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Auto-Ethnography as Reflexive Inquiry: The Research Act as Self-Surveillance

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[p. 100 ↓]

Auto-Ethnography as Reflexive Inquiry: The Research Act as Self-Surveillance

We've got a little girl of nine. It's quite difficult, this rejection. It's not the same rejection as other families. What they are rejecting is a child ... who they feel very guilty about bringing into the world and they would actually say, when they really get distraught, things like, 'really if she died it would be better for everyone concerned', and the father saying, 'well let's put it this way, if there was a shipwreck, I would save my wife and sons'. (Team Leader, Children and Families Team)

Social workers are very accustomed to providing accounts of their actions. For example, they may have to justify their decisions to the courts, to other professionals, to 'consumers' of services, or may be questioned as part of the audit activity which is increasingly a feature of welfare organizations in the UK and elsewhere. In the extract above, a team leader, working in a statutory child care team in the UK, is describing her thoughts and actions [p. 101 ↓] in relation to a case involving a child with a disability. There are a number of ways in which we could approach this account. Conventionally, it may be used to make a judgement about the team leader's practice. For example, was she following the imperative of the Children Act (1989) and treating children with disabilities as 'children first'? Had she properly assessed the risk to the child? Had she provided appropriate services? In this chapter, I should like to introduce you to a different way of reading professional accounts of various kinds. I shall argue that, by using transcripts of social workers' talk and by undertaking detailed description and analysis of everyday business, we may open up for debate previously unquestioned aspects of practice. Professional conversation (talk) and case files or reports (text) can be used to explore and make explicit taken-for-granted ideas about practice and hence can open these up for debate.

The study on which I draw is a multi-method ethnography of child care social work which I completed in 1997 (White, 1997b). During two years of participant observation, I adopted a 'complete-membership' role (Adler and Adler, 1996). That is, I conducted the study at 'home', in the department in which I was employed as a team manager. Hence, I have referred in the title to 'awto-ethnography as reflexive inquiry'. This is a rather fancy way of saying that doing ethnography at home, or indeed reading ethnographies about ourselves, can help us to examine, more self-consciously and analytically, what we are thinking about and doing in our professional practice. This does not mean that we will necessarily want to change it. We might want to debate, or to change some things some of the time, but we might even feel rather proud of other bits. However, I shall argue that we can only make these judgements once we have developed a particular kind of 'reflexivity' about our routines and practices. We may help nurture this reflexivity by undertaking the kinds of analysis I have referred to above and I shall illustrate in more detail in due course. However, first, we must consider what is meant by the rather peculiar term 'reflexivity', and how it differs from the more familiar concept of 'reflection'.

Reflexivity: Beyond introspection?

In recent years, researchers and welfare professionals alike have been told they must be 'reflexive' in their practices. However, reflexivity is a slippery term and there is considerable ambiguity and variety in the way it is interpreted (Taylor and White, 2000). It is often treated as a form of what, for the purposes of differentiation, I shall call, 'reflection'. This is a form of 'benign introspection' (Woolgar, 1988: 22): a process of looking *inward*, and thinking about how our own life experiences or significant events may have impacted upon our thinking, or on the research or assessment process. Typically, this form of reflection involves the researcher or practitioner keeping confessional diaries, which include narrative accounts of their actions 'in the field', and particularly in the context of social work may [p. 102 ↓] make reference to 'power differentials' or to (often failed) attempts to 'empower'. As one interpretation of the concept of reflexivity, self-disclosures of this type have become rather fashionable of late. Clifford Geertz, in pejorative tone, dubs this trend 'the diary disease' (1987: 90). There is, indeed, a danger that we learn little about what is claimed and a great

deal about the struggles and torments of the researcher or practitioner. Moreover, paradoxically, by 'confessing' to some misdemeanour, or error in the *past* and displaying their capacity to learn from such mistakes, the researcher or practitioner constructs their *current* interpretations and practices as new, improved, and hence more robust and less fallible. Although it is by no means true in all cases, the researcher or practitioner can cast themselves as a kind of born-again truth broker. This very effectively closes down opposition and fruitful debate - the very thing that reflective diaries are supposed to create.

Academics interested in the sociology of scientific knowledge have generated a more radical version of 'reflexivity' (for a collection of papers on reflexivity, see Woolgar, 1988). They argue that scientific knowledge is constructed through social and linguistic processes. However, since these sociologists also make knowledge claims of their own, this inevitably begs the question of how social scientific accounts of the social construction of scientific knowledge are themselves constructed through language, and so on. This interpretation of the concept of reflexivity has led to the development of innovative textual devices such as attempts to convey 'multi-vocality' (multiple voices or versions) by simulating conversations and arguments between the researcher/writer and themselves (cast in another role), about the production of the account. These 'literary' forms can be very revealing and useful (see Hall, 1997 for a particularly illuminating and worthwhile example of this technique applied to social work). However, some suggest that, in its extreme forms, this movement has drifted towards solipsism, producing a good deal of 'self-deconstructive' work about reflexivity, at the expense of detailed claim-making accounts on the sociology of science itself (Latour, 1988; Law, 1994; Pinch and Pinch, 1988).

Whilst both these forms of reflexivity are a good deal better than failing to think at all about what one is saying, writing or doing, in exploring the concept of reflexivity in this chapter, I shall be advocating a rather different reading.¹ Using the research experience as an exemplar, I want to interpret and apply the concept of reflexivity to denote a form of destabilization, or problematization of taken-for-granted knowledge and day to day reasoning. Treated in this way, reflexivity becomes a process of looking *inward* and *outward*, to the social and cultural artefacts and forms of thought which saturate our practices. So, for the reflective diary to become reflexive, it would need, as it were, to

reflect upon the narrative forms themselves and upon their socio-cultural origins and effects (cf. Ixer, 1999). Bourdieu has termed this process of problematization 'epistemic reflexivity'.

However, the process of problematization is not so simple. If something is taken for granted, if we are no longer aware of it, how may we open it up [p. 103 ↓] for study? This links to a larger, recurrent question from social science and philosophy - 'to what extent can we know ourselves?'. This question has generated a good deal of methodological debate, which we should consider before proceeding further.

Researching at home: The problematics of being on the inside 'out'

Marginals and natives: collapsing the distinction

Before examining the specific ways in which the debates relate to social work and social work research, it is worthwhile summarizing some of the social scientific opinion on auto-ethnography. There is a rich literature, originating particularly within social anthropology, on conducting research within one's own culture. Some anthropologists, with their traditional predilection for the exotic and remote, appear to have been rather troubled by questions of reliability and validity within what has become known as 'auto-anthropology'. One of the guiding metaphors, transported into ethnographic studies from anthropology, is that of the ethnographer as a naive 'child', 'apprentice', 'stranger'. Of course, when conducting research within familiar surroundings, it may be extremely difficult to achieve this 'anthropological strangeness'. However, the 'marginal native' metaphor sits uncomfortably alongside the imperative that the ethnographer should develop 'deep familiarity' with the setting and its members.

The fieldworker is always a marginal person who, if he [sic] is *successful*, is permitted relatively free access to the backstage area of the social scene. (Pelto and Pelto, 1978: 248)

In other words, roll up your sleeves and muck in, but under no circumstances 'go native'.

However, as anthropologists have debated the in/out, stranger/native, familiar/unfamiliar dichotomies, it has become clear that either/or distinctions of this kind are difficult to measure or sustain in ethnographic field-work. An understanding of the setting is allegedly built for the ethnographer, through the search for regularities, involving the collection and analysis of descriptive data, leading to the gradual discovery, over time, of insights into 'the interpretations of reality as seen by the group members' (Agar, 1980: 195). Clearly, in order to access these interpretations, the researcher must place considerable dependence upon informants (insiders), and this blurs the ostensible boundary between inside and outside. The ideal-typical ethnographer may be on the outside 'in', but the informants selected by researchers may themselves, in some way, be on the inside 'out'.

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The requirement for reflexivity on the part of the researcher *and* the informants is explained further by Geertz (1979), who points to the necessity for translation back and forth of 'experience-near' and 'experience-distant' concepts:

Confinement to experience-near concepts leaves an ethnographer awash in immediacies as well as entangled in the vernacular. Confinement to experience-distant ones leaves him stranded in abstractions and smothered in jargon. (Geertz, 1979: 227)

The collection and abstraction of experience-near concepts is not simply a matter of extracting accounts from those who know best. Experience-near concepts are characterized by their high level of integration into the natural attitude, or, in Bourdieu's (1977b) terms, the habitus, of subjects. Thus, experience-near concepts will not be treated as *concepts* at all, but simply as the only right and proper way to think. Hence,

there is a need for reflexive activity on the part of researcher *and* informants for whom the ordinary and everyday must already have been problematized.

Moreover, there are few settings so homogenous that they contain no one on the inside 'out'. For example, Strathern, referring to Okely's (1987) ethnographic study of Travellers, casts doubt on the distinction between the familiar and unfamiliar, saying that such a criterion would involve 'impossible measurements of degrees of familiarity' (Strathern, 1987: 16). What defines being 'at home' for Strathern is whether the researcher and researched share the conceptual frameworks which inform ethnography, thus:

whether anthropologists are at home qua anthropologists, is not to be decided by whether they call themselves Malay, belong to the Travellers or have been born in Essex; it is decided by the relationship between their techniques of organizing knowledge and how people organize knowledge about themselves. (Strathern, 1987: 18)

So, it seems that validity and reliability do not depend on the ethnographer being an alien, or outsider in the setting. Indeed, as I shall go on to argue, the advantage of turning the ethnographic gaze upon the familiar is precisely that it holds the possibility of *defamiliarization* of certain routines and practices (Aull Davies, 1999). So, how does all this relate to social work?

Defamiliarization and 'practitionerresearch'

To answer this question, we need to examine some of the arguments for and against, so called, 'practitioner research'. You will see from Jan Fook's contribution to this volume, and the discussion in Chapter 10, that there is not one single variety of practitioner research, and there is no doubt that increasing numbers of social workers and managers are undertaking [p. 105 ↓] 'research' activity of various kinds. For example, in the UK, during the 1980s and early 1990s, as a consequence of neo-liberalism and the new managementism, professionals were urged to monitor outcomes, demonstrate effectiveness and generate performance indicators with enthusiasm and vigour. The concepts of evaluation and outcome measurement have gathered

momentum and have, in some services, become routine activities. Moreover, a particular rational-technical variety of practitioner 'research' has been fuelled by New Labour's 'modernization' agenda (e.g. Department of Health, 1998a, 1998b), reaching its pinnacle in the evidence-based practice movement.

Whilst some have argued that *only* practitioners in a particular field can produce research which is relevant to practice (see Hammersley, 1992 for a counter-argument to this view), the idea that practitioners can or should research themselves is not uncontroversial (e.g. Hammersley, 1992; Atkinson and Delamont, 1993). It is sometimes said that practitioner research is undertheorized, and that its problem-driven and solution-focused nature can preclude proper 'unfettered', critical engagement with the phenomena in question. In short, the argument runs that practitioner research sometimes moves far too quickly from exploring what *is*, to advocating what *ought* to be the case. There is some cogency to this argument, and elsewhere, with John Stancombe, I make a similar point in relation to psychotherapy process research. With the fundamental and unshakable belief that therapy is a good thing and an impassioned desire so to prove, we argue, clinician-researchers rarely find anything other than what they had commonsensically anticipated at the start of their enquiry (Stancombe and White, 1997).

This may be the case, but any *wholesale* dismissal of practitioner research must rest on the presupposition that it is impossible, in some sense, to research oneself. One cannot, it is implied, be on the 'inside' and achieve any 'distance' from the forms of thought one is researching. Under such circumstances, the argument runs, practitioner research becomes self-referential, simply reproducing dominant forms of thought. However, we have seen from the debates about anthropological fieldwork that the inside/outside distinction has proved extremely tenuous. I should like to suggest here that it is perfectly possible for practitioners to develop a critical, or analytic orientation to their practices. Reflecting on my own case, it seems that there were two principal ways in which this orientation developed.

First, it was in part a product of particular personal and professional experiences. For example, I remember that, on one or two occasions when my second child was very small, I took him into work with me for a short while. He would sometimes be clingy and sometimes very independent, preoccupied with play, or other people, and hence almost

indifferent to my presence. In the company of a group of my social work colleagues, I became acutely conscious that his behaviour could easily be read on any of these occasions as one of the many varieties of 'attachment disorder'. [p. 106 ↓] Had this been a clinical assessment, I thought how vulnerable I would have been to such a diagnosis, and how resistance to it could easily have been written off as defensiveness or denial. Having used the theory routinely in my work for many years, this experience made me much more aware of its incredible malleability and virtual incorrigibility. There are few permutations of infant behaviour which escape its prolific explanatory potential. I came to see attachment theory (indeed all theory) less as a convenient tool, or template, and more as a powerful coloured lens, with the capacity to clarify (by eliminating the 'glare' which we experience when we try to make sense of complex relationships), but which may also cast the world in an over-simplifying monochrome.

The second way in which a more meta-analytic orientation to day-to-day practices may develop, has been noted by Strathern above. It is related to 'techniques of organizing knowledge' (Strathern, 1987: 18). If practitioners are exposed to different analytic and meta-analytic frameworks from outside their primary discipline, this increases the likelihood of them understanding their practices in new ways. In my own case, this influence came from my academic studies in sociology and social theory.

Of course there must be many more routes to 'marginality', and echoing the anthropologists, I should like to argue that social work is 'heterogeneous enough to provide its own outsiders' (Shokeid (Minkovitz), 1970: 113). However, this does not mean that the defamiliarization upon which reflexive practice depends is straightforward or easy, or that all practitioners have the motivation to acquire it. It is important now to examine in more detail the processes involved and their effects.

Reflexive inquiry as defamiliarization: Towards a more realistic realism

Being a fish

Clearly, the main advantage of researching amongst one's own kind lies in the familiarity and ordinariness itself. This means that one can avoid the sorts of problem Law describes below:

I had been told that I could sit in on meetings ... But I could only attend meetings if I knew when and where they were taking place. And this was not so easy. 'You can't ask about something if you don't know it exists' ... I'm not implying that anyone deliberately tried to stop me learning about meetings ... It was more that they thought I wouldn't be interested. For it turns out ... that people think that sociologists will not be very interested in 'technical details'. And what of the discussions and conversations that didn't take place in meetings? I had no way of plugging into these at all. (Law, 1994: 44)

In contrast to Law, I found that my relationships with practitioners and managers facilitated access to sensitive material, and my 'insider' [p. 107 ↓] knowledge helped me to identify effortlessly what were the important meetings. So, a 'complete membership role' (Adler and Adler, 1996) has some advantages. It allows for the checking and rechecking of observations and analytic inductions against constantly accessible 'business as usual' (cf. Pollard, 1985).

However, that is not to say that there are *no* difficulties and dilemmas associated with conducting an ethnography 'at home', indeed there are many. However much one desires it, defamiliarization does not come easily. The 'invisibility' of 'experience near' concepts referred to by Geertz above amplifies the need for reflexivity on the part of the researcher. This can take considerable effort and will always and necessarily be partial.

It will be helpful at this point to examine my own ethnography of child care social work in more detail (White, 1997b).

As I explained earlier, between 1989 and 1995, I was employed as the manager of a hospital-based, local authority 'children and families' team. In 1993, I began two years of participant observation research in my own authority. By this time, 'doing being' a team manager was second nature to me. Like other professionals, I had my own 'cook book knowledge' (Atkinson, 1995a: 116) and I had learned the recipes by heart. Although my academic background in sociology had given me the kind of alternative interpretive repertoire referred to by Strathern above, defamiliarization was not unproblematic. For example, at the beginning of the research, I found myself able to spot *unusual* practices (often in a very critical 'wouldn't have taken that decision' manner!), or to discuss attachment theory at a meta-theoretical level, or to comment sociologically on cases that were somehow distinctive, or on organizational cultures or bureaucratic practices. However, many of the *ordinary and everyday* explanatory frameworks and models of causation seemed to me to be quite simply the obvious and only way to think about cases. At this stage, I had barely approached the first stage of analysing the construction of social work practices - the task I had set myself in the research proposal. I was unable to see that some things could be other than the way they were. I had failed to see that 'X as it is at present, is not determined by the nature of things, it is not inevitable' (Hacking, 1999: 6).

For example, during fieldwork, I undertook an analysis of documentary sources of data. I had recognized the importance of case files, because it is in such records, and in the reports contained within them, that social workers produce their rationalizations for past interventions. Records are also time travellers and form the basis for sense-making in the future. They are thus of considerable significance both organizationally and analytically. Taking notes from an initial sample of a hundred of these files was a very time-consuming task, and at the beginning of the exercise, I had the alarming experience of not being able to see the wood for the trees. All I could see was 'ordinary' and highly predictable case recording. Similarly, I had identified the weekly 'allocation meetings' as a rich source of data. In these meetings, cases which have been referred during the previous [p. 108 ↓] week are talked through and allocated by the team manager to a particular social worker. I taped these meetings in two separate teams over several weeks, but during the initial transcription I was unable to imagine what I

would possibly find to say about them. The forms of thought were so very familiar to me - like the proverbial fish, I had yet to discover water.

Discovering water

Gradually, by reading and rereading these data it became clear to me that there were certain preferred ways of ordering cases in professional narratives of various kinds. Incidentally, it is for precisely this reason that research instruments like tape-recorded talk can be useful, because they allow for a more distant and microscopic analysis of the taken-for-granted pragmatics of the ordinary and everyday (West, 1996), whether the analyst is outside 'in' or inside 'out', and whoever they are.

For example, by persevering with the transcription of the contents of the case files, I began to perceive patterns, routines, typifications and strategies which comprised 'competent' recording. For example, the records revealed what I called a hierarchy of accounts, with the versions of events offered by some categories of referrer or referred more likely to be reported as factual (e.g. child, other professionals and sometimes mother), and others more often coded as uncertain or contestable (e.g. fathers, particularly estranged fathers or step fathers, neighbours, some professionals with a reputation for over or under reacting, sometimes children denying abuse had taken place).

I also began to see subtle 'Warnings' and to notice that certain causal accounts offered by families were usually reported with scepticism. For example, in the absence of corroborative medical or psychiatric diagnoses, parental reports that their children were temperamentally (intrinsically) difficult were routinely subverted and an alternative professional reading offered, which redefined the 'problem child' as a product of deficient parenting or family relationship problems. Amongst the hundred case files analysed, I could find no 'discontinuing' cases. That does not mean that none exist, but it does suggest that the assumption of 'parental culpability' (used here to mean 'responsibility' - Pomerantz, 1978) forms part of social workers' 'prototypical causal *gestalt*' (Bull and Shaw, 1992: 640).

In relation to the allocation meetings, by preparing and reading the transcripts it became increasingly clear to me that the notion of 'risk' is actively produced, or artfully accomplished in social workers' talk. This does not mean that social workers make it up. Rather, through active selection and assembly, fragmented and ambiguous information is ordered into a coherent story. This story, in the telling, attains the status of fact, attributes causation, accomplishes subtle blamings and anticipates certain effects. It also silences or quietens other potential readings of the case (cf. Hall et al, 1997). Again, scepticism about parental accounts, and a display of personal commitment to 'child-centred practice' is integral to competent [p. 109 ↓] professional performance. Here, again, social workers both invoke and reproduce a dominant cultural notion of childhood as an age of passivity and *potential* personhood (Burman, 1994; Marks, 1995; Rose, 1989, 1998; Stainton Rogers and Stainton Rogers, 1992; White, 1998b). Reaching personhood depends, not on the child's 'programming' (biology or nature), but on them receiving 'good enough parenting' (nurture).

This discourse of parental culpability is at its most apparent when it is challenged or breached. This occurs when a child's embodied characteristics are classified and defined by biomedicine as, in some way, deviant or pathological. In these circumstances social workers' accounts explicitly seek to reconcile biological explanatory frameworks with their dominant professional imperative to assess risk and judge parenting. This can be illustrated in the following, heavily edited extract in which a team leader is describing to me a very 'difficult case'.²

We've got a little girl of 9. It's quite difficult this rejection. It's not the same rejection as other families. What they are rejecting is a child ... who they feel very guilty about bringing into the world and they would actually say when they really get distraught things like, 'really if she died it would be better for everyone concerned', and the father saying, 'well let's put it this way, if there was a shipwreck I would save my wife and sons'. You are getting a very clear message that really this child they wish she wasn't there. They wish they had never produced her. From the child's point of view she's autistic as well as deaf, totally deaf. We know that she recognizes her family and we know that she gets excited when she wants to go home with the family and we know that she has

got into a certain routine that she knows her mother and father and her brothers, now she has got some kind of relationship and communication with them whatever their feelings are... .

I certainly spoke to child protection on a couple of occasions. I remember when we investigated it. Well the child was - we had a complaint that the child was outside on her bedroom window, sitting outside on the first floor window. When we investigated it the child was sent to her room quite a lot, and there were no safety bars on the window, so we talked about, well the mother said if I find myself getting uptight the only thing I can do, she actually has this screech, is put her in her bedroom, which is a coping technique ... so what we did was to put bars on the window, which was in the child's interest, but it still bothered us that they were using that perhaps to excess and this is a time when I was offering the family resource worker because she was saying 'it's terrible I can't spend anytime with my two other sons at tea time', and yet when I offered a family resource worker she tells me she employs a nanny everyday at tea time, so she really wasn't making much sense and I couldn't respond to what she was saying. The other thing that we had was this child was made to wear a hat all the time, and she wouldn't go anywhere without this hat, but we were worried why she had to wear this hat and it was that she was pulling her hair out, so they insisted on her wearing a hat. Now you can say, this is the dilemma with disability, is it cruel to make her wear a hat or is it really the only thing you can do to make her stop damaging herself and people ... The wearing of the hat has improved in that she has different kinds of hats now and she [p. 110 ↓] likes to wear a hat, but they are not actually these bonnet type things that tie under her chin, so she can wear a summer hat and she can change them and increasingly she is being encouraged to leave the hat aside for certain activities that she enjoys and we'll concentrate on that and actually her hair is growing. So, I think the parents resort to very serious preventative measures rather than coaxing and distracting that we would want them to do, so it's whether they've got that kind of investment. She started biting, biting

her clothes and what they did was give her a horrible rubber ring. I don't know what it was off, it was almost like one of those dog rings, to bite on. The school refused to give her this rubber ring and with my backing we said we are not prepared to do it, if she wants to bite her clothes we'll attempt other methods of distraction, but we are not introducing a dog ring to this child.

As I said at the start of this chapter, there are several ways of reading this account. For example, it could be used to make a variety of normative judgements about this team leader's competence. However, it could also be examined for its fundamental organizing principles. What knowledges or rationalities does it invoke, and what are its effects? This latter form of analysis, which operates with a position of 'indifference' (Garfinkel and Sacks, 1970) as to the adequacy of the explanation, or the actions it purports to describe, can yield insights into taken-for-granted aspects of contemporary social work.

From this position of 'indifference', we can see that, throughout the story, the team leader seeks to reference the child's humanity - 'We know that she recognizes her family and we know that she gets excited when she wants to go home with the family.' At the same time, she assigns the child to 'deviant' categories - 'she is autistic, as well as deaf, totally deaf; 'she's so damaged'. The team leader struggles to assign culpability to the parents for aspects of their parenting which would usually be defined as 'bad' (e.g. wishing the child had never been born; confining her to one room for long periods when she is distressed; insisting that she wears a hat at all times; giving her a rubber ring to chew on). The account continually juxtaposes 'blaming' talk about parental management techniques, with the more 'expert' strategies recommended by the social workers. For example, we are told 'the parents resort to very serious preventative measures rather than coaxing and distracting that we would want them to do', and also 'we'll attempt other methods of distraction, but we are not introducing a dog ring to this child'. The deviant nature of this action is amplified by the phrase 'we are not introducing a *dog* ring to this *child*'. This is a ring not fit for a child (it could have been called a teething ring for example), it is a *dog* ring.

The team leader's problems are multiplied by the pragmatic difficulty in 'measuring' any damage to a child whose development patently deviates from the usual markers of developmental psychology. The team leader finds it very difficult to mount a challenge

to the parents' moral accounts of their situation, and is 'forced' to accept the story that they are providing the best care that they can. This is further reinforced by the team leader's [p. 111 ↓] practical knowledge that they are providing the best care *available*. That is, that she would struggle to find an alternative placement for the child. This sits uncomfortably alongside normative judgements about 'rejection', and their style of parenting which would 'normally' have resulted in the child being 'removed'.

Thus, although moral judgements *are* made about the parents of children with classified and named 'intrinsic' problems, the usual practical responses to these judgements (e.g. 'investigation', 'case conference') are rendered exceedingly problematic. However, the fact that stories are constructed by practitioners in the manner illustrated above, underscores the dominance of the 'parent as culpable' discourse, in that deviations are clearly recognized as 'accountable' phenomena, which require that social workers and managers tender justifications and disclaimers.

A fish out of water?

Thus, through the fieldwork experience, I became aware of the pervasive and unquestioned nature of the notion that children are 'made not born'. This does not mean that I am asserting that the discourse of child centred-ness is wrong. However, once one has become aware of it, one develops in response a critical control over one's thinking. Moreover, some components of 'sense-making' become more explicit and hence are opened up for debate. For example, it became increasingly clear to me that, although formal knowledge (e.g. developmental milestones, attachment theory, immunization status, medical, forensic and psychological opinion) is palpably displayed in social workers' forms of talk and written records, many narratives have a transparently qualitative, evaluative and profoundly moral design. Indeed, rational-technical or evidential materials are often invoked to authorize *moral* judgements. So, a mother may be 'blamed' for being 'emotionally unavailable' to her infant and hence for failing to 'promote a healthy attachment'. In short, it became clear to me that theory and even apparently 'forensic' evidence could sometimes be invoked to provide *ex post facto* a normative warrant for decisions taken on other grounds.

Once routinized forms of thought have been destabilized in this way, it becomes extraordinarily difficult to continue to think as usual. Towards the end of my fieldwork (and of my career as a team manager), I became increasingly conscious of a dialogue between myself as researcher and myself as social worker. As the research progressed, I became more and more self-conscious about this, which was a rather strange and destabilizing experience. However, rather than this being a bad thing, it opened up to question my taken-for-granted presuppositions. Practice is inevitably remoralized, and rendered more contestable and debatable as a result of the epistemological and ontological shift (cf. Giddens, 1984). I have often been asked whether this destabilization was the catalyst for my departure from practice into an academic post. It may well have been, since the [p. 112 ↓] published results opened up that possibility. However, it was and is perfectly possible, if not always comfortable, to continue to act, and also to 'see' oneself acting. Academics, too, have their cherished discourses about pedagogy, student assessment and research which can themselves be problematized. Exam boards are as fruitful a source of rhetoric and moral accounting as any allocation meeting! There is no escape. So, yes, defamiliarization can be hard work, but it is worth it. As I shall conclude below, it is worth it because it offers the possibility of more realistic realism about professional judgement and hence of more robust ethical debate.

Conclusion: Towards a realistic practice ethics

One may realize that something, which seems inevitable in the present state of things, was not inevitable, and yet is not thereby a bad thing. But most people who use the social construction idea enthusiastically want to criticize, change or destroy some X that they dislike in the established order of things. (Hacking, 1999: 7)

As Hacking notes above, the kind of analysis I have undertaken in this chapter can easily be read as a criticism of social workers' understandings and practices. The idea of normative critique is so embedded in our modern, reforming consciousness that it is almost impossible to study and describe anything without being accused of wanting to

change or destroy it. That has not been my intention. For example, it remains a *material* fact that children's bodies are damaged by their parents, and it is social workers and other child care professionals who are charged with the task of dealing with these situations. It is an occupation dealing with life *in extremis*, and this is reflected in the forms of thought, in particular the quest for certainty in assessment. Moreover, social workers have room only for invention within limits. Their activities and professional mandates are heavily circumscribed by statute.

However, 'grounding' professional activity in the very material, embodied/acis of child abuse and neglect and in the prescriptions and proscriptions of policy does not have to lead back into an acceptance of a linear relationship between theory, policy and research and professional practice. I have tried to show here that social work practice depends on a variety of 'rationalities'. For example, in relation to causation or risk, 'objective' or 'forensic' criteria are vitally important, but social workers must also make judgements about the veracity and moral adequacy of particular accounts, and about the creditworthiness and blameworthiness of various parties, including the other professionals. Obviously, these judgements are not arbitrary, but neither are they neutral. Moreover, the grounds for the judgements are rarely fully self-conscious. They are influenced by taken-for-granted tacit presuppositions which are socially and historically constituted [p. 113 ↓] and often intrinsically moral. For example, social workers may find a mother's account of her reasons for 'not knowing' that her child was being sexually abused either morally adequate, or in some way inadequate based on common-sense criteria such as consistency of the story over time, and by making judgements (and that is what they are) about whether she is in other ways a 'good mother' (as currently understood).

I am not suggesting that these forms of sense-making are inferior, bad and must be dispensed with. Rather, they are essential and unavoidable in many, or dare I say all, health and welfare occupations (for examples from other professions see, Atkinson, 1995a; John, 1990; Latimer, 1997; Marks, 1995; Stancombe and White, 1997; Taylor and White, 2000). They become problematic if they are treated as though they are a detached and neutral 'mirror' of reality. Social work is a practical moral activity and, as such, its judgements and 'rationalities' need to be explored and debated. This kind of analytic rigour, paradoxically, is spawned by a recognition of undecidability and

indeterminacy. Decisions should be warranted not by sustaining the myth of certainty, but by looking at the problem of judgement for what it is, and opening it up for debate.

On this note, I am struck by the relevance of a debate between Rorty and Derrida on the subject of judgement. I have sympathy for Derrida's position when he states:

... whatever choice I might make, I cannot say with good conscience that I have assumed my responsibilities ... If I conduct myself particularly well with regard to someone, I know that it is to the detriment of another ... And this is why undecidability is not a moment to be traversed and overcome.... (Derrida, 1996: 86–7)

However, against this, Rorty argues that the only criterion needed for the justification of action is the prevention of cruelty, and that Derrida unnecessarily complicates the issue:

Derrideans tend to think that the more questioning, problematizing and *mettant-en-abime* you can squeeze into the day's work, the better. Deweyans, on the other hand, think that you should only question when you find yourself in what Dewey called a 'problematic situation' - a situation in which you are no longer sure of what you are doing. You may not be sure what you want, or you may not be sure that your old tools are the best way of getting what you want, or your perplexity may involve both kinds of uncertainty at once. But unless you suffer from some such uncertainty, you should save problematizing for the weekends. (Rorty, 1996: 44)

Social workers and their managers need to be helped to steer a course between Derrida and Rorty. Practitioners do not have time for the kind of self-indulgent epistemologizing beloved by Derrida. They resolutely must continue to judge and to act upon their judgements. However, where [p. 114 ↓] Derrida falters in undecidability, Rorty strides forth a little too sure-footedly. Rorty refuses to acknowledge the critical potential of the work of the likes of Foucault and Derrida, but it is not always clear whether something is broken until one has deconstructed it and made it problematic (Critchley, 1996). What is needed is an approach to practice, which is at one and the same time, problematized and 'doable'.

The acknowledgement of uncertainty does not lead inexorably into a descending vortex of relativism. We do not need to maintain the simplistic dichotomy between realist and relativist approaches. For example, Bruno Latour advocates a more 'realistic realism' (Latour, 1999: 15) (which he also calls 'sturdy relativism' - Latour, 1999: 4) which acknowledges that we can be relatively sure about quite a few things, but that we still need other sorts of judgement. The acknowledgement of the complexity of social workers' different ways on knowing makes reflexive and analytic practice more not less important. In the absence of algorithmic methods to help us resolve uncertain situations, we must think very carefully about what we do.

Foucault points to the need for agents to build an ethics based on an understanding of the socially and historically constituted nature of their knowledges:

People have to build their own ethics, taking as a point of departure the historical analysis, sociological analysis, and so on that one can provide for them. I don't think that people who try to decipher the truth should have to provide ethical principles or practical advice at the same moment, in the same book and the same analysis. All this prescriptive network has to be elaborated and transformed by people themselves. (Foucault, 1994: 132)

They cannot do this, however, whilst their presuppositions and shortcuts remain taken-for-granted. By using detailed ethnographic data as part of a dialogical model of applied social science, social workers can be helped to see the problem of versions and hence to become more reflexive, analytic and systematic in their sense-making activities. By attending to *how work gets done*, rather than to how it *should* be done, ethnographic data can form the basis for fruitful dialogue between research and practice.

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[p. 115 ↓]

Notes

1. My reluctance to deconstruct or disavow the ethnographic claims I make in this chapter does not mean that I consider them to be infallible. Rather, because all such claims are constructed and inevitably interpreted we may as well get on with making them, so that people, if they so wish, may argue with us (cf. Latour, 1988; Law, 1994). However, this is not the place for a detailed analysis of my own ethnographic findings (see, for more detail, White, 1997b, 1998a, 1998b, 1999b), nor is there space here for a systematic consideration of the important issue of validity. For a wide ranging discussion on validity in ethnography see, *inter alia*, Hammersley, 1992 and in social work, *inter alia*, Shaw, 1999a.

2. The general claim I have made is supported by many different sources and types of data (e.g. documentary sources, interview transcripts and tape recordings of naturally occurring talk). This is not the place for me to provide analyses of these sources. I am using this exemplar to illustrate a general point about reflexivity. You may of course contest my reading of this extract, and that is precisely why recorded talk is so valuable.

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