Ethnography as Practice: Is Validity an Issue?

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Abstract

This article addresses current debates on the validity of qualitative research by conceptualising ethnography as a form of social practice. Ethnographic research in criminal justice is described as a cross-field practice, where researchers move from an academic field to the field of observation. It is argued that validity issues arise as a result of the material structures and methodological conventions that shape the practice of ethnographic research. Using three examples from criminal justice research that the author took part in, this article reaffirms the usefulness of triangulation and concludes that validity is not simply a methodological, but also an ethical, issue in criminal justice research practice.

Introduction: Validity of ethnographic research

Since the 1980s there have been various waves of disputes among researchers (the 'paradigm wars': see Denzin 2010) regarding how social research should be conducted. One central controversy among combatants in these 'wars' has been over the issue of validity — whether validity is enhanced through triangulation, whether validity can be measured, and whether validity should be a concern at all. While the validity of qualitative research is not a new issue, it is not a topic that is often openly discussed in criminology and criminal justice. This article aims to examine issues of validity that have been central to judging the credibility of social research by focusing on the practice of ethnography[1] in criminal justice.

[1] Ethnography in this article covers a range of research techniques, including participant observation, the use of audio or video recording, formal and informal interviews of groups or individuals, and examination of artefacts and documentary evidence in the course of fieldwork. Ethnography is the general research approach, while participant observation is a data collection technique, and fieldwork is the location of data collection (Delamont 2007).

The dominance of positivist and post-positivist methods in criminological research has, even to this day, set up a false assumption that the validity of quantitative research is more readily achieved and demonstrated than that of qualitative research. It is instructive to compare the situation of criminal justice research with that of health research, where qualitative researchers must conform to the format of the dominant paradigm of quantitative research. In a review article, Kitto, Chesters and Grbich (2008) outline a set of criteria for authors and assessors for submitting and assessing qualitative research articles for the *Medical Journal of Australia*. After contrasting the aims and methods of qualitative and quantitative research, the authors suggest that:

> The conventional methodological criteria of quantitative research — validity, reliability and empirical generalisability — are generally not directly applied to qualitative research because of the different frameworks, sampling approaches, size of sample and goals of qualitative research. Instead, terms such as *rigor* (thoroughness and appropriateness of the use of research methods), *credibility* (meaningful, well presented findings) and *relevance* (utility of findings) are used to judge the quality or 'trustworthiness' of a study … (Kitto et al 2008:243).

The article then presents a set of criteria for assessing qualitative research, including clarification and justification, procedural rigour, representativeness, interpretive rigour, reflexivity and evaluative rigour, and transferability. Such an approach has the advantages of allowing qualitative researchers to be accorded a place in a field (such as health research) that is dominated by quantitative methods, and providing the tools for qualitative researchers to demonstrate the credibility and rigour of their research to policy makers (who are naturally wary of 'soft' research data). However, some would regard these guidelines as too restrictive and overly 'scientistic', thus confirming the subordinate position of qualitative researchers in the field.

Denzin, Lincoln and Giardina, long-time practitioners and analysts of qualitative research methods, have rejected an international trend for governments 'to regulate scientific inquiry by defining what counts as "good" science', which they see as raising 'fundamental, philosophical epistemological, political and pedagogical issues for scholarship and freedom of speech in the academy' (2006:769–70). Yet the issue of validity is of crucial importance to criminological research, especially when it relates to policy or political actions:

> Validity cannot be dismissed simply because it points to a question that has to be answered in one way or another: Are these findings sufficiently authentic (isomorphic to some reality, trustworthy, related to the way others construct their social worlds) that I may trust myself in acting on their implications? More to the point, would I feel sufficiently secure about these findings to construct social policy or legislation based on them? (Guba et al 2011:120)

Though there are controversies regarding how validity can be judged and whether such judgments are inextricably bound to research paradigms, Guba and Lincoln's 'authenticity criteria' are a good starting point for discussing validity. These include 'fairness, ontological authenticity, educative authenticity, catalytic authenticity, and tactical authenticity' (Guba and Lincoln 1989:245–51, quoted in Guba et al 2011:122). Briefly, *fairness* is about balance in the sense of being inclusive, and avoiding the marginalisation of certain voices; *ontological and educative authenticity* are related to a 'raised level of awareness' among research participants and others and reflect a 'capacity to engage in moral critique' (2011:122); and *catalytic and tactical authenticity* reflect the ability of the research to lead to action by participants or involvement of researchers in training participants in social or political action. Other forms of validity discussed include 'transgressive validities' (that seek to 'rupture' and problematise the discourse of validity by using experimental writing)

and 'validity as ethical relationship' (2011:123). Lincoln (1995; quoted in Guba et al 2011) extracted seven 'new standards' for judging research quality:

> positionality, or standpoint, judgments; specific discourse communities and research sites as arbiters of quality; voice, or the extent to which a text has the quality of polyvocality; critical subjectivity (or what might be termed intense self-reflexivity …); reciprocity, or the extent to which the research relationship becomes reciprocal rather than hierarchical; sacredness, or the profound regard for how science can (and does) contribute to human flourishing; and sharing of the perquisites of privilege that accrue to our positions as academics with university positions (Guba et al 2011:123).

The political and ethical dimensions of validity are especially significant for criminal justice researchers as knowledge produced by researchers has potentially real consequences for criminal justice practices and those subject to such practices. For those who are realistic and pragmatic about policy-relevant research, the 'criteria for quality include the "truth value" and plausibility of findings; credibility, impartiality, and independence of judgment; confirmability, consistency, and dependability of data; and explainable inconsistencies or instabilities' (Patton 2002:93). Committed, critical researchers who value reflexivity, reciprocity and giving voice to those marginalised by society are nevertheless mindful of 'validity' issues, by checking the 'accuracy of field observation and the veracity of ethnographic interviews' through the triangulation of sources (Maher 1997:215).

This article seeks to cut through current debates on validity of qualitative research by conceptualising ethnography as a form of social practice. Instead of regarding differences in research methodologies as fundamental divisions in philosophical and political terms, such divisions are seen as reflecting variations in practice by researchers located in different fields and socialised in different disciplines. Ethnographic research into criminal justice institutions is conceptualised as a type of cross-field practice, where researchers move from an academic field to the field of observation. To make sense of what they observe in the field, researchers draw on their academic knowledge and conventions to construct piece by piece the structure, process, action and meaning of the type of practice they observe.

It is argued that validity issues arise as a result of the material structures and methodological conventions that shape the practice of ethnographic research. To illustrate this point, the article identifies and discusses the validity issues in two of Manning's (2009) models of ethnographic research (the consensual and the transferential). Using three examples from criminal justice research that I took part in — a study of media practices in reporting crime and deviance in the 1980s, a study of police practices in the 1990s, and a study of restorative justice practices in recent years — the article reaffirms the usefulness of triangulation as a method of enhancing the credibility and authenticity of research findings. The article concludes that validity is not simply a methodological, but also an ethical, issue in criminal justice research practice.

Ethnography: Research practice and researching practice

The notion of practice — 'an organised constellation of different people's activities' — has been discussed by social theorists such as Bourdieu and Giddens (Schatzki 2012:13). I have made use of Bourdieu's framework (see Bourdieu and Wacquant 1992) for theorising police practice (Chan 1997; Chan et al 2003) and criminological practice (Chan 1996; Chan 2000). The basic idea is that all social actors operate in a *field* of practice (cf Schatzki's 'material arrangements':2012:16). The field is a space of competition where there are various types of

capital or resources that are valued and sought after. Actors operating in a field are motivated (consciously and unconsciously) to maximise capital, be it social capital (in the form of network), economic capital (in the form of income or wealth), or symbolic capital (in the form of prestige or influence). They also share certain dispositions (Bourdieu's *habitus* or Giddens' *practical consciousness*) that subtly integrate the necessity of the field; these dispositions include taken-for-granted assumptions, established ways of thinking and acting, values, and even physical characters (bodily hexis). Social practice is thus a product of the habitus interacting with a field. Schatzki postulates five types of relation between arrangements and practices: 'causality, prefiguration, constitution, intentionality, and intelligibility' (2012:16).

Ethnographic researchers interested in understanding criminal justice practices operate in two fields: the *academic (or research) field* and, through their fieldwork, also the *field of observation* (for example, criminal justice institutions such as police, courts, prisons, and sites of 'informal justice'). Researchers are usually located in an organisational field such as academia or government agencies. In these organisations they hold various types of capital (according to their rank, reputation and academic achievements). Ethnography is a subfield in the field of criminal justice research. Historically it has occupied a relatively less powerful position in criminology, given the dominance of scientific models of criminological research. There have been very few researchers who adopt this style of research because of a combination of reasons: not many researchers have been trained to do ethnographic research; it is not easy to gain access to criminal justice organisations to carry out observation work; ethnography can be hard work, demanding a great deal of energy and skills from researchers; ethnographic research generates results that are often regarded as 'anecdotal' and thus not 'representative' or 'reliable' by policymakers.

Ethnographic researchers who study criminal justice practices move between their positions in an academic[2] field of research and the field of criminal justice. They draw on academic knowledge and conventions, including ethnographic research techniques, to make sense of the criminal justice field and construct piece by piece the 'arrangements', rules of the games, tacit and explicit knowledge, actions and discourses, and values operating among criminal justice practitioners. But ethnographic researchers are not alone in creating knowledge. Through observing, interacting and questioning criminal justice actors and interpreting what they observe, they often co-create knowledge with criminal justice actors that they study. Though temporarily operating in two fields, ethnographic researchers are primarily players in the academic field. (Of course, researchers may participate in the practices of other fields. For example, those interested in real-world outcomes often take part in a policy or political field devoted to the causes of reforming systems or redressing injustices.) The validity (credibility, authenticity) of ethnographic research findings is limited in a number of ways. Structurally, researchers do not usually have full access to the criminal justice field they are studying; nor are they players in that field in terms of their aspirations and motivations. This is part of the material constraints of doing ethnographic work as an observer. Culturally, researchers' practices are primarily driven by their desire to conduct meaningful inquiries which are governed by academic conventions consistent with their theoretical/disciplinary orientation with its theory of knowledge, methods, and judgments of validity. While they will try to understand and empathise with the values and

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2   Researchers who are not employed by academic institutions are in a different position, with different types of capital and habitus operating, even though they would abide by similar research conventions as academic researchers. It is probably not a coincidence that ethnographic techniques are rarely used by researchers employed in government departments.

aspirations of actors they observe in the field, as researchers their job is to report the findings of their inquiries from a researcher's perspective.

As a cross-field practice, ethnographic researchers can vary in the degree of their immersion in and identification with the field of observation: they can range from being completely detached observers to totally committed participants. Two of Philip Manning's (2009) three models[3] of ethnography illustrate the validity issues related to the more conventional 'consensus model' exemplified by Mitchell Duneier's (1999) study of street vendors in New York City and the 'transferential model' found in Loic Wacquant's (2004) study of pugilists. The transferential model is an extension of the consensus model, but the two models differ in the nature of cross-field practice and the maintenance of insider/outsider boundaries. The *consensus model* of ethnography involves three important elements:

> An often tacit acceptance of the broad assumptions of symbolic interactionism as set out by Blumer (focusing on the shared meanings of objects), a set of data collection techniques based on long-term observations, as systematized by Hughes and Junker, and, lastly, an acceptance of the role of ethnographer as 'observer as participant,' that is, as an outsider with detailed knowledge of a group's practices (Manning 2009:763).

In this model of ethnography, the researcher is firmly operating from the academic field, with the observation work guided by the academic habitus, and maintains a clear identity as an outsider. A strong criticism of this model is that, as an outsider, the ethnographer cannot full understand the practices of the people being observed, nor does he or she have access to the 'full range of backstage activities' (Manning 2009:766; see debate between Wacquant and Duneier). This limitation suggests that the results of research may not be fully credible or relevant, especially when applied to policy formulation.

In contrast to the consensus model, the *transferential*[4] *model* of ethnography involves complete immersion in the field and a high degree of reflexivity on the part of the researcher. Wacquant (2004) was personally 'embroiled' in the boxer world he studied in order to understand the habitus of the pugilist through his own experience:

> This required Wacquant to submit his life, body, and psyche to the contingencies of the everyday world of the boxer in order to become his own informant. Practically, this put him into the position of having 'backstage' knowledge (Goffman, 1959) of the hidden aspects of the boxing world. Physically and emotionally, this meant that he experienced for himself the pain, ambivalence, regrets, and celebrations of the boxers themselves (Manning 2009:757).

Manning regards Wacquant's ethnography as both powerful and risky. There is real difficulty in experiencing the world as an insider while maintaining 'the detachment of an outsider, who can stand back, explore, and challenge the legitimacy, even the authenticity of that experience' (Manning 2009:775). The need to 'stand back' may limit the research process and, by extension, the validity of the results, yet a failure to 'stand back' would put at risk the viability of the ethnographic research project.

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[3] Manning also discusses a third, 'comparative' model of ethnography as exemplified by Erving Goffman's (1961) study of total institutions. This model is omitted in this discussion as it differs from the consensus model only in terms of its purpose — what Manning calls an 'ethnography of concepts' as opposed to an 'ethnography of places' (2009:767).

[4] Manning (2009:771–2) describes in detail three versions of the psychoanalytic term 'transference' (referring to Prager 1998): (a) the conventional definition of transference, where a patient develops strong sexual or hostile feelings towards his or her analyst, (b) a sociological definition that is similar to Bourdieu's notion of habitus, and (c) the psychoanalytic concept of 'transference neurosis', where the feeling of attachment to the analytic relation becomes so intense that all other aspects of the patient's life pale into insignificance.

The validity issues that arise in both models are very similar and have to do with how researchers negotiate their position and disposition as players in the academic field while being immersed in the field of observation. As long as they are committed to their identity as researchers, they need to come to terms with whether and how their research would be judged in the academic field as trustworthy, credible or authentic. The academic field is, of course, not homogeneous, so a researcher's academic habitus would reflect the disciplinary and theoretical position (see Brubaker 1993) with which the researcher identifies. Nevertheless, as argued earlier, validity issues cannot be dismissed, regardless of disciplinary or theoretical paradigms. How to address these issues is something I wish to focus on in the rest of this article.

Practising ethnography

In this section I will discuss how validity issues in ethnography can be addressed through a more considered approach to research design. Using three examples from criminal justice research I took part in, I will explain how the use of multiple methods and multiple sources of data can improve the trustworthiness and authenticity of ethnographic accounts. The research projects are (a) a study of media practices in reporting crime and deviance (Ericson et al 1987, 1989, 1991), (b) a longitudinal study of police socialisation and police practices (Chan et al 2003; Chan 2007), and (c) a study of restorative justice practices (Chan et al 2011). In each of the projects, in addition to participant observation, a range of research methods and techniques was employed, including quantitative survey of media content (Ericson et al 1991), interviews with news sources (Ericson et al 1989), multi-waves of questionnaire survey of police recruits and interviews with police academy staff and field training officers (Chan et al 2003; Chan and Doran 2009), and archival analysis of victim offender conferences and interviews with conference participants and conference facilitators (Chan et al 2011). Given the limit of space, the full details of each study will not be discussed (except for the restorative justice study which has not yet been published). Readers are invited to go back to the original publications for further information. In each of the three examples, only one validity issue is selected for discussion, although there may be numerous others that have arisen, given the complexity of the projects.

Researching media practices

The main aim of this study was to understand the role of the mass media in defining and shaping public knowledge of crime and deviance. The ethnographic fieldwork component involved 'more than 200 researcher days in the field, and a total of about 2500 hours including the preparation of research notes' carried out in 1982 and 1983 in Toronto, Canada (Ericson et al 1987:86). Each of the three authors spent about six months in the field observing mainly journalists from the *Globe and Mail*, a broadsheet newspaper, and CBLT, the Toronto television channel of the Canadian Broadcasting Corporation ('CBC'), and also (less intensively) newspaper, television and radio reporters from other news organisations. Fieldworkers approached journalists for permission to follow their story's production from assignment to production (Ericson et al 1987:86–7). Assignment editors were also systematically observed and interviewed. Detailed field notes were written at the conclusion of fieldwork (more than 2000 pages of field notes were analysed).

The validity issue I would like to focus on relates to the material constraints of the observer and how these may affect the credibility and authenticity of the results. As outsiders to the media organisations, it was inevitable that we were not always welcome or

that we did not always have full access to all relevant information or events relating to the construction of the story we were observing. For example, journalists were concerned about the impact of our observation on their work process and how the observation findings would be used. One newspaper editor refused to participate in the study, thus excluding us from being present in stories that involved him. We were excluded in other situations, such as where there were sensitive issues or anonymous sources involved in a story and the journalist did not wish to have an observer present. Once we became aware of the informal 'rules' being applied, we would occasionally exclude ourselves from highly sensitive or personal situations without being told. The focus on two 'quality' media outlets also limited our understanding of media practices in 'popular' outlets, although, where possible, we would look for opportunities to observe these journalists. For example, I was able to gain the trust of a commercial radio 'police' reporter who allowed me to follow him for a number of days. There were also limitations in terms of recording details in field notes after a long day of observation during which only minimal notes were taken.

In this study the use of other research methods and multiple sources of information served to strengthen the validity of the research findings. Having three observers was one way of cross-checking the information and insights recorded in field notes. In the second component of the study (Ericson et al 1989), 93 'news sources' from criminal justice practitioners to other government and non-government sectors were interviewed on a range of issues including the strategies and tactics they used in dealing with the media. Because these sources dealt with the full range of media outlets, they were able to provide information on aspects of media practice that we might not have had access to. The third component of the study (Ericson et al 1991) involved both a qualitative and a quantitative analysis of media content, focusing on whether there were variations of such content by medium (newspaper, television or radio) and by market (popular or quality). The quantitative analysis was based on a cross-sectional sample of 1485 news stories over 33 days in 1983 chosen from six media outlets: two newspapers (*The Globe and Mail* and a tabloid), two evening (6 pm) television news broadcasts (CBLT and a commercial TV channel) and two evening (5 pm) radio newscasts (on CBC a commercial radio station). The qualitative analysis examined news stories longitudinally, taking into account both media contents on the same story covered by different outlets and insights from the ethnographic fieldwork.

These additional analyses undoubtedly provide a more comprehensive and in-depth understanding of the role of mass media in defining and shaping public knowledge of crime and deviance. More specifically, the combination of ethnographic fieldwork, interviews with sources and systematic content analysis enriches our appreciation of how variations in crime and deviance news content are linked to the *field* of media practice (the types of capital valued in different mediums and markets) and the *habitus* of the media workers and of media sources. While theoretically it is possible to document the work of journalists, the role of media sources and the content of media stories through fieldwork alone, it would require unlimited access, energy and resources to arrive at a comprehensive and systematic understanding of the research problem. The validity of the research is not simply of academic interest. The role of the media in constructing and representing crime and punishment is a perennial discussion point in criminology, but there is no simple 'fix' for the problem. Understanding the social organisation of newsmaking, the crucial role of news sources and the variation between markets and mediums is a prerequisite for finding workable policy responses.

Researching police practices

The main aim of this study was to understand the socialisation of police recruits and the concept of police culture. In particular, the study was designed to investigate whether a more 'professional' police education, emphasising integrity, community accountability and reflective practice, would make a difference to police practice. The research design of the original study (Chan et al 2003) combined both longitudinal (over two years) and cross-sectional (an entire class of recruits) data on the attitudes and experiences of police recruits — employing questionnaires, interviews (of a randomly chosen half of the class), field observation (of a sample of recruits) and documentary data — from their first week at the NSW police academy to when they had become police constables. A follow-up study in 2003–04 using only questionnaires and interviews was conducted on members of the cohort still working in the same police organisation (Chan and Doran 2009). Ethnographic fieldwork was only conducted in the original study; it involved more than 900 hours of observation (115 eight-hour shifts) in police stations over various stages of police training. Because of the limited time available for observation (especially in Phases 2 and 5), a number of research staff were involved in the observation simultaneously. The choice of stations (13) and shifts (day, evening and night) was dependent on the stage of training and was designed to represent a spread of geographical coverage, station workload and gender. Like the media study cited above, this project took a mixed method approach from the start; hence the ethnographic fieldwork was one of several research strategies.

In Chan (2012), I discussed a range of validity issues that arose from the original and the follow-up studies; these include problems of access and 'construct validity', inter-observer reliability, causal inference and the need for selectivity in presenting qualitative data. The validity issue I would like to focus on here relates to interpretation of the survey data. One of the most important findings of the longitudinal surveys was that by the end of their 18-month training police recruits had become significantly more negative towards the community (the general public), the criminal justice system, the rule of law, and police work (Chan et al 2003:ch 6). These results were confirmed by what recruits themselves told researchers in interviews: that they had become 'more cynical, suspicious, less tolerant and more likely to stereotype people' (Chan 2012:305). These results were also consistent with the conventional wisdom about the corrupting influence of police culture (see, for example, Fitzgerald Report 1989). However, such a simplistic interpretation of the survey data would be misleading. As discussed in Chan (2012), the use of multiple research techniques (including observation) in this study led to a more dynamic and nuanced understanding of police socialisation and police culture. First of all, we found that police culture was not homogeneous (different practices and values co-existed within the same organisation) and was in fact in a state of flux, partly as a result of the Wood Royal Commission (which was established around the same time as the original study). Second, police recruits were much more reflective and strategic than conventional wisdom would suggest:

> Recruits' lack of confidence and practical experience when they first entered the operational field made [the Field Training Officer's] criticisms [of academy training] particularly poignant. Their desire to 'fit in' and be accepted by operational police also meant that they would never openly contradict these criticisms. But when we interviewed probationers away from the field, some of them admitted that the negative view of the Academy was partly perpetrated by officers in the field — a fact confirmed by our observation during Phase 4 ... A few recruits told us that they did not agree with these criticisms of the Academy. They thought that the Academy provided the basic knowledge which could then be drawn upon in the field. They felt that there was a place for theoretical knowledge and Academy training was not meant to replace field training, which was where practical training was supposed to take place ... Probationers also commented on how workers adapted to the inconsistent styles and

expectations of different shift supervisors and shift partners … When given the opportunity to express their views, some probationers were quite willing to criticise and distance themselves from certain practices. Individual probationers formed judgments about police work and the police organization based on their own experiences and were able to distinguish between 'good' and 'bad' models (Chan et al 2003:307–8).

Finally, the documentary data, especially regarding the work and impact of the Wood Royal Commission, helped interpret some of the changes in attitudes among recruits in relation to corruption, integrity and solidarity. Changes in the field of policing — tighter accountability, stricter regulation of misconduct, and the enforcement of integrity requirements — might not have changed police culture in a dramatic way, but they had certainly made police socialisation a much more heterogeneous, unstable and unpredictable process.

This discussion highlights the importance of taking validity issues seriously. Police reforms are complex; to the extent that research can make a contribution to policy decisions, researchers must take seriously their responsibility to produce knowledge that is credible and defensible. A superficial interpretation of the survey data in this study would have led to a very pessimistic conclusion about improving police education as a reform measure. The use of multiple sources of data over a longer period of time has, in my view, strengthened the validity of the findings and provided a more nuanced understanding of the possibilities and limits of reforms.

Researching restorative justice practices

The main aims of this (still ongoing) study are to understand the practice of victim-offender conferences ('VOCs') for serious offences and to examine the structural and situational factors affecting the processes and outcomes of these conferences (Chan et al 2011). The research design includes three components: an archival analysis (based on documentary data and interviews with conference facilitators) of VOCs conducted by Corrective Services NSW since 1990; an in-depth analysis of current conference cases (based on field observation of conferences, pre- and post-conference interviews with the main conference participants and the facilitator); and a five-year follow-up study of completed conferences (based on questionnaires and interviews). The NSW program is described as follows:

> The Victim Offender Conferences run by the Department take many months to prepare (sometimes up to 6 months or even a year), usually last 4–5 hours, and are followed by as much de-briefing as requested by victims, offenders and families (contact can remain for more than a year). The case load is small; about 10 VOC a year. Referrals may come from offenders *or* victims and are undertaken voluntarily whilst the offender is either still in prison or on parole. Because it is a *post*-sentence program, obviously the conference has no sentencing implications, and taking part in a conference has no impact on their eligibility for parole. The aim of the program is to provide a forum for discussion and to facilitate the resolution of emotional impact of the crime on victims and their families and communities of care ... (Bargen et al 2011).

The validity issue I would like to focus on relates to the trustworthiness and authenticity of the field observation. There are numerous material constraints on the observation of conferences (see Bruce 2008 for a discussion of time and logistical constraints in relation to the observation of youth justice conferences). Access is a perennial problem. Given the private and emotional nature of these meetings, not all participants are keen to have an observer at the VOC. But even when access is granted, there are logistical problems, such as whether one observer can capture the dynamics of interactions and complexities of emotions that are integral to this type of encounter. We have decided to use two observers for each

conference so that each observer can report on the participants in the circle that are visible to them. We typically sit outside the circle. Even with two observers taking notes, there are problems of being able to hear and record accurately or verbatim what was said, being able to see and interpret correctly the emotions being expressed by participants, and being able to understand and record adequately the attitudes and experience of the participants during the conference. Part of our research strategy involves getting to know thoroughly details of the offence(s) and the history of the case, and interviewing all key participants before and after the conference. Even though these interviews convey participants' personal accounts that may or may not have been constructed to convey a particular impression to the researcher, we have found these accounts to be extremely helpful for piecing together an overall picture of the conference. We found that interviews with the facilitators were particularly important for understanding what they regarded as the moods, dynamics and turning points in the conferences.

To illustrate the validity issue about the trustworthiness and authenticity of the field observation, I will briefly describe a VOC that was observed as part of the research project (RJConf007). The amount of information we collected was extensive: in addition to two of the researchers observing and completing field notes on the three-hour conference, we had access to the case file information, which contained details of the offences, sentence outcomes and other background data. We conducted three pre-conference interviews (with five victims and the offender) and seven post-conference interviews (with the offender, three victims, one support person — a prison psychologist — and a prison officer who was present but not part of the conference). The offence involved was the sexual assault and murder of an elderly woman by a young man who was a neighbour. The offender pleaded guilty and was sentenced to 20 years of imprisonment with a non-parole period of 15 years. The VOC took place 17 years after the offence and was attended by the offender (together with his sister and brother-in-law, a prison chaplain, a prison psychologist, and a probation and parole officer) and the victim's daughter, son and grand-daughter (together with their spouses and other support persons) — a total of 18 participants, not including the facilitator. Space does not allow a full description of the conference and its dynamics in this article. Very briefly, for both the offender and the victim's family, the conference was regarded as a great success in that the victim's family members were able to tell the offender how the offence had affected them (mental health, wellbeing, family relationships), the offender fully accepted his responsibility and said he was sorry for the murder, and the victim's family offered forgiveness and contributed actively to the formulation of an agreement at the end of the conference.

A number of very important validity issues arose for interpreting the observation, for example: How genuine were the apparent lack of hatred among the family members and their outpourings of forgiveness? Apart from the observable display of reconciliation (talk of forgiveness, handshaking between some of the victim's family with the offender, the signing of an outcome agreement and so on), how truly 'restorative' was the VOC for the participants, and how did this vary among the participants? How reliable was our observation of the emotions shown by the participants? What were the hidden emotions? How significant was the part played by emotions in the VOC? How much of the reconciliation was caused by what happened at the VOC, and how much was caused by other factors (such as the family's religious faith, the preparation by the facilitators, the passage of time, and the health condition of the offender)? Generally, how reliable was our observation of the conference dynamics and how transferable are the findings of this research to knowledge about restorative justice? It is safe to conclude that without the multiple accounts (from researchers, participants, a non-participant observer and the

facilitator) that provided a range of perspectives and interpretations of the VOC, we would have been much less confident of the conclusions to be drawn from the observation. Restorative justice conferences are complex events that reflect a web of antecedent factors and concurrent dynamics which may or may not be apparent to observers. Displays of emotions may be overt or subtle, while impact on participants can rarely be determined by observation alone.

The importance of being confident of the reliability and validity of our conclusions is not simply a matter of rigour in an idealistic scientific sense; it is in fact about our ethical responsibility both as researchers and as potential generators of policy recommendations on restorative justice practices. Findings of the project will have a variety of policy implications. For example, they will shed light on how restorative justice programs for serious crime should be evaluated: What are the appropriate indicators of success? How important are the role of facilitators and their preparation of participants? How relevant is offender recidivism as a measure of success? Similarly, there are other related policy implications: Does restorative justice have relevance for parole decisions? What do restorative justice programs tell us about how to identify and support the needs and wellbeing of victims (Bruce et al 2012)? Does restorative justice have any impact on victims' fear of crime? Researchers need to be convinced (as well as convincing) about the veracity of their findings and their inferences for policy actions.

Conclusion: Ethnography, validity and the ethics of criminal justice research

This article has focused on the validity of ethnographic research as a methodological issue and argued that it is an issue that ethical criminal justice researchers ignore at their own peril. While there may be problems with using scientist notions of validity (for example, construct validity, internal validity, external validity and reliability) as yardsticks for assessing ethnographic research, the three examples I cited from criminal justice research have illustrated how the veracity and credibility of research findings can be strengthened by using multiple sources of data or multiple methods, in other words, the use of triangulation.

As Denzin points out, triangulation has had a long and chequered history in the practice of qualitative research. It was a 'fad' in the 1960s when '[s]cholars were racing to design research that was valid, objective, and sensitive to threats to internal and external validity and reliability' (Denzin 2010:419). Denzin himself advocated the use of a mixture of research methodologies for such a purpose: 'Because each method reveals different aspects of empirical reality, multiple methods of observations must be employed' (1978:28). He distinguished between triangulation of data, investigator, theory and methodologies (1978, 2010:425 n 2). It was in the 1980s that triangulation became problematic: the so-called 'paradigm war' between the post-positivists ('QUANS') and the constructivists ('QUALS') was fought over whether the two camps represent fundamentally different and incompatible paradigms which cannot be combined (Denzin 2010:421). A second 'war' then occurred in the 1990s to mid-2000s between various paradigms, including multiple versions of critical theory paradigms. In the US at least, there has been a third paradigm war (from 2005 on) that was pitched between 'evidence-based methodologists and the mixed methods, interpretive, and critical theory schools' (2010:421).

While advocating a form of triangulation, I am not unaware of the possibility of different sources of data leading to contradictory conclusions, nor do I believe that by using multiple

methods/sources of data, researchers will capture fully 'the reality'. In my view it is important to start from a theoretical standpoint with its inherent assumptions and conventions, so that it is possible to discuss both convergences and divergences in research findings from different research approaches. Divergences may well provide fertile ground for theory building and enhancement of understanding:

> a common misunderstanding about triangulation is that the point is to demonstrate that different data sources or inquiry approaches yield essentially the same results. But the point is really to *test for* such consistency. Different kinds of data may yield somewhat different results because different types of inquiry are sensitive to different real-world nuances. Thus, understanding inconsistencies in findings across different kinds of data can be illuminative. Finding such inconsistencies ought not be viewed as weakening the credibility of results, but rather as offering opportunities for deeper insight into the relationship between inquiry approach and the phenomenon under study (Patton 2002:248).

When researchers' findings enter into the *policy field*, something else happens. The politics of knowledge favours research findings that are 'rigorous', 'objective', 'credible' and so on, and thus in some ways researchers may need to strategically appropriate the language of science. While admitting that 'absolute objectivity of the pure positivist variety is impossible to attain' (Patton 2002:93), researchers may nevertheless believe that there are compelling reasons for taking validity issues seriously in criminal justice research. The use of multiple methods within one research project is one sensible and defensible way of dealing with the trustworthiness and authenticity of ethnographic accounts. While I welcome further dialogues between researchers who work from different theoretical paradigms, it is important to recognise that research 'has always been and will always be a moral, political, value-laden enterprise' (Denzin 2010:424–5). Researchers should not let the politics of evidence take over their ethical obligations to ensure that the knowledge they produce is credible, fair and relevant.

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