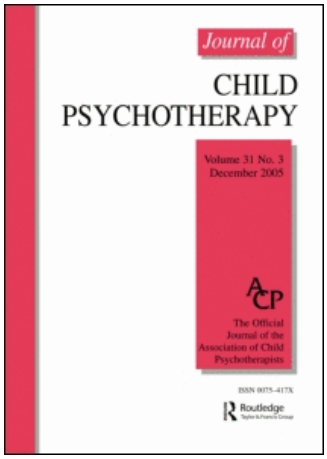

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The 'inseparable bond between cure and research': clinical case study as a method of psychoanalytic inquiry

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Abstract *Since Freud's own time, there has been great deal of debate about the most appropriate research methodology for investigating psychoanalytic psychotherapy [Fonagy,* Journal of Child Psychotherapy, *29 (2): 129–136, 2003; Rustin,* Journal of Child Psychotherapy, *29 (2): 137–145, 2003]. The single case study, which has a long tradition both within child psychotherapy and the wider research field, has been widely criticised as an approach to research, even while its contribution to clinical practice, the development of new ideas and teaching have been acknowledged. After reviewing the history of case study as a research method, this paper argues that there are a broad range of approaches to the study of the single case, each of which may be appropriate depending on the particular research question. Each of these approaches, however, must respond to the three perceived weaknesses of the clinical case study as a research method: the 'data problem', the 'data analysis problem' and the 'generalisability problem'. This paper outlines the nature of these criticisms and, using many examples of actual research projects, suggests various ways in which the criticisms can be addressed, in order for the single case study to re-gain its place at the heart of psychoanalytic research.*

Keywords Psychoanalytic research; single case studies; qualitative and quantitative methods; clinical research.

Introduction

In 1934, Freud received a letter from an American psychologist, Saul Rosenzweig. The letter told him about the results of an experimental study that Rosenzweig had carried out – involving two groups of subjects being given a series of jigsaw picture-puzzles to solve – in which he had found scientific 'proof' for the Freudian theory of repression. After a period of anxious waiting for a response, one can imagine the delight Rosenzweig would have felt when a reply arrived from Vienna, in Freud's own hand. But Freud's brief letter would surely have been enough to put anyone off attempting the arduous

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task of designing procedures to test psychoanalytic concepts. Freud wrote, with barely concealed contempt:

> My dear Sir: I have examined your experimental studies for the verification of the psychoanalytic assertions with interest. I cannot put much value on these confirmations … Still, it can do no harm.
>
> (Quoted in Wallerstein and Fonagy, 1999: 91/2)

In fact, poor Rosenzweig might have saved himself a great deal of effort if he had read a paper that Freud had written a few years earlier. In his post-script to *The Question of Lay Analysis* (1927) Freud had laid out very clearly what he considered to be the appropriate methodology for the exploration of psychoanalytic ideas. 'In psychoanalysis', he wrote:

> … there has existed from the very first an inseparable bond between cure and research. Knowledge brought therapeutic success. It was impossible to treat a patient without learning something new; it was impossible to gain fresh insight without perceiving its beneficient results. Our analytic procedure is the only one in which this precious conjunction is assured.
>
> (Freud, 1927: 256)

This is a view that many psychoanalytic researchers have continued to maintain. Michael Rustin, for example, in a special issue of this Journal on 'the clinical relevance of research in child psychotherapy', has argued that the consulting room must be considered the primary 'laboratory' in which psychoanalytic research takes place; indeed, that it is the only method of inquiry that is suited to the unique subject of psychoanalysis: the unconscious and the inner world. He declares emphatically: 'the method is in no way broken, and therefore does not need fixing' (2003: 142).

Rustin's powerful defence of research in the consulting room came in response to an equally powerful argument made by Peter Fonagy, who questioned Rustin's belief that psychoanalysis can be considered to have 'an alternative epistemology to that of scientific research' (2003: 131). In his paper, Fonagy argues that such a viewpoint maintains an inferior position for psychoanalysis, noting that reviews of the evidence-base for psychological therapies do not even consider clinical case studies as 'scientific evidence'. He calls for the incorporation of data-gathering methods from social and biological science that 'go beyond the anecdotal' (p. 134). His conclusion about the research agenda for child psychotherapy is directly contrary to Rustin's, but equally blunt: 'Child psychotherapy needs to change' (p. 131).

Yet beyond the headlines, these two advocates for psychotherapy research have more in common than may at first appear. Fonagy, after all, is aware that those 'who work at close quarters with the human mind will inevitably have an impression of reductionism when they see the full complexity of an individual's struggle with internal and external experience reduced to a single 100-point scale' (p. 130), while Rustin, for his part, recognises that 'its chosen methodology of the consulting room does leave the child psychotherapy profession very vulnerable to criticism and misunderstanding' (p. 142). He even suggests that the use of certain systematic coding procedures, blind rating and

the use of audio- and video-recordings of sessions might be beneficial. After all, 'it does not generate confidence in outsiders if professional insiders appear reluctant to let anyone else have independent access to what they are doing' (p. 143).

So are there ways in which child psychotherapy can maintain its focus on the consulting room as the primary research laboratory, remaining true to the complexity of the human mind and the therapeutic process, while also addressing some of the criticisms made by outside observers about the way in which psychoanalysts and psychotherapists have attempted to study their field? More specifically, is the clinical case study a method of inquiry that can be part of a solution to these multiple demands, or is it the problem?

A brief history of 'case study' methods

According to the Oxford English Dictionary (O.E.D.), a 'case' is 'a thing that befalls or happens; an event, occurrence', but also 'the actual state of matters; the fact'. This points to one of the central features of case study research shared by all the various approaches – it involves the study of a particular instance or event (whether this be related to a single individual, or a whole community or society), with the aim of achieving an in-depth understanding, one that captures 'the actual state of affairs'. The O.E.D. goes on to illustrate the meaning of the word within a number of *professional* contexts – a policeman, a lawyer and a doctor will all deal with 'cases', whether seen as something to be investigated, disputed or diagnosed; the lawyer will build up a knowledge of 'case law' while the doctor may write a 'case history'.

These various definitions of the word 'case' point to the fact that the word existed within certain occupational fields long before it became part of the language of research. Hammersley and Gomm show how the meaning of the term 'case study' can be traced back to its origin in medicine or in the social worker's 'case work', but that over time 'case study research has become increasingly distant from the practical treatment of cases' (2000: 1), as it gradually emerged as an important method of research within the social sciences.

The *clinical* case study, as a specific form of psychological research, was already a well-established methodology when Freud and Breuer came to publish their *Studies on Hysteria* in 1895, going back at least as far as Philippe Pinel's 'little stories' (*historiettes*) in the early 1800s (Goldstein, 2001). Freud's case studies differed from what had gone before by the depth and intimacy with which they portrayed the inner lives of his patients, but the methodology itself continued to be widely used well into the 1920s, in many different spheres of clinical research. Watson and Rayner's famous case of 'Little Albert' (1920), illustrating the learned nature of fear-reactions (a loud noise was paired with the appearance of a rat in the presence of this 11-month-old infant, as a way of illustrating how fear can be taught), is just one example – a particularly disturbing one – of the importance of case study research *outside* psychoanalysis.[1]

When it comes to the history of child psychoanalysis and psychotherapy, there is no doubting the centrality of the clinical case study. The very first work in this field, *Little Hans*, was written in the form of a case study; many of the major developments within child analysis have been introduced through the narrative account of a particular child

and his or her treatment; and the case study is central to all child psychotherapy trainings, both in terms of what is studied (the classic accounts of child analysis, varying depending on the particular theoretical orientation of the training school) and in the importance of the 'qualifying paper'. And although the number of 'straight' accounts of clinical treatments is diminishing within most psychotherapy journals, it still continues to be an important genre within the professional literature for the communication of our work and ideas.[2]

So what is the aim of such narrative accounts of our work? While the focus may be on an individual child and his or her treatment, most papers tend to aim for more than simply telling us about that one particular therapy. For example, Freud wrote *Little Hans* (1909) after his *Three Essays on Sexuality* (1905) was criticised for making claims about *infantile* sexuality based almost exclusively on constructions made in the analyses of *adult* patients. He saw the study of this 5-year-old boy as an opportunity to confirm some of the theoretical discoveries of psychoanalysis that had already been elaborated in his earlier works (Midgley, 2006).[3]

Freud – as both Klein, in her *Narrative of a Child Analysis* (1961), and Winnicott, in his case study of *The Piggle* (1977), were later to do – gives detailed, almost word-for-word accounts of conversations with Hans, based on notes taken by Hans' father at the time. Yet most contemporary case studies tend to be much briefer. Partly this is a matter of space. The treatments of Hans, Richard and Gabrielle were all relatively short, yet the case studies range from 200 to almost 500 pages. Most case studies written today are around 15 pages long (the length of a journal article or a one hour spoken presentation), and – within the English-language literature, at least – tend to follow a fairly standard format.[4]

Clearly such case presentations serve a number of functions, both explicit and implicit (Michels, 2000; Spurling, 1997). As well as providing 'evidence' or 'clarification' of certain theoretical ideas already held, case studies can also lead to the emergence of new ideas. As a form of *learning* they give us the opportunity to integrate our own clinical experience with theoretical concepts; and as a form of *teaching* they can allow others to get a sense of what goes on in the private space of a clinical treatment, providing us with what Clare Winnicott and Shepherd, in their foreword to *The Piggle*, described as 'the rare opportunity of being admitted to the intimacy of the consulting room and of studying the child and the therapist at work' (1980: vii).

Yet in the wider field of social science research, the case study, after a period of dominance at the start of the twentieth century, had for many years fallen out of favour. If one wished to be foolhardy, one could date this turning point in the popularity of case study research to one week in 1935, at the annual meeting of the American Sociological Society. At that meeting, as Hamel (1993) describes it, scholars from the Chicago School – who had dominated American sociology up until that date, with their classic case studies of urban life – were challenged by a group of scholars emerging from New York's Columbia University. While the conflict between these groups had as much to do with institutional power politics as issues of methodology, the Columbia University academics focussed their critique on the case study method itself.

The critics of the Chicago School argued that the method of case study research was lacking in representativeness and lacking in rigour. The collection and analysis of data was biased by the subjectivity of the researchers, and the results were not properly

generalisable. Case study research, if it was to be used at all, they argued, should only be used for the purposes of preliminary investigation of a field, and had little research value beyond this. Although a fierce debate about these issues continued for many years after this, and the Chicago School approach did not entirely disappear, this meeting marked a decisive turning point towards a more positivist, quantitative model of research in the US and beyond.[5] By the 1930s, as Kazdin has pointed out, 'journal publications began to reflect the shift from small sample studies with no statistical evaluation to larger sample studies utilising statistical analyses' (1982: 6). The case study method was not dead, but its status and popularity was diminished significantly.

If case study methods characterised a period of research within the social sciences up until the mid-1930s, and between-group statistical comparisons have characterised the period from then until the 1980s, then the last 20 years could be described as a period of relative pluralism, in which the dominance of statistical methods has begun to be challenged within the research field (Midgley, 2004). While group-focused, quantitative analyses, exemplified by the randomised controlled trail (RCT), are still regarded by many as the 'gold standard' in clinical research, especially among those focussing on outcome and evidence-based practice, there has been increasing dissatisfaction with a sole reliance on this approach (McLeod, 2001).[6]

As a consequence of this, there has been something of a renaissance of case study approaches within social science research, reflected in the publication of a number of key texts within sociology and psychology (Bromley, 1986; Gomm *et al.*, 2000; Kazdin, 1982; Stake, 1995; Yin, 1994). These more recent works all argue that the case study is a legitimate method within social science research, which needs to be assessed by criteria appropriate to its own methods, not by those deriving from experimental research. They argue that single case studies are often the most relevant way of studying causal influences and mechanisms; that they are a good basis on which to move towards a gradually wider level of understanding; that they are often more clinically meaningful; and they therefore play an important role in helping to bridge the gap between research and clinical practice.

Yet despite all of these strengths, the clinical case study continues to be fiercely criticised as a method of investigation. In this paper, I will argue that there are a whole range of methodologies that can be described as 'clinical case study research', and that in addressing criticisms of this approach, we need to be aware of such diversity. Some authors (e.g. Hilliard, 1993) have tried to categorise a number of sub-types of case study research, but I will argue that it is more helpful to consider the *underlying* challenges that have to be faced by all researchers wishing to make use of single-case designs, and then consider a range of ways in which these challenges can be met (Kazdin, 1981; Turpin, 2001).[7]

In brief, the criticisms of the clinical case study that will be discussed in this paper can be broadly categorised into three main areas:

- **The 'data problem'.** The basic observations or data that are used in the clinical case study are unreliable.
- **The 'data analysis problem'.** The ways in which the basic observations of the clinical case study are analysed and reported lack validity, and do not allow us to assess the truth or accuracy of any particular interpretation or hypothesis.

- **The 'generalisability problem'.** Even if the basic data of the case study are reliable, and they are analysed in a way that makes the interpretation credible, this approach is of limited value because it is not possible to generalise beyond the particular case.

In what follows, I will elaborate further on what is meant by each of these criticisms. While we may not wish to fully accept these views, it is important that we are aware of what the difficulties are seen to be with the case study as a form of research, and that some effort is made to respond to these concerns. As I hope to show, there is no 'correct' response to each of these points, but there are a number of different strategies that can be considered if we wish to make the case for the case study as a legitimate form of psychoanalytic inquiry.

Part One: the 'data problem'

(a) Should we use a therapist's own process notes as the primary data in clinical case study research?

The basic observations of the clinical case study are commonly the therapist's own notes, usually recorded soon after the session, based on the therapist's own memory of what happened during the hour. Melanie Klein, in her preface to the *Narrative of a Child Analysis*, gives a detailed account of this process of 'data collection', showing an awareness of potential criticisms and offering her justification for using this approach. She writes:

> I took fairly extensive notes, but I could of course not always be sure of the sequence, nor quote literally the patient's associations and my interpretations. This difficulty is one of a general nature in reporting on case material. To give verbatim accounts could only be done if the analyst were to take notes during the session; this would disturb the patient considerably and break the unhindered flow of associations, as well as divert the analyst's attention from the course of the analysis . . . For all these reasons I am sure that notes taken as soon as possible after each session provide the best picture of the day-to-day happenings in the analysis, and therefore of the course of the analysis. Hence I believe that – allowing for all the limitations I have enumerated – I am giving in this book a true account of my technique and of the material.
>
> (Klein, 1961: 11)

Klein, despite her awareness of the limitations of this approach, claims to be giving a 'true account' of what went on in the sessions with Richard, but what status can we give to the facts of the case as reported by Klein – or by any other child analyst or therapist? Does a 'clinical fact' exist out there, in the real world, or is it a construct within the therapist's mind (O'Shaughnessy, 1994)? While a therapist's account of a session may appear to be an account of what happened in a session, it is clearly a very selective one, and there are a great many aspects of the session that are (inevitably) not included. Most

accounts tend to shift between what happened in the session and the therapist's *understanding* of what happened in a way that is sometimes difficult to disentangle.

Klein attempts to make a clear distinction between these two processes by having her commentary separated out from the narrative of the sessions by the use of footnotes. Most clinical papers do not make this distinction so clearly. Often a process of selection and re-inscription (to use Freud's own term for the process by which memory is constructed retrospectively) has taken place on at least two levels: the *therapist's* selective memory of what took place in the session (perhaps recorded soon after the session) and the *author's* selective re-editing of the material in writing up the case study. And yet systematic studies of the reliability of analytic process notes in adult psychotherapy have suggested remarkable variation (not to mention inaccuracy) in what is recorded and how it is recorded, leading to concerns about how legitimate it is to use the therapist's process notes as the basic data to provide evidence for the therapist's own hypotheses (Wallerstein and Sampson, 1971).

Aware of such criticisms, the American Psychoanalytic Association undertook a review of the reporting of psychoanalytic case studies in the late 1980s. They noted that there was a significant lack of primary clinical data reported in the 15 most-cited articles from the major psychoanalytic journals at that time, and that most were 'over-generalised' and based on 'untestable assumptions'. While recognising the compelling nature of clinical case reports, and their importance in capturing the psychic reality of the clinical event, the list of problems the working party identified was extensive. Klumpner and Galatzer-Levy, summarising this report, wrote:

> Most analytic data are gathered retrospectively. Process notes seem to present primary data, but rarely do. Even when material appears verbatim it is often unclear how much is selected or revised. The reader has little chance to make direct contact with the clinical data to reach his own conclusions. Observation is conflated with inference. Undue reliance on narrative and brief vignettes obscures analytic experience and supports received theory. Lack of detailed clinical descriptions makes alternative understanding difficult.
>
> (Klumpner and Galatzer-Levy, 1991: 727)

(b) Is video- and audio-tape of clinical sessions a better alternative as the basic data of clinical case study research?

One possible response to these difficulties is the introduction of other recording devices, most notably audio- and video-recordings of sessions. This is not a recent idea to have surfaced within psychoanalytic research. Wallerstein and Sampson (1971) note that the first use of audio-recordings in a psychoanalytic setting took place as early as 1933, when the American analyst, Earl Zinn, created dictaphone recordings of psychoanalytic sessions with a patient at the Worcester State Hospital.

Yet such an approach has traditionally been opposed by clinicians, fearful of the clinical impact of such an intrusion on both patient and analysts. 'The talk of which psychoanalytic treatment consists brooks no listener', Freud asserted, as early as 1916,

arguing that the patient 'would become silent as soon as he observed a single witness to whom he felt indifferent' (1915/16: 17). Klein, writing in the Preface to her *Narrative of a Child Analysis*, clearly agreed:

> Another possibility of obtaining literal accounts is the use of a recording machine, either visible or hidden – a measure which, in my view, is absolutely against the fundamental principles on which psycho-analysis rests, namely the exclusion of any audience during an analytic session. Not only do I believe that the patient, if he had any reason to suspect that a machine was being used (and the unconscious is very perspicacious), would not speak and behave in the way he does when he is alone with the analyst; but I am also convinced that the analyst, speaking to an audience which the machine implies, would not interpret in the same natural and intuitive way as he does when alone with his patient.
>
> (Klein, 1961: 11)

Yet from the *research* perspective, there are clearly many advantages to the use of audio- and video-recordings, while many of the *clinical* objections appear to have been countered by the actual experience of psychoanalysts and therapists carrying out such work. Moreover, the use of such recording equipment helps to separate the clinician's role from that of the researcher; it seems to offer a much more 'complete' account of what has gone on in a therapy, free from the 'dangers' of subjective distortion; and any transcript of the sessions remains as a (relatively) public document which can then be re-examined from a number of different perspectives.

More importantly, video-recording of sessions can potentially allow a much more fine-grained account of the kind of moment-to-moment interactions between patient and therapist that are clearly of interest to the researcher in her attempt to understand the processes of change as they emerge in the clinical setting.

But as Wallerstein and Sampson recognised as long ago as 1971, the use of recordings and transcription is not always an improvement, even when considered purely from a research perspective. From a practical viewpoint, there is a real danger of 'data overload' when dealing with transcripts of psychotherapy sessions, leading to the possibility that one gets so lost in detail that one is unable to see the bigger picture. Moreover, it can be argued that video- and audio-recording are not only an intrusion into the privacy of the clinical setting, but also fail to capture some of the most important clinical data of the analytic setting – most noticeably transference and counter-transference data and the thoughts and feelings of the treating analyst. As Freud himself had commented (although not specifically in reference to the use of audio-taping):

> It must be borne in mind that exact reports of analytic case histories are of less value than might be expected. Strictly speaking, they only possess the ostensible exactness of which 'modern' psychiatry affords us some striking examples.
>
> (Freud, quoted by Wallerstein and Sampson, 1971: 18)

Clearly the myth of 'absolute completeness', just like the myth of 'absolute exactness' that audio- and video-recording seems to support, is just that – a myth. Rather than

such recordings as inherently better or worse than process notes as the 'basic observations' of clinical research, it is more productive to consider what *type* of data is most appropriate for what *kind* of research questions.

For example, in Alvarez and Lee's single-case study of 'early forms of relatedness in autism' (2004), the authors use clinical observations (i.e. the therapist's own observations during the course of therapy, written first as process notes after each session and then condensed into termly summaries) to give a general account of the qualitative features of the behaviour of a 4-year-old boy with autism over a 3-year period; but they complement this with a more detailed study of aspects of this child's interpersonal relatedness using ratings of behaviours observed in video-recordings of the same clinical sessions. Such videotape allows not only for more 'objective' assessments of certain behaviours (such as the frequency and duration of looks to another person, which can be minutely tracked and timed using video-recordings), but also allows detailed micro-analysis of moment-by-moment interactions which would be almost impossible to record using process notes.

(c) Are there ways in which the research value of the therapist's own process notes can be improved?

Even if a research study is only making use of the therapist's own process notes, there are certain things that can be done to make them more appropriate or helpful to a research study. For example, the Menninger Clinic's enormous research study of 42 lives in psychoanalytic treatment (Wallerstein, 1986) made extensive use of process notes for research purposes, but the therapists, in writing their notes, were guided by certain checklists of features (specifically related to the areas of investigation) that they were expected to observe and record. A similar procedure has taken place in the Anna Freud Centre's Young Adults Research Scheme (Fonagy and Tallandini-Shallice, 1993), in which 12 young adults with borderline disturbances were offered intensive psychoanalytic treatment. Session reports have been a central part of the data collected for this study, but (following guidelines based on the American Psychoanalytic Association's report on the reporting of clinical material, as reported by Klumpner and Galatzer-Levy, 1991), analysts were asked to use a standard format in writing their notes. This included:

- Nearly verbatim reporting of both patient and analyst's speech, clearly identifying the speaker;
- Recording the patient's non-verbal behaviour and affects separately from approximate or exact wording of speech;
- Recording the analyst's own thoughts and counter-transference separately, and in an alternative format.

Unlike many clinical case studies published in journals today, such a procedure is, in fact, fairly close to what Freud, Klein and Winnicott do in their respective case studies, where the analyst's commentary is clearly separated from the clinical material, which is in itself written up in precise detail. This is of enormous benefit to anyone wishing to

explore the material from a different perspective, because it gives scope for alternative readings and interpretations (Midgley, in press).

But as this last point implies, to consider the clinical case study as a method of research implies more than just attending to the reliability and appropriateness of the basic data itself. All research also involves making some *interpretation* of that data. And while psychoanalysis, as a field of study, has no shortage of interpretations, there are very few accepted ways to distinguish between those interpretations that are (relatively more) true and those that are (relatively more) false. This is a major difficulty when considering the clinical case study as a method of research, and one to which we will now turn.

Part Two: the 'data analysis problem'

(a) What are the difficulties with the way that the clinical case study usually selects and interprets the basic data of a psychotherapy treatment?

In his own introduction to *The Piggle*, Winnicott comments on the way in which he 'purposely left the vague material vague, as it was for me at the time when I was taking notes' and how he has 'added comments, but not enough – it is hoped – to prevent the reader from developing a personal view of the material and its evolution'. The usual case study report, however, has gone through a process of condensation and interpretation, which creates a more finished, self-contained narrative. Many case studies (as both Freud and Klein stated explicitly with Hans and Richard respectively) are written in such a way as to persuade others of the validity of a particular point of view, and the process by which the case study is constructed reflects this aim. The writing of a clinical paper often involves going through process notes in a fairly unsystematic way and picking out vignettes or sessions that seem to 'fit' the story that one is wanting to tell, then putting this material together in a narrative form that tells a compelling and persuasive account of the treatment.[8]

Yet the very sense of aesthetic pleasure and narrative completion that one often experiences in reading a 'good' case study is one of the common criticisms of case studies from a research perspective. David Tuckett, in 'Some thoughts on the presentation and discussion of clinical material in psychoanalysis', has written:

> There is the possibility that a good, well-told and coherent story creates the risk of seduction, which in the context of communication to others can be summed up thus: the more a narrative is intellectually, emotionally and aesthetically satisfying, the better it incorporates clinical events into rich and sophisticated patterns, the less space is left to the audience to notice alternative patterns and to elaborate alternative narratives.
>
> (Tuckett, 1993: 1183)

Spence (1997) goes further, and argues that the main character in every case study is not actually the patient, but rather the clinician – or at least, the clinician as the embodiment of psychotherapy itself. He described the clinical case study as following the classical narrative structure of the hero (or heroine) 'overcoming obstacles on the

way to a pre-ordained resolution', where a challenge (the patient's disturbance) is presented and ultimately overcome (the successful therapeutic outcome). Within such a narrative structure, as Bion himself put it, 'the reader is prepared for the triumph of psycho-analysis in contrast with the patient's previous misfortunes', thus creating a text that may be compelling – or perhaps even seductive – at the expense of denying space for alternative understandings or even for doubt and uncertainty (quoted by Ward, 1997: 7).

From a research perspective, the danger with the traditional method of selecting and interpreting material within the clinical case study is that episodes that tend to fit with one's own theoretical preconceptions tend to get emphasised, while those that might contradict such an interpretation are simply ignored. And even if the clinical material does appear to confirm a particular theoretical point of view, this has little significance if we do not know how many 'failures of confirmation' there may have been within the clinical data (Spence, 1993). What may appear like a compelling confirmation of our theory may turn out to be simply an ingenious piece of post-hoc reasoning. As Fonagy and Tallandini-Shallice have noted, the psychoanalytic literature is full of successful case studies, which all seem to support different psychoanalytic theories and therapeutic methods, and this has worrying implications:

> The abundance of clinicians claiming, on the basis of case reports, that their theory and technique are indispensable, is the gravest indictment of the logic of case-study methodology.
>
> (Fonagy and Tallandini-Shallice, 1993: 6)[9]

How, then, can this situation be improved?

(b) Are there more systematic ways of analysing clinical material?

One way in which researchers have attempted to respond to this criticism is by developing more explicit methodologies for analysing process notes, in contrast to the way in which clinical case studies usually select data in a purely subjective manner (and primarily to provide evidence for a particular idea). As a number of commentators have noted, the process of discovery in psychoanalysis has many similarities to the method of 'grounded theory' as developed by Glaser and Strauss (Anderson, 2004; Midgley, in press). Within the field of child psychotherapy, researchers such as Reid (2003) and Anderson (2003) have made use of the grounded theory approach to develop research studies that use the traditional method of data collection (process notes) but analyse these texts in a more systematic and explicit way. Certainly, such an explicit approach to data analysis is likely to help prevent any one 'over-valued idea' (Britton and Steiner, 1994) from dominating the clinician's mind as she attempts to build up an understanding of the clinical material.[10]

But grounded theory is not the only qualitative method of data analysis that is suitable for studying the single case and developing new ways of understanding the clinical process. For example, Varvin and Stiles have developed what they call the 'intensive qualitative case study', as a method of deepening their understanding of the

assimilation of problematic experiences in the treatment of traumatised patients (e.g. Varvin and Stiles, 1999). Likewise, transcripts of therapy sessions across a whole treatment have been analysed using a number of widely recognised qualitative methodologies from the social sciences. These include content analysis (e.g. Pole and Jones, 1998), thematic analysis (e.g. Meier and Boivin, 2000), discourse analysis (e.g. Madill and Barkham, 1997), conversational analysis (e.g. Peräkylä, 2004) and narrative analysis (e.g. McLeod and Balamoutsou, 1996). Others have developed methods specifically for the qualitative study of psychotherapy sessions, such as Phelps' 'process charts', used in the study of the process of therapy with children in foster-care (Phelps, 2003). Each of these methods highlights a particular aspect of the clinical encounter and may be appropriate to explore different aspects of the therapeutic process.

To give but one example, Peräkylä (2004) has used conversational analysis to explore how psychoanalysts use language to construct certain kinds of interpretations, in particular those linking two different domains, such as a past experience and the 'here and now' of the transference. In a detailed study of 45 'interpretative sequences' identified in the transcripts of a series of 27 analytic sessions, Peräkylä describes a number of ways in which the analyst used language to establish persuasive links for the patient. The author found, for example, that the analyst often used a 'circulation of figures', linking together two different areas of experience described by the patient with a shared figure of speech. For example, by taking up a particular phrase used by the patient to describe a past experience (e.g. 'mother just completely got crushed'), the therapist then used the image of being crushed to describe an aspect of the current situation, so making a linguistic connection for the patient between the two experiences. Peräkylä also identified the way in which the analyst would lay the grounds for such an interpretation by a process of 're-explicating the patient's experience', often building up the case for an interpretation over a long stretch of talk preceding the actual interpretation. Using highly detailed transcripts of patient-analyst talk, this study was able to explore in great detail what actually goes on when a clinical paper might simply say, 'I interpreted to the patient . . .'; and explores it in a way that may well deepen our understanding of this particular process.

(c) Can the qualitative analysis of a clinical case be used to test hypotheses?

A study such as the one described above supports the view of the analytic interpretation as a co-construction between patient and therapist; and it implies that interpretations are a 'creative act' that may well be effective because of a particular *rhetorical* persuasiveness. Peräkylä, however, is clear that the study is purely descriptive, and though these findings may be the basis for further research (perhaps looking at the performance of interpretations in different types of therapy, or linking the type of interpretative style to outcome), the study does not aim to provide evidence to support or refute any particular hypothesis.

While such a study may be useful within the 'context of discovery', where the researcher's aim is primarily to develop new ideas or to reach a deeper understanding of a process, it is arguably less valuable within the 'context of justification', in which the researcher's aim is to provide *evidence* for a particular hypothesis. But while some have

abandoned the very idea of using the single case study as a form of hypothesis-testing, others have argued that the single case offers a unique opportunity to build up causal arguments that are far more compelling than a mere statistical correlation.

For example, Robert Elliott begins his account of the single-case study of a 49-year-old man seen in process-experiential psychotherapy, with the question: 'What would it take to make a convincing case that therapy caused a reported change?' (2002: 3). He argues that two types of information are necessary if one is to make such a claim: (i) other evidence that the change occurred (corroboration); and (ii) plausible ruling out of alternative possible sources of change.

Corroboration derives from the use of other sources of data besides the clinical material itself. For example, in their case study of 'Paul', a 10-year-old adopted boy, Lush *et al.* (1998) combined process notes of the therapy with educational psychology assessments (including an assessment of IQ and projective testing), therapist questionnaires and semi-structured, open-ended questionnaires and interviews with both Paul and his adoptive parents. On the basis of this range of measures, the authors could, with some confidence, claim that Paul, one year after therapy ended, was 'basically managing well, though sometimes lacking in confidence and needing more adult approval than is usual in adolescence'. Perhaps more importantly, the authors go on to make a good *prima facie* case for arguing that this was unlikely to have been the case if Paul had not received therapy, although their argument does not address the 'plausibility criteria' as fully as Elliott's single-case study referred to above suggests we should.

But what are the criteria by which one can make such claims? Or that other factors were not responsible for the change, rather than the therapy? Or perhaps more importantly (assuming that the therapy was significant), how can we know what *aspect* of the therapy was most responsible for the change? The ruling out of other alternative (and plausible) explanations for change is essential if we are to show not simply that change took place, but also that the change can be attributed to a certain aspect of the therapy itself.

Marshall Edelson, in a series of papers in the mid-1980s (e.g. Edelson, 1985, 1986), has made one of the most powerful arguments for suggesting that the clinical case study does not have to be abandoned, even as a means of providing *evidence* for certain psychoanalytic ideas – that it can even have a scientific value in the testing of hypotheses, as long as it is used in appropriate ways. He suggests, however, that most case studies fail to achieve such an aim, because the evidence used to support a particular hypothesis is not credible. In a series of papers, he suggests that for the clinical case study to be convincing in scientific terms, a number of things must be done:

(1) The study should clearly and prominently state the hypothesis being tested in the case;
(2) Facts or observations should be clearly separated from interpretations of these observations;
(3) It should be clearly shown how the hypothesis about the case explains or accounts for the observations;
(4) Observations which, if they had occurred, would have been grounds for rejecting the hypothesis should be specified;

(5) Observations that appear to contradict the hypothesis should be reported and clear grounds for dealing with these counter-examples should be given;
(6) Since any set of observations can be explained in different ways, some argument should be given for why the observations are better explained by this hypothesis rather than a particular alternative hypothesis;
(7) Even if the observations can be better explained by this hypothesis rather than a rival one, the study should consider what factors operating in the clinical setting may have resulted in the obtaining of favourable data, even if the hypothesis were false;
(8) The study should make clear to what extent the hypothesis about this case can be generalised to similar cases or treatments.

(Edelson, 1985)

A good example of this approach is Fridhandler *et al.*'s (1999) case study of a middle-aged woman suffering from pathological grief following the premature death of her husband. Drawing explicitly on Edelson's work, the authors evaluate a 'grief-as-self-punishment' hypothesis in relation to this case and contrast it to a rival hypothesis, i.e. that the grief was simply the result of multiple losses, and show in some detail how they have taken into consideration each of the eight points that Edelson has made about how to strengthen the plausibility of claims made in single case research.[11]

(d) What place do quantitative methods have in clinical case study research?

But even if systematic methods of qualitative data analysis, such as those described above, are made use of, there are still certain arguments for a *quantitative* approach to single-case research, as well as the use of statistical methods of analysis. According to Fonagy and Moran, the 'advantages of quantification are clear':

> ... numerical representations of data provide access to statistical techniques and reduce the complexity of observations to a relatively small number of indicators. Quantitative data are also easier to inspect in searching for patterns of relationships or finding a useful format for presentation.
>
> (Fonagy and Moran, 1993: 69)

Traditionally, it has been considered that most statistical forms of analysis have been inappropriate for single-case research, so that data has usually been presented in simple visual diagrams, showing a time-line along one axis and the dependent variable (e.g. some kind of outcome measure) along the other (ibid, p. 86). The Alvarez and Lee (2004) study is a good example of such an approach, which illustrates the way that a 4-year-old autistic child's quality of emotional engagement (as recorded by a number of reliable measures) gradually improved over a period of three years. But as the authors themselves note, such a correlation, while clinically interesting, cannot say anything about what aspect of the therapist's intervention (if any) played a role in these changes.

But recent developments in statistical analyses have allowed far more sophisticated analyses of data from a single case, as Moran and Fonagy's case study of the

psychoanalytic treatment of a girl called 'Sally', with poorly controlled diabetes, has illustrated. In this case, daily measurement of blood glucose levels were taken in order to assess Sally's fluctuating level of diabetic control before, during and after the treatment. Alongside this, Moran and Fonagy (1987) developed a sophisticated content analysis and rating-scale based on the process notes of the treatment itself. Eventually 10 analytic themes were categorised as central to the pathological structures underlying diabetic mismanagement for this particular girl (e.g. feeling unloved by her father and angry with him for his lack of responsiveness, or anxiety and guilt feelings over her death wishes towards her parents) and each of these was then used by independent raters to code each weekly report on a five-point scale (from 'definitely present' to 'definitely not present'). Inter-rater reliability for such coding was also established and achieved for seven of the 10 themes.

A 'time series analysis' (Gottman, 1981) was then used to test the association between diabetic control and the therapist's ratings of these seven themes in order to test the hypothesis that there is a relationship between insight into unconscious conflict and the amelioration of neurotic symptoms and whether improvements in glucose control followed on from the emergence of certain material or preceded it. Using complex statistical computations, it was possible to trace a pattern of detailed interactions between certain therapeutic actions and both intra-psychic and behavioural changes.

While such 'time series analysis' allows a sophisticated exploration of causal relationships as they emerge across a considerable period of time (something that is eminently suited for a therapy such as long-term psychotherapy or psychoanalysis), it is not the only quantitative method for investigating the single-case study. Fonagy and Moran (1993) contrast this method with what they call the 'replication by segmentation' approach, in which transcripts of therapy sessions are sampled on the basis of a particular type of event (e.g. therapist interpretation of transference) and broken down into episodes. Another variable assumed to be causally related to this (e.g. patient's level of insight) is then identified and measured and statistical analyses are used to discover whether any associations between these two variables can be identified. Perhaps the most thorough investigation of a single case using this method is the study of 'Mrs C', whose psychoanalytic treatment was studied by a team of almost 50 researchers from the Mount Zion Psychotherapy Research Group over a period of almost 15 years (Weiss and Sampson, 1986), and has since been further investigated by a number of other research teams.[12]

Part Three: the 'generalisability problem'

But even when such a detailed analysis of a treatment, such as that of Mrs C, has been carried out using the most sophisticated analytic techniques, how far is it possible to draw general conclusions? If the findings of each study are only able to tell us something about this one particular patient, we might doubt whether the effort expended has indeed been worthwhile. In other words, if the study is not in some way generalisable, is it really of value? And does single-case research have the capability to make any such generalisations? It is this, which is perhaps the most fundamental criticism of the single-case study, to which we must now turn.

(a) Do we need to study large groups if we want our findings to be generalisable?

As most standard texts on research methods remind us, researchers are ultimately not just interested in a particular individual, but want to be able to generalise to a wider population, and this can be a problem for clinical research. 'An obvious weakness of a single case study', explains Janis, 'is that it can provide no indication as to whether the relationship applies to all other, many other, a few other, or no other human beings' (quoted by Wallerstein and Sampson, 1971: 41).

The traditional response from the research community to these questions about how to generalise on the basis of a single case study has been to make the group, rather than the individual, the primary focus of research. If, for example, one studies a large group of children referred because of ADHD, and compares the outcomes of therapy for those who are offered psychotherapy with those treated by CBT, then, they argue, one might have the beginnings of a more general statement about the therapeutic effectiveness of one form of therapy rather than another. If the two groups are big enough, and the difference between them considerable, then the results may even be 'statistically significant'. Furthermore, if one has been able to specify the variables tightly enough, so that the children seen in the two groups were, overall, roughly similar, then one might feel that there were grounds for generalising from this specific group of children seen in the study to a wider population of children diagnosed with ADHD.

Such a solution to the 'problem of generalisability' has been the most common one within the research community to date, and leads to the kind of research designs based on group-comparisons that make up the substance of most evidence-based reviews. Yet there are a number of problems with this approach, known as 'statistical inference'. For one, it is not at all easy to make a group of patients 'representative' of a wider population, for however many variables one tries to control for (age, level of disturbance, gender etc.) people tend to remain irresolutely singular. There is a real danger that the statistical inferences one makes actually lack 'external validity', in which case the general conclusion one reaches may be misleading. Some would go further than this and argue that the very attempt to establish context-free, general truths is based on a discredited reductionism that is the legacy of a positivist approach to science (Lincoln and Guba, 2000).

Even if one does not accept such philosophical critiques, there are practical limitations to statistical inference based on group comparisons. By averaging scores across individuals, group-based results fail to attend to individual differences in a way that may be highly misleading. A result may be true on average, but it is still notoriously difficult to 'generalise' the findings about a group to a *specific individual* (Leitenberg, 1973). This is one of the main reasons why clinicians struggle to make use of evidence-based findings in a real-life, clinical setting.

(b) Are researchers always aiming to generalise from a single case?

Yet generalisation is not always the purpose of a research study. For example, if a single-case study finds that someone suffering from a neurotic disturbance was not sexually abused as a child, then a theory that suggests (as Freud's 'seduction theory' did) that *all*

neurosis in adulthood is the result of childhood abuse must be revised, and a new theoretical model that can explain both abused *and* non-abused histories must be developed. Such 'falsification' studies, to use Popper's term (1963), can be of immense value in carrying forward our knowledge and often have significant impact on practice. They may not tell us what is common, but they do teach us what is *possible*.

(c) Is it possible to generalise from the study of a single case?

But even if one *is* aiming at generalisation, statistical inference is not the only way of generalising the findings of a piece of research, and nor is it necessarily the best. As Fonagy and Moran have argued:

> The belief that knowledge based upon groups of individuals is somehow more likely to be generalisable (that is, applicable beyond the specific locus of its discovery) than is the case for knowledge based upon individual cases is fatally flawed.
>
> (Fonagy and Moran, 1993: 65)

Such a misconception, based on statistical theories of sampling developed in the 1930s, are not easy to dispel. In reality, however, it is very questionable to what degree it is possible to generalise from Randomised Controlled Trials and other such studies, and problems of sampling and questions of generalisability 'trouble group design as much, if not more, than individual case studies' (ibid., p. 90). After all, single-case studies, when systematically replicated with other individuals, can help us not only to understand what aspects of the original study's findings are transferable, but also those that are not. When a different result is found, one must try and work out what is specific to this second case which makes the results different to the first study, and in this way one's understanding is gradually enriched and one's understanding refined.

Such an approach, which is sometimes referred to as 'aggregated single-case study', has a long history within psychoanalysis, going back to Freud and Breuer's *Studies on Hysteria* (1895) and continuing to the present day – with great effect – in the emerging field of neuro-psychoanalysis (Kaplan-Solms and Solms, 2000), where a theory is built up on the basis of the understanding that emerges from a series of case studies. Dreher (2000) refers to this process as 'intuitive induction', in which 'a series of observations is used to infer basic principles or patterns that are suspected to be an integral part of the phenomena under discussion' (p. 25). Such an approach has been widely used in child psychotherapy research, whether it involves a single clinician drawing inferences from a large number of cases over a considerable period of time, or a number of clinicians working together with a particular group of children to establish some more general understanding of a particular phenomenon.[13]

Studies such as these contribute to what Stake (2000) has described as a process of 'naturalistic generalisation', which can avoid some of the dangerous over-simplifications inherent in more traditional approaches to generalisability in research. Janet Phelps argues that the model of aggregating single case studies is a highly appropriate one for psychoanalytic researchers and can be seen as equivalent to the development, for lawyers

and judges, of 'case law', in which the comparison of successive cases leads to incremental conceptual refinements and reformulations (Bromley, 1986; Phelps, 2003; Stake, 2005).

(d) Are there ways in which we can improve the generalisability of aggregated clinical case studies?

One of the common weaknesses of the aggregated case study approach is that the *choice* of cases for comparison is often fairly unsystematic and depends more on chance than planning. The principle of 'theoretical sampling', as described by Glaser and Strauss (1967), in which each subsequent case is chosen specifically to further the analytic understanding (e.g. by choosing a subsequent case which might seem to deliberately contradict the theory one has developed on the basis of previous cases, in order to challenge one's emerging conceptual framework), is rarely used in aggregated single-case research in psychotherapy, resulting in samples which are *ad hoc* and in danger of being selected simply because they support the developing account.

The use of more specific principles of case-selection for aggregated case studies, such as those described by Sidman (1960) as 'direct replication' and 'systematic replication', would gradually help us to establish *how far*, and *in what ways*, the findings of any one case can be generalised to others. Moreover, such an approach would provide us with the sort of 'generalisation' that is clinically significant, and avoids many of the shortcomings of typical group-level comparisons. One could even argue that, despite the practical difficulties and time involved, the use of carefully designed single-case designs is the only *meaningful* way of achieving generalisation in this field.

Conclusion: an inseparable bond?

This paper began with Freud's confident assertion about an 'inseparable bond between cure and research', and the way in which two contemporary researchers in the field of child psychotherapy – Michael Rustin and Peter Fonagy – have offered diametrically opposed views of the correctness of this view.

As I hope to have shown, the clinical case study could have an important role to play as a method of psychoanalytic research that brings together these divergent views, despite many of the criticisms that have been made about this approach. Yet if it is to have such a role (as opposed to its other roles, e.g. as a form of teaching, or as a means of clinical communication about technique etc.), then we need to be aware of – and provide some response to – the limitations of this approach as they have been identified by the wider research community. In some cases this simply means being able to justify why we do things in a certain way; in other cases it means changing the way we have traditionally done things, and introducing more explicit and systematic forms of investigation.

But are such time-consuming and (at times) tortuous methods necessary in the field of psychotherapy research? Why not accept the traditional clinical case study, with all its strengths and obvious weaknesses, for what it is? I think Donald Spence gives the answer to these questions most elegantly:

> Because the case study is our primary means of reporting clinical happenings, the defects of the case study genre have important implications for the progress of psychoanalysis as a science. If the case study tends to be anecdotal, selective, consciously and unconsciously self-serving, and biased towards a singular solution, it can be seen that our literature is seriously incomplete.
>
> (Spence, 1993: 40)

One solution – perhaps the most common one since the 1930s, when Saul Rosenzweig first sent his study of repression to Sigmund Freud – has been to turn to *non-clinical* methods of investigation to give scientific credibility to the findings of clinical psychoanalysis. Such an approach was given added force by the work of Adolf Grünbaum in the 1980s, who argued that evidence for psychoanalysis 'cannot be obtained without well-designed *extraclinical* studies of a kind that have for the most part yet to be attempted' (1986: 217). The rise of 'evidence based medicine', with a research paradigm derived primarily from the natural sciences, has further increased the argument for the irrelevance of clinically focused, single case research, even as it has increased the perceived gap between clinicians and researchers (Stiles, 1994).

But as I hope to have shown, this is not the only response possible to the problems that have been identified with the traditional clinical case study. While the report of a psychotherapy treatment may not, in itself, be sufficient as a way of doing research, this is more a matter of *degree* rather than *kind*, and there are ways in which the clinical case study may be set on a firmer research footing.

What would Freud have made of the attempts to develop more formal ways of investigating the single case? It was, after all, Freud himself, in the *Studies on Hysteria*, who commented on the strange fact that his 'case histories should read like short stories and that, as one might say, they lack the serious stamp of science' (1895: 160). Would he then have approved of the attempt to combine the literary and the scientific elements of the case study, retaining the focus on the individual while also introducing methods to increase the reliability, validity and generalisability of such studies?

In all probability, the answer to this question is 'no'. Freud was explicit about the limits of any attempt to bring objective scientific methods to the field of psychoanalytic investigation. Writing to Marie Bonaparte, Freud declared:

> Mediocre spirits demand of science a kind of certainty which it cannot give, a sort of religious satisfaction. Only the real, rare, true scientific minds can endure doubts which are attached to all knowledge. I always envy the physicists and mathematicians who can stand on firm grounds. I hover, so to speak, in thin air. Mental events seem to be immeasurable and probably always will be so.
>
> (Quoted by de Mijolla, 2003: 96)

While Freud was clearly right that research will never provide us with absolute certainty, he was equally careful in this letter not to equate science with this demand. It is mediocre spirits, he reminds us, who demand certainty, and only the 'true scientific minds' who are able to endure doubt – who have both the freedom to create and discover and the discipline and patience to rigorously challenge and investigate. Freud

described his own personality as a 'succession of daringly playful fantasy and relentlessly realistic criticism' (1915: 55), and it is this combination that perhaps describes best what is necessary for a truly scientific – and creative – approach to research.

I would like to end with some words that the American psychoanalyst, Arnold Cooper, has written, when describing some of the consequences of taking such an inquisitive, creative and critical approach to our own work:

> Many of our favourite ideas will be shown to be wrong or, what amounts to the same thing, not useful. Psychoanalysts should learn to entertain this prospect as good news rather than bad news [. . .] Psychoanalysis is invigorated by the increase of our empirical research and by the shedding of concepts that no longer yield new knowledge . . . We pass a critical test for any healthy therapeutic field – we change as we gain new knowledge and experience.
>
> (Cooper, 1993: 391)

This is a point of view that Freud himself would surely have approved of, and is a view that both Peter Fonagy and Michael Rustin, despite their contrary views of the child psychotherapy research agenda, would likely endorse. And this is what makes the re-discovery of the clinical case study method as a form of research such an important task for our profession.

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Notes

1 As Hamel (1993) clearly illustrates, the rise of the case study as an approach to research went hand in hand with the development of an inductive approach to knowledge-generation, influenced by the principles of 'analytic induction'. This approach assumes that the researcher is aiming to formulate propositions that can apply to all instances of a particular phenomenon; but that the method of achieving this is by studying a particular instance of the phenomenon, developing hypotheses to fit this instance, and then moving on to all other instances in a 'to and fro' process of constant hypothesis-revision until all instances appear to be explained.

2 A quick comparison between the *Journal of Child Psychotherapy* [30, 2004] and a corresponding volume 30 years earlier (3/4, 1974) suggests the truth of this comment. In the 1974 volume, five of the seven papers are case studies, describing either the whole of a treatment or the first year of a therapy. In contrast, the 2004 volume has only one clinical case study. Of the other five articles, one is purely historical, and the other four all use clinical examples or vignettes to discuss a particular theme. This move away from clinical case studies to the use of vignettes and examples appears to be widespread in psychotherapy journals, and is no doubt partly a response to increased concerns about issues such as confidentiality and informed consent. However, it also raises important issues about the validity of such 'evidence' (Widlocher, 1994).

3 Melanie Klein, in publishing her *Narrative of a Child Analysis* (1961), had somewhat different aims. Although she also wished her case study to provide evidence for certain theoretical views she had already developed, she also saw this detailed case study as offering an opportunity to demonstrate a certain way of working with children analytically: 'In presenting the following case-history, I have several aims in view. I wish first of all to illustrate my technique in greater detail than I have done formerly. The extensive notes I made enable the reader to observe how interpretations find confirmation in the material following them. The day-to-day movement in the analysis, and the continuity running through it, thus become perceptible. Furthermore, the details of this analysis clarify and support my concepts' (1961: 11).

4 The standard clinical paper in English-language journals tends to begin with a brief introduction, followed by an account of the background history and referral; then a narrative account of the treatment itself, using a combination of broader narrative and themes and more detailed accounts of particular sessions, especially at the assessment, the first holiday breaks, 'turning point' sessions and those approaching termination. Most papers then end with a brief 'discussion' section, in which the clinical material is linked to the wider psychoanalytic literature. But this is not always the case. 'If we read case-histories from different periods, or from different analytic schools, we notice different emphases. More or less of the patient's history is included; it is assumed to be more or less objectively true or distorted by instinct and transference; different kinds of traumatic events are included in the account' (Budd, 1997: 40/1). These differences are not random, but reflect differing theoretical orientations and preoccupations.

5 In fact, the shift away from the case study paradigm had already been signalled 10 years before this fateful meeting with the publication of R.A. Fisher's classic work on statistical methods (1925). Along with other developments in the field of statistical testing, this work made the case – and provided the tools – for the study of variance within the large-scale comparison of groups.

6 While the positivist/quantitative model continues to have significant weight, the 'new paradigm' research (Reason and Rowan, 1981) has picked up on a distinction that Allport initially made between *nomothetic* and *idiographic* approaches. While nomothetic approaches are concerned with establishing general laws about human behaviour, the idiographic approach is primarily concerned with understanding the specific case, the unique instance. Such an approach has obvious affinities with case study research.

7 In a previous paper (Midgley, 2004) I have argued that qualitative approaches offer a potential way forward for child psychotherapy researchers wishing to navigate the perilous path between the Scylla of large-scale, quantitative research on the one hand, and the Charybdis of the traditional clinical case study on the other. In the current paper, which can be seen as a companion piece to the earlier one, I am suggesting another way of moving forward that avoids the potential pitfalls of large-scale quantitative work or the traditional clinical paper: the development of more sophisticated forms of single-case methods – *whether these make use of quantitative or qualitative data* – or indeed a combination of both.

8 Greenwood and Loewenthal (2005) gave a more positive account of this process, describing the clinical case study as a 'phenomenological-hermeneutic' form of research, in which the therapist goes through various stages of reflection to develop an understanding of the case. Here the aim is to provoke the reader into thought, rather than persuade them of the 'truth' of the interpretation in anyone objective sense.

9 As is often the case, it was Freud himself who first recognised this danger most clearly. In his 1920 case study, 'The psychogenesis of a case of homosexuality in a woman', Freud offered a

compelling construction of his patient's early history which would explain her homosexual development as a late adolescent. But having done so, he voices a word of caution. He writes: 'So long as we trace the development from its final outcome backwards, the chain of events appears continuous, and we feel we have gained insight which is completely satisfactory and even exhaustive. But if we proceed to reverse the way … then we no longer get the impression of an inevitable sequence of events which could not have been otherwise determined. We notice at once that there might have been another result, and that we might have been just as well able to understand and explain the latter … in other words, from a knowledge of the premises we could not have foretold the nature of the results' (Freud, 1920: 167).

10 Britton and Steiner (1994) distinguish between a 'selected fact', seen as 'a creative integration of disparate facts into a meaningful pattern', and an 'overvalued idea', which can 'likewise be used by the analyst to give a sense of integration to otherwise disparate and confusing experiences [… but here] the integration is spurious and results from the facts being forced to fit an hypothesis or theory which the analyst needs for defensive purposes' (1994: 1070). While recognising the danger of confusing these two phenomena, the authors suggest that a 'selected fact' stays close to the patient's own material and is 'emergent', whereas the 'overvalued idea' is often a 'pre-selected fact' used defensively to buttress a fragile sense of stability in psychic space.

11 Transcripts of two sessions in which the patient explored her grief were analysed in some detail, and an argument made that analysis supports the view that this woman's prolonged grief was related to her guilt about her ambivalent feelings about her deceased husband. The researchers then sought out other segments of the case in which the patient's grief was explored, which might seem to explicitly contradict this hypothesis, and considered what observations might have led them to abandon their hypothesis (e.g. if the therapist had implanted ideas about grief, by bringing this topic up; or if there was evidence that this patient was prone to self-punishment as a general character trait) and discuss any observations which might seem to conflict with the formulation (e.g. the fact that the patient continued to feel depressed even after the guilt had been identified). Finally, they consider an alternative hypothesis (grief as a result of multiple losses) and give evidence from the case history, which would suggest that this would not adequately explain the material presented.

12 Jones and Windholtz (1990) made use of a Q-sort methodology to help create a chronicle of the whole course of Mrs C's therapy in a way that could be considered both formal and reliable, while Spence *et al.* (1993) have explored selected sessions to examine the effect of certain interventions on associative freedom. Bucci (1997), in a computer-assisted analysis of discourse patterns in Mrs C's treatment, pointed to themes in Mrs C's material that had not been noted by previous researchers and suggests that she therefore reached 'essentially opposite conclusions from those of Jones and Windholtz, using the same set of seventy sessions' (p. 172). This is both evidence for the importance of multiple perspectives, and a timely reminder that the most 'rigorous' methods cannot guarantee that we will reach a shared 'truth' about the meaning of a case.

13 Some notable examples of this approach, coming from the Tavistock Clinic, are Meltzer *et al.*'s (1982) research on children with autism and Boston and Szur's (1983) work with severely deprived children. Likewise, Moran and Fonagy's study of a child with poorly controlled diabetes, discussed above, was also followed up by replication studies with three other children (Fonagy and Moran, 1990), in which the finding of a contingent relationship between successful therapy and physical growth was reproduced in each case, thus

strengthening the case for the generalisability of this finding (within a specific population of children with poorly controlled diabetes).

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