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Beyond Retroduction?— Hermeneutics, Reflexivity and Social Work Practice

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SUMMARY

Over the last three decades qualitative research methodologies have been in the ascent within social science. Yet social work evaluation studies have tended to be quantitative in nature, conventially relying upon the generation of criteria against which interventions are retrospectively judged. The generation of such criteria inevitably depends upon pre-suppositions, which in themselves go unresearched. As a consequence the *sense making* activities on which social work interventions depend are rendered immune from critical analysis. This reflects a broader tendency for social work to cling to naive realist epistemologies, which are arguably obsolete within the interpretive paradigm in which its activity is properly located. By examining the debates within interpretive social science, this paper argues for an approach to social work assessment which avoids the pitfalls of naive objectivism and the nihilism of anarchic relativism, whilst retaining *creativity*, *imagination* and *hope*.

Over the last three decades, qualitative research methodologies have been in the ascent within the social sciences: certainly, within sociology, there is little doubt that they have become the new orthodoxy (Hammersley, 1992). Alongside this development, the drive for efficiency within welfare services has fuelled attempts to find ways to evaluate social work practice. However, evaluation studies have tended to be quantitative in nature, conventionally relying upon the development of measurable criteria against which interventions are retrospectively

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judged. The generation of such criteria inevitably depends upon certain presuppositions which will, in turn, be influenced by the social, political and historical contexts in which social work is located. Thus a concentration on *outcome* alone can render certain intricacies of the social work *process* immune from analysis. This reflects a broader tendency for social work to cling to naive realist epistemological positions, which are arguably obsolete for the kinds of interpretive activity in which it is engaged. I intend to focus upon social work as a 'sense making' activity and, to this end, it is helpful to draw analogies between assessment and ethnographic research, the validity of which has been hotly debated within interpretive social science.

I will argue that, as a consequence of the pursuit of certainty, social work has limited the possibilities open to its practitioners. To an extent, my analysis builds upon earlier contributions, particularly Holland's (1993) work on meta-theory and reflexivity in social work, and the more recent paper by Sheppard (1995). Sheppard convincingly explodes the false duality that has been constructed between social science and practice wisdom by arguing that social work assessment may be compared to the processes of analytic induction characteristic of qualitative methodologies. On this point, I wholeheartedly agree with him. However, I should like to push the argument a little further and I shall go on to show that in treating the 'retroductive' research strategy as uncontested, Sheppard leaves a fruitful area of social inquiry (namely hermeneutics) relatively unexplored. Within the context of contemporary sociological debate, this apparent neglect is difficult to justify and it appears to seduce Sheppard into the tacit reinforcement of dominant practice ideologies. His analysis requires another layer of abstraction and, to this end, I should like to consider some philosophical issues about 'truth' and 'relativism' and their implications for practice.

ETHNOGRAPHY AND ASSESSMENT—CAPTURING THE 'REAL'?

Social work is often a pragmatic and reactive activity; as such, it has not traditionally concerned itself with philosophy. A similar disregard has been noted amongst ethnographers and, as Hammersley (1992) notes, there is indeed a danger that abstruse philosophical debates can simply become a distraction from more earthly and immediate research problematics. However, Hammersley continues

> there is no escape from philosophical assumptions for researchers [social workers]. Whether we like it or not, and whether we are aware of them or not, we cannot avoid such assumptions. And, sometimes, the assumptions that we make lead us into error (Hammersley, 1992, p. 43).

At the heart of these assumptions is the belief that there is an independent reality to be discovered and that the products of any research (or assessment) should directly correspond to that external reality. Positivism is the archetypal example of this tradition. Treating social facts as things to be uncovered by *objective* scientific endeavour, it is represented in the litany 'if it can't be measured, it doesn't exist'. However, such a model sits uncomfortably with the interpretivist view of lay understanding and the associated commitment to uncovering the *meanings* which social actors use to understand their world. Positivism has been largely unconcerned with lay accounts, treating them as corrigible or irrelevant (Blaikie, 1993). The interpretivist position, on the other hand, necessarily encompasses the view that 'reality' is relative, mediated by human consciousness.

By and large, the status of lay accounts has been marginalized or even ignored in evaluative studies of social work practice. However, for reasons which will become clear, it is my contention that this issue is pivotal to the processes of assessment and intervention. First, whilst many social workers would (cl)aim to possess the external and detached ('professional') qualities revered by positivism, the central notion of empathic understanding seems contradictory to this position, for it necessarily involves some notion of immersion in the lived experience of the service user. The subject, therefore, merges with the object.

Secondly, returning to the issue of authenticity, if this is not achievable by detachment, how is it to be demonstrated? Ethnographic research is characterized by the researcher's 'deep familiarity' (Lofland, 1995) with the objects of study. The demonstration of this familiarity is a rhetorical strategy used by ethnographers to authenticate their knowledge claims (Atkinson, 1990). This status as 'witness' is also a crucial aspect of social work assessment and, paradoxically, it can prove difficult for service users to challenge these 'insider' views. The convention for home visiting in social work is evidence of more than the intrusiveness of the state panoptican; it is central to social work's epistemology. However, the understandings so generated may differ markedly from those of the service user themselves and may be loaded with tacit professional assumptions and received ideas (Philp, 1979; Rojek *et al.*, 1988), which often go unexplored and hence are able to masquerade as neutral knowledge.

A similar contradiction is evidenced in social work's commitment to anti-oppressive practice, and its attempts to achieve critical distance from ethnocentric or patriarchal (Western scientific) interpretations, whilst simultaneously asserting the validity of its own professional interpretations. We simply cannot have it both ways for, if our own interpretations are granted ontological privilege, then this must raise serious challenges for anti-oppressive practice. However, if relativism is applied

across the board and we treat all our knowledge claims as contingent upon a particular set of pre-suppositions, we enter an abyss in which the progressive nature of knowledge acquisition must be questioned and nothing taken for granted. Under such a regime, patently cruel child rearing practices (or even infanticide) could be treated as acceptable so long as the account of the parents was internally consistent. This kind of nihilism is patently not an option for social workers.

However, there may be a way out.

> There is a great danger of backing ourselves into a corner by deploying a dichotomy which obscures the wide range of epistemological positions available. We can maintain a belief in the existence of phenomena independent of our knowledge claims about them . . . without assuming that we can have unmediated contact with them and therefore that we can know with certainty whether our knowledge of them is valid or invalid. The most promising strategy for resolving the problem . . . is to adopt a more subtle view of realism (Hammersley, 1992, p. 50).

This version of 'realism' (cf. Sayer, 1992) acknowledges the existence of an independent reality but equally accepts that research (or assessment) cannot faithfully reproduce it. Any representation must *always and necessarily be partial, delivered from a particular perspective*. This allows for the coexistence of competing but *equally valid* claims about the same phenomenon. Hammersley argues that such claims should be treated as evidence, with the central research task becoming the reflexive monitoring of the assumptions and actions taken as a result of them. A view of research (assessment) as the *reproduction* of reality is abandoned with a recognition, instead, that research can achieve no more than a 'selective representation' thereof. It thus becomes vital that the background assumptions and theoretical models informing a particular interpretation are made explicit. Holland (1993) makes a similar point in relation to social work practice. He suggests that the knowledge and skills social workers need for practice should be built upon an awareness of the consequences of seeing the world through a particular set of theoretical lenses. He introduces the concept of a 'paradigm map' on to which the various competing theoretical models can be plotted, along with the philosophical assumptions which underpin them.

However, if the (naive) correspondence theory of truth (that analytic descriptions should directly correspond with the immediately given reality) is rejected, what should replace it? For social workers, the link between data (observation and dialogue) and subsequent 'problem' construction cannot be side-stepped, since the ideas so generated have powerful material consequences for service users. Therefore, we must have some mechanisms for evaluating the ways in which workers seek to make sense of the lives of others.

Again, it is useful to examine the ways in which ethnographers have sought to authenicate their knowledge claims. As a result of the paucity of criteria through which to evaluate research within the interpretivist paradigm, ethnographers have left themselves open to criticism. To summarize what is in fact a complex area, there appear to me to be two primary concerns, which may equally be applied to social work assessment:

1 A particular account may satisfy basic and mundane criteria of plausibility and may be supported by evidence which suggests a particular causal relationship. It will nevertheless remain problematic if we are not persuaded that *other* competing explanations are less adequate. There has been, it is suggested, insufficient attention to the analysis of *negative* cases which arise during data analysis, so that alternative and competing explanatory relationships remain obscured. Furthermore, the frequency of occurrence of a particular phenomenon is often ignored. As a result, ethnographies (and assessments) are potentially indistinguishable from anecdote and prejudice.
2 Ethnographers are criticized for being unreflexive:

> they give no attention to the social processes that impinge upon and influence their data. They do not adopt a critical attitude toward their data. . . . Thus the strengths of the data are exaggerated and/or weaknesses under emphasised (Brewer, 1994, p. 233).

Sheppard's recent paper deals satisfactorily with the first criticism by advocating the use of the hypothetico-deductive method (discussed below). However, the latter point is not sufficiently developed in his paper, leading him to ignore certain crucial areas of inquiry.

THE RETRODUCTIVE RESEARCH STRATEGY

> Social workers, when making assessments, confront the world rather like qualitative social researchers. They are concerned with issues of description, accuracy, understanding and meaning, and this information is gained largely through interviews, direct observation and documentary evidence (Sheppard, 1995, p. 273).

Sheppard argues that the concept of 'retroduction'may be used as a heuristic device to help us to understand and evaluate this essentially interpretive process of assessment. He is using the term to cut across the traditional distinction between inductive and deductive research strategies. In the former, observation of the world is the central tenet, and ideally this observation should be undertaken without preconceptions. Clearly this is problematic, since it is not possible to undertake

conceptually neutral inquiry. The latter approach is exemplified by the 'hypothetico-deductive' or 'falsificationist' method expounded by Popper (1959), within which it is freely accepted that theory inevitably penetrates inquiry. The remedy is to subject any hypothesis to a rigorous process of falsification, so that knowledge develops on a survival of the fittest basis (Chalmers, 1982). On closer analysis, it becomes clear that induction and deduction collapse into one another, since one cannot advance knowledge without observation any more than one can escape the conceptual backcloth which circumscribes any scientific endeavour. Hence the concept of retroduction was born.

Attending first to the strengths of Sheppard's argument, he sees the retroductive method as a potentially useful yardstick to be used in the evaluation of social work assessment. The rigorous search for disconfirming evidence and adoption of a sceptical attitude are seen as central to the pursuit of the best possible 'fit' between the phenomenon 'out there' and the worker's assessment of it. The major potential deficiencies and pitfalls of assessment are identified as follows:

> Poor practice is marked by a lack of clarity in hypothesis formulation. . . . Sensitivity to disconfirming evidence has two dimensions. First, it is possible for a practitioner to proceed in a manner which seeks to confirm initial impressions or preconceived ideas. . . . The second relates to evidence, although collected during assessment, which, because it contradicts explicit or implicit hypotheses, is ignored. . . . These instances represent the professional equivalent of 'jumping to conclusions' (Sheppard, 1995, pp. 278–9).

Indeed they do and, in this respect, Sheppard's analysis has much to commend it. However I simply do not believe that he goes far enough. He treats some of the dominant theoretical models in use in social work as unproblematic whilst inadvertently subscribing to a naive realist epistemology. For instance, Sheppard implies that, if an appropriately rigorous approach is adopted, it should be clear which are the better and the worse interpretations of a given presenting problem. There appears to be no acceptance of the possibility of the existence of different and *equally valid* accounts. Instead he enters into a sort of serial monogamy with each version of 'reality', giving the complexities of family life a static quality, as if only one person can be 'right' about what is going on. It is worth quoting at some length an extract from Sheppard's paper in order to illustrate this point.

> A 14 year-old may be referred by his . . . parents because he is disobedient and close to being 'out of control'. He has been stealing money from his mother's purse, truanting from school and is increasingly aggressive. . . . The parents may themselves present this as a personality issue: this is an awkward

life stage and a nasty egocentric boy. As a loose initial hypothesis . . . this can be examined by seeking evidence. Initial interviews with him may show him to be more sensitive than presented by the parents. . . . Such evidence negates simple explanations of egocentricity. We then may search for alternatives, examining family dynamics. We may discover that his mother has been increasingly 'snappy' over the past year, has, unlike in the past, shown no interest in what he is doing and communicated this lack of interest dismissively and with verbal aggression. Both parents have been arguing. Such a line of investigation contains an implicit hypothesis that the parent-child relationship is poor. . . . Further investigation may reveal the father and mother have been arguing frequently, and this relates to poor performance of her traditional (maternal) role. . . . We may then hypothesise that the woman is depressed because she feels trapped within the limits of her traditional role expectations. Although the boy's problems cannot be ignored, *the central problem is in fact the mother's depression*, arising from her individual experience of oppression (Sheppard, 1995, p. 276, emphasis added).

I am sure that such a formulation would do little to lessen this mother's depression (or oppression) and it is, I believe, no accident that Sheppard concludes in this particular manner, given his past interest in maternal depression (1994*a*; *b*). This is an example of the kind of error that can easily occur in practice, if theoretical preferences are not rigorously monitored and if the meanings service users give to situations are simply treated as obstacles to be overcome. There are any number of ways in which the events described above could be interpreted and settling on one version will largely be governed by whose perspective one *treats* as more real. I am not suggesting that this choice is arbitrary, but neither is it neutral.

Holland (1993), has argued that, in adopting a particular theoretical perspective, it is essential that the worker is aware of the *consequences* of doing so. There is more to this than a good analytic 'fit'. It requires imagination and a particular form of reflexivity which recognizes one's own location within a discourse or professional ideology, call it what you will. It is on these grounds that the retroductive method has been criticized within the philosophy of science. The choice of hypothesis is far from unfettered; it 'occurs in the context of ontological, conceptual and theoretical assumptions' (Blaikie, 1993, p. 168) held by the researcher (practitioner).

Returning to social work, the particular hypothesis a worker is 'selling' to his or her manager, to a court or to another professional, is more likely to be treated as credible if it conforms to certain dominant professional background expectancies (compare Hak, 1992). These will shift over time and can become hegemonic to a point where there appears to be no other way of viewing the situation. For example,

Parton (1991), Howe (1992) and Thorpe (1994) have variously charted the dominance of child protection within social work discourse. I have argued elsewhere (White, 1996) that the ascendancy of attachment theory in child care practice and its incorporation into assessment tools like *Protecting Children* (Department of Health, 1988) has led to certain 'preferred' professional formulations. Burman (1994) has similarly pointed out that attachment theory may be used to explain polar opposites of infant and child behaviour (for example separation anxiety *and* indifference can equally be read as pathology). A hypothesis may thus achieve a 'universal relationship' (Robinson, 1951, cited in Sheppard, 1995, p. 275), in the sense that nothing seems to refute it, but this alone does not make it the most helpful interpretation. A similar point has been made by Kaye (1995), in the context of psychotherapy research:

> [R]esearch questions posed from within the parameters delimited by the canons of scientific research tend to be disconnected from psychotherapy and indeed transform it into something else. For not only are the questions we bring to therapy theory laden, but our theories construct the phenomena they are designed to explain . . . *a question, by proposing a distinction, constructs its own answer*. That is, the theory-driven distinctions we draw determine the questions we ask, the nature of our findings, and thus the picture we form on the basis of our results (Kaye, 1995, p. 38).

There is thus an intrinsic circularity embedded in the research strategy advocated by Sheppard. Retroduction alone does not sufficiently advance the hypothetico-deductive method to make it adequate for the purposes of evaluating the *content* of the social work encounter. Evaluation is about more than falsification and scepticism, it is about *meanings* and it is about *consequences*.

BEYOND RETRODUCTION—HERMENEUTICS AND REFLEXIVITY

The problems with the retroductive approach, outlined above, have been debated within sociology and philosophy resulting in what has been called the 'abductive' research strategy.

> The Abductive research strategy is based on the Hermeneutic tradition, and is used by Interpretivism and approaches which include Interpretive ontological and epistemological elements. . . . Abduction is the process used to produce social scientific accounts of social life by drawing on the concepts and meanings used by social actors, and the activities in which they engage (Blaikie, 1993, p. 176).

Whilst it usually co-exists with some form of retroduction, since we can never fully escape theoretical and conceptual constructions, this approach seeks to explicate the meanings which social actors give to their world and is exemplified (*inter alia*) by ethnomethodology (for example Garfinkel, 1967) and by grounded theory (Glaser and Strauss, 1968). Narrative therapeutic approaches (for example Epston and White, 1992), which treat people's accounts of their problems as 'stories' which are then potentially amenable to 're-authoring', have been influenced by a similar set of assumptions. Identities are seen as largely socially constructed and reproduced through language. This approach, similarly to the abductive research strategy, is intended to overturn the view that lay accounts are inferior to professional constructions.

On a more theoretical level, it is within the abductive research tradition that are properly located those versions of feminist thought (see, *inter alia*, Stanley and Wise, 1983; and, for a review, Holmwood, 1995) which have sought to overturn the (Cartesian) dualisms—reason/emotion, subjective/objective, self/other—with a celebration of subjectivity and emotional connectedness. Likewise, Critical Theory (for example Habermas, 1984) and Postmodernism (for example Lyotard, 1984), somewhat uneasily, share the space in the abductive camp; but, whilst the former seeks to retain an emancipatory project within a hermeneutic epistemology, the latter sees the search for such 'truths' as a constraint on the freedom to spin off new ideas and possibilities. It is not profitable to develop these debates here but enthusiasts may refer to Bernstein (1983), whose analysis of objectivism and relativism has not, in my opinion, been bettered.

The abductive strategy may appear to open the door to 'anything goes'. However, there are so many competing professional interpretations of the same phenomenon that there is scant evidence that the imposition of formal 'scientific' categories results in a less anarchic practice terrain. For example, speaking of women with mental health problems, Ussher states:

> [a] woman who is unhappy, angry and withdrawn may be told by a psychiatrist that her hormones are in flux, by a psychologist that her cognitions are faulty, by a sociologist that her environment is responsible, or by a psychoanalytic therapist that she is repressing her unconscious desires. . . . Who is right? (Ussher, 1991, pp. 103–4).

It is precisely this kind of problem that has led to the shift within social science, from naive realism and empiricism towards hermeneutics, yet social work appears to be dragging its feet. I would argue that, under circumstances such as those described by Ussher, any evaluation of the competing perspectives must take account of the *consequences* of a

particular interpretation. Indeed, it is precisely because the medical model has been perceived as a potentially negative and 'labelling' perspective to apply to many of the problems of service users, that social work has traditionally been critical of it. Yet we have, by and large, remained strangely resistant to the reflexive monitoring of our own potentially stigmatizing interpretations, as is illustrated in the extract from Sheppard above. This brings me back to the concept of reflexivity.

REFLEXIVITY

Within the social work literature, I would argue, the concept of reflexivity has been used to describe processes by which workers seek to develop a capacity for self-monitoring in respect of their *values* and, in Holland's sense, their *theoretical* perspective. Both these elements are vital, but there is more to reflexivity than this. Bourdieu (see Bourdieu and Wacquant, 1992) has drawn a distinction between what he calls 'textual' and 'epistemic' reflexivity. By textual reflexivity, Bourdieu is referring to the trend for researchers to bare their soul in the writing of their ethnographies, disclosing their value positions and treating the reader to an autobiography of the research experience. This kind of reflexivity is mirrored within social work. Bourdieu is dissatisfied with these attempts and argues that researchers need to be aware of the ways in which their interpretations are affected by their membership of a particular discipline or professional grouping, hence he argues

> [w]hat distresses me when I read some works by sociologists is that people whose profession it is to objectivize the social world prove so rarely able to objectivize themselves, and fail so often to realize that what their apparently scientific discourse talks about is not the object, but their relation to the object. (Bourdieu, in Bourdieu and Wacquant, 1992, pp. 68–9).

Bourdieu recognizes that sociologists (and social workers) will impose interpretations on the behaviour of those they study and, since this is the case, they must 'work to neutralize the specific determinisms to which their innermost thoughts are subjected' (Wacquant, in Bourdieu and Wacquant, 1992, p. 46). Without a properly reflexive orientation, practitioners become 'like the carpenter who, possessing a hammer, tends to see every problem as a nail' (Blaug, 1995, p. 425). So epistemic reflexivity may only be achieved by social workers becoming aware of the dominant professional constructions influencing their practice. For example, within contemporary child-care services these pivot around notions of parental dangerousness and fragile childhoods. This does not mean that these constructions have to be rejected wholesale, simply

that workers should be explicitly aware of the need to consider the consequences of their analyses and formulations, remembering that

> the identities and needs that the social welfare system fashions for its recipients are *interpreted* identities and needs. Moreover, they are highly political interpretations; and, as such, are in principle open to dispute. Yet these needs and identities are not always recognized as interpretations. Too often, they simply go without saying and are rendered immune from analysis and critique (Fraser, 1989, pp. 153–4).

CONCLUSION

I have argued that ethnographic research and social work assessment share certain characteristics, principally their methods of data collection and analysis. The recognition, within interpretive sociology, that it is impossible for any research strategy to achieve a faithful representation of 'reality' has posed certain challenges to traditional tests of reliability and validity. Some attention must thus be paid to the development of alternative evaluative criteria.

Moreover, as assessment differs from ethnography, in the sense that it forms the foundation for action, and is immediately and often profoundly consequential for service users, there is a pressing need to attend to the interpretive processes and presuppositions upon which social work activity depends. In presenting themselves (or being presented) for a service, clients (or referrers) are generally seeking the amelioration of some difficulty. I suggest that this pushes social workers towards a 'naive realist' epistemological position; a diagnostic *modus operandi*, which necessarily embodies a kind of linear causality. Many of the theoretical models in use display these characteristics and social work has (paradoxically, given its pre-occupation with oppressive practices) been slow to develop the reflexivity to recognize its shortcomings.

I have already referred to attachment theory, many of the current interpretations of which have a very deterministic quality, reducing the complexities of multiple problem presentations to failures of attachment. Similarly, adverse responses to social work intervention are all too easily written off as denial or resistance, rather than a failure on the worker's part to achieve a satisfactory 'fit' between their analyses and the meanings ascribed by the family or individual themselves. I do not intend to imply that culpability should always be assigned to the worker on the basis that they have made an inadequate assessment. For example, sometimes the views held by the adults in a family are patently discrepant with children's needs (even if their versions of 'truth'

are internally consistent on their own terms) and may need to be challenged and sometimes overruled to protect the relatively powerless. However, remembering Bourdieu's 'epistemic reflexivity', when this course of action is taken social workers should be explicitly aware of their 'preferred' professional formulations.

So what *should* be done to ensure that the qualitative and interpretive process of assessment is as rigorous and as *useful* as possible? First, assessments should attend to the measures of reliability and validity appropriate to the interpretivist paradigm. Sheppard has described many of these, concerning the explicit awareness of theoretical influences, the generation of possible competing explanations and the rigorous search for disconfirming evidence. I should like to add a further simple criterion, that assessments should give an indication of the *frequency* with which a particular event occurs. This is particularly important where inferences are being drawn on the basis of observed behaviour, such as parental 'coldness', for example. This simple counting may also serve to expose those situations in which observations are being selectively reported.

Secondly assessments should, as far as possible, 'fit' with lay explanations. There is little value in redefining a parent's request for day care as covert rejection and offering family work instead. Whilst it is sometimes necessary to try to change the stories families offer about their lives (for example where individuals within families are being hurt or abused), equally, redefinitions can deny parents access to resources which may help them to manage better.

Thirdly, it is essential that social work moves away from a naive realist epistemology and accepts that it is futile to try to achieve a simple correspondence between 'reality' and an assessment of it. In the field of human relations, reality is rarely so static. Rather than seeing the existence of multiple explanations as negative, it should be embraced as a potential asset; for along with uncertainty comes hope for change. This is postmodernism at its most positive, embracing pluralism and possibilities. The deterministic explanations which can so easily be applied to people's lives have consequences, which brings me to my final point. If we accept that there are a number of ways in which a given situation may competently be interpreted *and* that, by using the criteria above, we have eliminated the possibility of wild inaccuracy, how may we go about 'searching for a better story' (Pocock, 1995)? Some stories are more hopeful and helpful than others and the more helpful stories will shift from case to case: thus the process cannot be reduced to mechanistic technique. For example, as Pocock suggests, it is a step forward to redefine a child's challenging behaviour as a consequence of her anger at her parents' separation, rather than as intrinsic

badness. In another case, however, it may be better to settle on an interpretation of a baby's excessive crying as the consequence of colic rather than reinforce a desperate mother's view that it is the result of her inability to provide her child with enough love. These approaches are not incompatible with orthodox 'scientific' theory, since, even if there were 'attachment problems' in such a case, the difficulties would be unlikely to be alleviated by the daily torture of guilt and hopelessness which would result from the latter story.

> Some stories, such as those which support depression, seem both fixed and hopeless and lack any awareness of personal strengths. A deep belief in one's uselessness or unlovability or irredeemable guilt may act as a prison from which there is no simple release. Implicit in the concept of a better story is that such truths are never final but ultimately provisional and open to revision. . . .Lack of certainty may be the first step to hope (Pocock, 1995, p. 167–8).

An important caveat to this (relatively) relativist position is that, in seeking a better story for one person, things are not made worse for another, more vulnerable individual. There is little point in alleviating a mother's guilt to such a degree that she believes her baby deserves a good thrashing for making such unreasonable demands. There are limits to relativism—and this is the problematic face of postmodernism—some 'stories' stink. Social workers need an approach to assessment which avoids the pitfalls of naive objectivism and the nihilism of anarchic relativism, whilst retaining *creativity*, *imagination* and *hope*. Social work continues to court certainties and Sheppard's recent analysis, whilst seeking to close the gap between social science and practice wisdom, maintains the preoccupation with the pursuit of elusive apodictic truths. I suggest that such an approach jeopardizes the very 'capacity to shift from one perspective to another . . . that sets off the social scientist from the mere technician' (Mills, 1959, p. 112).

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