RESOURCES

The Nettle

Ron Britton

Paper presented at the Annual Research Lecture of The British Psychoanalytical Society on 1st March, 2000

Like Peter Fonagy, I believe in the necessity for scientific research, that psychoanalysis has a place in the Natural Sciences, and that its findings and its practice should generate research and be scrutinised in a spirit of scientific inquiry. I have been involved in scientific research in the past, I am not unfamiliar with numbers and I have always enjoyed devising projects. The reason why I need to make this disclaimer at the outset is because I do not want to be dismissed as research-phobic. If I seem Cassandra-like, it is because I see my role in this duologue as the nettle leaving the grasping to Peter.

People trust the familiar and mistrust the unfamiliar. Peter Ustinov tells the story of a cousin in the large literary and artistic Ustinov family, who confronted his parents with the news that he wanted to be an engineer. Shocked and discomfited by this, they said, 'Why engineering? We don't know any engineers… why couldn't you take up something safe - like poetry?'. We might say to some fact-seeking progeny embarking on a career in neuroscience, why don't you do something down to earth, like psychoanalysis?

Unfamiliarity leads to suspicion and to hostility. Most of us, at least at the beginning of analysis, are unfamiliar with the unconscious content of our own minds. Analysis tries to help us to make a friend of our unconscious, no easy task, for some never possible. We should not be surprised therefore that a profession that espouses the existence and importance of the unconscious is likely to be treated with suspicion and hostility. This has been said so often and for so long, sometimes as an explanation, and sometimes as a rationalisation, that we are tired of hearing ourselves say it and even more of listening to our colleagues say it. So why repeat it?

Because we might not any longer believe it. If we do not remember that it remains true, we will expect enlightenment to deal with the prejudiced and objective evidence to persuade the sceptical. This is no reason for not embarking on outcome or any other kind of research but it has to be kept in mind when thinking about the likely reception of our published findings.

The philosopher who originally described the theory of pragmatism, C. S. Peirce, took an idealised and distinctly non-pragmatic view of scientists. He wrote, 'The scientific man is not in the least wedded to his conclusions. He stands ready to abandon one or all as soon as experience opposes them' (Ayer & O'Grady, 1992, p338). Max Planck, who was a real scientist and the originator of Quantum mechanics, took a different view. He said, "A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it". (Ayer & O'Grady, 1992, p343).

I think, by and large, psychoanalysts are apprehensive about research, or perhaps researchers. Our psychoanalytic experience - both of ourselves and others - gives us good grounds for distrusting human beings who claim to be objective. It is also true that we have been badly bruised in the past by some very blunt instruments wielded by such adversaries as Hans Eysenck. There is always someone wanting to disprove the reality of psychoanalysis. We are not alone in this: Darwin and the theory of evolution is under serious attack in the USA at present. The validity of Natural Sciences has been questioned by some post-modern social deconstructionists, on the grounds that science is just one belief system of many and that scientific truth is a social construction invented by scientists to support their hegemony over knowledge. Discrediting individuals historically linked to the ideas that they have generated is a common form of attack. Psychoanalysis is particularly vulnerable to this approach; the idea is commonly held that, if the historical person Freud can be discredited, the work of thousands of analysts influenced by his ideas is nullified. But the answer to all this is not less research but more. The answer to Freud-bashing is more Freud scholarship not less.

Putting aside personally motivated bias, there is also a problem when approaching research in the fields of psychiatry and psychology of ensuring that there is a shared starting point. There is the question of the basic assumptions about the nature of the mind and the effects of mental activity. Before particular psychoanalytic ideas about unconscious mental life can even be considered, or the efficacy of psychoanalytic psychotherapy evaluated, the question does the mind cause anything has to be addressed. Do thoughts have effects or are they only the accompaniment of physical changes? The theory that mental states are nothing but the accompaniments of physical events is called epiphenomenalism. It underlies a good deal of psychiatric and psychological thinking.

T. H. Huxley (1874), one of the proponents of epiphenomenalism, considered consciousness to be 'an epiphenomenon of molecular changes in the brain and hence all mental events to be the effects of physical events but never the causes of either physical or other mental events' (Flew, 1979, p101). No one who believes that all human behaviour has either an immediate physical or simply a direct social cause is going to be persuaded to accept any psychoanalytic explanation, however many favourable outcome studies there might be. Unless a researcher or reader of research results considers that what an individual believes influences his feelings and actions, he/she is not operating in the same cognitive universe as a psychoanalyst.

At the heart of psychoanalysis is the proposition that human beings are profoundly affected by what they believe. We share this opinion with most philosophers; certainly in the Anglo-Saxon schools of academic philosophy. They perhaps are our natural allies. Like them, we place value both on the objective methods of the natural sciences and the significance of subjective mental life. Our own particular psychoanalytic addition to the notion that beliefs have effects is to demonstrate that this is true, not only of conscious but also of unconscious beliefs. Ironically, it is we who can therefore offer an explanation for that part of therapeutic trials usually statistically discounted, namely the placebo effect.

Empiricism, defined as the philosophy that claims only to be guided by observation and results, is usually regarded in England as self-evidently virtuous. We should be warned, however, by the second dictionary definition of empiricism; namely, 'the practice of medicine or surgery only by efficacy without scientific knowledge, otherwise known as quackery'. The notion runs deep in British thinking that there is a special scientific virtue in the wholesale collection of facts without theories and only then deriving ideas by induction from them. This is usually described as the Baconian method. Darwin was so intimidated by that intellectual model that he claimed to have arrived at his theory of Natural Selection by this method. He wrote in his autobiography, 'I worked on true Baconian principles and without any theory collected facts on a wholesale scale' (Gould, 2000). In truth, he had his ideas for twenty years before publishing them and spent the interval trying to accumulate the facts he thought he needed to support them and to refute the expected opposition. In contradiction to his autobiographical claim to be a true Baconian, he wrote in a letter of 1863, 'How odd it is that anyone should not see that all observation must be for or against some view if it is to be of any service' (Gould, 2000, p254). According to Stephen Jay Gould, Bacon has been misrepresented, his project was not to found an ideal scientific method but to urge us to free ourselves from tenaciously held pre-existing beliefs, which he called idols, and look at the facts.

Empiricism in English thinking is usually linked to utilitarianism, the belief that only what is useful is relevant. John Stuart Mill, probably the most famous proponent of utilitarianism in the nineteenth century, was also its most subtle critic. His comments on Jeremy Bentham (the founding father of utilitarianism) could be applied to many current believers in epiphenomenalism. '[Bentham] was a boy to the last', wrote Mill. "Self-consciousness ...never was awakened in him. How much of human nature slumbered in him he knew not, neither can we know. He had never been made alive to the unseen influences which were acting on himself, nor consequently on his fellow-creatures...He measured them but by one standard: their knowledge of facts, and their capability to take correct views of utility, and merge all other objects in it". (Mill, 1838, p62-3).

In the psychological area, such utilitarianism and quasi-Baconian empirical research tends to go with what is called common sense. Using this unexamined source of native wisdom, collections of facts are explained, usually in the most banal way.

There is a particularly pernicious variety of this that takes the form of employing sophisticated and rigorous statistical methods to operate on doubtful data in an impoverished theoretical context. Thus statistical rigour gives credence to poorly conceived interpretation of dubious data. This has been the norm for psychiatric publication in this country for some time. Why concern ourselves with it? Because it creates a model of what can be called scientific, and resemblance to the model is then taken to confer the status scientific on it. We can be seduced into imitating it in our hunger for acknowledged scientific credibility or, if in despair of being understood we turn our back on any structured research, we can be described as uninterested in verification.

There is a lot to be said for testing the theoretical model but it needs to be in the environment to which it belongs. At the beginning of Ben Jonson's play, 'The Alchemist', we see him in his library perfecting his swimming strokes: 'My master is the greatest swimmer in Europe', says his valet, 'How does it go in the water?', asks the visitor. 'Ah, we haven't tried that yet', said the valet (Jonson, 1995).

All that I have said is not intended to cast doubt on the validity of a statistical approach but there is always, I think, a risk of distortion when employing highly regarded models of verification derived from other fields of inquiry. The risk is that the relevance of the psychological data is defined not by its real significance but by its suitability for the intended model. It is as if those factors that can be measured in a certain way are to be regarded as important and those that do not lend themselves to that kind of measurement disregarded. My assertion is that the research model devised needs to be sensitive to the transactions it purports to examine and to be sufficiently inclusive to accommodate all the possibly relevant variables operating in the field that is being studied.

As one approaches psychoanalysis in practice and any psychoanalytically based therapy with this intention, one sees what a daunting task it is. I think the hard work we face is not simply devising models that are sufficiently rigorous to claim validity and reliability but also models that are relevant and actually accommodate themselves to the subject being scrutinised. One strategy might be to narrow the field of inquiry by limiting the research to simple questions. We should not ignore minor issues nor be scornful of small studies. Instead, perhaps, of undertaking a thirty-year prospective study of the outcome of psychoanalytic treatment at the London Clinic, never to be completed, one might try to answer a few simpler questions. For instance, to find out what factors influence the premature termination of treatment, by collecting and collating existing data. If it limited itself to studying easily identified data, it might produce a modest yield in the way of information. Information such as age, sex, source of referral, marital status, employment record, flexible/rigid response to appointments, length of time on the waiting list, supervisor's assessment comments, the amount of experience of the treating analyst and so on. The modest findings might have a spin off in further useful research and provide a control base for specific inquiries. For example, does a history of anorexia nervosa increase the possibility of premature termination? Or does a period of therapeutic consultation prior to analysis reduce the dropout rate?

Essentially I think of research, in a professional context where it is valued, as opportunistic and individualistic. It is at is best when it seeks to discover something, it is okay when it attempts to verify something and at its worst when it aims to justify something.

If the efficacy of psychoanalytical psychotherapy is the subject of the enquiry; the simpler the question and the more direct the comparison of like with like, the more probable there will be a relevant finding. To give a simple example, one might compare the effectiveness of short-term therapeutic consultations conducted by experienced analysts, with once-a-week psychotherapy for a year by trainees under supervision. This might yield a result. Some might be tempted to complicate this with a control group, let us say, for example, an equal number of those who spent a year on the waiting list. Now it is no longer so clean or simple. It brings in so many different unexamined factors and what can be taken to be unifactorial is almost certainly multifactorial.

What about research into psychoanalysis itself, its theories and methods? I think - for a number of reasons - there is not at present in existence a research model that could be used to prove or disprove the most relevant of our ideas. Any research model derived from other sciences distorts the process of psychoanalysis or misinterprets the results. In particular, those methods of statistical verification derived from linear mathematics, the bell curve, and simple probability equations are inappropriate to the phenomenology of psychoanalysis. Thanks to the mathematics of what is called Chaos theory, we know that complex interactive systems with feedback do not follow simple probabilities but have mathematics of their own. Nina Hall wrote,

"Chaos theory has resulted from a synthesis of imaginative mathematics and readily accessible computer power. It presents a universe that is deterministic, obeying the fundamental physical laws, but with a predisposition for disorder, complexity and unpredictability. It [Chaos theory] reveals how many systems that are constantly changing are extremely sensitive to their initial static position, velocity and so on. As the system evolves in time, minute changes amplify rapidly through feedback. This means that systems starting off with only slightly differing conditions rapidly diverge at a later stage". (Hall, 1992, p8).

This sounds like our psychoanalytic world! In that case we should be encouraged by the further discoveries about randomness in complex systems, they are not as random or as chaotic they seem. Chaos theory really is a misnomer.

Thanks to the computer’s ability to handle millions of steps a pattern is revealed due to repetition. Within the overall shape, there lies a repetitive pattern whose exquisite substructures characterizes the nature of chaos, indicating where predictability breaks down (Hall, 1992, p8-9).

These patterns are called fractals. We could say that we deal with psychic fractals. Such complex systems as this determine the weather, for example, making it pointless to attempt prediction by ordinary mathematical methods beyond the few days of visible and measurable change. The weather, like the mind, is subject to a variety of interactive effects; it is influenced by its own reactions and has the potential to be dramatically altered by small changes. So as predictors of psychic events we have more in common with meteorologists than we have with astronomers. We might predict storms or lulls in the next few days, we might make useful statements on the mental climate, we might see a repetitive pattern, like psychic fractals, where things regularly break down. But we cannot predict with precision psychic events as astronomers can predict the movements of the heavenly bodies.

This is not due to the relative ignorance of meteorologists or psychoanalysts but the nature of the events under consideration.

If we are thinking of testing the validity of psychoanalytic hypotheses within psychoanalysis, we need to seek help to find what mathematical models might do justice to the phenomenology of psychoanalysis.

More immediately available are research projects that are a spin-off from analytic findings and theory applied in other fields. Mando Meleagrou's use of psychoanalytically informed interviews to discover the psychological influences that determine women's choices of antenatal tests for foetal abnormality, and Caroline Garland's work on the psychological effects of trauma and the efficacy of intervention are examples of such research in other fields. Interestingly, in both of these examples, the research is based on what is simultaneously a therapeutic intervention and a form of inquiry. This will often be the case and brings heart and mind together into the research: this is likely to make it more authentic.

However, that should not mislead us into accepting the idea that a successful therapeutic outcome is necessary to vindicate the background theory. Psychoanalytic theories are either approximations to the truth or not. We can understand so much more than we can do.

Some clinical presentations configure themselves into crude patterns easily understandable by psychoanalytic theory, and yet the same cases would make an experienced practitioner very wary of predicting a favourable therapeutic outcome. And yet, even within the citadel of psychoanalysis itself, voices can be heard, particularly in the USA, saying that it is true if it works, that the only test of psychoanalytic truth is therapeutic outcome. This is a bastardisation of William James' pragmatic formula that a belief is true if it works or if it produces fruitful results. This can be a dubious doctrine when applied to clinical trials. I have previously referred to the clinical trial held in Augsburg in the 16th century between an exorcist and a sceptic to test the validity of the theory of demonic possession. Needless to say, demonic possession won the day. Alas they did not content themselves with the conclusion that exorcism works but took it that demonic possession was experimentally proven (Roper, 1994, p179-80).

If, as I am suggesting, outcome does not settle psychoanalytic theory, and if there is not yet an available model to quantify or systematise psychoanalytic data for research purposes, what research should be done in the meantime? I think a good deal needs to be done to clarify what we already know and don't know in preparation for the day when neuroscience's research on the brain will need to be correlated with the accumulated knowledge of psychoanalysis about the mind. This is no easy task. For a hundred years, it has grown but now it is time we picked the fruit from the vine, keeping what is good and discarding the rest. We need to disentangle our theories from the outdated science that surrounded psychoanalysis at its inception and the general scientific assumptions of former times that have been incorporated into them.

For the rest, we should get on with our work and continue developing and refining the ideas that spring from it. The constant exchange of information through publication and increasing use of the Internet provides the best means of influencing and informing each other. The best hope of formal research developing in our own field is the development of a climate in which it is seen to be relevant and not defensive; one in which it is valued and perhaps, at a post-graduate training level, expected. Then individuals, maybe in partnership, might look for opportunities to use their ingenuity to devise their own projects.

Our experience as analysts can generate some ideas that systematic research could only answer. For example, it occurred to me recently that there is a generational escalation of psychopathology in some cases and de-escalation in others. Put simply; that some patients in analysis are more disturbed than their parents and others less so. The child-murderer Mary Bell is an example of generational escalation, the degree of psychopathology increasing from one generation to the next. Other patients one sees in analysis are clearly less disturbed than their parents. I have a hypothesis that, in the cases where there is escalation as compared with the cases where there is de-escalation, there is more negativism and destructiveness in the transference relationship than in the other group. One analyst's practice can never provide an answer to questions like that. The collection of data in some form or other might provide the opportunity for someone with enough ingenuity to devise some way of further addressing such questions. Alas, actual research consists of a lot of hard work on time-consuming detail. As I make the proposal that doing research is what will generate more research, I am uncomfortably aware that, like many other virtuous things, it is something one encourages other people to do.

Finally, if I may, I would like to quote two laws that defy the calculations of statistical probability: one is Sod's Law that says if anything can go wrong it will go wrong, and the other is Brittan's Law, not Ron Britton but Sam Brittan, the eminent economist. His law is that if anything can be misunderstood it will be misunderstood. Therefore, to avoid misunderstanding, let me summarise: in principle, I am in favour of psychoanalytic research and wary of it in practice.

References

Ayer, A. J. & O'Grady, J. (1992). A Dictionary of Philosophical Quotations.

London: Blackwell.
Flew, A. (1979). A Dictionary of Philosophy. London: Pan Books.
Gould, S. J. (2000). Deconstructing the 'Science wars' by reconstructing an Old Mold. Science 287, No. 5451, p253-261.
Hall, N. (1992). The New Scientist Guide to Chaos. London: Penguin Books.
Jonson, B. (1995). The Alchemist. Woolland, B. (Ed.) Cambridge: CUP.
Mill, J. S. (1838). Bentham. In Mill on Bentham and Coleridge, F. R.**Leavis (Ed.)**, Cambridge: CUP, 1980, p38-98.
Roper, L. (1994). Oedipus and the Devil. London: Routledge.