If all of a sample of experimenters obtain similar data we will not err very often if we assume that... no effects whatever associated with the experimenter have occurred.’ (Rosenthal & Rosnow 2009, p.567) The methodology of a science necessarily involves the activity of a scientific community, and secure results are widely agreed upon, having been tested and confirmed by the results of many researchers.

We can appeal to corroboration, and on this basis reply to the charge of suggestion, for a limited, but central, range of claims in psychoanalytic theory.¹¹ Rubinstein (1976) famously argued that psychoanalysis is best seen as a ‘purely clinical’ science (see also Klein 1976), and Wallerstein (1990, 2005) develops the case. He defines the clinical theory as the theory of transference and resistance, conflict and compromise, noting Freud’s famous definition of psychoanalysis as ‘Any line of investigation which recognizes these two facts [of transference and resistance] and takes them as the starting point of its work’ (1914b, p.16). Wallerstein argues that the clinical theory is substantially held in common between the many psychoanalytic schools in mutual disagreement over metapsychology and etiological claims.¹² While Wallerstein’s views have caused controversy, this has focused almost entirely on his claims about the relation of metapsychology to clinical data. Few commentators have taken issue over the consilience regarding clinical theory, and those that have focus more on the theory of clinical technique than the psychodynamic model of the mind and its clinical manifestations (see Abrams et al 1989). Over the existence and nature of unconscious defences against psychic conflict, and the implication of both such conflict and

¹⁰ It is important to distinguish between suggestion and confirmation bias. Suggestion *changes* the clinical data in support of the theory; confirmation bias *misinterprets* the data in support of the theory.

¹¹ For a more extensive defence of this claim, see Lacey (forthcoming b).

¹² There is a common conception that the metapsychological and etiological disagreements are sustained by, and evidence of, the operation of suggestion, such that analysands tend ‘to bring up precisely the kind of phenomenological data which confirm the theories and interpretation of their analysts’ (Marmor 1962, p.289). The common conception is unsupported, however: confirmation bias could produce the same result. Hence it may *appear* that analysands produce data of the kind that supports the theoretical views of their analysts, but this appearance is in fact a product of the selection and presentation of the data by the analyst. Wallerstein (1990, p.11) argues similarly that the data of different schools are comparable, but they are explained through different general theoretical frameworks, and this is possible because the theories are not tightly linked to the data.

defence in the etiology of neurosis and a range of psychological traits, there is little disagreement (Rosenblatt 1989, pp.90-91).

This is not a final answer to the problem of suggestion. Expectancy effects are put to rest more soundly when we get corroboration by different schools, and insofar as psychoanalysis can be considered a single school of thought in psychology, we must seek corroboration by non-psychoanalytic theories. This makes a general point about the nature of science – *no* school of thought should stand in isolation from its rivals. But where corroborative results within a school are established by a defensible methodology, its explanations enter as equal competitors, not to be dismissed from the outset as ‘unscientific’. We have not yet secured this result for psychoanalysis, for the absence of the correctives in the social sciences – triangulation, quasi-statistics, and comparisons – and the threat of confirmation bias remain as reasons to think that, in its traditional form, psychoanalysis is not properly a science. We will turn to the issue of current and future developments in §3.

2.3 Our third methodological challenge comes from Adolf Grünbaum, who argues that the method of inferring the causes of clinical data from the clinical data is flawed (1984, Ch. 3; 2004).¹³ Causal inferences must be justified by use of Mill’s canons (Mill 1904): to establish that X is the cause of Y, we must show that Xs make a difference to the occurrence of Ys *by comparing (classes of) cases*, either comparing the incidence of Ys in cases in which X occurs with ones in which it does not, and/or by comparing cases in which Y occurs to see whether X occurred or not. An example: to establish whether smoking causes lung cancer, we must compare the incidence of lung cancer in smokers and non-smokers; and/or we must examine cases of lung cancer to see whether the person smoked or not. If there is a higher probability that a smoker will have lung cancer than a non-smoker, and/or a higher probability that someone with lung cancer is a smoker than non-smoker, we may infer that smoking causes lung cancer. The clinical data, however, cannot furnish this kind of evidence. Take the central claim that neurosis results from repression (or better, chronic psychic conflict together with the failure of psychic defence). Nothing in the clinical data alone could establish that the difference between neurosis and its absence is psychic conflict, because it does not record whether, in the general population, there are people who are not neurotic but nevertheless suffer from psychic conflict.

On the face of it, psychoanalysis proceeds by supposing that the interpretation of free associations can discover the psychological states that are the unconscious causes of the clinical data via connections in intentional content. In everyday life, Hopkins (1988) argues, we interpret voluntary behaviour by placing it in relation to a motive or set of motives, which we take to have caused the behaviour. The motive both explains and causes the behaviour in virtue of its intentional content, while the behaviour ‘inherits’ its content from its cause. Most of the time, we must rely on background information and a range of behaviour, e.g. to understand that someone is seeking revenge, we need to know more than that they deliberately seek to harm someone. We revise our interpretations, and so our causal inferences, as further evidence emerges, in line with the virtues of inference to the best explanation (accuracy, scope, coherence, simplicity, and plausibility). If this were what we, and psychoanalysts, are doing, Grünbaum argues, then we commit a fallacy of ‘thematic affinity’: connections in intentional content or meaning between

¹³ For this objection to apply, motives (and other mental states) must be causes of the behaviour they bring about, i.e. the hermeneutic interpretation is false. I shall assume, with Grünbaum, that this is so. If the hermeneutic interpretation is correct, however, the general issue of how we justify our interpretations of the meaning of behaviour still arises, and the discussion that follows may be understood in that light. For a more detailed discussion of Grünbaum’s objection, see Lacewing (forthcoming a).

mental states, or between these and behaviour, are no indication, on their own, of a causal connection. At the very least, we must draw upon background knowledge that rests on Mill's canons and establishes that certain types of mental state are the causes of certain types of behaviour.

Grünbaum's conclusion, it will emerge, is defensible for a range of causal inferences in psychoanalysis, in particular those pertaining to the existence of metapsychological structures and the childhood causes of adult mental illness. But the general claim regarding thematic affinity is false. There is a logical or conceptual connection between the intentional content of a desire and the range of behaviour it *typically* causes. What makes a mental state a desire, rather than some other type of state, is precisely the pattern of its causal and normative relations to behaviour and other mental states. What gives a desire the content it has is also determined (in part) by such relations. The desire to get a drink is a *desire* and a desire *to get a drink* in virtue of its typical causes and effects. Hence to understand a desire is already to understand it in thematic affinity with the behaviour it typically causes. This conceptual analysis entails that relations of intentional content are *prima facie* evidence of causal connections, and so Mill's canons are not necessary in the generation of background knowledge regarding the kinds of causal relations in which desires typically participate. Grünbaum is therefore wrong to think that appeal to thematic affinity is always fallacious.¹⁴

Nevertheless, experimental work, most famously by Nisbett and Wilson (1977) and Nisbett and Ross (1980), presents overwhelming evidence that people are poor at correctly identifying (all) the causes of their behaviour, and prone to inventing false rationalizations. (Some examples were given in §1.2.) Instead, our inferences are guided by 'judgments about the extent to which a particular stimulus is a plausible cause of a given response' (1977, p.231). Wilson (2004) revisits and revises the argument in terms of a distinction between conscious and unconscious processes. Many processes, occurring as part of the 'adaptive unconscious' mind, influence our preferences, decisions, and behaviour, and in ways we would not think 'plausible'. As a result, our inferences from behaviour to motives are accurate only when actual causal factors are plausible, and no plausible-but-irrelevant factors are present.¹⁵ However, Nisbett and Ross (1980, p.211) are clear that such everyday inferences do well enough most of the time, at least for voluntary behaviour with a conscious motive. Furthermore, there is good reason to believe that typical mistakes in causal inference identified by Nisbett et al are more common in situations that encourage casual

¹⁴ Two points. First, the conclusion can be made stronger: we *cannot* use Mill's canons to generate the background knowledge, because we cannot *conceptualise* or think coherently about the desire to get a drink without already thinking of the typical causes of getting a drink. For instance, it is incoherent to ask if the desire to get a drink might, in fact, typically cause people not to seek drink, but to seek sleep instead – for if, *per impossibile*, this were the case, we would no longer be talking about the desire for a drink. If X is, in part, constituted by its causal relations to Y, we cannot investigate whether X causes Y in the way Mill describes. Second, it should of course be acknowledged that behaviour that *typically* manifests one motive may, in particular cases, be caused by another. Inference to the best explanation is not infallible, and we must be sensitive to patterns in behaviour that would indicate this. But in particular cases, Mill's canons do even worse. Mill's canons establish relations between *types* of cause and their effects. For example, neither a statistical analysis of other occasions on which a singer has sung the national anthem, nor a statistical analysis of the various motives for which people generally tend to sing the national anthem, will alone provide an answer to why this singer sings it now (e.g. to impress her audience of patriots). Hopkins' method of interpretation, by contrast, is applicable to the particular case, even as it deploys general background knowledge.

¹⁵ A further condition I do not discuss here is 'availability' (see Nisbett and Wilson 1977, p.251).

or automatic responses, while thoughtfulness, motivation to understand, and social intelligence increase accuracy (Fletcher 1995, pp.73-9; Gawronski 2004).¹⁶ Hopkins' method is robust within limits.

But can the *everyday* method of inferring motives from behaviour on the basis of intentional content be extended to become the basis for new *psychoanalytic* theories of unconscious motive? The three conditions that improve accuracy of inference are well-satisfied by psychoanalysts working in the clinical setting. As a result, such inferences may be rather more reliable than everyday ones. Nevertheless, I suspect the answer is 'only so far', and this is why Grünbaum's challenge to psychoanalytic methodology is defensible. We should distinguish inferences from clinical data to

- a) the present motives of the analysand in behaving as he does,
- b) complex mental processes, such as defence mechanisms,
- c) psychological structures, such as the superego or Klein's two 'positions', and
- d) etiological accounts of the origins of any of the above.

Our everyday method of interpretation moves from current patterns of behaviour to current motives (broadly construed). The forms of inference in (a) and (b) can be defended as an extension of this form of reasoning. With this information also comes information about psychic conflict. It is a central claim of psychoanalysis that such conflict is at the heart of neurosis, and the method of interpretation is capable of linking clinical data and such conflict in a way that can justify such a claim. (It is also on these claims that we argued there is corroboration.) However, our everyday method of interpretation has little (reliable) to say about inferences of types (c) and (d), and we need a further argument to think that metapsychological and etiological theories could be established in this way, without recourse to any data generated through the use of Mill's canons. There are too many factors of potential causal relevance to the formation of long-term dispositions, and many relevant non-psychoanalytic sources of evidence regarding the structure of the mind. Hence its traditional clinical methodology, if employed in isolation, does not enable psychoanalysis to provide scientifically respectable answers in these areas.

2.4 To finish our discussion of whether psychoanalysis is a science, it is worth switching perspectives. Rather than seeking to meet objections, is there an independent argument for a positive answer? One such argument can be made by appealing to the clinical results of psychoanalysis. Grünbaum (controversially) purports to find evidence of the following argument, defending Freud's central clinical claim that neurosis results from repression, in Freud's writings:

(1) only the psychoanalytic method of interpretation and treatment can yield or mediate to the patient correct insight into the unconscious pathogenesis of his psychoneurosis, and (2) the analysand's correct insight into the etiology of his affliction and into the unconscious dynamics of his character is, in turn, *causally necessary* for the therapeutic conquest of his neurosis. (1984, pp.139-40)

¹⁶ This evidence undermines the widespread view, first put forward by Ross (1977), that we suffer from a misleading theory about human behaviour called the 'fundamental attribution error': we assume that behaviour is primarily the result of our 'enduring and consistent dispositions' (Nisbett and Ross 1980, p.31) rather than a response to the particular characteristics of the situation in which we act. We don't hold a faulty *theory* of human behaviour, but we are, in *practice*, often too quick to attribute behaviour to the agent's dispositions.

If these two claims are true, and if psychoanalysis is therapeutically successful, then this success validates the etiological hypotheses of psychoanalytic theory. The interpretations of the analyst must ‘tally’ with the real causes of the analysand’s neurosis, or cure will not follow (hence Grünbaum calls this the ‘Tally Argument’). However, the empirical support for (2) has been unforthcoming – ‘outcome studies’ show that non-psychoanalytic therapies can be successful and there is even spontaneous remission. The argument fails.

In fact, recent evidence suggests caution about this conclusion. Certainly, there is no evidence to support that psychoanalytic therapy is *necessary* for ‘cure’, but medical science does not work with necessary causes, only claims of relative causal efficacy. First, compared with spontaneous remission (no treatment), recent studies (e.g. Leichsenring 2005; Fonagy, et al 2005; Westen & Bradley 2005; Milrod, et al 2007) show that psychotherapy (in general, not psychoanalysis specifically) has a considerable positive effect, with 80% of those treated ending up better than no-treatment controls. This effect size is as large as many medications for physical complaints, and greater than almost all treatments in cardiology, geriatric medicine, asthma, flu vaccination, and cataract surgery. Second, when it comes to comparing rival psychological therapies, some evidence is just now beginning to emerge that psychoanalysis is superior (see Shedler 2010 for a review). Why now?

First, most outcome studies compare rival psychotherapies with *short-term* psychoanalytic psychotherapy (up to six months), while some recent studies (Leichsenring & Rabung 2008; Leichsenring 2011; de Maat, de Jonghe, Schoevers & Dekker 2009; Knekt, Lindfors, Härkänen et al 2008; Knekt, Lindfors, Laaksonen et al 2011) indicate that the benefits of psychodynamic psychotherapy become much more pronounced after six months’ treatment, and that long-term psychodynamic psychotherapy *is* more successful in the long run, at least for chronic distress, mood, anxiety, and personality disorders. Second, outcome studies need something clear to measure, and it is less expensive to measure it over a short period of time, and so very few studies have measured what it is that psychoanalysis *now* would claim to be uniquely best at achieving, e.g. improving the patient’s ability to tolerate a wider range of emotional experiences and be more emotionally ‘alive’, to have a more satisfying sex life, to understand themselves and others in more nuanced ways, to live with greater freedom and flexibility. The empirical work is yet to be done on establishing whether there is an appropriate interpretation of ‘therapeutic success’ according to which psychoanalysis uniquely delivers such success. However, such evidence would not demonstrate that it is veridical insight into the *origins* of the analysand’s psychic structures of defence that is responsible for the therapeutic success, so etiological claims would still not be evidentially supported.

2.5 Our discussion has given us reason to believe that, as it is traditionally practiced, psychoanalysis is not a science. It faces challenges of confirmation bias, the absence of three standard correctives in the social sciences (triangulation, quasi-statistics, and comparisons), the absence of consilience and the difficulty of defending its method of drawing inferences. The clinical theory does best, rebutting the last two objections at least, but this result could be greatly strengthened by finding solutions to the other issues. Given this assessment, it makes sense to ask:

III. Could psychoanalysis be(come) a science?

There is a time lag in §II, and a narrowed focus on psychoanalysis as ‘traditionally practiced’. The discussion ignores important developments of the last thirty years (with roots that extend throughout the history of psychoanalysis). A letter by Mary Target to the *New Scientist*, 27

October 2010, responding to an ill-informed article on the scientific status of psychoanalysis by Mario Bunge, states ‘The 54 signatories to this letter include distinguished researchers in psychoanalysis in the science faculties of leading world universities, who have acquired major public grants and have published papers in high-impact, peer-reviewed scientific journals.’ On this evidence, psychoanalysis is becoming accepted as a science. Two recent developments in psychoanalytic research are primarily responsible for this: an expansion of the methodology used to analyse the clinical data, and an interdisciplinary approach that utilizes findings from across psychology, including neuroscience. A third development, I shall argue, is additionally necessary, relating to the training and education of psychoanalysts. I consider the methodological issues first, and then comment briefly on training, before finishing with a comment on the interdisciplinary ‘turn’.

3.1 The traditional clinical methodology of psychoanalytic theory involves defending claims on the basis of case studies. It does not utilize a variety of (statistical and non-statistical) forms of analysis of larger data sets. But the clinical data per se does not prevent deploying other methodologies,¹⁷ and these may provide robust solutions to the objections considered in §II. Robert Holt argues that

[a] good deal can be done to test nonetiological hypotheses with the undeniably rich data of psychoanalyses, but only if they are fully recorded..., the data carefully if minimally censored to prevent recognition, and made available to all qualified researchers. Then at last we will have public and replicable data; then the researcher can be someone other than the therapist, and therefore truly disinterested and uncontaminated. (1985, p.296)

Early examples of this kind of work include Dahl (1972), Luborsky (1977), and Gill (1982; and Gill & Hoffman 1982). Since then many such papers have appeared, testing hypotheses across clinical cases using statistical and quasi-statistical methods. Expanding clinical methodology in a different way, Kächele et al (2008) have developed the single case study and significantly improved the rigour and objectivity of inferences drawn from it. They present the case study in 80 pages, followed by a 130-page analysis, with the evidence base of 517 audiotaped sessions made available to other researchers. A number of databases of clinical material now exist, open to scrutiny and collectively running to tens of thousands of hours.

The introduction of these empirical methods of data analysis meets the three outstanding conditions on reducing bias left over from §2.2. Researchers can ‘triangulate’ their conclusions, using a variety of methods, including ‘quasi-statistical’ analyses and the comparison of findings across groups, either control groups or those used in other studies. An example: Luborsky (2001) seeks to identify the preconditions for recurrent symptoms, such as momentary forgetting, in psychotherapy. He examines the recurrence of seven symptoms manifested across seven audio-recorded analyses. The occurrence of symptoms are sampled in the context of the preceding 30-50 words and the preceding 300-400 words. These contexts are examined for psychological conditions they have in common. 12 differentiating qualities were identified across the cases, and these were then rated for significance and intercorrelation, and five identified as most significant (viz. hopelessness, lack of control, anxiety, feeling blocked, and helplessness). A control group of contexts in which the symptoms were not manifest, chosen by arbitrary principles, was

¹⁷ There is an ongoing debate over whether recording clinical sessions may influence the data; and whether more data is available to the analyst ‘live’ than any recording can capture. It may be true that a fine level of detail and accuracy regarding the individual patient is lost, but there is good reason to think, from the work that has already been done, that enough remains – and remains the same – for general hypotheses to be tested.

compared. Independent judges and a variety of scoring systems were deployed in generating the results. The five psychological conditions for symptom onset were then compared with a range of classical psychoanalytic theories, with 'the most impressive conclusion' being the high match between the five conditions and those predicted by the theories (2001, p.1148).

Similarly, the earlier appeal to corroboration, as a response to the challenge of suggestion, is strengthened if the corroboration remains once we deploy the new methodologies.

With enough recorded treatments from a sufficient variety of analysts of all schools, it should be possible to find out just how far their patients' dreams, fantasies, childhood memories, etc. do systematically differ, and to what extent hypotheses of Freud's clinical theory hold, regardless of the nature of the treatment. If positive findings occur disproportionately more often in classical psychoanalyses... that would imply that they are a consequence of suggestion. (Holt 1985, p.295)

The absence of such positive findings is *prima facie* evidence of the absence of suggestion. Furthermore, from their interpretations of unconscious wishes and phantasies, psychoanalysts can make predictions regarding analysands' behaviour (not specific acts, but classes of behaviour, e.g. relation to authority figures) and postdictions regarding childhood events. Holt (1985, p.301) notes that 'Because so many thousands of us [psychoanalysts] have successfully made predictions and postdictions about particular cases informally... the impression naturally arises that psychoanalytic theory has been thoroughly validated in clinical use.' But he rightly argues that a stronger case rests on the new methodologies: 'Predictions and postdictions must be regularly recorded, and then all relevant evidence recorded too, finally being judged blind, with control data, for relevance to the prediction (or postdiction).'

These developments do not take us beyond or outside psychoanalysis. The clinical data, and so the interaction between analyst and analysand, remain central, as the new methodologies are applied to clinical data. Only if psychoanalysis is mistakenly separated from the development of the psychoanalytic theory of the mind, e.g. by being identified exclusively with the attempt at facilitating individual self-interpretation, can the objection be made that these methods are no part of 'psychoanalysis'.

These methods do not solve the challenges facing the metapsychological and etiological theories noted in §§2.3 and 2.4.¹⁸ Here, psychoanalysis needs to turn towards interdisciplinary collaboration, on which I comment in §3.3.¹⁹ But if we can meet the challenge of confirmation bias, these methods are sufficient to secure the clinical theory of psychoanalysis as scientific.

¹⁸ The disagreements between psychoanalytic schools regarding etiology and metapsychology are evidence either that everyone outside one's favoured viewpoint is incompetent in drawing conclusions from clinical data or that clinical data doesn't determine the correct metapsychological theory. I prefer the second, more humble conclusion. It is noteworthy that as psychoanalysis has developed, so greater emphasis has come to be placed on working in the transference in the present, and less emphasis has been given to a reconstruction of the origin of neurosis. The usual reason offered for this shift is that the clinical data cannot reliably support historical reconstructions, but can give insight into present conflict.

¹⁹ Of particular relevance to the etiological and developmental theories of psychoanalysis are the results of attachment theory (Ainsworth et al 1978; Main & Goldwyn 1985; Cassidy & Shaver 1999; on the connections with psychoanalysis, see e.g. Eagle 1997; Holmes 2000; Fonagy 2001; Fonagy & Target 2003, Ch. 10).

3.2 As noted in §2.2, one corrective to confirmation bias is to seek disconfirming evidence. Lord et al (1984) and Hirt & Markman (1995) show that subjects who consider an *alternative explanation* of the evidence to the one they favour correct their confirmation bias. This simple strategy is more effective than attempting to be 'fair and unbiased'. It forces subjects to seek out evidence that distinguishes between rival explanations, which in turn leads to a better, more objective assessment of the evidence available. Thus, to avoid confirmation bias, the psychoanalyst needs to demonstrate that their preferred explanation is not merely 'an' explanation, but better than alternatives. I.e. psychoanalysis must explicitly adopt inference to the best explanation as its method, which is not merely a corrective to confirmation bias, but a key component of scientific method generally (Lipton 2004).

There are at least two 'environments' in which psychoanalytic explanations compete. The first, 'near' environment is the range of alternative psychoanalytic theories. What is necessary in developing theoretical psychoanalytic claims is an account that demonstrates one set of inferences is superior to those made by rival schools. The second, more 'distant' environment comprises the range of non-psychoanalytic theories concerned with or impacting upon the data from which psychoanalytic inferences are drawn (e.g. alternative theories of dreaming and neurosis). To assess an inference as the best explanation, we need knowledge of the alternatives, and many of these may be generated outside the consulting room.

It may be that many of the phenomena that psychoanalysis seeks to explain simply do not have satisfactory alternative explanations, and that unifying power of psychoanalytic explanation continues to provide reason to accept it. However, we can only know this to be true, if it is true, *if we know what these alternatives are*, and what the evidence is that supports them. Both factors change constantly – what was the best explanation may cease to be so, either because new evidence contra-indicates it or because a new, more powerful explanation is generated. The 'environment of competition' can change, and has, of course, changed dramatically since Freud's initial proposals regarding the origin of neurosis 120 years ago.

Historically, psychoanalysts have not been good at practising inference to the best explanation as described, failing to compare their claims to alternative theories. In this vein, a former editor of the *International Journal of Psychoanalysis* remarks that 'by and large our standards of observation, of clarifying the distinction between observation and conceptualisation, and our standards of discussing and debating our observations are extraordinarily low; and they have received far too little attention' (Tuckett 1994, p.865). In principle, at least, this issue can be resolved in the education and training of psychoanalysts. Such training would need to encourage psychoanalysts to test and compare explanations and theories, which in turn requires both that an atmosphere of intellectual enquiry and rigorous questioning is engendered and that trainees are provided with an education that covers a range of theories. Otto Kernberg (2000, 2006, 2007, 2010) complains that both are missing, and advocates a series of significant reforms to psychoanalytic training and education. He argues that many psychoanalytic training institutes fail to teach research skills and actively oppose original thought, and they ignore both the contributions of other schools of psychoanalysis and relevant information in non-psychoanalytic disciplines. The future development of psychoanalysis depends upon the integration of new knowledge from bordering disciplines.

A further reason to accept this last suggestion is that, to have confidence that a particular inference from clinical data is correct, we need to ensure that it does not directly conflict with well-established data elsewhere. It may be that a hypothesis has implications, e.g. relating to childhood development or mental processes, which are undermined by non-clinical data. On the other hand, non-clinical investigations may provide corroboration for an explanation first arrived

at from clinical data, either in its existing form or in a modified form. This interaction between clinical and non-clinical data refines the explanation that is finally justified as 'best'. (This relation of psychoanalytic theory to non-clinical data should not, of course, be seen as a one-way street. Other psychological explanations and theories are just as beholden to well-established psychoanalytic clinical claims.) Working explicitly with inference to the best explanation as described can meet the objection from confirmation bias well enough to confirm the credentials of psychoanalysis as a science.²⁰

3.3 It is well-known that Freud drew upon non-clinical sources in constructing psychoanalytic theory, most famously the work in neurology on which the economic model was based. But the full extent of his borrowings, laid out in Kitcher (1992), is rarely appreciated. From neurophysiology, Freud took ideas of neurons, psychic energy and the reflex model of the mind; from psychology, associationism and the division of the mind into functional units; from psychiatry, the theory that neurotic behaviour could be product of unconscious ideas, theories regarding the sexual origin of neurosis, and from his (and others') studies in aphasia, the claim that the idea of a thing and the idea of a word for a thing are separate (a claim that forms part of his theory of repression); from sexology, he took notions of infantile sexuality, stages of sexual development, and component instincts; and further borrowings are made from philology and sociology. From anthropology, he adopted an approach to understanding the mind by tracing lines of development from primitive ancestors, which can be detected not only in ancient cultures but in children and 'primitive' people in the present. The same historical approach was taken from evolutionary biology and the theory of recapitulationism, which claims that ontogeny repeats phylogeny, i.e. the development of the individual organism passes through the stages of its evolutionary ancestors. Freud applies this form of explanation most famously to his theory that mental illness originates in earlier stages of psychosexual development.

These remarks show that engaging in interdisciplinary collaboration does not lead psychoanalysis away from its roots, but returns it to them. More importantly, however, they show that in accepting any of Freud's claims that were influenced by or rested upon his borrowings from non-clinical sources, psychoanalysts are indirectly resting their views upon evidence from other disciplines. And this opens psychoanalysis to a new challenge to its scientific status, resulting from the dangers of interdisciplinary work. A mark of scientific enquiry, as Popper noted, is to amend one's theory in the light of new evidence. Where the evidence comes from a discipline other than one's own, it is easy to fail to keep up with advances and to overestimate the strength of the evidence, as one is not party to the continual debate. For example, the pleasure principle, as Freud formulated it throughout his life, depends upon the reflex model of the nervous system: pleasure consists in the nervous system divesting itself of energy. In 1895, the model was defensible in neurology, but by 1940, it was not. Even by 1906, Sherrington had argued that there were severe difficulties facing the model. Yet no trace of this uncertainty appears in its adoption by psychoanalysis. Later psychoanalysts, e.g. Fenichel (1945), took their neurophysiology straight from Freud, and hence from 1895, and not from contemporary developments.

Of course, the significance of this particular example can be debated, given the development of psychoanalysis away from the economic model (though I doubt that contemporary

²⁰ Confirmation bias still occurs in scientific enquiry. For example, if a scientist hypothesizes that two events are causally related, this increases the chance that they will find supportive evidence and decreases the chance that they will find disconfirming evidence (Nisbett and Ross 1980: 97). Nickerson (1998, p.194) suggests that it is the operation of science as an institution or community that neutralizes confirmation bias as far as it does.

psychoanalytic models of the pleasure principle have truly freed it from the influence of its original form). But given Freud's extensive borrowing from multiple disciplines, other examples may take its place, e.g. recapitulationism continues to affect psychoanalytic thinking about mental illness as the re-emergence of earlier forms of mental functioning (Westen 1990). The central point cannot be argued away: As gaps open up between the contemporary developments in a discipline and its historically frozen representation in psychoanalysis, so psychoanalysis is made to 'appear to rest on more and more outmoded, simplistic, and speculative ideas' (Kitcher 1992, p.163). If the disciplines on which Freud drew have moved on, rejecting ideas that psychoanalysis has embedded, those ideas need to be revisited within psychoanalytic theory as well. Evidence is being generated in other areas of psychology that is directly relevant to the truth of psychoanalytic claims.

Kitcher is critical of psychoanalysis for its failure to engage appropriately with the disciplines from evidence was borrowed. But in discussing Freud alone, she can, in turn, be criticized for failing to keep up with developments in psychoanalysis which seek to make good this error. It is noteworthy that she is equally critical of contemporary cognitive science, which she diagnoses as making the same mistakes in interdisciplinary theory construction as Freud. Her argument is not to object to interdisciplinary work, nor to argue that it is unscientific, but to advise caution. The interdisciplinary approach taken by Freud and advocated here strengthens the claim of psychoanalysis to be scientific, but avoiding the pitfalls that go with that approach will require careful attention.

Psychoanalysis cannot avoid this challenge as it cannot avoid interdisciplinary work if it is both to be a science and involved in defending a psychological theory that seeks to give an account of the structure and development of (aspects of) the mind. I have argued not only that these are appropriate ambitions of psychoanalysis (§I), but that contemporary developments in the methodology of psychoanalysis are sufficient to secure the scientific status of claims regarding the motivational structure and complex mental processes involved in defence. In principle, these developments meet the challenges of suggestion, bias, and causal inference levelled against psychoanalysis in the historical debate. Even here, an awareness of relevant evidence in other fields is important. But a wider evidential base is crucial for metapsychological, etiological, and developmental claims, not only because the method of inference from clinical data is insufficient to establish such claims, but also because a historically dated evidential base is already implicated in psychoanalytic theory and requires constant revisiting and updating. I conclude that psychoanalysis – the form and method of generating a psychoanalytic model of the mind – can become, and is becoming, a science.²¹

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