

## Section 3. Method and methodology

### Ruth Pinder and Anne McKee

#### The design of the study

We conducted a questionnaire survey of London course organisers to establish what types of case discussion groups (if any) that they ran. We subsequently visited four Balint and four non-Balint groups as previously reported.<sup>24</sup> From these, we selected two VTS groups that demonstrated good small-group work practice. One ran a formal Balint group, and the other a regular small group with defined aims that was not a Balint group. They are referred to respectively as 'Highville' and 'Jamestown'. We decided that our main focus should be an in-depth exploration of the Balint group, Highville, and a detailed but less extensive exploration of Jamestown. The rationale for this is given below (see 'The control and comparison question' on p. 6).

Six three-hour observations of the group were conducted at Highville. Jamestown was observed twice over a period of six months. Semi-structured interviews with 13 group participants (ten from Highville and three from Jamestown) were held. Discussions were also held with the course organisers of each group.

The study group of doctors selected for interview represented the range of group participants in terms of age, gender, ethnicity, experience of training and of Balint or small-group work. There were few male doctors in the groups, so the study group reflected this imbalance, consisting of three men and ten women. Two doctors were hospital Senior House Officers (SHOs) and 11 others were GP registrars. The average age was 30.7. Four had been in the groups for less than six months, whereas one had been a group member for six to nine months and eight had been in the group for nine months or more. Reflecting the ethnic diversity of the locality, participants came from Nigeria, Hong Kong, France, Namibia, New Zealand, South Africa and Vietnam. Five were British born (three from the Jamestown group and two at Highville).

A key strategy was to interview the doctor presenting a case for discussion in the sessions observed at each group. The questions that formed the basis for the subsequent interviews were as follows:

- What happens in groups where Balint work and purposeful non-Balint work is introduced?
- What did participants feel they gained from it? How did it gel (or not) with previous experience? How might they build on it?
- Given that no tradition can accomplish everything, what learning opportunities were offered by the group experience and what were closed off?
- How did the doctors in the groups understand, accept, transform – or contest – the learning process offered? Was Balint being subtly transformed in the process? If so, how?

- What compromises, if any, did course organisers have to make to get it to 'take'?
- What lessons might the experience have for other course organisers wishing to follow in their footsteps? What mistakes might be avoided and what aspects capitalised upon?

With the doctors' consent, interviews were taped and transcribed. The observations and interviews were written up as case studies and negotiated with participants to establish their fairness, accuracy and relevance. All the observations and interviews were carried out by RP. The study also included interviews by RP with five experienced, Balint-trained GPs and two psychotherapists/educational consultants in general practice. Observations were also conducted at three Balint leaders' workshops, and a demonstration Balint group at the British Society of Psychotherapists. The annual Balint dinner, Oxford Conference, and Memorial Lecture were also attended. The observations and interviews were carried out between March 2002 and July 2003.

Analysis (which we also explain in more detail below) was an iterative process. This involves weaving to and fro between the literature, the data and emerging concepts in the search for patterns and regularities, and, as importantly, for instances that did *not* fit. The ethnographer learns *as* he or she goes, not *before* he or she goes, allowing emerging understandings to broaden and deepen the research process itself.<sup>25</sup> It broadly followed grounded-theory procedures,<sup>26</sup> but was not rigidly confined to them. Field notes, and an ongoing analytic record, were kept throughout the research process.

#### Ethical considerations

Early in the planning phase of the project, we sought advice from the Chair of the Camden and Islington Local Research Ethics Committee. Her advice was that formal ethics committee consent would not be required, as patients were not directly involved. Nonetheless we ensured that fully informed consent was obtained from all participants and confidentiality maintained by using pseudonyms to identify both the groups and their members. We concealed the identity of the cases discussed by removing or altering all identifying material.

Prior to each group visit, the course organiser was asked to obtain the group's consent to the visit, which was to be part of a research project to be published. An information sheet detailing the project and a consent form were given to each participant at the beginning of the visit. The record of each case discussion was submitted to the doctor who had presented the case in order to confirm its accuracy and make any necessary amendments. Every effort was made to ensure accuracy and to confirm permission to publish the material.

Prior to publication we again wrote to participants seeking their confirmation that they were happy with how they may have been portrayed in the paper and again sought their consent for publication. Apart from one or two participants who we were unable to trace, all have agreed to publication.

The verbatim speech was our raw data but in the interests of readability we've omitted the 'um's' and 'er's' and the obvious grammatical errors of normal speech, as well as making minor amendments where meanings were obtuse. In one or two places we have substituted more appropriate terminology where the original could have inadvertently caused offence to readers. Otherwise we have been very careful to preserve the integrity of what the doctors have said.

### Methodological issues

Ethnography has its roots in anthropological inquiry. It has been applied particularly in developing countries, where the ethnographer lives and works with the people to be studied, learning their language and mores. Of course nothing so ambitious could be attempted here. But the close, textured observation of what people say and do is an ethnographer's raw data. It takes what people say seriously – in fact very seriously – but not literally. The question is not simply always 'What are the facts?' but 'How can we think about the facts in a way that is illuminating?'

Ethnography is as much a way of seeing as the application of any set of techniques. Arguably it is a more fluid and intensive form of qualitative work than the survey-plus-single-interview formula currently favoured in health services research. As an open-ended pluralistic approach to inquiry, ethnography aims to capture both the sense and the sensibility of things. It is oriented towards cultural interpretation and the exploration of cultural patterns. In particular it is concerned to uncover meanings as they are articulated from the inside looking out, searching out the *shoulds* and the *oughts*, the ideals and the realities of life.<sup>27</sup> It asks not simply what the problems and possibilities are, but why any difficulties persist; not only what is going on here, but how did things get to be the way they are? This means questioning what is self-evident, a process that almost always involves learning to ask more nuanced questions. It cannot be assumed *a priori* that the questions that initially frame a project are read in the same way by those being studied.<sup>28</sup>

### The control and comparison question

The difficulties of moving between description, interpretation and analysis quickly became apparent in the team's discussions of the qualitative experimental method that was first proposed. Working out why we decided to jettison it, and view the non-Balint group as a satellite case study site – another lens with which to explore small-group work – was far from straightforward. If we wanted to understand Balint and its contribution to small-group work, Balint did not need another experimental field site to explain itself. Ethnographic work scores in depth not breadth. With limited time available, the mandate to study two different sites was likely to

diminish the attention available for each. Crucially, treating Balint as a 'variable' that was present in one group but not the other might well mean reducing qualitative work to investigating relationships already imagined. The results would be circular, part of the research problem, not part of its solution. Our job was to question the frame, not remain within it.

Ethnographic comparison is a different matter. The ethnographer is the research instrument *par excellence*, ethnography the analysis of what emerges from bringing different understandings to bear on a problem.<sup>29,30</sup> It is a matter of continual comparison and contrast. So we found it was the dialogue between us on the Steering Committee as much as the encounter with the VTS groups studied that brought issues into sharp relief. The difficulties of take-up referred to in our introduction could not be divorced from an understanding of what was being taught in Balint groups, and how Balint training was being conceived and transmitted.

### Reflection and reflexivity

While all research tries to be as objective and rigorous as possible in data collection, analysis and reporting, an interpretive approach argues that we cannot escape the social world in order to study it. All observation is theory-laden; there is no such thing as a no-view view.<sup>31</sup> Ethnographic inquiry is an iterative process that tacks to and fro between data collection, emerging concepts, theory and more data, involving constant self-exploration, comparison, reflection and interrogation. Its power and rigour lie in its reflexivity as much as its techniques. It is more demanding of the researcher in terms of integrity, courage and self-awareness than any survey or questionnaire.<sup>7,32</sup>

With a background in sociology and anthropology, RP is trained to explore the social in the personal, the personal in the social, to see connections between phenomena, continuity as well as change. Mentally she kept breaking the rules, straying across boundaries that were supposed to be distinct, and, contrary-wise, seeing distinctions where none were intended. For example, it was impossible to see the doctor-patient relationship as bracketed off from the web of other relationships that framed the consultation.<sup>4,33</sup> This may have led to an underestimate of the degree of difference between Balint and non-Balint, a bias with which RP continually wrestled. However, rather than trying to smooth the bias away or pretend it didn't exist, such reactivity is integral to the ethnographic process.<sup>31</sup> It was the good bias that gave us the edge to see. The study would have been the poorer without it.

### Research as conversation

We suggest that what initially seemed to be a weakness became a strength: namely the interplay of two different perspectives, (loosely) one from the sciences, the other from the arts. They mirror the process of Balint groups – a process that attempts to analyse scientifically the emotional ('artistic') aspects of general practice consultations. Each perspective rests on a different vocabulary (and we exaggerate the differences to make

the point). The scientific (or literal) sees the world in terms of discrete objects that can be isolated and examined, and their attributes precisely identified. It tests a specific hypothesis, or set of hypotheses. It is systematic, verifiable, experimental and universal. This allows general laws to be formed and predictions made about behaviour, independent of context. By contrast, the artistic (or metaphorical) perspective sees the interconnectedness of things. It is intuitive, emergent, relative and particular. It argues that the object of inquiry is always altered by context. Hence attempts to categorise objects with precision are not seen to be of much help in comprehending events. It *ends* with a hypothesis, or set of hypotheses, rather than beginning with one.

At best, *both* approaches set out to uncover and explain, not to think in lists, but to think conceptually. In many ways science and art are essentially comparable. However, the scientific method expects to find truth behind the final screen; the artistic perspective suggests that 'behind each screen lies another concealment'.<sup>34</sup> Bringing the two perspectives together had implications for the research right down the line: from formulating and reformulating the research question, to the development of case studies, and hence to the move between descriptive and conceptual thinking using some insights from narrative theory. Perhaps issues concerning impartiality, emotional commitment and ownership strengthened the differences in research perspective.

The GP researchers on the team were, and are, advocates and practitioners of Balint's approach. Their challenge was to discipline their advocacy and rethink their assumptions as part of the research. For the ethnographic researchers, the task was to allow their approach to accommodate to the context in which it occurred and the audience to which it was to be reported. Methodological compromises such as that described with the satellite case study were made. More arduous was a shift in writing style; we discovered that finding simpler ways to convey complex concepts is not about oversimplifying them. Between us we discovered much about how long it takes for things to say what they mean to us.

### *Case studies*

With these issues in mind, it was logical to use case studies as a way of exploring how individual doctors respond to Balint training. The approach has a long tradition in social anthropology and was, after all, richly employed by Balint himself.<sup>2</sup> As a teaching and research tool, case studies are now emerging as a distinct method in education, organisational research and evaluation.<sup>35,36</sup> Drawing on cases as they present themselves in the field (a single person, a group or a critical incident), the approach focuses on the perceptions and meanings of people's actions rather than on their quantification.

No method is perfect. Case studies can never traverse the terrain or tap the range of variables. Moreover, there is always the danger that they attribute excessive significance to limited segments of data. At their best, however, they may powerfully illuminate the area of study, thus unsettling assumptions, pointing to flaws in the claims made of populations, or revealing subtleties

hitherto off the map.<sup>28,36,37</sup> In particular, case studies may challenge the supposed natural order of things. Standing outside one's own conceptual system is always difficult, but they have the power to make those opaque structures visible, illuminating the contours of the goldfish bowl we all inhabit.

Thus this ethnographic approach is necessarily both different from and similar to that of Balint-oriented research. The same frame of reference would compromise creativity and risk merely reproducing what is already well-known. We hope this new approach is familiar enough to be recognisable but sufficiently fresh to encourage imaginative thinking. Inevitably it is but one of many possible interpretations, and the understandings reached are necessarily partial and contingent. Neither can they be legitimated by faultless technique or precise calculation. Rather it is an invitation for doctors to test an outsider's views against Balint's insider experiences: to look again at what goes without saying.

After all 'the objective of . . . collaboration in the study of the human condition is to achieve not unanimity, but more consciousness, and more consciousness always implies more diversity'.<sup>38</sup>

### *Linking practice to theory*

The question of generalisability remains troublesome for case study research.

Inferences cannot be tested statistically here. Nor can cases predict, or derive, universal laws independent of time and context in the way relied upon in experimental design. However, the possibilities for transferable learning would be bleak if nothing of wider significance could be said. If they are to be of value, case studies need to link experience to context. How else to counter the sceptic's 'So what'? Whilst the case may in some respects be unique, we can still learn from it in much the same way that Hamlet teaches us about anxiety or Macbeth about guilt. The difficulty is in knowing the degree to which theoretical generalisation is possible.

Theories tend to get above themselves; they come and go, perhaps nowhere more so than in psychoanalysis. Understanding what was '*too much*' theory and what was '*not enough*' only emerged as the research progressed. For example, in the various groups attended, it was apparent that there were subtle differences in the extent to which group leaders made psychoanalytical theory explicit. This made RP's earlier efforts to draw on contemporary psychoanalytic theories as analytical tools problematic. It was only later that we found that we all agreed that we liked stories. The case studies in the groups took narrative form, as did the process of inviting doctors to reflect on their experiences during interview. So stories it became. It was an empirical grounding: a theory with a small rather than a large 't'.

### **The turn to narrative**

*Culture is . . . an ensemble of stories we tell ourselves.*<sup>39</sup>

One of the best ways to understand others is by telling and re-telling stories. The tale well told gives us the capacity to imagine ourselves anew, and to revisit experience with fresh eyes. The stuff of narratives lies in

the gap between what ought to be and what is, how these dissonances are read and variously dealt with. Hovering between realism and fantasy, they have the power to reveal deep truths about ourselves and the times we live in.<sup>40,41,42</sup>

Stories about what are too easily called emotions are simultaneously stories about society. Even as they address individual uniqueness, the cornerstone of Balint teaching, narratives are encoded deep in the cultural values of a particular time and place. They make sense of experience because they already produce it in culturally specific ways, anticipating as much as making sense of action already taken. They are therefore conservative as well as liberating. Stripped of such context, however, narratives may lose depth, and become simply biographical solutions to systemic problems: the finger points – in one direction only.

Hence the emphasis here is not on whether the stories doctors tell about themselves and their patients unravel the inner self. Ultimately the self remains unknowable. Rather, the following case studies focus on which stories mattered, which were displaced, and how well publicly shared narratives in the groups dovetail with doctors' private accounts of themselves. They give an entrée to the way that narratives, and the feelings they engender, are not simply discovered, but created and performed, allowing doctors to explain themselves to themselves and to others in plausible ways. As miniature social dramas,<sup>43</sup> the stories told both shaped and reflected key social values within medicine, producing order out of the disorder of everyday experience.