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Research Ethics: Regulations and Responsibilities

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INTRODUCTION: SCIENCE NOT ETHICS?

We wish to begin this chapter with a characterization of academic research, which seems to leave little space, or have little need, for ethical reflexivity.

Proper academic research is a process defined by scholarly detachment from the subject studied. It is a rational, dispassionate, and largely technical endeavour, not emotional, ideological, or personal. It is characterized by value freedom and the eradication of biases. It is properly distanced from political debates about the purposes to which such research might be put. If asked how ethics comes into the frame then, apart from avoiding the obvious issues of falsification of data or blatant abuse of subjects, questions of ethics, like other emotive or subjective issues such as biases, values, and political or religious views, should be largely expunged from the research process lest they taint the dispassionate pursuit of knowledge.

For some readers this characterization might appear to be a 'straw man', a bald caricature of the disengaged academic. For others, given the disparate nature of academic research on organizations, it may approximate an

ideal to aspire to. We present it here to begin what we intend to be a questioning, at times sceptical, examination of research ethics across the paradigmatically diverse field of organizational research: starting with this view which, perhaps more than any other, seems to pit academic research – presented as 'science' – against or, perhaps more tellingly, *above* ethics.

Such a view of science can trace its history to European eighteenth-century Enlightenment thinkers, their popularization of a fifteenth- to seventeenth-century scientific world view and extension of such views from the areas of natural science to the study of humanity (Christians, 2005). Located within a political struggle with the landed aristocracy and church hierarchy over the distributions of privileges, inequalities, and social status and an ideological struggle with the Aristotelian-Christian ideas, which legitimized these, the Enlightenmenters promoted reason, observation, facts, and the autonomy of the individual as a direct counter to the church's imposition of an incontestable moral and religious authority. In such a context, that which was apprehended by revelation or superstition, or imposed by tradition or domination, was representative

of the old order, of the church's and the landed aristocracies' claims to divine rights. To free people from such constraints, reason was to be divided from and elevated above faith, facts from values, the material from the spiritual, truth from belief, 'is' from 'ought'. We have here the emergence of a view of science, the legacy of which we operate with today, where the scientific pursuit of hard facts appears largely separate to and divorced from the realm of morals or ethics.

However, this apparently clear cut picture of Enlightenment science as separate from, or in antithesis to, morality or ethics, is more ambivalent than may at first appear. Woven within this view of the liberating force of reason and science is a strong normative, ethical, commitment to the values of progress, truth, and freedom. The freedom of the sovereign individual, acting through reason, freed from domination by religious or other traditional moral authority was upheld as the highest ethical end. Thus, though the *process* of science was understood to be necessarily freed from issues of beliefs, values or such, science itself was strongly wedded to a liberal humanist ethics. Science and reason were cast as key forces through which humankind would move to a progressively less prejudiced, less hierarchical, more rational social order. Seidman (1998: 21–2), summarizing the views of the eighteenth-century European thinker *Condorcet*, for example, presents this era as asserting that 'the very nature of science – its reliance upon facts and observation, its openness to criticism and revision – inevitably promotes individualism, tolerance, equality, and democracy. Hence the progress of science automatically translates into social progress'.

Though still perhaps an inspiring ideal for many – and, we suggest, the enduring implicit justification for science and academic research more generally – faith in the inherently liberal, humanist, or progressive ethics of science has been seriously tested in more recent times through historical revelations, which have implicated scientists in atrocities against humanity (Punch, 1994).

Neither is *social* science free from charges of complicity in deeply ethically problematic practices. Prasad (2005: 76–78), for instance, charts how ethnography, considered by many to be the pre-eminent qualitative methodology, has its roots deep in the politics of colonial and American frontier expansion. Urgently needing more information about the 'native' populations so as to subjugate, pacify and/or better exploit them, colonial powers and frontier administrators commissioned and supported ethnographic field work to provide invaluable 'authentic' accounts of the 'natives'. Prasad perceptively highlights how the problematic legacy of colonialism still runs like a seam through contemporary ethnographic research and writing. Such a legacy surfaces, for instance, in quasi-colonial assumptions about the ability of the expert academic researcher to apprehend the native's point of view – to, one might argue, treat the researched as a colonized people or subject race such that the expert researcher can assume to 'know them and what is good for them better than they could possibly know themselves' (Said, 1978: 35; also Wray-Bliss, 2002b: 94).

For some critics, particularly those drawing on feminist and postmodernist traditions, such examples where science has functioned in the service of oppression are not merely dismissible as 'bad' science. They also raise deeper ethical questions with the understandings of academic research methods and the authority of academic knowledge, which we have inherited from the Enlightenment. The researcher's detachment and distance – from research subjects, from values or biases, from consideration of the ends that knowledge may be put to – are not, according to these critics, markers of an impartial scientific process that enhances human knowledge and progress. Rather, such detachment represents a discredited, even dangerous, way of apprehending the world that distances the researcher or scientist from an appreciation of their and others' humanity. Glover (2001) provides a compelling, far-reaching account of the consequences of detachment or divorce from humanity during the twentieth century.

He argues that this century's atrocities, wars, and genocides compel us to re-examine our ethical assumptions and ethical philosophies. Included amongst the historical experience he considers is the following correspondence between scientists at the I.G. Farben Chemical Trust and officials at the concentration camp, Auschwitz, regarding the procurement of research subjects:

In contemplation of experiments with a new soporific drug, we would appreciate your procuring for us a number of women ... We received your answer but consider the price of 200 marks a woman excessive. We propose to pay not more than 170 marks a head. If agreeable, we will take possession of the women. We need approximately 150 ... Received the order of 150 women. Despite their emaciated condition, they were found satisfactory ... The tests were made. All subjects died. We shall contact you shortly on the subject of a new load. (Glover, 2001: 338-9)

While such horrific examples of the divorce of humanity from the pursuit of knowledge are most thankfully rare, a number of other critics have also raised strong ethical objections to more subtle forms of researchers' detachment from the subject and objects of their research. Included amongst such criticisms are arguments linking the treatment of subjects in research as passive objects of study, to the broader objectification and oppression of women and marginalized members of society (Hooks, 1989; Said, 1978; Stanley and Wise, 1993). Critics have also highlighted how conventional practices in academic writing – including writing in the third person, excluding from the text data that is ambivalent or contradictory to the main argument, perfunctory technical sounding summaries of research methods used, and the citing of an author's qualifications and institutional allegiances in biographical notes – push for the elevation of the authority of the 'dispassionate' and 'objective' academic voice (Smith, 1990). Such authority, while useful in making the academic voice heard, may also have the effect of rendering research subject's voices subordinate and subject to that of experts and thereby less able to contest

or contradict academics' and other experts' representation or appropriation of their lives (Opie, 1992). For some, the critiques of the Enlightenment view of science and scientific method are of a magnitude that obliges us to overturn the historical separation of science and ethics and make the explicit articulation and debate about our ethical positions and intentions central to our research practices (Bauman, 2005; Seidman, 1998).

From the preceding broad brush discussion we can perhaps begin to appreciate how matters of ethics intertwine with research both through the *process* of carrying out research and its *legitimacy* or whether it should be conducted at all. In organizational research, these areas of ethical consideration are no less pertinent. First, in relation to the process of research, the dominance of quantitative research practices which draw on a positivist epistemology, may serve to reduce the status of research populations from that of active, knowledgeable, and diverse subjects to passive, easily categorized objects of knowledge. The reproduction of the distanced, detached voice of the author in published texts, in both quantitative and qualitative research, 'mainstream' and 'critical' studies, can obscure the normative and inherently contestable basis of research and push for the presumption of academic authority over the lives of those studied (Wray-Bliss, 2002a). This may render less-powerful members of organizations, who already have few opportunities to have their voices and views publicly aired, more silent still.

Second, in relation to the legitimacy of organizational research as a worthwhile project, this can be argued to rest, as did the Enlightenment view of science before it, upon largely unexamined assumptions about the goodness of its ends – in this case the 'goodness' of organization. For though each research topic and subdiscipline or field will have its own local rationale – extending our knowledge of organizational strategy, for instance, or optimizing distribution and supplier networks – organizational research in general is validated by an implicit appeal to the furtherance of organizational effectiveness.

This constitutes an assumed ethical end that serves to legitimize the organizational research arena. However, some critics have argued that such assumptions cannot continue to underpin the ethical legitimacy of organizational research. For example, the view that the rightful purpose of organizational research is to aid us in a movement toward progressively more rational and efficient organization, expressed through notions of evidence based practice (Learmonth, *this book*), may itself be questioned. Critical organizational researchers (Alvesson and Deetz, 2000), working in such fields as labour process theory and critical management studies (Grey and Willmott, 2005) have highlighted and critiqued the normative, pro-managerial, pro-western (Griseri, 2002) and masculine (Gherardi, 1995) interests and assumptions underpinning and directing mainstream management and organizational research. Such concerns around the unacknowledged normative assumptions of organizational research, the systemic privileging of managerial concerns and voices and the silencing of the interests of a range of other organizational participants and stakeholders are longstanding. Writing in 1974, Braverman, for instance, was highly critical of the failure of industrial sociologists to examine the degradation of the working conditions and processes faced by nonmanagerial labour. He attributed this failure to sociologists' own unacknowledged managerial assumptions. By sharing with management 'the conviction that this organization of the labour process is "necessary" and "inevitable"', Braverman argued:

This leaves to sociology the function, which it shares with personnel administration, of assaying not the nature of the work but the degree of adjustment of the worker. Clearly for industrial sociology the problem does not appear with the degradation of work, but only with overt signs of dissatisfaction on the part of the worker. (Braverman, 1974: 29)

Difficulties concerning the ethical legitimacy of organizational research driven by partial, managerial interests may be rendered particularly acute in the contemporary

climate, when the majority of organizational research takes place within the context of business schools which, as Parker (2002: 130) observes, are 'pressed up hard against the utilitarian reality of corporate managerialism'. Researchers working in this context may feel, in innumerable material and symbolic ways, the need to be 'relevant to' and 'in the service of' business impressed upon them – rendering re-examination of the 'goodness' of commercial organizational ends that much more difficult. Yet some critics have called for a re-examination of largely taken-for-granted assumptions about the goodness of modernist organization per se. Echoing and extending concerns regarding the rationalizing and homogenizing pressures in western society and large scale bureaucratic organization, the postmodern social critic Zygmunt Bauman has powerfully questioned the beneficence of the modern organizational form. His work has linked treasured modernist features of organization – its complex hierarchy, focus upon procedures, instrumental orientation, delineation of responsibilities, and removal of personal sentiment from the efficient performance of organizational roles – with the potentially catastrophic, systematic erosion of moral responsibility in organization (Bauman, 1989/1991). Following Bauman, even the goal of more rational or efficient organization, an end upon which the legitimacy of organizational research at least implicitly relies, cannot be assumed to be an unquestionable good. Furthermore, the nature of goodness itself – or more specifically questions of how we are to decide on matters of ethics – is also contested. The long history of moral philosophy, while aspiring to some universal and eternal understanding of goodness, has achieved the production of multiple well-reasoned but contested and at times conflicting formulations of ethics (MacIntyre, 1998). The modern citizen, including of course the academic, is the inheritor of this legacy, which has become structured into our institutions, laws, customs, and individual moral subjectivities. Such that now, notions including duty, virtue, responsibility, fairness, consequence, and even happiness carry echoes of different

philosophical ethical traditions that each encourage us to place different weight on different outcomes, intentions, customs, or contexts in our deliberations of what is ethical.

From the preceding discussion, several points emerge, which we wish to carry forward in this chapter. First, research ethics is informed by wider philosophical debate and discourse, but it is not an abstract, peripheral, or principally philosophical concern. Instead, it encompasses a set of empirically and theoretically driven issues that permeate the entire research process. Second, organizational research has a cumulative, and sometimes immediate, effect upon participants, which cannot be assumed to be necessarily beneficial. Third, ethical considerations run through all types of organizational research, both quantitative and qualitative, affecting choices of method, degree of detachment from or relationship with the researched, the way that the organization, data, or other's experience is represented in the text and the way that the academic voice is authorized. Finally, the credibility and authority of organizational research rests on either an implicit or explicit ethical warrant, which ensures its ongoing support. One of the principal ethical warrants on which organizational research is founded is the assumed goodness of a managerial agenda and/or the beneficence of modernist organization. These assumptions in turn draw on the historical legacy of the Enlightenment. This legacy is wedded to liberal humanist notions of progress, reason, and the furtherance of humanity and which understands science to be at the heart of this process. It has passed down a view that the researcher's detachment from central aspects of their own and others' humanity is at the core of the scientific method. However, we have suggested that we can no longer, in the current context, make assumptions about the progressive nature of this detached scientific process, of science and research itself, or indeed of the goals of modernist organization. This is not to say that researching organization or organizational research processes are inherently

unethical or harmful – far from it. At a time when the reach, power, and complexity of organizations risks outpacing our traditional processes of democratic accountability, intellectual comprehension, and moral imagination, understanding organization is, we would argue, fundamental to wellbeing and survival. Rather, the implication of the above would seem to be that we, as a community of organizational researchers, need to subject our research practices, agendas and assumptions to continual and sustained ethical scrutiny. In the following section, we explore how the organizational research community currently engages with such matters.

The status of ethics in organizational research

Within organizational research, we can identify what we might call implicit and explicit engagements with ethics. Implicit engagements include a myriad of choices and assumptions, which may not be explicitly articulated as ethical issues but which nevertheless imply ethical choices. These include a researcher's choice of topic, the nonfalsification of research data, avoidance of plagiarism, judgements around whose 'voice' will be represented in the research and how (Wray-Bliss, 2004), even decisions regarding where or if to publish the research and how it will be written up imply choices that may impede or enable other's access to the research, to knowledge about their world or access to how they have been represented and understood in the wider society (Parker, 2002; Grey and Sinclair, 2006). However, the above issues are seldom explicitly articulated or engaged with as ethical issues. More often than not, they will be seen as merely matters of academic convention in the particular organizational research field. Instead, 'research ethics' are normally understood more narrowly and discretely as concerned with the need to follow a set of clearly defined precepts concerning appropriate and acceptable research conduct. These explicit engagements with ethics involve consideration of how

researchers should treat the people who form the subjects of their investigation and whether there are certain actions that should or should not be taken in relation to them. This approach tends to result in the generation of a set of ethical principles, which organizational researchers are expected to show that they have considered, usually at the design stage of the research. Failure to adequately address these issues may result in censoring by the organizational research community.

The main mechanisms for ensuring compliance to such ethical precepts have traditionally been self-disciplinary. These have taken the form of adherence to codes of ethical practice developed by professional associations to which organizational researchers belong. The origins of these codes and the ethical precepts within them can be traced to the military tribunal known as the 'Nuremburg Trial' involving twenty-three German physicians and administrators who were accused of war crimes and crimes against humanity for their participation in inhuman experimentation on those deemed unworthy of life during the Second World War. The appalling nature of these atrocities, and the shock of the international community at the role of science in their perpetration, led the trial judges to formulate a universal standard for human experimentation in an attempt to pave the way for a reconstituted moral vision (Katz, 1992). Argument submitted to the United States Counsel for War Crimes by doctors for the prosecution sought to define legitimate research on human subjects and to protect them from potential harm through the principle of voluntary informed consent. In a section entitled '*Permissible Medical Experiments*', the verdict established ten points, which covered the areas of: (i) ensuring the voluntary consent of the subject; (ii) experiments must be for the good of society; (iii) sufficient prior research must have been conducted; (iv) unnecessary suffering or harm must be avoided; (v) experiments with the chance of injury or death are prohibited; (vi) the risk should never outweigh the humanitarian importance of the problem studied; (vii) preparation and facilities must be in place

to avoid harm or injury; (viii) only properly qualified persons may conduct the research; (ix) the subject can withdraw consent; and (x) the scientist must terminate the experiment if the risk of harm warrants it. These ten points have subsequently become known as the 'Nuremburg Code' and have informed both biomedical and social science ethics codes since.

For nonmedical organizational researchers, professional bodies such as the *Academy of Management*, or related social science disciplines like the *Social Research Association*, *American Psychological Association*, or *British Sociological Association* set out the main ethical issues that researchers tend to face and provide guidance on how they should respond to them. That there has been relatively little variation in the content of these codes since the 1960s (Bryman and Bell, 2003; Bell and Bryman, 2007) suggests that a broad consensus has emerged surrounding the definition of ethical issues in social science research founded on the principles established by the Nuremburg Code. Such codes universally specify the avoidance or minimization of harm or risk of harm, whether physical or psychological, and the need to ensure that research subjects are fully informed about the nature of the research and give their consent to being involved with it. In addition, they stress the necessity to maintain the privacy of research subjects, ensuring the confidentiality and anonymity of data where appropriate, minimizing the possibility for deception at all stages of the research process, from data collection to dissemination. There may also be an obligation to declare sources of funding and support that have the potential to affect the affiliations of the researcher or cause a potential conflict of interest. In each case, it is the researcher's responsibility to ensure that these ethical issues have been considered and that steps have been taken to address them where necessary.

Despite, or perhaps because of, the convergence of ethics codes there has, in recent years, been an increase in attention and debate concerning ethics in social scientific

research (Sin, 2005; Truman, 2003; Kent et al., 2002; Nelson, 2004; Wiles et al., 2007). Such sensibilities have informed a terminological shift away from a focus 'on hapless research participants, conventionally termed, *subjects*, a word reflecting the concept that research participants have things done to them' (Lincoln and Guba, 1989: 222, emphasis in original) towards the use of the term 'participants' (Birch and Miller, 2002). These debates have been most vocal amongst qualitative and reflexive researchers who draw on feminist, postmodern, and postcolonial critiques to critically re-examine the ethics of their own research practices. This may have had the unintentional effect of encouraging more positivist and/or quantitatively minded researchers to assume, mistakenly, that consideration of ethical issues is more relevant to, or only becomes problematic in relation to, qualitative research.

In comparison to the broader social sciences, organizational researchers have been more reticent in reflecting on the implications of these debates for research practice (see Brewis and Wray Bliss, 2008; Bell and Bryman, 2007). Yet, there is little doubt that their impact is likely to be significant, not least because they have been paralleled by a rise in ethical governance, as the agencies and institutions that fund or disseminate organizational research or employ organizational researchers have become more active in seeking to regulate ethical conduct. For example, recent revisions to the code of ethics produced by the Academy of Management allow researchers to be penalized for ethical breaches through a process of formal adjudication, which can result in their exclusion from the Academy (although it should be borne in mind that there is no requirement on organizational researchers to belong to this or any other professional association). In addition, academic institutions that employ researchers, driven in part by a concern to avoid potentially costly litigation, are taking a more active role in overseeing the ethical conduct of their members. Within universities, research ethics committees (RECs) or, in North America, institutional review boards

(IRBs) have been established with responsibility for judging adherence to ethical precepts in research proposals submitted by research students and academics. These developments signal the establishment of ethical governance structures, which are more formalized and less 'light touch' in comparison to former voluntarist systems (Kent et al., 2002). Such developments may be interpreted on one level as a welcome indication that the academic research community is engaged in a serious and sustained way with ethics. However, a number of concerns have been expressed regarding the current trend toward greater formalization of ethical accountability in the social sciences, which we outline in the section that follows.

Critiques of ethical formalism: Committees and codes

Characterizing the historical treatment of issues of morality and ethics in civil society, Bauman (1993) argues that we have tended to reduce matters of ethics – understood as that unending innermost compulsion to seek to do what is *right*, to know one's own heart, to be responsible toward the Other – to matters of following predefined rules, laws or codes. In so doing, a small proportion of the population have been granted the moral status to reflect, philosophize, and codify what is ethical while the rest have been allotted the roles of merely following these rules and norms, socialized into doing so through the mechanisms of organizational and state bureaucracy. For Bauman, such rule-following, bureaucratized behaviour does not represent ethics but rather its antithesis – the abdication of individual ethical agency and its replacement with conformity. Whether we take Bauman's broad-brush depiction of society at face value or not, we can begin to appreciate that the formalization or bureaucratization of ethics may be questioned. Such a critique focuses on whether ethical formalism enhances accountability and responsibility or limits and constricts our engagement with matters of ethics.

If we apply these considerations to research ethics we can perhaps begin to appreciate some of the concerns that have been voiced regarding ethical governance as promoting a rigid and inflexible understanding of ethics, one that encourages the ticking of boxes rather than ongoing ethical reflexivity. Critiques of ethical formalism in social scientific research are driven by concerns about the disciplinary effects associated with these mechanisms and by the possibility that this will lead to the imposition of a hegemonic view of what constitutes 'good' research. Critics seek to argue that social science research operates differently from other research involving human subjects (Coomber, 2002) and that the principles that are used to regulate the community need to reflect this. One of the criticisms that can be made of the standardization of research governance is that it does not necessarily encourage or support researchers in the process of becoming morally-active individuals, since it is only through conducting and writing up research that we develop a full awareness of ethical issues. A further critique is that they may be functioning so as to prohibit, or make significantly more difficult, valuable forms of research that do not readily conform to a narrowly defined set of bureaucratic norms,

'if the ways of ensuring ethical research are heavily reliant on RECs, this may not serve research practice well. This seems to be especially the case where processes are rigid and concerned only with assessing the ethical issues prior to the project start, when the information needed to make the assessment is not available' (Richardson and McMullan, 2007: 1127).

These points can be illustrated in relation to the principle of informed consent, where the role of the REC typically is to:

... a) make sure that the research proposals passed by the committee are appropriate and safe, and b) that the prospective participants are fully apprised of the research and of their rights (e.g. to anonymity or to stop the research at any time without prejudice). The method increasingly used to achieve the second of these concerns is that of the signed consent form. (Coomber, 2002, section 1.1)

The adequacy of form-filling as a method of ensuring that participants understand the implications of consenting to participate in a research investigation has been questioned by several commentators (c.f. Edwards and Mauthner, 2002; Bhattacharya, 2007). Sin (2005) and Wiles et al. (2007) suggest that consent is contingent and situated, varying according to whom one is dealing with and how definitions are operationalized, a process that relies on an ongoing process of negotiation, which cannot be adequately addressed by getting participants to sign a form. This is a particular issue for qualitative researchers and those working with theoretical perspectives that seek to overcome traditional knowledge hierarchies and power relations in the research process through, for example, the adoption of feminist, participatory or decolonizing methodologies (Bhattacharya, 2007; Boser, 2007). RECs tend to be predicated upon a medical model of the research process that is insensitive to more flexible, emergent research strategies (Truman, 2003; Wiles et al., 2007; Lincoln and Tierney, 2004; van den Hoonaard, 2001). Methods such as participatory action research (*Chapter 2, this volume*) make it virtually impossible to anticipate all conventional ethical issues in advance of a study being undertaken (Bell and Bryman, 2007). The problematic nature of informed consent is also apparent in relation to ethnographic research, where the researcher's daily participation and routine appearance in the lives of those she or he researches can encourage participants to forget that their interactions and conversations are the subject of study.

Bhattacharya (2007: 1110) argues that there needs to be space within regulative structures for addressing 'the implications of fluidity and the contingent nature of qualitative research'. In such research, relationships with participants are continually deferred and not necessarily defined purely in terms of a short-term, instrumental academic need to collect data. Although it is also important to note that all types of research can deal with complex ethical issues, which may not become apparent until after the study

has begun. The requirement to obtain signed consent also has the potential to adversely affect the participation of particular groups in research, such as those who wish or need to remain anonymous because for example they are involved in committing illegal acts, by increasing rather than reducing their anxiety about participating in a research investigation (Coomber, 2002; Nelson, 2004). Ethical regulation based on principles intended to protect vulnerable research subjects from harm has the potential to make it more difficult to study organizations and individuals who do not want their activities to be studied, using covert methods for example (Galliher, 1982; Herrera, 2003). Finally, while informed consent protects research participants from harm, it does not allow for the possibility of participants causing harm to others (Koro-Ljungberg et al., 2007).

Ethical formalism has also resulted in the widespread convention that research should be anonymized in publication to protect participants from the risk of potential harm (Cassell, 1978). While this might initially seem to be a clear-cut ethical good, Grinyer (2002) has questioned this assumption, noting that in some cases it can be extremely important to participants that their accounts are not anonymized so as to maintain their authenticity. While in organizational research there are many instances where organizations that authorize research access are not anonymized in publication, increased ethical formalism poses a challenge to this convention. Furthermore, an argument may be made not to anonymize research subjects on the grounds of a felt ethical responsibility to expose and document forms of organizational exploitation, abuse or malpractice. Within organizational research, there has been only limited acknowledgement of the implications of unequal power relations in the research process. Relating to participants in organizational research as vulnerable subjects can unwittingly serve to reproduce existing power relations and hierarchies (Wray-Bliss, 2003a). Uncritically adopting, or being compelled to adopt, research protocols that have been specifically designed to protect

vulnerable subjects may institutionalize forms of practice that take-for-granted and merely seek to mitigate the effects of assumed power inequalities between 'researcher' and 'the researched'. In organizational research, the majority of participants are adult, employed, and often highly educated. This potentially enables them to exert considerable influence in shaping the nature, direction, and outcomes of the research to a far greater extent than ethics codes generally allow for.

Such discussions fuel concerns about the rise of an audit culture (Power, 1997) within research ethics where having been seen to have 'done ethics' becomes more important than meaningful engagement with the issues raised. This encourages a view of ethics as a separate component in the research process that is dealt with at a certain point, potentially having the effect of discouraging researchers from continuing to reflexively engage with ethical issues throughout the research process (Mason, 1996). As Sin (2005: 289) argues, 'the tick-box approach to ensuring compliance in ethical standards ... is an oversimplistic representation of the "messy" realities surrounding the research process'. Moreover, the formalization of ethical governance is by no means consistent, as Richardson and McMullan (2007: 1126) note, 'RECs vary greatly from institution to institution and there is no consistency even about whether the research ethics committee operates at the level of the discipline, the level of department/school, at faculty level or the level of institution'. The apparent inability of ethical governance structures to deal with certain types of qualitative research is suggested to have led some RECs to reject qualitative research projects for being unscientific and ungeneralizable. The disciplinary effects associated with such a discourse are, therefore, potentially significant:

Narrow definitions of scientific inquiry – and evidence – serve ultimately to circumscribe painfully and dangerously the range of what is considered useful knowledge and, consequently, to limit the kinds of studies supported by a range of

administrative and managerial structures, including IRBs, funding agencies, foundations, and state governments and agencies. The long-term results are systematic constriction of academic freedom (for individual researchers) (Akker, 2002), a seeping loss of institutional autonomy (for institutions of higher education), and a loss of needed epistemological perspectives on persistent social justice issues. (Lincoln, 2005: 173)

Within UK health research, there is evidence that the rigidity of ethical governance structures is causing researchers to modify their research strategies or even to stop doing research in this type of organization altogether (Richardson and McMullan, 2007). Since ethical governance structures in health are increasingly being used as a model for other disciplines, it is entirely possible that over time this pattern will also affect researchers studying other types of organizations.

As the preceding discussion illustrates, by defining what is acceptable or 'good' organizational research, ethical governance structures have significant disciplinary effects (Lincoln and Cannella, 2004a, 2004b), their power being derived from having the ability to change the way that research is conducted. Funding bodies may make grant awards conditional on the researcher having demonstrated that ethical issues have been considered while RECs may grant researchers conditional approval, requiring modifications to be made in advance of the study. Such structures have the ability to restrict researchers' ability to follow research questions they wish to pursue. There are fears that in so doing academic researchers' independence and ability to speak 'truth to power' may be diminished. For some, this has evoked a concern about academic research being manipulated to support government, executive, or other institutional or organizational interests – what in the US has come to be known as 'Bush Science' (Lather, 2004). As Koro-Ljungberg et al. (2007) argue, the notion of 'value-free science' disguises a subtle form of oppression, which is based on the notion that those in power act neutrally to defend the 'common good'. Adopting a Foucauldian perspective, they argue that IRBs constitute

a type of 'examining apparatus' through which the 'normalization' of researchers and research projects is achieved as a form of control.

Lincoln (2005) suggests that researchers need either to communicate with RECs or to serve on them as a means of representing their interests and getting their voices heard, or even to try to work in partnership to try to understand their worldview (Boser, 2007). Similarly, Bell and Bryman (2007) argue that the organizational research community needs to become more actively engaged in these debates if they are going to be able to influence ethical governance structures in a way that ensures that the particular issues it faces are taken into account. These concerns find additional purchase for organizational researchers when we consider the way that ethical codes are being shaped by bodies such as funding councils. For example, the Research Ethics Framework launched in 2005 by the Economic and Social Research Council, the main funding body for UK social science research, is concerned to minimize the risk of harm to not only individual research participants but also their organizations or businesses. This constitutes organizations as well as individuals as the subject of ethical considerations (Bell and Bryman, 2007) and in so doing potentially recasts ethics in a way that restricts the ability of researchers to examine critically certain issues such as forms of oppression within organizational structures, which are subordinated to the organization's right to be protected from critique.

Given that ethics codes originated with the Nuremberg Trial's concern to give justice to human subjects who suffered at the hands of those who were integrated into the organized atrocity of the Nazi holocaust (Bauman, 1989/1991), the extension of definitions of harm to include protection of organizations might seem a perverse departure. However, it must be acknowledged that the definitions of goodness upon which ethical oversight regimes are based are not neutral – they are founded upon a modernist, Enlightenment view of science outlined at the start of this

chapter. Ethical formalism can encourage a view of ethics as a series of abstract, neutral, objectively measurable principles, which can be universally applied, rather than a series of politicized judgements about the legitimacy of research as a worthwhile project based on assumptions concerning the goodness of academic knowledge about organization. We end this chapter by considering the implications of this final point and suggesting some possible responses to the issues we have raised in this chapter so far. As part of the organizational research community ourselves, these proposals are of course informed by our own theoretical and epistemological affiliations. We, therefore, offer them for debate and consideration, rather than in any attempt to set an agenda for others to follow.

Relating to and representing the other

Throughout this chapter we have sought to emphasize the importance of ethics, not only in relation to the *process* of doing organizational research, but also in terms of what is perceived to be the legitimacy of its *purpose*. How we as an academic community respond to the rise of ethical governance structures is in large part determined by the answers to these broader questions. Addressing them involves evaluating the ethical intentions that underpin organizational research; in other words, why we do it and the purposes it serves. This not only entails sustained examination of the relationships between the researcher and individuals and organizations who become participants in organizational research – the traditional focus of discussions about research ethics. It also involves exploration of the relationships that organizational researchers have with other groups, including audiences of managers and students who read and apply organizational research, universities and funding bodies that provide financial support and other researchers whose work informs it or who continue to develop it.

Some initial insights into these issues can be gained from considering how organizational

researchers have responded to ethical oversight regimes and critiques of research ethics. One potential response has been simply to push for the reduction or even the rejection, of ethical formalism. This is justified on the grounds that either there is a commonality of interests between organizational researchers and those individuals and organizations that constitute the focus of their research, or that the professional integrity of researchers is such that no such external disciplinary mechanisms are required. This latter point would seem to evoke a deontological ethical position, where the researcher searches their own heart and reason to determine their ethical obligations. If this assumption is correct, it may seem to follow that formal guidelines on research ethics, developed in large part to protect the interests of the researched, may be dispensed with. Alternatively, we might argue, drawing implicitly upon a utilitarian ethical formulation and weighing up the moral worth of consequences for the majority, that the goals of scientific research have an importance that overrides issues of research ethics. However, in the context of the postpositivist questioning of the modernist faith in science, truth and ideology in recent decades discussed earlier in this chapter, we suggest the political naivety of the first position and arrogance of the second are dangerous bases upon which to assume a commonality of interests or to override ethical concerns.

If we accept that the formalization of research ethics is a trend that cannot be reverted then another response would be to engage directly with ethical oversight regimes to ensure that the concerns of organizational researchers are effectively represented. This response introduces other dynamics since it potentially lends ethical oversight regimes greater authority through which to require researchers' conformity to predefined ethical codes. Furthermore, while the representation of different ethical issues or academic disciplines may mitigate the narrowness of definitions and applications of ethics, the constitution and function of such bodies tends to suggest pressures toward universalising and normalizing

research ethics. This generates the same problems of exclusion, the application of potentially inappropriate ethical criteria and questions of how to apply such criteria across disciplinarily, methodologically, and politically diverse fields and subfields of organizational research. In short, we remain unconvinced that such processes are the only or indeed the best way of encouraging organizational researchers to actively and routinely engage with the complex range of ethical issues that doing research generates.

A further strategy would be for the organizational research community to move away from the ethical universality implied by compliance-oriented ethics codes and instead to embrace ethical pluralism. This approach would involve different communities of organizational researchers attempting to articulate ethical practices for their specific communities; positivist organizational researchers could have one set of ethical practices, interpretive researchers another, critical researchers yet another (a situation which some may suspect already pertains though is rarely explicitly articulated). However, the adoption of a strategy of ethical pluralism introduces other challenges, for if we follow the logic that ethical responsibilities vary according to the research community studied, we can envisage arguments for responsibilities not only to be enhanced when, for instance, vulnerable or historically silenced organizational subjects are studied but also diminished when the research subjects constitute a community that, in some way, has forfeited our normal ethical regard. For organizational researchers drawing upon postmodernism and Critical Theory, acutely conscious of the political inequalities of organization and the dangers of universal truth claims, for example, a localized or selective research ethics, one that could vary according to the political status of the community studied, might not be inconceivable. Such selective ethics, however, raises difficult issues about the nature of ethics and politics of academic research, which have yet to be systematically explored (Collins and Wray-Bliss, 2005).

Any movement toward ethical pluralism will have to engage with the historical context of the attempt to universalize research ethics. The Nuremberg Code was devised as a universal code precisely so that groups of individuals – be they Jews, the disabled, or indeed those whose politics one vehemently disagrees with – could not be removed from the status of moral beings. Movements toward dismantling the aspiration to universality – the desire to write a code that says to all humanity for all time that the potential atrocities and abuses of science are not to be tolerated in sciences' name – will need to think carefully about what protections or prohibitions have the gravity to take its place.

While we find ourselves having some sympathy with each of these arguments we believe that there is still a need for greater transparency and reflexivity than these approaches invite and that this necessitates a process of cultivating greater ongoing critical self awareness in relation to ethics. Our final suggestion, therefore, entails shifting the emphasis away from approaches that are informed by teleological and deontological theories and concerned with 'what to do', toward an approach which instead seeks to develop dispositions such as honesty, sensitivity, respectfulness, reciprocity, and reflexivity, driven by a concern with how the researcher should 'be' (Wax, 1982; Lather, 1986; Lincoln, 1995; Wray-Bliss, 2003b; Bell and Bryman, 2007; Richardson and McMullan, 2007; Brewis and Wray-Bliss, 2008). This invites conceptualization of the ethical relationship between the researcher, their topic, and those whom they research as an ongoing, active, personal, and relational responsibility. In addition, it encourages a shift away from a controlling orientation to ethics as primarily involving formalized compliance to bureaucratic rules, toward one that is a more enabling and aspirational. It entails a focus on how organizational researchers choose to conduct themselves, casting ethics as a matter of beliefs, thoughts, and values as well as actions. For critical management researchers who are concerned with exposing and redressing the political

inequalities of organization an approach to research ethics, which takes into account the relative power and influence of the groups represented in the research might seem particularly appropriate (Bell and Bryman, 2007; Brewis and Wray-Bliss, 2008; Wray-Bliss 2003b). Such a position also evokes feminist and postmodern approaches to ethics that emphasize responsibility to the Other (Bauman, 1993; Benhabib, 1987) through valuing connectedness with the actual researched populations who may be particularly vulnerable or have historical reasons for being particularly suspicious of the intentions of academic researchers (McLaughlin and Tierney, 1993; Bishop, 2005). This approach might further necessitate organizational researchers devoting more attention to ethics. For example, by particular journals becoming more proactive in specifying ethical standards to which their prospective contributors must attend and encouraging greater discussion of ethical issues in published articles, or through the education of organizational researchers in a way which casts ethics as an ongoing aspect of their professional development. Most fundamentally, however, rather than only being relevant to certain methodological approaches, ethics would be treated as a central aspect of the conduct of all research involving organizations.

We have, in this final section, outlined a number of possible responses to ethical formalization in relation to organizational research. We do not regard any of these responses as ideal. Nor do we envisage the possibility of an ideal response, either for the communities of organizational researchers or for individual researchers. This is because matters of ethics centre on the fundamental question of how we should live. Moreover, while much moral philosophy has aspired to universal ethical pronouncement on the good life, we are wary of the potentially subordinating power effects of such claims. For us then, research ethics involves explicit consideration of how we should relate to research participants, other researchers, institutions that support research, and research

audiences, including those who read and apply our research. Such questions, as we have sought to emphasize throughout this chapter, do not lend themselves to easy solutions. Our position here has been, therefore, to suggest that ethical issues permeate the entire research process for all researchers whether positivist, interpretivist, postmodernist, and whether dealing with quantitative and/or qualitative data. Furthermore, while ethical formalization is a trend which in our view cannot be easily avoided, the way that each of us, as individual organizational researchers, engages with such challenges has the potential to vary enormously. We can all, of course, fall back on the path of least resistance and minimally comply with whatever research ethics principles are prescribed to us by our funding bodies, journals, supervisors, or university ethics committees. Alternatively, we can seek to embrace the possibilities that ethics introduces for reinvigorating and redefining the way that we think of and do organizational research.

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Rhetoric and Evidence: The Case of Evidence-Based Management

Mark Learmonth

INTRODUCTION

Evidence-based management has become a particularly hot topic, now, in the early years of the twenty-first century. And this chapter is in line with some of its ambitions; for example, it is intended, in part, to be a call for academics (and practitioners) to use more forms of evidence to inform practice. However, the chapter is also concerned to provide a critical exploration of some of the ideas about 'evidence' and 'management' that have become associated with evidence-based management. Or at least, evidence-based management in its most prominent form – which is to say the version that promises applications of science to management through which we can: 'hear and act on the facts [in order] to make more informed and intelligent decisions' (Pfeffer and Sutton, 2006: 14).

Curiously, however, for a movement that is so 'now', this promise is based on a version of science and evidence that is somewhat dated. For in today's intellectual environment, it is only rather traditional versions of science that fail to question the idea that scientific facts are the products of impersonal, disinterested, and rigorous observations concerning the evidence. This traditional view of science and evidence was most influentially propounded by Robert Merton (1973/1942) for whom science's 'truth-claims, whatever their source, are to be subjected to *preestablished impersonal criteria*; [t]he acceptance or rejection of claims entering the list of science is not to depend on the personal or social attributes of their protagonist; [o]bjectivity precludes particularism' (1973/1942: 270; italics in original). Evidence, in this traditional view, can be misunderstood, it can even be falsified or invented; nevertheless, the only judge we

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Job Satisfaction in Organizational Research

Alannah E. Rafferty and Mark A. Griffin

INTRODUCTION

Job satisfaction is one of the most frequently studied aspects of organizational life in the psychological sciences (Cranny et al., 1992; Fisher, 2000; Landy, 1989; Locke, 1969, 1983). Psychologists have argued that the way that an individual feels about work and the meaning that work holds in his or her life is a critical element of the employment experience (e.g., Judge et al., 2001a; Tait et al., 1989). The degree of interest in job satisfaction is reflected in the number of studies that have been published in the area. Entry of the search term 'job satisfaction' in the psycLIT database reveals well over 5,500 published studies in refereed journals. Importantly, however, as reflected in our analysis of articles published in the *Journal of Applied Psychology* and the *Journal of Organizational Behavior* since 2000, relatively few studies have focused on the job satisfaction construct itself.

As noted by Locke (1983), it is impossible to conduct a comprehensive review of the job satisfaction literature in a single chapter. We adopt a psychological perspective on

job satisfaction and focus on methodological issues that have emerged in the literature in the last few decades. We begin our review by exploring why job satisfaction has been the subject of such interest. Next, we examine classic studies, including the Hawthorne Studies (Roethlisberger and Dickson, 1939), Hoppock's (1935) and Schaffer's (1953) study, and Herzberg and colleagues' (1957, 1959) work. We then define the construct of job satisfaction, noting some of the methodological debates in the area. Next, we discuss the distinction between global and facet job satisfaction. We then examine the major methodological approaches and measurement instruments that have been used when studying satisfaction. Our analysis then turns to recent empirical research. We conduct a review of articles published since 2000 in two major organizational psychology journals, the *Journal of Applied Psychology* and the *Journal of Organizational Behavior*. Finally, we identify some emerging issues in the job satisfaction literature, including dispositional approaches to job satisfaction and multilevel research.

Why study job satisfaction?

There are a number of perspectives that motivate the study of job satisfaction. While our review of recent empirical studies reveals that job satisfaction is often treated as a dependent variable, some authors argue that job satisfaction is a crucial construct, because it mediates relationships between working conditions and organizational and individual outcomes (e.g., Dormann and Zapf, 2001). Researchers have been particularly interested in exploring relationships between job satisfaction and individual performance. The satisfaction-performance relationship has been the subject of hundreds of studies, and a number of meta-analyses have summarized results of these studies (Bowling, 2007; Iaffaldano and Muchinsky, 1985; Judge et al., 2001b; Petty et al., 1984). Landy (1989) described the ongoing search for confirmation of the link between job satisfaction and job performance as the 'Holy Grail' of Industrial and Organizational Psychology. Indeed, researchers have persevered despite evidence that there is only a weak relationship between these constructs. In the most recent meta-analytic review, Bowling (2007) concluded that the satisfaction-performance relationship is largely spurious and is influenced by individuals' dispositional characteristics. Researchers have also studied other outcomes, including voluntary turnover (e.g., Hom et al., 1992; Wright and Bonett, 2002), absenteeism (e.g., Tharenou, 1993), and counterproductive behaviors (e.g., Gottfredson and Holland, 1990), to name just a few.

A second perspective that has motivated research on job satisfaction is interest in the link between job and life satisfaction. Some researchers have argued that since work is a central life activity it is likely to influence individuals' satisfaction with life (e.g., Rain et al., 1991; Rode, 2004; Tait et al., 1989). Empirical research indicates that there is moderate positive relationship between job and life satisfaction ($r=.44$; Tait et al., 1989). However, as with research looking at satisfaction-performance relationships, authors have suggested that the job-life

satisfaction relationship is influenced by dispositional factors, which are related to both constructs (e.g., Heller et al., 2002; Judge and Hulin, 1993). We examine research exploring the relationship between dispositional characteristics and job satisfaction in greater detail later in this chapter. In the following section, we examine some classic studies that have had an important impact on the methodologies and approaches used when studying job satisfaction.

Historical perspectives

Research exploring job satisfaction began in the 1920s and 1930s with the Hawthorne Studies (Roethlisberger and Dickson, 1939) and work by Hoppock (1935). Although these two sets of studies were conducted at roughly the same time, the Hawthorne Studies have had the most enduring influence on job satisfaction research (Locke, 1983). We also describe studies conducted by Schaffer (1953) and Herzberg and his colleagues (1957, 1959).

The Hawthorne Studies

The Hawthorne Studies were a series of studies conducted from 1924 to 1932, which provided the impetus for research focused on psychological and social factors in the workplace and are generally acknowledged as one of the starting points of the Human Relations school of thought (Judge and Larsen, 2001; Locke, 1969, 1983). This series of studies was initially designed to study the effects of work conditions, such as the timing of rest breaks, the length of the working day and pay rates, and on worker fatigue. However, the emphasis soon shifted to the study of 'attitudes,' as workers did not respond in expected ways to systematic changes in working conditions. Roethlisberger and Dickson's (1939) use of the term 'attitudes' encompasses operators' attitudes to management, their moods in the workplace, and the reactions to their supervisor and the experimental team.

We draw on one of the early studies, the Relay Assembly Room study, to provide an example of how 'attitudes' were assessed in the Hawthorne Studies. The Relay

Assembly Room study involved a small group of female workers who were isolated in a room from the rest of employees. In one of the 13 experimental periods, a survey consisting of eight questions was administered to operators. This initial questionnaire assessed whether operators had experienced any changes in their 'mental health habits and mental attitudes.' An additional questionnaire was then administered that focused on specific issues in the test room that could be responsible for these positive responses to the test room. Twenty-one questions were asked of operators, who suggested that they liked the smaller group in the test room, liked having 'no bosses,' enjoyed the 'freedom' available in the test room, and 'the way they were treated.' In summary, in the Relay Assembly Test Room study researchers were beginning to focus on individuals' cognitive and affective responses to their job and were interested in determining the particular aspects or facets of work that were responsible for these responses.

The Hoppock Study

Hoppock (1935) published the first intensive study of job satisfaction, based on interviews with adults in a small town and 500 schoolteachers. Hoppock's research was driven by two underlying questions (Landy, 1989). First, are workers happy? Second, are some workers happier than others are? Hoppock used verbal self-reports collected via interviews to assess job satisfaction. When summarizing his findings, he identified many factors that could influence job satisfaction, including worker fatigue, job conditions such as monotony, supervision, and autonomy. Results indicated that only 12% of workers could be classified as dissatisfied and that occupational groupings influenced satisfaction with higher status occupational groups reporting higher satisfaction. In summary, Hoppock began to systematically analyze job satisfaction and identified a range of factors contributing to job satisfaction that are still studied today (e.g., Rose, 2003).

Schaffer (1953)

Schaffer (1953) argued that whatever psychological mechanisms make people satisfied or dissatisfied in life also make them satisfied or dissatisfied in their work. He suggested that the amount of dissatisfaction experienced by an individual is determined by; a) the strength of an individual's needs or drives, and b) the extent to which an individual can perceive and use opportunities in the situation for the satisfaction of those needs. Schaeffer identified 12 needs that need to be met and overall satisfaction was said to be predicted by the two most important needs held by a person. If the individual's two most important needs are met by the job then they are satisfied.

Based on his analysis, Schaffer (1953) developed a questionnaire measure to assess the strength of each of the 12 needs, the degree to which each of the needs were satisfied by the individual's job, and the individual's overall satisfaction. The overall satisfaction scale consisted of three items, two of which were based on the work of Hoppock (1935), while the third item was an open-ended question. In summary, Schaffer's work acknowledged that individual differences are an important antecedent of job satisfaction, a topic that has recently received considerable attention.

Herzberg: The two-factor theory

By the late 1950s, Herzberg and colleagues (1957) were able to review the available job satisfaction literature in a systematic way. They concluded that there was sufficient evidence to confirm a relationship between satisfaction and outcomes such as performance, turnover, absence, and psychosomatic illnesses. In addition, Herzberg et al. (1957) concluded that satisfaction and dissatisfaction were two distinctly different phenomena. Herzberg et al. (1959) conducted a follow-up study to explore this idea, and they interviewed 203 accountants and engineers. Herzberg et al. (1959) concluded that specific factors were related to feeling positive about the job, including: achievement and recognition, the work itself, responsibility, advancement or promotion opportunities, and salary.

In contrast, dissatisfaction with a job was related to another set of factors, including: company policies and administration, technical supervision, salary, relationships with supervisors, and working conditions.

On the basis of this study, Herzberg and his colleagues (1959) proposed the two-factor theory or the motivator-hygiene theory and a number of empirical tests of the two-factor model were conducted (e.g., Ewen et al., 1966; Graen, 1966). Ewen et al. tested the theory using a sample of 793 male employees from a range of different jobs and failed to support the theory. Subsequent research also failed to support the two-factor theory and research on this topic declined dramatically. Landy (1989) suggests that while this theory is probably reasonable at the descriptive level, it is not an explanation of how job satisfaction develops and of its relationships with other constructs. Despite the limitations of the theory, it stimulated research into job satisfaction and prompted new directions of investigation.

Defining job satisfaction

The definition of job satisfaction has been debated throughout its history. Many researchers have defined job satisfaction as an emotional reaction to the job (e.g., Cranny et al., 1992; Locke, 1969; Spector, 1997). Cranny and colleagues (p. 1) state that 'there is general agreement that job satisfaction is an affective (that is, emotional) reaction to a job that results from the incumbent's comparison of actual outcomes with those that are desired (expected, deserved, and so on).' Locke (1969: 316) defined job satisfaction as 'the pleasurable emotional state resulting from the appraisal of one's job as achieving or facilitating the achievement of one's job values.' Although job satisfaction is usually described as an affective response, it is usually measured as an evaluative assessment of job attitudes compared to an individual's internal or external standards (Fisher, 2000; Niklas and Dormann, 2005; Pekrun and Frese, 1992).

The distinction between affect and evaluation has been addressed by research in the

social psychology of attitudes, and recently, scholars have begun to draw explicitly on the attitudes literature when defining job satisfaction (e.g., Fisher, 2000). Fisher identified two components of job attitudes, an affective or emotional component and a cognitive component. Weiss (2002) identified three distinct, but related aspects, of job satisfaction. He argued that job satisfaction incorporates an individual's evaluation of their job, their beliefs about their job, and their affective experiences on the job. Importantly, however, Weiss identifies evaluation as the central component of job satisfaction and in line with this statement, he defined job satisfaction as 'a positive (or negative) evaluative judgment one makes about one's job or situation' (p. 175). An important consequence of this perspective is that affect and beliefs are understood to be different causes of job satisfaction.

The definitional debates in the job satisfaction literature have important implications for how we measure job satisfaction and the most appropriate research designs to use. Weiss (2002) argues that current job satisfaction measures are limited, because they do not enable researchers to assess the three component model of job satisfaction that he identified. He argues that since affective states and beliefs structures have important behavioral and cognitive consequences, then they are likely to have added value over the cognitive evaluations, which have been the focus of measurement efforts to date.

Some empirical research has explored the extent to which existing job satisfaction measures capture cognitive versus affective information. Brief and Roberson (1989) conducted a study with 144 students, and explored the extent to which the Job Descriptive Index (JDI; Smith et al., 1969), the Minnesota Satisfaction Questionnaire (MSQ; Weiss et al., 1967, and the female Faces Scale (Dunham and Herman, 1975) captured cognitions and positive and negative affect. The different measures of job satisfaction varied in the extent to which they captured the affective and cognitive aspects of job satisfaction. Brief and Roberson suggest their

results as suggesting that the Faces measure is the 'most balanced' of the scales as it displayed relationships with cognitions and positive and negative affect.

Similarly, Fisher (2000) argued that it is useful to think of existing job satisfaction measures as lying on a continuum assessing the cognitive component of job attitudes to assessing a combination of cognitive and affective components. Based on an empirical study that used an experience sampling methodology to collect data from 121 employees over a two-week period, Fisher concluded that most job satisfaction measures only assess affect to a limited degree. In contrast to other researchers, Judge and Larsen (2001) argued that it is not productive to classify job satisfaction measures as either cognitive or affective. They argued that there is no need to measure new, affectively laden job satisfaction measures or to supplement or replace job satisfaction measures with measures of affect. Judge and Larsen argue that while cognition and affect can help us better understand the nature of job satisfaction, separate measures of these aspects of job satisfaction are not likely to prove useful.

Niklas and Dormann (2005) argue that findings suggesting that existing job satisfaction measures primarily tap the cognitive components of satisfaction have important implications. While cognitions are relatively stable over time, affective reactions are unstable, so different methodologies are required to assess cognitions and affect. In addition, while the issue of time frame is relatively unimportant when measuring cognitions, this issue is critical when assessing affect, as retrospective reports of affect tend to be biased. As such, there is a need to assess affect on a time-contingent basis. To date, however, researchers studying relationships between trait and state affect and job satisfaction have retrospectively measured affect over the last weeks, months, or years (Niklas and Dormann, 2005). This is a critical limitation of existing approaches, because people tend to overestimate the intensity and frequency of positive and negative affect in retrospective measures, and retrospectively reported affect

is likely to be affected by current affect (Fisher, 2002). In addition, negative affect is more easily recalled than positive affect (e.g., Baumeister et al., 2001).

In summary, there is still considerable debate concerning how to define job satisfaction. This debate has important methodological implications. Our review suggests that the different definitions of job satisfaction require authors to develop different measures of satisfaction. For example, if we define job satisfaction as an attitude that consists of cognitive evaluations, affect, and beliefs then theorists argue that there is a need to develop measures of affect and beliefs independent of evaluations. Niklas and Dormann's (2005) work highlight the fact that affective reactions are unstable, which means that different methodologies are required to assess cognitions and affect. Researchers have recently begun to address the methodological issues associated with assessing affect when studying relationships between dispositional factors and job satisfaction.

Facet satisfaction versus overall job satisfaction

Most scholars recognize that job satisfaction is a global concept that also comprises various facets (Ironson et al., 1989; Judge and Larsen, 2001; Smith, 1992). Researchers have examined relationships between global job satisfaction and facet satisfaction (Smith, 1992; Weiss, 2002), the impetus for which lies with Locke (1969). He suggested that overall job satisfaction is the sum of the evaluations of the elements of which satisfaction is composed. Global or overall job satisfaction scales ask an individual to combine his or her reactions to a job in an overall or integrated response (e.g., all things considered, how satisfied are you with your job?). In contrast, facet satisfaction scales are designed to examine specific aspects of job within the more general domain (Ironson et al., 1989). The most typical categorization of facets was proposed by Smith et al. (1969), who identified five facets, including: pay, promotion, co-workers, supervision, and the

work itself. Another common distinction is between extrinsic and intrinsic satisfaction. Satisfaction with pay and promotions has been considered forms of extrinsic satisfaction. In contrast, satisfaction with co-workers, supervisors, and the work itself are considered to reflect intrinsic satisfaction (Judge and Larsen, 2001).

Empirical studies have indicated that global measures of job satisfaction and facet satisfaction measures are only weakly to moderately related (e.g., Aldag and Brief, 1978). These findings have resulted in a debate as to why global measures of job satisfaction are not equivalent to the sum of facet satisfactions (e.g., Highhouse and Becker, 1993; Scarpello and Campbell, 1983). Scarpello and Campbell examined relationships between single item global measures of job satisfaction and facet measures of satisfaction taken from the short-form of the MSQ (Weiss et al., 1967). These authors concluded that it is not appropriate to use the sum of the facet measures as a measure of overall job satisfaction as global measures of job satisfaction appear to capture information relating to satisfaction with occupational choice, life off the job, in addition to capturing information about job satisfaction. As such, global evaluations of job satisfaction are more complex than the sum of the job satisfaction facets that are typically measured. In summary, empirical research findings suggest that global satisfaction and facet satisfaction measures assess distinctly different issues and are not equivalent measures of job satisfaction.

The facet satisfaction and global job satisfaction distinction also becomes problematic if we consider Weiss's (2002) discussion of job satisfaction. In particular, if job satisfaction is a cognitive evaluation then Weiss argues that the facets of job satisfaction are not distinguishable from the overall job satisfaction evaluation. Rather, Weiss suggests that there 'are only discriminable objects in the work environment that we evaluate' (p. 186). The basic argument being proposed by Weiss is that while we may evaluate issues such as how satisfied we are with our supervisor or our pay, these

evaluations combine to create an overall evaluation of how satisfied we are in the workplace. This argument suggests that what have been called facets of job satisfaction are simply evaluations that individuals make about their work environment. In summary, an analysis of existing research suggests that global measures of job satisfaction appear to tap a broad domain of satisfaction that incorporates life satisfaction. In contrast, while facet job satisfaction measures are designed to assess a broad domain they appear to assess a more limited and focused aspect of job satisfaction involving specific evaluations. In addition, the recent emphasis on job satisfaction as an evaluation that is caused by affect and beliefs also calls into question the facet versus global satisfaction distinction.

Research design issues in the study of job satisfaction

The predominant methodological approach that has been used when studying job satisfaction is the field study, with authors using both cross-sectional and longitudinal approaches. While early research on job satisfaction used qualitative data collection approaches such as interviews, very little recent research has adopted this approach. In addition, a number of quasi-experimental field studies have also been conducted that assess job satisfaction. However, job satisfaction has not been the key variable of interest in these studies. More recently, with increasing interest in the role of affect in job satisfaction, a number of studies have focused on intraindividual processes. These studies have used experience sampling methodologies. In the following section, we focus on exploring early research that used interviews, quasi-experimental studies and studies using experience methodologies to study intraindividual processes.

Qualitative studies

Qualitative studies are typically conducted in applied settings and are generally broadly focused studies that are exploratory in that they are not driven by explicit hypotheses.

These studies are designed to gather data on individual experience and meaning, and many different methods such as interviews, conversations, observations, personal experiences, and textual analysis can be used to collect data. Qualitative studies emphasize depth of information and analysis as opposed to breadth of information and analysis (Kidd, 2002). Next, we explore studies that have adopted qualitative approaches when exploring job satisfaction.

A number of early researchers used techniques such as interviews in order to develop an understanding of the nature and antecedents of job satisfaction (e.g., Hoppock, 1935; Herzberg et al., 1959). For example, Hoppock interviewed adults in a small town and 500 schoolteachers in order to explore whether workers were happy in their jobs and whether some individuals were happier than others. However, a search of recent studies using PsycLIT revealed that very few authors have used qualitative approaches to study job satisfaction. In those studies that we did identify that used some type of qualitative approach, interviews were generally used as a supplement to quantitative analyses rather than the primary data collection approach (e.g., Lyons and O'Brien, 2006). For example, Lyons and O'Brien conducted a study in order to explore the determinants of job satisfaction for a sample of African-American employees. These authors collected data using surveys and also collected data using an interview to administer an open-ended question ('what makes you or would make you a satisfied employee?'). Lyons and O'Brien concluded that the interview data was consistent with the quantitative results.

We identified one study that did use an interview as the primary data collection tool. Bussing and Bissels (1998) conducted an exploratory study to test a model of work satisfaction and conducted semistructured interviews with 46 German nurses. Participants were asked to give a detailed description of issues such as their working situation and their overall satisfaction or dissatisfaction in the workplace. Bussing and Bissels argue that the use of qualitative

analysis allowed them to identify substantially different qualities of work satisfaction. In addition, the authors argue that they were able to identify apparently inconsistent statements that reflected the contradictory experiences and pressures placed on nurses in the workplace. In summary, we identified very few recent studies that have used qualitative methods when studying job satisfaction. One potential explanation for the lack of such studies is that the amount of qualitative work published in APA journals in the last 50 years is quite small (Marchel and Owens, 2007). Despite this, however, Bussing and Bissels' (1998) work suggests that there are important benefits to be gained from using qualitative methods when studying job satisfaction.

Quasi-experimental field studies

A number of researchers have conducted quasi-experimental studies where job satisfaction has been included as a measure of interest (e.g., Lam and Schaubroeck, 2000a, 2000b; Logan and Ganster, 2005; Seibert, 1999; Workman and Bommer, 2004). A quasi-experiment is an 'experiment[s] that ha[ve]s treatments, outcome measures, and experimental units, but do[es] not use random assignment to create the comparisons from which treatment-caused change is inferred' (Cook and Campbell, 1979: 6). Rather, comparisons occur between a nonequivalent control group and an experimental group or groups who differ in many ways apart from the treatment of interest. One of the challenges of using a quasi-experimental approach involves determining what effects are attributable to the treatment of interest versus those effects due to the initial noncomparability of the experimental and control group. However, quasi-experimental studies are important, because they allow researchers to study causal relationships in a complex field setting. In addition, these studies also allow us to determine whether results obtained in the laboratory generalize to real world environments. In the case of job satisfaction and many other organizational constructs, it is essential that we study these variables *in situ* using rigorous methods that

allow us to consider issues of causality. In the next section, we examine one quasi-experimental study that incorporated job satisfaction.

Seibert (1999) conducted a pretest-posttest nonequivalent control group quasi-experimental design, which was designed to test the effectiveness of a formal mentoring program in a Fortune 100 company over a one year period. Job satisfaction was a key outcome variable in this study. Results suggested that the facilitated mentoring program had a significant effect on job satisfaction. One year after the initiation of the program, employees who had been mentored reported significantly higher job satisfaction than their nonmentored counterparts. In summary, we could only identify a small number of quasi-experimental studies that assessed job satisfaction as one of many outcomes of interest. While quasi-experimentation has been discussed as a powerful tool for drawing casual inferences in applied settings (Cook and Campbell, 1979; Cook et al., 1992), researchers do not seem to have fully utilized this approach when studying job satisfaction. We argue that one issue that researchers can focus on when studying job satisfaction is the use of quasi-experimental research.

Intraindividual processes

The majority of job satisfaction research has focused on differences between individuals. In recent years, however, increasing attention has been paid to the way individuals' job satisfaction changes over time. Although earlier research had investigated the stability of satisfaction (e.g., the genetic basis of job satisfaction; see Arvey et al., 1989), little attention had been paid to the fluctuations in satisfaction from month to month, day to day, or within even shorter time periods. The development of diary studies and other experience sampling methodologies has encouraged a better understanding of these fluctuations within individuals. The traditional cross-sectional, between-subjects approach used in job satisfaction research assumes that constructs are stable over time

and that variations around the average level of a variable are random (Ilies and Judge, 2002). Experience sampling methods allowed the possibility that this within-person variance is systematic and meaningful. From this perspective, job satisfaction might develop over time, change in response to transient events, be moderated by more stable factors, and might itself influence diverse within-person factors such as insomnia for example. We discuss one study that has explored intraindividual processes in the following section.

As described by Ilies and Judge (2004) experience sampling methodologies ask individuals to report on their momentary feelings or their subjective feeling states, and/or to report on their current physiological state. This approach has been said to be beneficial, because it eliminates error associated with selective recall processes. An example of what experience methodology involves can be seen in a study conducted by Ilies and Judge. These researchers conducted a study involving 33 participants from two state universities in the United States. In the first phase of data collection, participants completed a measure of trait affectivity and asked a significant other to complete the same measure. In Phase 2, one week later, participants reported their momentary mood and job satisfaction three times a day, for two weeks, using a diary method. That is, participants were asked to record their responses to survey questions assessing mood and job satisfaction at regular intervals throughout the day. The measure of job satisfaction collected through this approach was called 'experience-sampled' job satisfaction. In the final phase of the study, Phase 3, which occurred two months after Phase 2, participants responded to an overall job satisfaction survey and reported on their beliefs about their job. The results of this study revealed that experience-sampled job satisfaction was associated with the overall job satisfaction measure collected in Phase 3. In summary, a number of research studies have studied intraindividual processes and job satisfaction. This methodology offers an

important means by which to understand the impact of affect on job satisfaction.

Measurement of job satisfaction

A number of different techniques have been adopted when studying job satisfaction. By far the most common approach involves the use of questionnaires. A variety of questionnaire measures have been developed over the last 70 years. Some of the most commonly used and well-validated measures are the JDI (Smith et al., 1969) and the MSQ (Weiss et al., 1967). Two other measures that are commonly used when assessing job satisfaction include the Michigan Organizational Assessment Questionnaire (MOA; Cammann, Fichman, Jenkins, and Klesh, 1983), and Brayfield and Rothe's (1951) Overall Job Satisfaction Measure. In the next section, we discuss these measures in some detail.

Questionnaire measures

Job Descriptive Index The JDI was developed by Smith et al. (1969) and the original scale consisted of 72 items that assessed five facets of job satisfaction, including: satisfaction with work itself, pay, promotion opportunities, supervision, and co-workers. Roznowski (1989) assessed the JDI and made a number of changes to the measure. In particular, modifications were made to reflect issues such as changes in technology that have occurred since the original scale was developed. Roznowski demonstrated that the items loaded on five factors and that the scales have good reliability, which has been supported by other researchers (e.g., Judge and Hulin, 1993).

Minnesota Satisfaction Questionnaire The 'long-form' of the MSQ consists of 100 items that assess 20 subscales, such as individuals' satisfaction with the extent to which they get to use their abilities on the job, achievement, advancement, moral values, and so on. Twenty of the items in the MSQ have been used to assess general job satisfaction and form the 'short-form' of the MSQ. The short-form consists of 12 questions assessing

intrinsic satisfaction. Intrinsic satisfaction items measure issues such as accomplishment obtained from one's job and opportunities to use abilities on one's job. Eight items assess extrinsic satisfaction. Examples of issues assessed in the extrinsic scale include satisfaction with promotion opportunities, and satisfaction with pay. The short-form of the MSQ has been shown to have good reliability (Mathieu, 1991; Wong et al., 1998).

Michigan Organizational Assessment Questionnaire Cammann et al. (1983) developed a measure of overall job satisfaction when they developed the Michigan Organizational Assessment Questionnaire. The satisfaction scale consists of three items such as 'all in all, I am satisfied with my job.' This scale had demonstrated adequate reliability (e.g., McFarlin and Sweeney, 1992).

Brayfield and Rothe's (1951) Overall Job Satisfaction Measure Brayfield and Rothe (1951) developed an 18-item measure of job satisfaction. However, a number of researchers have adopted a 6-item version of this measure and this measure has displayed adequate reliability (e.g., Aryee, Fields and Luk, 1999). The 18-item measure has also demonstrated adequate reliability (Pillai, Schriesheim and Williams, 1999).

Graphic measures

Faces Scales Kunin (1955) developed the General Faces Scale because he stated that attitude surveys, which translate 'one man's feelings into another man's words' (p. 65), result in distortion of meaning. To address this issue, Kunin conducted research to establish scale values for two series of faces that varied on a continuum from happy to unhappy. Respondents are asked to circle the face that reflects the way that they are feeling about their job. Dunham and Herman (1975) developed a female version of the faces scale, and demonstrated that the male and female versions can be used interchangeably to measure job satisfaction. The Faces Scales have been used by a number of researchers (e.g., Dunham, Smith, and Blackburn, 1977;

Niklas and Dormann, 2005; Rousseau, 1978). Authors such as Fisher (2000) and Niklas and Dormann argue that the faces scales are affectively laden measures, as they found that job satisfaction as assessed by the General Faces Scale is strongly determined by state affect.

Recent empirical research

To assess the range and focus of recent job satisfaction research, we conducted a review of studies published since 2000 in the *Journal of Applied Psychology (JAP)* and the *Journal of Organizational Behavior (JOB)*. We identified 84 studies assessing job satisfaction with fifty of these empirical studies published in *JAP*, while 34 studies were published in *JOB*. In the following section, we outline some of the methodological characteristics of these studies. Job satisfaction was treated as a dependent variable in the majority of these studies. Specifically, in 41 of the 50 studies published in *JAP* and 29 of the 34 studies published in *JOB*, job satisfaction was treated as a dependent variable. Only nine *JAP* studies and four *JOB* studies treated job satisfaction as an independent variable. Studies where satisfaction acted as a mediator or moderator were included in the independent variable study count. Researchers in two of the major organizational psychology journals appear to use job satisfaction primarily as a broad outcome measure and have paid less attention to the meaning of the construct and its impact on other outcomes.

Next, we summarized the measures that were used to assess job satisfaction in *JAP* and *JOB* since 2000. Analysis revealed that 15 studies used either the Michigan Organizational Assessment Questionnaire (Cammann et al., 1983) or Brayfield and Rothe's (1951) measure. The next most commonly used measure was the JDI (Smith et al., 1969), which was used six times. The MSQ (Weiss et al., 1967) was used in four of the studies. Importantly, however, 13 studies used measures specifically designed for the research or did not clearly identify where the job satisfaction measure was obtained

from. Another notable feature of the measures used was that over 40 separate measures of satisfaction were used in the 84 studies. This result indicates that there is little consistency in the use of job satisfaction measures.

Finally, we reviewed the research methodology used when conducting job satisfaction studies in *JAP* and *JOB*. Our analysis suggests that both cross-sectional and longitudinal field studies were used. Of the studies published in *JOB*, 26 adopted a cross-sectional survey approach while seven studies used a longitudinal survey methodology. One study (Workman and Bommer, 2004) used a quasi-experimental methodology when collecting data in the field. There was a very even split between cross-sectional survey studies and longitudinal survey studies in *JAP*.

Emerging issues

Our review of the job satisfaction literature reveals that interest in this construct has not waned in the 80 years since its inception. Indeed, researchers have continued to study questions concerning the relationship between job satisfaction and performance relationship despite discouraging results (e.g., Bowling, 2007). Recent theoretical developments regarding the dispositional influences on job satisfaction and methodological advancements concerning the measurement of the affective aspects of job satisfaction, suggest that interest in satisfaction will continue to grow. Our review of recent job satisfaction literature reveals a number of emerging trends. In particular, increasing attention has been devoted to exploring the role of dispositional factors as antecedents of job satisfaction and as third variables responsible for job satisfaction-outcome relationships. A second theoretical issue that has emerged in recent research are multilevel studies of job satisfaction. Next, we explore these issues in some detail.

The role of dispositional factors

In the past, cognitive judgement approaches (e.g., Locke's (1976) needs-values conflicts

approach) and social influence approaches (e.g., Salancik and Pfeffer, 1978) have dominated the field as explanations for the causes of job satisfaction. In the past two decades, however, increasing attention has focused on the dispositional bases of job satisfaction (e.g., Staw et al., 1986; Staw and Ross, 1985; Weiss, 2002). Staw and Ross stimulated interest in this perspective when they argued that previous research had overestimated the influence of the situation on job satisfaction and underestimated the impact of an individual's dispositional characteristics. The dispositional approach suggests that a person's job satisfaction reflects their tendency to feel good or bad about life in general. This tendency to feel good or bad is independent of the job itself and its positive or negative features (Weiss and Cropanzano, 1996).

Researchers interested in the satisfaction-performance relationship have begun to explore the role of dispositional characteristics as an explanation for the relationship between these two constructs. Bowling (2007) concluded that the relationship between satisfaction and performance is largely spurious. Results of this meta-analysis revealed path coefficients of 0.09 to 0.23 between satisfaction and performance after personality was controlled. Judge et al. (2002) conducted a meta-analytic review of relationships between job satisfaction and the 'Big 5' personality traits (Goldberg, 1990). This review indicated that neuroticism, extraversion, and conscientiousness, displayed moderate relationships with job satisfaction. Neuroticism was most strongly associated (negatively) with job satisfaction, followed by conscientiousness, which displayed a positive association with job satisfaction. However, the 80% credibility interval for conscientiousness included zero, suggesting that this result may not be replicable. Other dispositional characteristics that have been the subject of considerable attention in recent times include trait affectivity (Thorensen et al., 2003), and core self-evaluations (Judge et al., 2005; Judge and Bono, 2001). Below, we examine research on positive and negative affectivity and then we

examine the research addressing the role of core self-evaluations.

Positive affectivity and negative affectivity

A great deal of attention has been directed towards examining relationships between positive affectivity (PA) and negative affectivity (NA) and job satisfaction (e.g., Thorensen et al., 2003). People high on positive affectivity tend to be sociable, lively, and are often in a positive mood. In contrast, people who are high on negative affectivity tend to be more distressed, unhappy, and irritable (Watson et al., 1988). A number of theoretical models have been developed to explain the relationship between job satisfaction and trait affectivity. Weiss and Cropanzano (1996) developed Affective Events Theory (AET), which focuses on affective experiences at work as proximal drivers of affective reactions, which influence attitudes and behaviors. This model implies that job events mediate relationships between dispositional affectivity and job satisfaction. Judge and Larsen (2001) proposed the stimulus-organism-response (SOR) model. The basis of this model is that PA and NA influence individuals' responses and judgments by moderating relationships between a stimulus and the organism. In addition, this model argues that affect mediates relationships between an organism and their responses. Finally, Forgas and George (2001) proposed the affect infusion model (AIM). This approach suggests that individual's attitudes to their job are partially a function of the affect that 'infuses' their cognitive processing when they are forming evaluations of objects. As such, the AIM is a direct effects model.

A number of empirical studies have explored relationships between trait and state affect and job satisfaction. These studies involve intraindividual processes described previously in this chapter. For example, Ilies and Judge (2002) examined the within-person relationship between mood (state affect) and job satisfaction, and also examined the role of

personality in moderating these relationships. Twenty-seven employees completed mood and job satisfaction surveys at four different times a day for four weeks. The results of this study indicate that 36% of the total variance in job satisfaction was due to within-person factors and mood explained 29% of this within-person variance. In addition, mood and job satisfaction were related both within and across individuals. Results also indicated that within-person variability in mood was related to within-person variability in job satisfaction and variability in mood and job satisfaction was predicted by neuroticism. Finally, individuals who scored higher on neuroticism were more likely to have their mood affect their job satisfaction.

A number of meta-analyses have summarized relationships between trait affectivity and state affect and job satisfaction (e.g., Thorensen et al., 2003). In the most recent review, Thorensen et al. reported an estimated corrected mean population correlation of 0.27 between trait PA and satisfaction, while state PA had an estimated mean population correlation of 0.38 with satisfaction. Trait NA had an estimated mean population correlation of -0.27 with job satisfaction while state NA had an estimated mean population correlation of -0.31. However, Niklas and Dormann (2005) argue that the conclusion that there is a relationship between trait affectivity and state affect and job satisfaction is problematic for a number of reasons. While results have been interpreted as suggesting that state affect is a result of job events, which then causes job satisfaction, these relationships are open to interpretation, because trait affectivity was not controlled when estimating relationships between state affect and job satisfaction. As such, the correlation between state affect and job satisfaction may in fact be spurious. In addition, Niklas and Dormann argue that there is a need to determine whether state affect only influences job satisfaction at the time when it is measured or whether state affect has a lasting influence on satisfaction.

Niklas and Dormann (2005) conducted a two week diary study with 91 employees to address these concerns. Participants

completed questionnaires four times a day. Facet job satisfaction was assessed as was general satisfaction using the Faces Scales. Trait job satisfaction and trait affect were measured and participants were asked to indicate how satisfied they usually are with work and how they usually feel. When state job satisfaction and state affect were assessed, participants were asked to indicate their current attitude toward their job and their current affect. The results of this study revealed that within-person aggregated state job satisfaction was strongly correlated with generalized job satisfaction. In addition, state affect and state job satisfaction were related even when trait affect and generalized satisfaction are controlled. Niklas and Dormann suggest that this study provides evidence that the relationship between state affect and job satisfaction is not due to the influence of a third factor, trait affectivity. Rather, they conclude that positive or negative work events lead to state affect, which has an impact on job satisfaction above and beyond that of affective disposition. Importantly, Niklas and Dormann state that job satisfaction depended more on affect elicited by current affairs than on personality dispositions. Because trait affectivity was controlled, Niklas and Dormann argue that state affect does not merely represent a mediator between trait affect and satisfaction. Rather, there must be situational factors that elicit state affect, which lead to changes in job satisfaction.

Core self-evaluations (Judge et al., 2005) defined core self-evaluations as fundamental, subconscious conclusions that individuals reach about themselves, other people, and the world. Four core self-evaluations – self-esteem, generalized self-efficacy, neuroticism, and locus of control – have been identified. Core self-evaluations can help elucidate the psychological processes underlying the dispositional source of job satisfaction. A growing number of empirical studies have explored the influence of core self-evaluations on job satisfaction. For example, Judge et al. (1998) conducted an empirical study and reported that core self-evaluations have consistent effects on job satisfaction,

independent of the attributes of the job itself. Judge and his colleagues state that results indicated that core self-evaluations had an indirect influence on job satisfaction, via their impact on individuals' perceptions of their job's attributes. These perceptions then influence job satisfaction.

Despite the progress that has been made in accounting for dispositional influences on job satisfaction, Johnson, Rosen, and Levy (in press) identified a number of theoretical and measurement issues with the core-self evaluation construct that are of particular concern. We focus on just two of the issues identified. First, Johnson et al. argue that, to date, core self-evaluation has been discussed as a superordinate construct with effects indicators, such that this construct is responsible for levels of self-esteem, generalized self-efficacy, locus of control, and neuroticism. In such models, causality flows from the construct to the traits, which are manifestations of the construct. However, Johnson et al. argue that core self-evaluation is more accurately modeled as an aggregate construct. In an aggregate model, core self-evaluation would not cause the traits but be caused by them. This is a critical distinction, because if core self-evaluation is modeled as a superordinate construct, then the likelihood of uncovering true effects is reduced. Second, the measurement of core self-evaluation does not align with how it is conceptualized. The use of summed scores is inappropriate, as an aggregate construct cannot be adequately represented by a summed score.

Multi-level studies of job satisfaction

Job satisfaction has also been incorporated in multilevel processes involving teams and organizations. Many permutations of relationship are possible when complex mediational processes are considered at multiple levels of analysis. We consider two broad and important types of multilevel problems that involve job satisfaction. The first multilevel problem involves top down processes whereby team and organizational processes shape the experience of job satisfaction of individuals. In these studies, predictors at

the team and organization level of analysis influence individual experiences of job satisfaction. For example, Jex and Bliese (1999) found that the collective self-efficacy of 36 US army companies positively influenced individual experiences of job satisfaction. In addition, collective self-efficacy moderated the relationship between work overload and job satisfaction such that work overload was less negatively related to job satisfaction in units with higher levels of collective self-efficacy. Other multilevel studies have investigated the way individual satisfaction has been shaped by aggregate processes such as leadership (Griffin et al., 2001), organizational justice (Mossholder et al., 1998) group racial composition (Mueller et al., 1999), and demographic diversity (Wharton et al., 2000).

Second, researchers have incorporated job satisfaction as an aggregate construct within systems of other variables that are assessed beyond the individual level. This approach seeks to extend theoretical relationships obtained at the individual level to higher levels of analysis (Chen et al., 2005). Ostroff (1992) invigorated research of this kind when she proposed that aggregate job satisfaction would be related to organizational performance. Her study extended the satisfaction-performance debate to the organizational level of analysis and provided a theoretical framework for understanding how individual attitudes might influence organizational outcomes. Patterson, Warr and West (2004) further extended this thinking to show that aggregate job satisfaction mediated the link between a company's organizational climate and objective measures of company performance. They argued that aggregate job satisfaction captured affective experiences that arose from the practices and procedures enacted in the company. In summary, researchers are increasingly concerned with studying multilevel relationships between job satisfaction and team and organizational variables. This research has opened up a whole new realm of possibilities concerning the role and the importance of job satisfaction.

CONCLUSION

Job satisfaction has been an active and productive area of research since the 1930s. Our review has revealed that there is still considerable interest in job satisfaction and that a growing number of methods are being used to study satisfaction. In particular, we reviewed studies using methodologies such as interviews, questionnaires, and diary studies. Recent developments in the methods used to study the affective aspects of job satisfaction reflect the continuing growth and sophistication of research methods and designs in the area. Overall, our review suggests that while working roles change for individuals and theoretical perspectives fade or rise for scholars, the experience of job satisfaction continues to play a central role in understanding life at work. We expect that this interest will continue across the spectrum of issues raised in this chapter. Debate regarding the definition and nature of job satisfaction will continue as insights grow from new methodologies, cross-cultural perspectives, and changing theoretical paradigms. This debate is important and will generate new methodologies as researchers seek to explore the implications of new definitions for theory, measurement, and practice. Recent advances in intraindividual and multilevel designs suggest that enormous scope remains for embedding the notion of job satisfaction within broader theories and practices. We conclude that the study of job satisfaction is a healthy topic of research that will continue to enhance understanding of how people interact with organizations.

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Studying Organizational Populations Over Time*

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INTRODUCTION

Most social scientists agree that formal organizations occupy a central role in modern society in that most collective action occurs within them. There is also an emerging consensus, evidenced by the research designs organizational scientists typically use, that a powerful way of studying organizations involves the collection and analysis of data over time. Yet, issues about how to study organizations over time sometimes engender confused debate. In this chapter, we aim to clarify some of the main issues involved, with a special focus on studying organizational populations over time. Throughout the chapter, we illustrate our general points with specific examples drawn from our ongoing study of the historical worldwide population of tape drive producers.

What is an organizational population?

Early studies of organizational populations were often simply called an 'industry study.'

In a typical study of this kind, analysts study a single industry over time by collecting information on all the firms or other organizations operating in a market. For instance, Epstein (1927) conducted a study of failure among American automobile producers. Such studies continue today (see Klepper, 2002; Mitchell, 1994; Tripsas, 1997). The definition of industry or market used in industry studies may be explicit and clear. However, it is usually not theoretically reasoned in any depth and often simply follows common sense or convention, as incorporated in directories, reports, or archives (used as sources for data). As a result, what is regarded as the relevant organizational population varies significantly from one study to the next – e.g., in some cases, it implies legal incorporation, in others, the production of goods or services, in yet others, the sale of goods or services possibly produced by other parties.

Attention to the conceptual foundations of the population-based design can be traced to the population ecology perspective on organization. Specifically, Hannan and Freeman (1977: 935–6) reasoned that,

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Researching Work and Institutions through Ethnographic Documentaries

John S. Hassard

INTRODUCTION

The aim of this chapter – which complements the discussion of feature films in Chapter 35 – is to describe and analyse the methods and methodology of ethnographic documentary for organizational research. This is achieved predominantly through an historical appreciation of the development of styles and schools. In so doing, we note how, in the British tradition, the ethnographic documentary has been deployed to provide grounded sociological insights into the ‘real world’ of institutions and occupations. The dynamic of this method is one of the attempts to gain evermore direct and unmediated images of organizations and work.

The chapter adopts the following structure. Initially, we introduce the methods and methodology of ethnographic documentary in order to prepare the way for an historical review of anthropological studies of work, occupations, and organizations. Subsequently, we document how such ethnographic evidence has been realized under four main

filmic styles – the *world-of-labour film*, the *free cinema film*, the *modern television documentary*, and the *video diary/blog* – and analyse each as a case example of *cinéma vérité*, or film-truth. Finally, we discuss the representational nature of ethnographic documentary and examine the extent to which the film-truth method achieves its objective of offering privileged access to the everyday reality of work and organizations.

History and method of ethnographic documentary

In *The Cinema and Social Science*, de Heusch (1962: 13) writes, ‘When Lumiere definitely established a technique and sent his cameramen all over the world, the ethnographic and sociological cinema was born’. Elsewhere Morin (1956: 14) notes how the precursors of the cinema intended to develop the cinematograph as a ‘research’ instrument in order to study the ‘phenomena of nature’. While the ingenuity of Lumiere’s techniques made it likely that the cinematograph

could, potentially, become a methodologically ‘objective’ instrument with which to research social and organizational behaviour, Morin (1956: 15–16) notes, however, that it was not long before the ‘illusionists’ had taken this new type of ‘microscope’ away from the ‘scientists’ and transformed it into a ‘toy’, thereby reviving the ‘tradition of the magic lantern and the shadow play’.

It is against this backdrop of methodological and technical development in which, as Morin suggests, the cinematograph becomes ‘snatched away’ from science, that we find ‘cinema truth’ – for Morin, one of the basic aims of early film-making – being reintroduced into the film-maker’s art. Over the decades the century, documentary film-makers, in particular, have addressed the problem of how to make anthropological films more ‘real’ and ‘truthful’. The history of ethnographic documentary filmmaking has seen a range of styles emerge in this quest. For the most part these have been subsumed under one generic title – *cinéma vérité* (film-truth).

Philosophy and method

According to its proponents, the aim of *cinéma vérité* is simply to present the ‘truth’. In his *French Cinema since 1946*, Roy Armes (1966: 125), for example, argues that *cinéma vérité* offers a ‘rejection of the whole aesthetic in which the art of the cinema is based. An interesting visual style and striking beautiful effects are rejected as a hindrance to the portrayal of the vital truth’.

Over the years, however, critics have pointed to confusion about the basic aims of *cinéma vérité*, and suggested that under its banner all kinds of films have been made, many of which are unrelated to the original philosophy. Debate over the value and purpose of *cinéma vérité* was particularly heated in the early 1960s, and notably around the time of the 1963 Lyon ‘summit’ conference on *cinéma vérité*, organized by *Radio Television Francaise* and attended by such notables of the ethnographic film as Jean Rouch, Edgar Morin, Robert Drew, Richard Leacock, Michel Brault, and Albert and David Maysles. While proponents such as Armes

heralded *cinéma vérité* as the ‘true art of cinema’, critics branded it as ‘anti-art’. Among the critics, James Lipscombe (1964: 62), for example, argued that the term *cinéma vérité* had become used so loosely that most of the films produced in its name ‘have absolutely nothing in common except celluloid’. In a similar vein, Charles Ford claimed that *cinéma vérité* was the ‘biggest hoax of the century’, and that ‘nothing is more fabricated, more prepared, more licked into shape, than the so-called improvisation of *cinéma vérité*’ (Natta, 1963, quoted in Issari and Paul, 1979). Such diversity of views has made *cinéma vérité* one of the most controversial methods in film studies, with this controversy enduring to the present day.

While we explain later how, as a philosophy and method, *cinéma vérité* ostensibly starts with Dziga Vertov’s work on the concept of kino-eye in 1919, it was during the 1960s that the style became widely adopted and discussed in film-making circles. When it was adopted, it was in fact as a tribute to Vertov’s work that the term *cinéma vérité* was first used in a stylistic sense, in fact to describe *Chronique d’un Été* (1962), a film collaboration between the anthropologist Jean Rouch and the sociologist Edgar Morin. According to Issari and Paul (1979: 6), such was the impact of *Chronique d’un Été* that the term ‘spread like wildfire’, one result being a proliferation of definitions in which *cinéma vérité*:

...acquired different names from its founders, practitioners and critics in accordance with their understanding of the style. To Jean Rouch and Edgar Morin it was *cinéma vérité*; Richard Leacock called it ‘living camera’; Mario Ruspoli, the Maysles brothers, and Louis Marcorelles were amongst those who favoured ‘direct cinema’. William Bleum designated the style as ‘mobile camera’, but William Jersey selected the term ‘realistic cinema’. Italians named it ‘film inquiry’. To Armondo Plebe it became ‘synchronous cinema’; to Colin Young, ‘cinema of common sense’; to Jean Claude Bringuier, ‘cinema of behaviour’; to Norman Swallow, ‘personal documentary’ or ‘télé-vérité’. Others dubbed it ‘film journalism’, ‘truth film’, ‘direct shooting’ and ‘free cinema’ (Issari and Paul, 1979: 7).

Of all the terms coined, however, two have proved the most enduring – *cinéma vérité* and ‘direct-cinema’. Although methodologically they stem from largely similar historical roots, and while their associated films have many similarities, their proponents often testify to separate philosophies, the main difference residing in the function of the film-maker (and by extension ‘film-researcher’). While the practitioner of *cinéma vérité* (in the Rouch and Morin style) deliberately intervenes, hoping that greater spontaneity and ‘truth’ will be stimulated by the ‘subjective’ participation of the film-maker in the event filmed, the ‘direct’ film-maker claims to take an ‘objective’ stance, merely standing by in the hope that a situation of anthropological or sociological interest will resolve itself in a dramatic way (see Barnouw, 1975; Nelmes, 2003).

In terms of stylistic influence, it is ‘direct-cinema’ that has become the exemplar for the film-truth method generally. In this style, the film-maker tries above all to avoid sociological judgement and subjectivity. The aim is to convey the phenomenology of a social situation; to film people as they live their everyday lives. The philosophy suggests that the filmmaker’s role is to reveal the most essential parts of this reality, not to create a new one. The ‘direct’ filmmaker should dispense with those elements of method which serve to distort ‘truth’; that is, with the professional ‘camouflage’ of conventional film-making, such as scripts, scenes, studios, actors and so forth. Films are to be made of ordinary people in real situations, the only acceptable aids being portable cameras and sound equipment. As the filmmaker is only interested in events as they naturally unfold, no direction is permitted. In the more ‘pure’ forms, there is no plot, no preconceived dialogue, and no questions are either posed or answered. The philosophy states that, in shooting a film, the crew’s task should be confined to following subjects and filming their moments of personal drama. Decisions concerning action should rest with the subject – the filmmaker merely decides whether or not to

film a particular piece of action (Issari and Paul, 1979; Nelmes, 2003).

Pioneers and schools

We have noted how a range of ethnographic documentary approaches have been subsumed under the general heading of *cinéma vérité*. We will now attempt to place structure on the development of these approaches, first, by discussing the ideas and works of two of its founders, Dziga Vertov and Robert Flaherty, and second, by describing how *cinéma vérité* has been shaped in what we will call the French, American, and British schools of ethnographic documentary. We will show how each of these schools offers a distinctive approach to ethnographic filmmaking and, especially in the British tradition, with regard to work, institutions, and occupations.

Dziga Vertov and kino-pravda

As noted, it was in the hands of the Russian film-maker Dziga Vertov (1896–1954; real name Denis Arkadievitch Kaufman) that an ethnographic documentary style based on ‘film-truth’ or *kino-pravda* is first developed in theory and practice (Petric, 1978). Vertov is associated with what has been termed the ‘extremist theory’ of the ethnographic film, the view that the cinema should rid itself, as Sadoul (1940: 172) says, ‘of everything which has not been taken from life’.

In line with the prevailing philosophy of socialist realism in early Soviet film making, Vertov argued that *kino-pravda* required the nonparticipation of the filmmaker as a fundamental condition of attaining ethnographic authenticity. If the movie camera was to be an effective tool in influencing the masses, it was necessary to present realistic images of their everyday lives. Instead of staged sets, real locations would be used, in which peasants played peasants and workers played workers. If filmmaking was to become a truly proletarian art, Vertov believed its function was to portray the life of the people in all its intense and intimate detail (Lawson, 1964: 73). Given the technology available, however, Vertov was perhaps somewhat

overambitious in his claims to be providing a *kino-pravda* approach for anything more than short sequences of film. In order to achieve ‘truthful’ images with cumbersome technology, his early films saw the use of hide-aways and telephoto lenses, as in his famous filming of a family weeping at a grave. Written in the early 1920s, at a time when filmmaking technology was in its infancy, his original ideas on the ethnographic film reflect prophecy more than practice. Vertov, however, remains the father figure of *cinéma vérité* in that his philosophies reflect the core ideas of the movement.

Robert Flaherty’s Nanook of the North

The other founding father of the *cinéma vérité* documentary was the American trapper-pro prospector Robert Flaherty, who in 1920 and 1921 spent fifteen months making a publicity film in Hudson’s Bay for the French fur company *Revillon*, a film screened in 1922 as *Nanook of the North*. In its original version, *Nanook* is a silent 38-minute portrait of the world of the eskimo. The story develops through scenes, which alternate between descriptions of the everyday struggle for existence and the tenderness of family life. The first American film in the *cinéma vérité* style, *Nanook* remains highly acclaimed and widely screened (Nelmes, 2003).

The high quality of *Nanook*, however, is often attributed to the large amount of preparatory work undertaken by Flaherty (as also with his *Man of Aran*, 1934; see Calder-Marshall, 1963). Unlike in ‘purist’ forms of direct observation, where subjects receive a minimal briefing, Flaherty studied *Nanook* and his family in detail before filming. As de Heusch (1962: 35) notes, ‘Flaherty does not follow *Nanook* about in order to take pictures: he enters into conversation with him, he asks him to cooperate closely in the sociological portrait’. Because of the elaborate manner in which the scenes are reconstructed, the extent to which *Nanook* represents film-truth is obviously questionable. The well-known scene where *Nanook* and his family wake up in an igloo, for example, is achieved by Flaherty

removing the roof in order to have enough space and light to shoot (Winston, 1979). In Flaherty’s hands, the documentary becomes ‘a work of art imbued with rationality and truth’ (de Heusch, 1962: 35–6). *Nanook* is constructed like a fictional film in which the main character acts the part of himself within the framework of a prepared story. The story is conveyed in accordance with the demands of the director.

The French School: Rouch and Morin’s Chronique d’un Été

Historians of *cinéma vérité* commonly claim that the development of increasingly unobtrusive film-making technologies will bring the notion of the ‘cinema eye’ closer to reality (see Barnouw, 1975; Issari and Paul, 1979; Nelmes, 2003). It is claimed, for example, that a classic example of such technological progress was witnessed in the development of the 16 mm camera with portable sound equipment, notably in the work of the French anthropologist turned filmmaker Jean Rouch. Rouch is often considered the founder of ethnographic cinema, because he overcame a number of traditional filmmaking handicaps simply by using lighter technology. Synonymous with the so-called ‘French School’ of the ethnographic documentary film, Rouch is the figure most readily associated with the term *cinéma vérité* (Issari and Paul, 1979; Aitken, 2005).

As noted earlier, Rouch is best known for a film made in collaboration with Edgar Morin (and the French-Canadian film-maker Michel Brault) on the lives of Parisians, *Chronique d’un Été* (Chronicle of a Summer, 1961). According to Rouch, *Chronique d’un Été* was an attempt to combine ‘Vertov’s theory’ and ‘Flaherty’s method’ (Armes, 1966). Initially twenty-one hours long, but later cut to one hour and forty five minutes, the film begins by showing the reactions of passersby to the question, ‘Are you happy?’, and continues by interviewing them in detail about, for example, their likes, dislikes, hopes, and anxieties. In the final scenes, we witness the participants themselves viewing the film in a screening room, the footage

describing their reactions to the interview process.

Although technically *Chronique d'un Été* benefits from the development of real-time (rather than post-synchronization) sound recording, it appears to suffer methodologically from the fact that most of its subjects appear uneasy in front of the camera. More than any other landmark documentary, *Chronique d'un Été* highlights the issue of whether the camera should be used to 'record' action or to 'stimulate' it. In hands of Rouch and Morin, the *cinéma vérité* film becomes not so much a recording of what would happen if the camera was not there, as what happens because the camera is there.

The American School: Drew Associates' Primary

In contrast, the American tradition of *cinéma vérité*, based on the 'direct-cinema' approach, adopts a more Cartesian orientation. The exemplar of this is found in the collaborative work of Robert Drew, Richard Leacock and colleagues (including filmmakers Albert Maysles, Donn Pennebaker, and Gregory Shuker) – or Drew Associates – and their classic documentary in the 'living-camera' style, *Primary* (1960).

Providing funds to develop sufficiently mobile 16 mm equipment, the living-camera project was commissioned by *Time-Life*, for whom Drew Associates were to make a news-film equivalent of the magazine's well known news-spreads. To this end, Drew and Leacock negotiated an agreement with Senators John F. Kennedy and Hubert Humphrey to record their political campaigns in the 1960 Wisconsin primary election. The plan was to follow the candidates and film their activities 'without intruding'. No lighting was to be used; the crew would film only what was in front of them; they would go into situations 'cold'.

By employing this method, Drew felt that not only would the facts of a situation be revealed, but also the essence or feeling. The 'living' camera would report the emotional as well as the intellectual truth. 'Internal forces'

would be revealed by studying 'external forces' (Leacock, 1992). Like Vertov, Drew Associates presumed that the essence of a situation would be captured when subjects were too tired, relaxed, or involved to protect themselves from the scrutiny of observers. The film is commonly considered a breakthrough in that, with relatively small, quiet, and portable technology, the camera operator could now 'walk in and out of buildings, up and down stairs, film in a taxi or limousine', and all the time 'get sound and pictures as events occurred' (Leacock, 1992). Frequently screened, *Primary* is a film with a strong Vertovian flavour, in which the subject matter is treated in a largely disinterested fashion, the action following no plot other than the train of events which leads to Kennedy winning the election.

Work and institutions in the ethnographic documentary

Having described the basic philosophy and method of the ethnographic documentary, and offered a review of landmark contributions, we now offer a case account linked to researching work and organizations, that of the ethnographic description of occupations and institutions. From the 1930s to the present, and especially since the advent of television, large audiences have been found in Britain for documentary films, which centre upon the relationship between individuals and their workplaces. We document how such work has been developed under four main styles – the *world-of-labour film*, the *free cinema film*, the *modern television documentary*, and the *video diary/blog* – and discuss each as a case example.

World-of-labour films (1930s/1940s)

The origins of work-related ethnographic documentary in Britain lie in the early world-of-labour films of producer-theorist John Grierson (see Manvell, 1946; Barnouw, 1975; Beveridge, 1979; Forsyth, 1979; Aitken, 1990 and 1998; Nelmes, 2003). In his 1929 silent film on the herring industry, *Drifters*, Grierson effectively screened the

first European occupational documentary. *Drifters* was revolutionary in European filmmaking in that for the first time the focus for large screen entertainment was nothing more exotic than the work of fishermen. In important respects, Grierson's work reflects the influence of both Vertov and Flaherty. While the Soviet style is evident in Grierson's concern for social problems, Flaherty's romantic style underpins the quest to record the heroic nature of ordinary existence (Aitken, 1990 and 1998). Holder of a doctorate in social sciences from Glasgow University, Grierson's work is driven by a concern for the ethical and sociological purpose of the documentary film. As Agel (1953: 45) notes, Grierson's aim is 'to exalt human toil and civic virtues' and above all 'to magnify the unconscious beauty of the physical effort involved in labour'.

Grierson's work subsequently influenced the social and industrial documentaries of Paul Rotha, whose work represents an early attempt to demystify the structure of British industrial society, especially the role of the state. Among Rotha's best known films are *Shipyards* (1935), concerning the construction of a ship and the reactions of the community in which the shipyard is based, and *Rising Tide* (1935, later retitled *Great Cargoes*), which deals with extensions to the Southampton docks and the interdependence of various commercial activities (see Rotha, 1967; Aitken, 1990, 1998 and 2005). In his theoretical treatise, *Documentary Film Art* (1936), Rotha explains his approach to the ethnographic film through praising Flaherty's technique and commitment, but criticizing his over-romantic and idyllic conception of a primitive struggle with nature, an image far removed from, and with little to say about, the alienating realities of the modern industrial world. Rotha's view was that serious ethnographic documentaries should focus on the social realism of industrial civilization; they should concentrate on issues that confront large masses of the world's population (Rotha, 1967). For Rotha, the idyllic documentary largely avoids any conscious, critical social analysis.

Other quality documentaries set in industrial or other work-related settings find Grierson in collaboration with Alberto Cavalcanti or Harry Watt. For example, it was Grierson who produced Cavalcanti's *Coal Face* (1936), a study of the lives of miners, while Cavalcanti in turn produced Watt's *North Sea* (1938), a study of seafarers. The latter film departs, however, from the early requirements laid down by Grierson, in that it employs actors and tells a scripted story. This was also the case with Watt's classic of the period, *Night Mail* (1936), which tells the story of the night run by the postal train between London and Glasgow, and centrally the work of postal workers receiving, sorting, and dispatching mail during the run. A collaboration with Grierson and Basil Wright, like *North Sea*, the film was produced by the esteemed Film Unit of the General Post Office (see Aitken 1998 and 2005).

Free cinema movement (1950s/1960s)

In the post-World War Two era, the British ethnographic documentary escaped from its role in creating a largely propagandist view of British society through what came to be known as the 'free cinema' movement, whose aim was to return to a more direct approach to the, notably working-class, documentary. The origins of free cinema lie in the pages of the short-lived Oxford University magazine, *Sequence* (1949–52: 14 issues). The editors of *Sequence*, Lindsay Anderson and Gavin Lambert (and later Karel Reisz), severely criticized British filmmaking for presenting a view of the nation from the perspective of the pre-1914 bourgeoisie. It was Anderson's view, in particular, that British filmmaking was devoid of critical analysis, especially regarding the class system. According to Robinson (1973: 292), the *Sequence* view was that British film-makers had characterized 'the function of the working class [as] to provide "comic relief" to the sufferings of their social superiors'.

The term 'free cinema' was first coined in February 1956 as the headline for a National Film Theatre programme of short films. The notes for the programme explain how the

movement's films are 'free in the sense that their statements are entirely personal. Though their moods and subjects differ, the concern of each of them is with some aspect of life as it is lived in this country today'. The attitude is one of 'a belief in freedom, in the importance of people and in the significance of the everyday' (quoted in Robinson, 1973: 294). The *Sequence* philosophy was to show the way 'ordinary people' enact their 'everyday lives'. The objective was for these films to possess a 'feeling of freshness' in documenting the 'real environment in which people work and play' (Robinson, 1973: 294).

The classic of the Free Cinema movement is undoubtedly Anderson and Reisz's *Every Day Except Christmas* (1957), a film mainly about night workers at London's Covent Garden Market. The film begins with the image of a shed somewhere far out of London, in which a lorry is being loaded with food and flowers at the very moment when the BBC announcer is saying 'goodnight' to listeners. From every corner of the country, similar vehicles speed towards the capital and Covent Garden Market. While the lorries are unloaded, dealers prepare their stands: boxes are opened; apples are polished; workers pause for tea and a chat. A large well-dressed man gives orders; the major retailers arrive and bargain; and a small trader loads a handcart. Housewives and itinerant merchants buy after the retailers have gone; an old woman sells flowers; tramps pick up any damaged fruit. Finally, the lorries are loaded with empty packing cases and depart for the four corners of Britain.

In recording these scenes, the camera carefully studies the faces of traders, customers and the like and catches their subtle yet commonplace gestures. In so doing, *Every Day Except Christmas* achieves an ethnographic montage in which each face contributes to the sociological effect. As in much of Anderson and Reisz's work, it is the class structure of Britain that is reflected primarily in scenes which document the ebb and flow of work and rest. The philosophy leans firmly upon Vertov's theories in that

the goal is to bring to the audience critical reflections on the world in which it lives.

Modern television documentary (1960s–present)

From the 1960s onwards, television replaced the short or feature length film as the main medium for disseminating ethnographic documentaries in Britain. Indeed, since *The Family* (1974) made nationally famous the Wilkins of Reading, British television has played host to a wealth of film-truth series. The advent of mass television, however, did not alter the trend for ethnographic filmmakers to be preoccupied with understanding the nature of work, occupations, and institutions.

The 1990s, in particular, saw British audiences bombarded with *cinéma vérité* studies of work and organizations, with large audiences being found for series such as *Flying Squad* (police officers, BBC, 1990), *The Duty Men* (customs officers, BBC, 1991), *Fire* (firefighters, Thames, 1991), *D.E.A.* (drug enforcement officers, BBC, 1992), *Town Hall* (town councillors, BBC, 1992), *The Ark* (zoo keepers, BBC, 1993), *The Adventurers* (venture capitalists, BBC, 1993), *Turning the Screws* (prison officers, Channel 4, 1993), *Skipper* (trawlermen, BBC, 1993), *Coal* (coalminers, BBC, 1994), *The Factory* (factory workers and managers, Channel 4, 1995), and *When Rover Met BMW* (car workers and managers, BBC, 1996), for example.

Stylistically, British television-based ethnographic documentaries have been influenced considerably by the work of the American film-maker Frederick Wiseman, who in the mid-1960s made a series of investigations for US television into the relationships between individuals and institutions. As much as John Grierson, it has been Wiseman's technique of using documentary film for observing social relationships that has influenced a generation of high profile British *cinéma vérité* filmmakers, including Roger Graef, Nick Broomfield, Diane Tammes, Chris Oxley, and Molly Dineen.

An early expression of Wiseman's influence was found in Graef's series, *The Space Between Words* (BBC, 1972), which looked at 'communication' in five institutional settings; the family, school, work, diplomacy, and politics. The series was innovatory for British television in that it focused on 'ordinary situations' and presented the images with the minimum of introduction. Graef's idea was to 'let viewers develop their own theories of communication'. In filming the series, Graef's policy was 'never to use lights, or staged scenes, to keep the equipment out of sight, and to have the minimum of people in the room'. To remain as anonymous as possible, Graef even had a rule of 'never looking any of the subjects being filmed in the eye'. A radical venture at the time, Graef has noted, with irony, how the response of the media was to interview academics and critics in order to obtain 'professional' explanations of the issues raised by the films, a practice completely anathema to the project (Graef, 1992).

Graef is better known, however, for documentaries which examine the relationship between organizational practices and institutional politics. Examples here are his films on investment in new technology at British Steel, and the allocation of spare beds in the National Health Service. His most notorious work in this style, however, was a series documenting the lives and work of members of the Thames Valley Police Force (*Police*, 1982, with Charles Stewart). In this series, Graef wished to combine the apparent objectivity of direct-cinema methods with the analysis of contemporary social issues, which, in the case of one episode, 'A Complaint of Rape', led to widespread public debate. The *Police* series laid new ground for British ethnographic documentary in that it opened up issues which the subjects themselves were not aware of, thus instigating a self-discovery process.

During the 1990s, the institutional documentaries of Diane Tammes also reflected a direct-cinema style. In her series, *Casualty* (Channel 4, 1991), for example, an institution is described with relatively few edits or

guiding commentaries. The film reflects that variant of the direct-cinema philosophy associated with the British National Film School, which suggests that the longer a sequence is allowed to run the more 'truthful' the images are likely to be. Tammes, however, has admitted to becoming 'less strict' with herself as her career as a documentary filmmaker has progressed. While she suggests her early work was 'purist', in that, 'the camera was there simply to see how people interacted', subsequent work has often involved dialogue with subjects from behind the camera. 'Truth', she argues 'is about what people actually come away with at the end of the film'. The problem is that 'everyone who makes a film is just putting their own truth on the screen' (Tammes, 1992).

An interventionist style was also evident during the 1990s in the work of Molly Dineen, and in particular her highly popular series on zookeepers, *The Ark* (BBC, 1993). Originally aiming to make 'a film about a zoo', Dineen ultimately told 'a story about a British institution undergoing radical change'. Dineen explains how, as financial problems loomed at London Zoo, it became a case of 'suits versus beards' – in other words, 'the management men and the financiers against the scientists and academics' (*The Guardian*, 1993: 53). As the crisis came to a head, she documents how problems of 'human resource' management began to take priority over 'animal' management. Consequently, her style of filmmaking became increasingly investigative, critical, and interactive, with Dineen, from behind the camera, probing to uncover the layers of political intrigue underlying, for example, decisions on redundancy and restructuring.

From the mid-1980s, a further step away from the philosophy and practice of 'objective' or 'purist' ethnographic documentary was signalled in works which actually made overt the construction of the *cinéma vérité* film. This is nowhere better represented than in works by the celebrated film-maker Nick Broomfield. When in 1990, for example, Broomfield (with Joan Churchill) made a follow up for Channel 4 of his earlier

institutional film *Juvenile Liaison* (1975), he brought to the fore his own problems as a filmmaker, especially that of tracing the original participants and gaining their renewed consent. This style of focusing on what goes on behind the scenes was subsequently used by Broomfield to great effect in his film, *The Leader, The Driver and the Driver's Wife* (Channel 4, 1991) where, exploiting the off-screen drama, Broomfield documented his difficulties in trying to secure an interview with the South African right-wing politician Eugene Terreblanche, the result being a black comedy of trial and tribulation – one somewhat reminiscent of Michael Moore's attempt to hold an interview with General Motors CEO Roger Smith in *Roger and Me* (1989). This style of filmmaking could not be further removed from Broomfield's earlier work, for here the camera is anything but anonymous. Broomfield has remarked that, as his filmmaking style has 'matured', he has felt increasingly hide-bound by having to keep to a 'pure' *cinéma vérité* approach, for in the filmmaking process, 'things don't happen when they are supposed to happen'. Broomfield argues that the associations you make throughout a film are often much richer than simply documenting what happened. For Broomfield, therefore, 'the notion of objectivity is no longer tenable' in ethnographic filmmaking. By keeping the 'pretence of objectivity' and thus 'failing to share personal experiences', he argues that the audience is 'less able to evaluate your final product' (Broomfield, 1992).

Video diaries and video blogs (1990s – present)

The important film makers of the future will be amateurs (attributed to Robert Flaherty, c.1925, by Jean Rouch, 1992)

Since the mid-1990s, one of the most popular forms of television *cinéma vérité* has seen both professional film-makers and also members of the public use 8 mm video camera technology to record personal 'diaries'. It can be argued that the personal documentaries which accrue from these diaries reflect many

of the ethnographic qualities the *cinéma vérité* pioneers sought in terms directness and authenticity.

In work and organizational settings, the use of the video diary form has been regularly employed in the Channel 4 series *Cutting Edge*. Films in this series frequently take the form of covert ethnographies broadcast under the title *Undercover*. An early example of a programme employing this method, one that aroused considerable media attention, was *Low Paid Work* (1993), in which former BBC film-maker Sima Ray spent three months undertaking covert observation in shops, factories, restaurants, and nursing homes throughout Lancashire. Concealing a camcorder in a shoulder bag, Ray filmed the situations she encountered as a young Asian woman in search of employment and accommodation. During the period of observation, all the jobs she managed to secure were remunerated at less than the legal minimum rate, employers showing scant regard for the law. One of the lowest paid jobs Ray obtained was, in fact, obtained through the local Job Centre, a State-run agency. Through covert observation of the relationship between employers and employees – a method that would, perhaps, today fail to gain permission from a university research ethics committee – Ray's film portrays vividly the feelings of hopelessness and depression experienced by low paid workers, in this case during a period of high unemployment. The film drew a sharp contrast between the low paid worker's struggle for existence and the luxurious lifestyles enjoyed by many employers, these contrasting images being silhouetted against a background in which the State frequently failed to prosecute employers who broke the law, and where the Government was concerned to veto European Community legislation on minimum wages.

The most ambitious British video diaries project, however, has undoubtedly been the BBC's *Video Nation* series, a television project in social anthropology and audience interactivity. Starting in 1993, the BBC has encouraged people to record their lives on video, with a subset of these video

diaries being shown on television. Originally created in the Community Programme Unit for BBC 2, and founded and coproduced between 1993 and 2001 by Chris Mohr and Mandy Rose, the series initially involved a diverse group of fifty people, selected from across the UK, being trained in the use of camcorders and invited to record aspects of their everyday lives during the course of a year. The BBC team selected extracts from these recordings and presented them as short films immediately before *Newsnight*. More than 10,000 tapes were ultimately submitted to the BBC, from which approximately 1,300 films were edited and shown on television. Viewing figures were high (1 to 9 million) and led to themed series of *Video Nation* films, such as *African Shorts*, *Hong Kong Shorts*, *Coming Clean*, *Bitesize Britain*, and others. In 2000, BBC 2 decommissioned the series and built a website from the archive at <http://www.bbc.co.uk/videonation/>. As the website shows, a significant proportion of the *Video Nation* contributions concern either the relationship between people and their employment or personal commentaries on the organizations they work for. Such films often reflect extremely intimate and emotional impressions of the relationships between people, work, and organizations.

From around 2003, a Web-based continuation and expansion of video diaries has emerged in the form of video blogging or 'vlogging'. During the mid-1990s, when the *Video Nation* project was being developed, the bandwidth and processing power required to operate video online made it an extremely difficult operation for most amateurs. With the advent of greater computer and connection speeds, however, vlogging has witnessed a significant increase in popularity. For example, the Yahoo! Videoblogging Group saw its membership increase dramatically in 2005, while the most expansive video-sharing site to date, YouTube, founded in February 2005, was launched publicly between August and November 2005. In turn, the BBC launched its first official video blog in October 2006. Currently, many

open source content management systems, like WordPress or Drupal, enable posting of video content, thus allowing bloggers to host and administer their own video blogging sites. Furthermore, the convergence of mobile phones with digital cameras now allows publishing of video content on the Web almost as it is recorded. While the potential of vlogging for academic research into work and organizations appears exciting and significant, in comparison to earlier forms of ethnographic filming, a formal methodological philosophy is yet to be established. However, in terms of practical impact, *The Economist* on 15 March 2008 – in a side-bar titled 'On strike, virtually' – cited examples of management being under pressure from 'virtual workforce protests' in a piece that highlighted the use of video blogging for orchestrating protest and dissent.

Ethnographic documentary, realism, and representation

It has become more difficult to think of ethnographic films as definitive representations of events, independent of the process that produced them, and ethnographic film-makers have begun to look upon their work as more tentative forays into cultural complexity [and as] parts of a continuing enquiry (MacDougall, 1978: 415).

Having described the philosophies and aims of ethnographic documentary, and also the forms it has taken in producing studies of work, institutions, and organizations, we finally examine the integrity of the approach in relation to methodological constructs of 'representation' and 'reality'.

In contrast to the philosophies of Vertov and Grierson, a key argument of recent theoretical writing on film-truth is to regard the 'truth' claims of the genre as in some way 'fictional'. In contrast to the early 'purist' assumptions of direct-cinema, which rest upon the Cartesian belief that reality enjoys an independent existence 'out there', current methodological assessments of the professional ethnographic documentary suggest that its products are in many ways as artificial as those of popular cinema. It is argued, for example, that

through editing and associated processes, both genres come to contain similar elements of plot, character, situation, and event (Renov, 1986; Hall, 1991; Nicholls, 1991; Nelmes, 2003). As such, both offer cultural images, which present the viewer with challenges or dilemmas, build heightened tensions and dramatic conflicts, and ultimately terminate with resolution and closure. In sum, it is claimed that ethnographic documentaries operate with reference to a 'reality' that is constructed, one that is, above all, the product of signifying systems.

In being so constructed, we find the signification process of ethnographic documentary, like that for other fictional accounts, being subject to scrutiny over its role in (re)producing hegemony. The claim of film-truth to be accessing some privileged reality 'out there' becomes treated itself as an ideological effect (Nicholls, 1991). Acknowledging this constructed basis serves to undermine, for example, the claims to moral superiority of, for example, Vertov, Flaherty, the British world-of-labour filmmakers, and the proponents of direct-cinema. It serves also to suggest that *cinéma vérité* offers access to a world rather than to *the* world. While *cinéma vérité* films appear to direct us toward the world, it can be argued that they remain constructed texts, offering representations rather than replications. Such representations reflect truth claims not only of what we discover in the social world, but also of what interpretations, meanings and explanations can be assigned. The social world is made manifest through agencies of external authority, through representatives. What we experience is an historical world made available through picture windows in which social practices are telescoped, dramatized, and reconstructed.

One of the main hegemonic products of this process is our impression of encountering the *cinéma vérité* world as if it were being presented for the first time. The world made available to us is one we perceive as Newtonian and Cartesian; a world comprised of reason, realism, and common sense; a world waiting to be discovered through advances

in technology. We discover that which we take to be the only thing we can discover. We are presented with a universe which fits within the framework of its representations. In this representational process, elements of style, structure, and perspective freely mix. When the filmic argument takes shape, we move beyond the factual world to that of the construction of meaning through a system of significations. In so doing, we never experience a pure correspondence between evidence and perspective, for facts become fashioned by arguments, which in turn rely on strategies and conventions for their accomplishment (Shotter, 1993).

The so-called methodological 'objectivity' of the ethnographic documentary can, therefore, be read as an ideological representation which operates in the guise of 'common-sense'. Barthes (1974) calls this institutionally enforced nature of representation the 'zero-degree style', for it adopts a posture of innocence and neutrality in the face of systems which provide the foundations upon which institutional perspectives represent themselves. The direct-cinema documentaries of, for example, Wiseman and Graef are never as neutral as they appear, for they embody a distinctive view of institutions like hospitals and prisons in a way which reflects strategies of resistance over a mechanistic and bureaucratic logic. The 'third dimension' (Lukes, 1974) of this hegemonic process is that which is accomplished without overt commentary. In ethnographic documentary, we tend to perceive only the particular arrangements of sound and image. Objectivity is constrained by decisions of what should, and should not, merit being commented upon.

In sum, these representational and ideological arguments suggest that ethnographic documentaries always present *a* truth rather than *the* truth. *Cinéma vérité* films contain authorial voice and, as such, what we experience is less the world 'reproduced' as 'represented'. This is so, 'even if the evidence they recruit bears the authenticating trace of the historical world itself' (Nicholls, 1991: 118).

CONCLUSION

I think this objective-subjective stuff is a lot of bullshit. I don't see how a film can be anything but subjective (Frederick Wiseman, quoted in Levin, 1971: 321).

As the basis for considering its potential use in organizational research, this chapter has offered description and analysis of a range of ethnographic documentary filmmaking styles. In the main, these styles have followed the early philosophies of Dziga Vertov and John Grierson in advocating a progressively 'direct' and 'natural' approach to filmmaking in which everyday events are recorded without the traditional, cinema-based props of scripts, staging, lighting, and so on. Proponents traditionally explain their development by recourse to a determinist theory in which technological innovations (e.g., mobile 16 mm technology with synchronized sound in the early 1960s and 8 mm video technology in the 1980s) provide for the capture of increasingly unobtrusive images through the eyes of a metaphoric fly-on-the-wall.

The chapter has addressed this technical and stylistic history at two main levels, research methodology and sociological analysis. For the former, film-truth may appear initially appealing to organizational research in that it appears to promise progressive reduction in levels of obtrusiveness and reactivity in the observation process. Also methodologically, *cinéma vérité* seems to offer a textual layer additional to, yet different from, those employed in traditional organizational research investigations. The suggestion is that a film record can add to the diversity and richness of data collected on, for example, work, occupations and institutions. As such, there appears potential for ethnographic documentary to contribute to a spirit of triangulation through providing more 'immediate' images than can be obtained by other research methods.

This methodological review, however, was in turn complemented by a critical sociological analysis of the relationship between 'realism and representation' in ethnographic

documentary. This led to an awareness of significant shortcomings in the technological determinist argument, which underpins much of the methodological argument for *cinéma vérité*. Initially from the standpoint of method, but increasingly from that of deconstruction, it became apparent that an equally appropriate metaphor for fly-on-the-wall documentaries could be 'fly-in-the-soup'! Critical reviews of the ethnographic documentary, together with work on authority and knowledge, proved influential in directing the argument toward an appreciation of the ideologically enforced nature of representation. In the process, earlier methodological concerns with overcoming obtrusiveness became defused by discussions of the relativity of authorial presence and the ideology of 'common-sense'. Awareness of the ideological effects of *cinéma vérité* helped deconstruct many of the methodological assumptions upon which 'purist' approaches were founded. It was argued, accordingly, that *cinéma vérité* documentaries operate with reference to a reality that is essentially the product of signifying systems.

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PART III

Strategies: Approaches to Organizational Research

Focusing on approaches to achieving research aims, illustrating links between topic, aims, strategy, analytical framework, and theoretical development, and also demonstrating the range of choice and degree of creativity as well as technical knowledge underpinning research strategies

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Cross-Cultural Comparative Studies and Issues in International Research Collaboration

Mark F. Peterson

INTRODUCTION

The methods issues in cross-cultural organizational research are linked with the distinctive characteristics of the phenomena studied, the theoretical and metatheoretical perspectives taken by researchers, and the social process of collaborating across culture and distance. Whereas most organizational research can be simplified by assuming that the phenomena encountered in one's own part of the world are all that matter, cross-cultural research draws attention to phenomena that are sometimes different in nuance and at other times profoundly different. The methods for studying culture, the choice of theories and constructs, and the intellectual traditions in which researchers understand them vary not only with personal idiosyncrasies, but also with the societies in which researchers are geographically located. The need to access diverse populations to do global research

means that researchers increasingly find advantage in forming collaborative groups, which are challenging to participate in and to manage due to communication constraints, variability in preferred methods, theories, and traditions, and global differences in work situations and personal lives.

The aim of this chapter is to identify major controversies in cross-cultural organizational research methods, particularly those that rely on questionnaires, as well as in the social process of doing such research. I will first consider basic technical issues that produce ongoing controversies and common misunderstandings. Since even researchers who share a basic methodological perspective rarely agree about all of these issues, international teams struggle with technical controversies. Attention then turns to methodological implications of perspectives on culture and controversies in cross-cultural research. Third, the interplay of ideals about how to conduct cross-cultural

research, with the researcher's cultural and personal situation will be considered, and ways to work successfully with a multinational research team will be suggested. Given space constraints, readers are referred to further studies of cultural differences involving, for example, hierarchical moderated regression, multigroup structural models, and hierarchical linear modelling, and also for research using qualitative methods (Easterby-Smith and Malina, 1999; Raudenbush and Bryk, 2002; Van de Vijver and Leung, 1997).

Technical issues

Construct equivalence and level of analysis present the greatest technical challenges in cross-cultural research. Equivalence issues plague researchers who wish to compare one phenomenon, say an attitude like commitment, across several nations based on some theoretically meaningful aspect of national culture, such as individualism (Gelade et al., 2006). Here, we find that we are also comparing languages, thought patterns, social norms, regulatory environments, social structures, and probably other issues of which we are unaware. Consequently, the ability to control for factors that confound the substantive comparison of interest with the degree of precision that is typical in domestic research is only possible when comparing a few select phenomena in small numbers of nations (Leung, 2008).

Translation equivalence

The technical literature concerning equivalence starts from careful translation checking (Brislin et al., 1973). For example, *back translation* requires having one party translate a survey, another translate it back to the source language, and then both to engage in a process of comparison and correction. *Parallel translation* is achieved by having two people separately translate a survey into a target language, and then either the translators or others compare the translations and make corrections. Back translation is more consistent with current norms in most

parts of the world, but the decision concerning which to use is an issue of power as much as research norms. In practice, back translation puts power for making final judgments about translation adequacy in the hands of the party who designed the survey, usually in English. Parallel translation puts this power in the hands of the parties who are fluent in the target language.

The translation procedure used is important, but is now seen as secondary to quantitative indicators of translation quality. In the early years of comparative research, documenting the careful conduct of translation was considered state of the art for achieving equivalence. However, greater confidence is now placed in indicators of equivalence in measurement structures. Equivalence is assessed by comparing reliability coefficients or by using the measurement portions of structural equation modelling programs (Van de Vijver and Leung, 1997). The availability of quantitative indicators means that the results of a pilot study to evaluate measure equivalence is likely to be more meaningful than are debates about the virtues of back translation or parallel translation.

Most discussions of translation equivalence in comparative survey research assume that the structures of different languages are similar, leaving translation adequacy as the main explanation for nonequivalence. However, languages have long been known to differ, often in surprising ways (Nisbett et al., 2001; Sapir, 1921). Many controversies about the implications of phrasing questionnaire items in the generic (English) organizational literature concern the distinctive characteristics of English, such as whether pronouns are singular or plural. For example, how my superior treats *me* may be different from how my superior treats *us*. In comparative studies of leadership with colleagues in Japan, we faced the problem of communicating such differences in Japanese, where the subject of a sentence is often implicit, especially when the subject is personal like I, me, we, or you (Peterson, Maiya and Herreid, 1993). The awkwardness of explicitly representing

the 'me' versus 'us' distinction in Japanese is likely to evoke responses different from those one would expect from survey research in English. Researchers who collaborate in a multiple-language project also need to discuss whether their goal is to extend work that is established in one language into another. In that case, the success of a translation might be evaluated relative to the language of origin. Where a measure is being more substantially redesigned, as in a study that redesigned role stress measures (Peterson et al., 1995), or a new measure is created for global use (Leung et al., 2002), overall similarity among the translations being studied may be more important.

A full treatment of language structures is outside the scope of the present chapter. However, it would be surprising if any survey measure that has been carefully tailored to promote both reliability and validity in one language will have precisely the same meaning in languages from radically different language groups. Still, languages tend to be sufficiently analogous that effective communication is typically possible (Pike, 1982, pp. 132–133). A host of comparative survey-based projects show that measures which were designed to have optimal psychometric properties in one language can be effectively translated into many others with only a modest loss of precision. The reduction in a reliability coefficient from 0.80 for a measure in one language to 0.70 in another adds imprecision, but such differences are often treated as sufficiently manageable to allow comparison (Leung, 2008).

Conceptual equivalence

Issues of translation equivalence merge with issues of conceptual equivalence since both are rooted in language. Concepts correspond between societies to different degrees. For example, Farh et al., (2004) report an inductive study of organization citizenship behaviour (OCB) in China. Their analysis indicates that some constructs, such as 'taking initiative', appear to be almost identical to US concepts, such as 'conscientiousness'. Other Chinese concepts, such as 'helping

coworkers', shows nuances of a US concept, in this case 'altruism'. However, others, such as 'interpersonal harmony', are more uniquely linked to China's heritage. Smith (2008) reviews indigenous management concepts from understudied parts of the world that only roughly correspond to ideas familiar in the English literature.

As with measure equivalence, discussions of conceptual equivalence are increasingly framed in statistical terms. That is, conceptual equivalence is evident when a measure fits into a set of relationships with other measures in theoretically meaningful ways, such as a nomological net, in the same way in different parts of the world. When viewed in this way, discussions of conceptual equivalence are typically reflected in hypothesis development and testing rather than in validity assessment. Data analysis techniques used to establish such relationships range from showing that regression slopes are equivalent in different nations to applying even more complex multigroup structural models (Van de Vijver and Leung, 1997). Controversies about the technical merits of different methods for assessing cultural differences in relationships among measures concern the strengths and limitations of regression, structural equation modelling, and hierarchical linear modelling (Raudenbush and Bryk, 2002).

Response bias

Language differences suggest that item phrasing is likely to affect inter-item covariance, reliability coefficients, or measurement model equivalence. However, language equivalence issues and culturally linked communication style preferences can also affect the tendency to use high, low, or intermediate values in responses (Smith, 2004). Significant anecdotal evidence has been collected concerning cultural differences in response patterns (Van de Vijver and Leung, 1997). National differences have recently been explained in part by a link to Hofstede's (2001) concept of power distance (Smith, 2004). Average responses to surveys in large power distance nations are generally found to be more

positive than those in small power distance nations.

In addition to cultural patterns in response tendencies, nuances reflecting *degree* in specific words can create language differences for particular items. The sensitivity of responses to the phrasing of alternatives is well recognized for English-language surveys (Bass et al., 1974). However, adverbs of degree may not match precisely between languages. For example, in a set of response alternatives ranging from 'strongly agree' to 'strongly disagree', the English 'slightly agree' can convey slightly more agreement than the Chinese *you dian* agree. Similarly, the phrasing of the question stem may have a different degree of positive, neutral, or negative connotation in two languages. In a simple question like 'How hungry are you?', a Dutch translation that represents hunger as *honger* relates to the upper ranges of this phenomenon (overlapping with starving), whereas the alternative Dutch *trek* relates more closely to the more ordinary degrees of hunger experienced in developed nations.

One solution has been to design measures using forced-choice alternatives or rankings that make response patterns difficult or impossible. For example, the *Meaning of Working* project (1987) asked respondents to rank a set of fourteen goals to avoid possible cultural differences in the propensity to say that all work goals are very desirable. Even if respondents would like to say that each goal is the most important, the answering system only allowed a respondent to rank one as the most important. Another approach has been to adjust responses to Likert-scale measures using within-subject standardization or mean-centring. Within-subject standardization requires subtracting the average for each respondent across all items from the score of each item, and dividing the result by the standard deviation of the respondent's answer across all items (Hofstede, 2001). Some researchers have, then, helpfully rescored the result so that it corresponds to the original response alternatives (Hanges and Dickson, 2004). Mean-centring takes the first step of

subtracting each respondent's overall mean, but leaves out the second step of dividing by each respondent's standard deviation.

Unfortunately, these solutions to the response pattern problem have significant limitations. They all produce dependencies among items that limit the reliability of indices that combine items (except when adjustments are made to Likert-scaled items after indices are formed), and create covariances among the adjusted items that may not be meaningful. Consider, for example, two questions from the Organizational Commitment Questionnaire (Mowday et al., 1979) about which most people are likely to have similar attitudes; 'My values and the values of this organization are quite similar', and 'I am proud to work for this organization'. If respondents were asked to rank these obviously similar items as '1' and '2', their answers would necessarily have a correlation of -1.0 . A similar negative correlation appears if respondents answer these questions using Likert scales, and the researcher then standardizes the responses within subjects as described in the preceding section.

The extent to which such within-subject adjustments are troublesome is affected by two considerations. One is the number of items, such that the dependencies produced are smaller as the number of items increases. The other is the original covariances among the items, such that the tendency to reduce meaningful variance is increased as the original covariances increase (Saville and Willson, 1991; Smith, 2004).

Sample equivalence

Cross-cultural researchers also face the problem of sample equivalence. Referring to the example of commitment, one would not want comparisons to be confounded by other demographic characteristics besides nation. Demographic confounds are readily resolved in domestic research by statistically controlling for factors such as age or gender before comparing commitment levels across organizations. Such controls, though, typically assume that any implications of age and gender are reasonably consistent

across organizations. Such an assumption is generally reasonable within a single nation, but is less reasonable across nations. Cultural variability in the meaning of demographics is reflected in showing respect for age and gender (Peng and Peterson, 2008). For example, in Japan, only those women who are extraordinarily committed to remaining employed tend to continue working for a major employer beyond their mid-20s. It is also unusual for a man to remain with a major employer beyond age 55 unless he holds a senior management position. Glazer and Beehr's (2005) study of nurses in four nations indicates that simple statistical controls for other sample characteristics such as occupation or employer type are similarly problematic where the social status or employment conditions affecting a group vary by nation. The consequence is that implications of demographic characteristics may well need to be considered, but the limitations of the simple statistical controls used in domestic research must be recognized.

Data collection process equivalence

National or cultural differences can also affect the meaning of data collection procedures. In some nations, postal systems work efficiently, but in others mail surveys can be lost or delayed. In some places, personal interviewers who come to a private home are viewed as normal visitors, but elsewhere they are viewed as potential threats and may be at risk themselves. In some nations, research that includes separate-source performance evaluations or access to personnel records requires respondents to sign a consent form, but in other nations, it does not. In a study of local governments, I found that if a Japanese organization agrees to participate, then nearly all employees will voluntarily participate as well; in the United States, however, organization consent and employee consent tend to be separate issues. Superficially equivalent data collection procedures are not the same as substantive equivalence, and a degree of nonequivalence in the data collection process

is likely in comparative projects (Lincoln and Kalleberg, 1990).

Adapting to equivalence constraints

How do we solve issues of limited equivalence? The magic of randomization (Campbell and Stanley, 1963) is typically out of reach in cross-cultural research. We are left with the partially satisfactory recourse of making statistical adjustments to our measures, matching respondents as best we can, and interpreting the likely implications of uncontrolled contextual differences. Precise equivalence gives way to sufficient comparability to allow reasonably meaningful comparisons with caveats and needs for replication (Leung, 2008). Subjectively evaluating context to give meaning to comparisons can require even a quantitatively oriented researcher to be something of a linguist, anthropologist, economist, sociologist and psychologist, or to invite collaborators from other disciplines (House et al., 2004).

Level of analysis

Is culture a characteristic of a society or of its members? As a theoretical question, I will deal with this again in the following section, but it also has a technical side.

Many cross-cultural projects collect survey data from individuals then aggregate those data to create measures that are typically used to represent a national collective. The mechanics of creating nation scores is simple; take an average across a set of items and across a set of people. The basis for making decisions about what items to combine to create nation scores, however, is frequently misunderstood even by experienced scholars. Researchers typically create measures based on individual-level measurement structures, then perform a second set of analyses to see whether there is sufficient variance among units at a higher level (groups, organizations, nations, etc.) to aggregate the individual-level measures to that level (Peterson and Castro, 2006). Hofstede (2001), however, notes that the correlations and covariances between items on which measure decisions are

based differ depending on the level of analysis at which they are calculated.

Consider, for example, a hypothetical study that seeks to create measures to represent the aspects of individualism and collectivism that Oyserman et al. (2002) identify in their meta-analytical review of individualism-collectivism. In principle, one might be able to construct ten items that provide face-valid indicators of individualism and another ten relevant to collectivism. It is frequently reported that, when such items are analysed at the individual level, two or more constructs emerge that are internally homogenous and only modestly correlated with one another (Kirkman et al., 2006; Oyserman, et al., 2002). That is, it is not unusual for one person to express both individualistic and collectivistic values, or indeed to express neither individualistic nor collectivistic values. However, when such items are aggregated to the nation level, and correlations among these items are calculated, the items tend to combine into one rather than two dimensions (Hofstede, 2001; Kirkman et al., 2006). That is, societies in which many people express individualistic values also tend to be societies in which few people express collectivistic values.

More generally, measurement structures are often affected by level of analysis (Peterson and Castro, 2006) and need to be assessed at each level at which they are to be used (Inglehart and Baker, 2000). Whether or not individual- and nation-level measurement structures differ depends on the topic of concern. Personality measures seem to have similar structures at both levels (Gelade et al., 2006), while measures of values often seem to differ by level (Hofstede, 2001). The measures of managers' use of social structures as sources of meaning show some tendency to collapse into a smaller number of dimensions at the national level compared with the individual level (Smith et al., 2002). Decisions about what items should be combined to create measures of personal perceptions, attitudes, or values related to culture will differ from decisions about what items should be combined based

on nation-level measurement structures to create nation-level measures of culture.

Perspectives and controversies

The technical issues of equivalence and level of analysis tend to be the focus of most commentary on cross-cultural research methods, but these are expressions of more basic issues concerning the nature of culture and its representation. Cross-cultural studies have progressed from the 1980s when culture was treated as an amorphous concept evoked to describe differences between nations without providing an explanation (Child, 1981; Roberts, 1970). Subsequently, controversies have centred on which ways of understanding and representing culture are most useful. These debates often concern the adequacy of Hofstede's (2001) conceptualization of culture as 'the collective programming of the mind' (based on the dimensions of individualism-collectivism, power distance, uncertainty avoidance, masculinity-femininity, and short-long term orientation), and the utility of cultural distance and country cluster research that is closely tied to these constructs.

Although these controversies continue, others have recently arisen. One set of controversies concerns the relative utility of alternative systems for categorizing cultural values compared to the one that Hofstede proposed. A second concerns whether organization studies will benefit more by comparing societies based on value dimensions, or whether it is better to compare belief systems, social structures, or concepts suggested by neoinstitutional theory or theories of institutional systems (see David and Bitektine, Chapter 10, this volume). Another is whether national culture analysis should be abandoned in favour of research about the values and the cognition of individuals, or whether collective culture characteristics can instead provide a better basis than surveys of individuals for drawing inferences about cognition. A further controversy concerns whether there is so much variability within nations, or such strong patterns of multiple-nation similarity,

that other boundaries around culture should be considered. I will deal briefly with each of these after discussing Hofstede's view of culture and cultural dimensions.

Culture and the 'collective programming' metaphor

Many of the controversies surrounding Hofstede's project relate to theoretical perspectives on the nature of culture. Researchers often cite Kroeber and Kluckhohn's (1952) review of competing definitions. The metaphor of 'collective programming' does not derive from any particular line of theory, although it suggests several. Currently, the many definitions of culture in use reflect major theoretical perspectives. For psychologists who study attitudes, values, or personality, Hofstede's metaphor draws attention to constructs that will have adequate psychometric properties in a given society, indicating that individuals use them frequently and understand them intuitively (Kirkman et al., 2006). The metaphor also evokes ways of thinking that are shaped by the social norms reflected in what children are rewarded and punished for by their parents, teachers, and childhood friends (Rokeach, 1968, 1973). For psychologists who are closer to theories of scripts, schemas, and cognitive structures, it implies that the way in which such ideas are structured are shaped by socialization (Markus and Kitayama, 1991; Nisbett et al., 2001). For linguists and anthropologists, the idea of mental programming invokes the Sapir-Whorf view that language is an aspect of culture that shapes what people intuitively understand (Sapir, 1921; Whorf, 1956). For sociologists and political scientists using survey methods, the metaphor suggests the sort of nation-level measures of societal norms noted in the preceding discussion of level of analysis. For sociologists and political scientists interested in social institutions, collective programming might call to mind the sort of influence that institutions like governments and churches generate (Whitley, 1999), as well as the influence of less tangible institutions such as normative systems (Kostova and Roth, 2002).

"Hofstede proposed *the idea of collective programming at a time when dimension-based theories were prominent in psychology and sociology, and this idea has frequently ...*" frequently been interpreted in terms of value dimensions. Many of the alternative theoretical perspectives developed later could not have been anticipated when Hofstede presented his perspective in 1980. These alternatives, however, permit useful reformulations of the 'collective programming' metaphor, each of which is likely to produce different responses to the issues discussed in this chapter.

Dimensions and nation scores

Challenges to uses of Hofstede's nation scores are many and well known. The use of these scores is criticized for relying on old data about phenomena that are susceptible to change, for being collected in a single company (IBM), for being based on items that have little face validity, and for being too quantitative and missing too much about the most important aspects of culture. The responses are also well known. Researchers who seek to explain culture tend to consider aspects of a nation's heritage reaching much longer into the past than the date of the IBM survey, studying a single company controls for many extraneous variables, the measures have construct validity in relation to a host of external criteria, and the measures are useful but are incomplete compared to the insight gained from learning a language and living and working in a society. Hofstede reviews and responds to these controversies in depth in the second edition of *Culture's Consequences* (Hofstede, 2001).

Alternative national culture dimensions schemes

The first edition of *Culture's Consequences* in 1980 established a paradigm for national culture research. One key characteristic of the paradigm is that questionnaire surveys can be used to collect individual data, which are then combined based on nation-level measurement structures. The dimensions measured are indicators of values that characterize the

national norms and institutions that societies use to fulfil generic social functions. A number of projects fall within this paradigm, but do so creatively by proposing alternative systems of dimensions.

The four most influential are projects based on the *Schwartz Value Survey* (Schwartz, 1994; Schwartz and Ros, 1995), the *Riding the Waves of Culture* model (Trompenaars and Hampden-Turner, 1998), the *World Value Survey* (Inglehart and Baker, 2000), and the *Global Leadership and Organizational Behaviour Effectiveness* project (GLOBE; House et al., 2004). The Schwartz Value Survey project introduces seven nation-level culture dimensions, which contrast with one another—conservatism-intellectual autonomy and affective autonomy, hierarchy-egalitarianism, and mastery-harmony. Nation-level scores based on data from secondary school teachers and university students are available for a number of these (Schwartz, 1994; Schwartz and Ros, 1995). Trompenaars and Hampden-Turner (1998) collected an extensive consulting-based data set that identified seven variables, the first five of which are based on the 'theory of action' developed by Parsons and Shils (1951); universalism-particularism, individualism-communitarianism, specific-diffuse, affective-neutral, achievement-ascription, sequential-synchronic, and internal-external. Smith et al. (1996) applied multidimensional scaling to this data set to identify two dimensions; egalitarian commitment and utilitarian involvement, contrasted with loyal involvement—an interesting nuance on the individualism-collectivism theme. The GLOBE project (House et al., 2004) augments Hofstede's project by creating nine culture dimensions; power distance, uncertainty avoidance, future orientation, institutional collectivism, in-group collectivism, performance orientation, gender egalitarianism, assertiveness, and humane orientation. Inglehart and Baker (2000) identify two national cultural dimensions; traditional/secular versus rational, and survival versus self expression.

The illustrations of culture dimensions drawn from these projects should not discount

their significance as alternatives to Hofstede's original dimensions. Each project is at least as extensive in the number of individuals surveyed and nations represented, as was the Hofstede project. Each is also linked to a significant theoretical tradition, and they each provide alternatives within the paradigm of nation-level research about culture dimensions.

Alternatives to cultural value dimensions

The paradigm of national dimensions based on values competes with a reasonably compatible set of alternatives. The social axioms model, for example, represents cultural differences in attribution propensities or systems of beliefs (Leung et al., 2002). Another approach renews a neglected theme in comparative sociology, organization studies, and social psychology concerning the analysis of societal differences in emphasis on a set of social structures including roles, rules, and norms that influence the behaviour and thinking of the society's members (Peterson and Smith, 2000 and 2008; Smith et al. 2002). This research identifies five organizational role categories—superiors, subordinates, colleagues, staff specialists, and one's self, as well as organizational rules and organizational and societal norms.

Individual level research on cultural themes

Several cross-cultural researchers have suggested that the field should move away from studying national culture dimensions. Instead of treating national culture as an indicator of a range of experiences that shape cognitive structures, characteristics of individuals should be measured using personal values surveys, and these measures should be used to test individual-level hypotheses. A recent review (Kirkman et al., 2006) suggests that many if not most studies that evoke Hofstede's (2001) conceptual basis and culture dimensions proceed in this way by linking the values of individuals, rather than national culture values, to various criteria.

The challenge of research into individual expressed values is that such constructs

represent the more rationalized but less behaviourally influential aspects of cognition (Peterson and Wood, 2008). It is tempting to extend the value label too far by interpreting expressed values as implicit, taken-for-granted aspects of cognition. People do think deliberately and act based on conscious values or beliefs under circumstances that are unfamiliar, threatening, or emotional. The argument for representing national culture by aggregating responses to attitude and value surveys rests on the effects of socialization on such conscious thought. Unfortunately, using expressed personal values as *indicators* of culture has made it tempting for researchers to mistakenly *equate* culture with patterns of conscious thought. Cognition research since the 1970s suggests that most aspects of behaviour are driven by less accessible cognitive mechanisms (Kahneman, 2003; Nisbett and Willson, 1977). Indicators of the culture in which a person has been socialized may be better indicators of aspects of these cognitive processes than are the values that individuals overtly express.

Extending the national culture paradigm to individuals

The distinctions in meaning reflected in differences between the structures for measures of values at individual and national levels make it challenging to connect levels of analysis. Since the structure of values at the nation level differs from that at the individual level, it is inappropriate to conclude without testing that research findings at one level apply to the other. For example, if societal values of individualism and gross national product are associated at the *nation* level, then preferences for individualism may or may not be associated with wealth at the *individual* level. Nevertheless, individuals are certainly exposed to, experienced in, and socialized into the cultures of their nations, so it is reasonable to expect that societal culture does affect individuals. How, then, does one do research that connects levels?

Studies that connect societal culture characteristics to nonconscious cognitive characteristics other than attitudes or values

typically follow cognitive psychology research paradigms (Peterson and Wood, 2008). Individuals are randomly assigned at birth to a cultural group. In experimental studies, a researcher then assigns them to more immediate experimental conditions to study the joint effects of culture and other experimental manipulations. Although just like any other experimental manipulation, individuals respond differently to the culture to which birth assigns them, culture can be treated as a quasi-experimental condition to which they have been exposed (Campbell and Stanley, 1963). As in any quasi-experimental context, the analogy with experimental manipulation is approximate; cultural groups are more complex than any ordinary experimental manipulation, and those who participate in research are often atypical of their cultural group. Nevertheless, experimental studies of culture and cognition often treat the implications of national culture for structuring the cognitive processes of a society's members as more relevant than the values that individuals express in surveys.

Studies that include a method to check the applicability of national values dimensions provide one option that can be useful for individual level research that evokes culture explanations. Brett and colleagues (1997), for example, report several negotiation studies using Japanese and US managers attending executive training as participants. These researchers recognize that the participants in their studies are not a random sample of the participants' national cultures, but a sample of people who have found their way into managerial roles. Nevertheless, the researchers recognize that their participants live and work in a national cultural environment. The researchers, therefore, expect group differences with respect to participants' cultural values, but do not expect individual participants' expressed cultural values to act as mediators. Brett and her colleagues use their participants' cultural values as a form of quasi-experimental sampling check to evaluate whether the relative positioning of the national samples of managers is consistent with cultural theory (Jeanne M. Brett,

personal communication, October 2007). This approach is most evident among psychologists who emphasize cognitive structures, such as Markus and Kitayama (1991) and Nisbett et al. (2001). However, a similar logic of looking for ways to evaluate the validity of society-level culture implications for individuals can also be found in research emphasizing values in the Rokeach (1968) tradition.

Nation, subculture, or region

Studying individuals rather than cultural groups is not the only alternative to studying national cultures (Peterson and Smith, 2008). Although some subcultures are typically recognized as exceptions in the major comparative studies of nations, the significance of within-nation subcultures is sometimes advocated even more strongly (Lenartowicz and Roth, 2001). Projects advocating the analysis of within-nation subcultures have emphasized the use of regions for explaining variance in the values of individuals and have studied relatively small segments of the world. Further work of this kind must progress beyond these limitations to develop and validate measures based on subculture-level measurement structures that are analogous to those that have been used for developing nation-level measures. This work also needs to compare within-nation variability to between-nation variability on a global scale.

Studies of multiple-nation regions, sometimes referred to as 'civilizations' (Huntington, 1993), are another alternative for analyzing cultural groups (e.g., Ronen, 2006). Ordinarily, however, multiple-nation regions are proposed as a way of identifying global cultural forces that supplement and explain rather than replace analyses of national cultures.

International research collaboration

Handling the technical and metatheoretical issues in international research is a social process as well as an intellectual one. Two issues that remain in the background of most domestic research surface in debates in

international collaborative teams. One is the way researchers need to interact in order to move between theory and experience. Polanyi (1958/1962, pp. 69–131) discusses this in his chapter about 'articulation' as a social process of explicating tacit knowledge. A second issue concerns bringing ideas, theories, and methods developed in one part of the world into contact with the system of meanings used in another (Peterson and Pike, 2002). These issues appear in international collaboration in part due to issues of conceptual equivalence; ideas that researchers from one locale find important are seen as less important or omit the concerns of researchers in other locales. Hofstede (1996) and Erez (1990) identify topical preferences in different parts of the world, and these also occur due to the cultural basis of intellectual traditions that influence preferred theories and methods (Weber, 1951).

Two other contingencies that affect international research team dynamics are the researchers' institutional context and their project aims (Peterson, 2001). Unpleasant politics can develop when viewpoints become tied to the images and identities of particular parties (Dutton and Dukerich, 1991). From the GLOBE project, Hanges et al. (2005) provide examples of how viewpoints and identities can become linked in international collaborative projects.

Normative and descriptive international collaboration

The literatures about international research collaboration include both normative material about how such research should be conducted, and descriptive reports about the experiences of international teams. Normative commentary includes recommendations about how to avoid over-reliance on concepts and theory from one nation, particularly United States (Boyacigiller and Adler, 1991; Peng et al., 1991; Peterson, 2001), and how input from the locales in which data are to be collected should be obtained. Some recommended procedures focus on how to draw the most from *researchers* from different nations (Brett et al., 1997), while others focus on how best

to learn from *respondents* in different locales (Morris et al., 1999). These approaches to de-centring international research are complementary.

The effectiveness of procedures for de-centring research is also now debated as part of a descriptive literature on international team dynamics (Hofstede, 2006; Javidan et al., 2006). A few international teams have been reflective about their dynamics. Easterby-Smith and Malina (1999) describe the conduct of an ethnographic project. Graen et al. (1997), Hanges et al. (2005), IDE (1981), MOW (1987), and Teagarden et al. (1995) describe the dynamics of very large-scale projects. Meziar et al. (1999) illustrate what can happen when US researchers study the global programmes of a US multinational organization. Tannenbaum et al. (1974) provide an example of how to incorporate different researchers' interpretations of the same data. Peterson (2001) provides a useful review of these projects. An analogous descriptive literature about handling process issues in international management teams is also relevant (Baba et al. 2004; Maznevski and Chudoba, 2000). Current commentary on international research collaboration suggests that normative models should be seen as alternatives. No model is universally optimal. The recommendations that follow draw both from the normative and descriptive literatures of international collaboration.

Research purposes affect collaboration

At least four purposes affect why researchers become involved in leading or participating in an international collaborative research project (Peterson, 2001). A collaborative group is composed of people who are likely to have a variety of purposes, and groups as a whole are likely to vary in the level of agreement about purposes. Choices about the methods, concepts, and viewpoints discussed in the foregoing section are influenced by the purposes of individual collaborators. The distinction among the first three of the following purposes has been frequently discussed (Boyacigiller and Adler, 1991).

First, research that focuses on *replication* is constrained to repeat procedures followed in the original research. To the extent that this purpose is accepted, debates about methods and concepts derail the project. Previously fixed procedures are so central that even the personal involvement of the researcher or group that did the original work can be limited to providing advice rather than directing the new team. An example would be continuing use of the Schwartz Value Survey (Schwartz and Sagiv, 1995) once the measures had become established.

Second, some research uses a dispersed set of collaborators to develop insight into a *research problem that is viewed as universal*. This purpose characterizes particular members of many research teams, although it is less often characteristic of a team as a whole. In this case, it is the theoretical and technical views of the collaborators that are treated as central, rather than the unique qualities of their cultural backgrounds, or the context in which they will collect data. For example, at the time of writing, I am collaborating with a group of negotiation researchers to contribute ideas about culture research methods where this knowledge is viewed as not being based on my cultural background (Metcalf et al., 2007).

Third, international collaborative research frequently draws upon the *cultural knowledge and setting of collaborators*. That is, a collaborator is part of a group, either because of a personal intuition for how a research problem is likely to be best formulated in their setting, or because of the ability to collect data in a particularly setting. This is consistent with integrating multiple methods and perspectives. For example, the negotiation study that I am assisting includes researchers from several nations who have an intuition about how negotiation works in their country, and whose contribution reflects the purpose of promoting the project's applicability in many nations.

Fourth, *intended outlets and audience* influence project design and conduct. Universities in United States and Europe are increasingly driven by criteria from

accrediting and funding agencies that base faculty resources and rewards on research that is published in a list of frequently cited journals. The functioning of international teams is then shaped by personal values and reward systems that influence the importance that their members place on conforming to the norms associated with producing such publications.

Those who manage or collaborate in international teams must recognize that these purposes create very different ways of operating. Purposes for being involved in a project, though, are not personality traits. One can be involved simultaneously in multiple projects that have quite different mixes of these purposes.

Resource implications

The normative literature on international collaborative research tends to be abstract and idealistic, while the descriptive literature tends to be selective in focus. Figure 19.1 attempts to remedy both problems by drawing attention to the elements in setting up a research project. Most of these elements

are present in any project, but are easily overlooked when researchers are so intent on a normative model of how to conduct good cross cultural research that they lose track of the resources that are needed to do any kind of research. Resources designated in Figure 19.1 as 'social' are those typically not contracted in exchange for money, but are based on networks, understandings, and relationships. Contracted resources are those that typically require funds. Theory resources and data analysis resources represent the body of knowledge and practice that the group will need to access. Organizing skills are those needed to coordinate a project group and link it to outside parties in its academic discipline(s). Collaborators from different nations typically contribute to different subsets of these elements.

The process of setting up a group oscillates between finding collaborators, negotiating purposes, and assembling resources. If these elements seem to be in sequence, such that one is needed before the other, this is not intended. They are set out randomly to indicate that in crystallizing a research project, these tend to

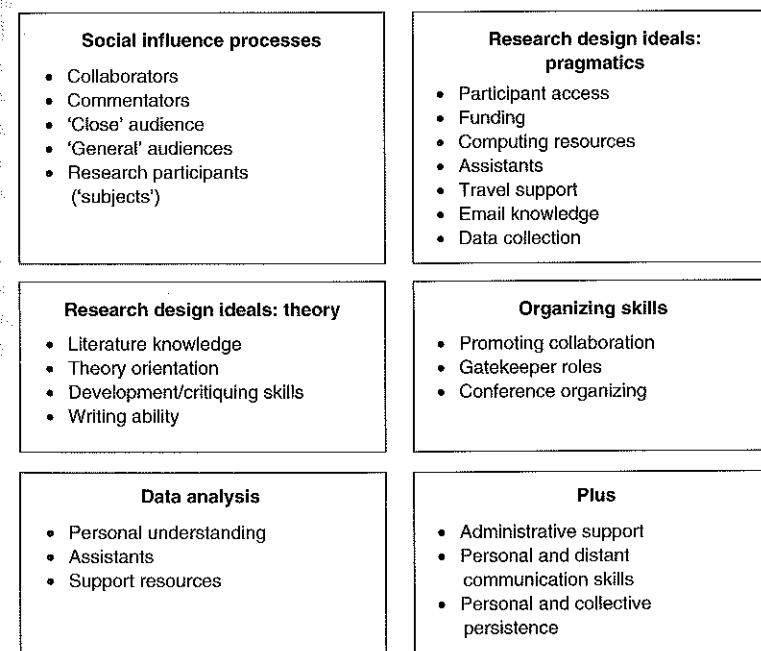


Figure 19.1 Elements in a research project.

occur chaotically. At some point, through the deliberate, proactive efforts of an individual, a few collaborators, or occasionally a larger group, the elements of a research project come to be formulated with sufficient clarity that the project can be announced. Other collaborators are then recruited and focused work can begin. Whichever elements remain indeterminate are either worked out or remain to haunt the project and threaten its completion.

Regardless of research process ideals or the explicit plan for a project, power considerations suggest that collaborators are likely to have credibility and influence to the extent that they can contribute the resources for handling one or more of the elements in Figure 19.1 that others in the group find important. The ability to provide those resources makes it predictable that large multiple nation projects will become centralized. If most project organizing experience, resources, and research skill comes from one or two people, any project is likely to reflect the preferences of a few individuals. Anyone joining an international research team would do well to consider what resources they bring, and any group taking an ideological stance about how the research process should unfold might consider whether its ideals are consistent with the distribution of resource contributions from its various members.

Project coordination

What are the options for coordinating an international collaborative project if one is in a high-power position, or for influencing the group where one's power is more limited or spread more evenly? Theories of social structures help identify these options (Peterson, 2001; Peterson and Smith, 2000; Smith and Peterson, 1988). Rules or norms about science, rules and norms negotiated by the group, guidance from an experienced leader, or ongoing collegial adjustments among experienced colleagues, all provide alternatives.

Two normative theories about how to manage international collaboration from Brett et al. (1997) and Morris et al. (1999) rely on universal arguments about the nature of doing good social science to develop a set

of procedures. As normative theories, they rely on *norms* of science to guide social behaviour, specifically variants on Berry's (1969) adaptation of Pike's (1960) ideas about emics and etics, concerning the use of cross-cultural surveys. As attempts to promote particular scientific norms, these views do not directly discuss the issue of whether the members of a collaborative research team will accept them. The variant proposed by Morris et al. (1999) applies best when a project includes a substantial replication element as well as including the potential for adaptation. The Brett et al. (1997) variant applies best where new theory and new measures are being designed. If a team genuinely accepts one of these or some other normative model, the model can well provide a procedure to follow that will combine prior research, the unique cultural intuition of collaborators, and the input of research participants in a fruitful way.

Considering other aspects of social structure besides norms provides other alternatives for guiding a collaborating group's work. Drenth and Wilpert (1980) explain the process for crystallizing and transforming local group *norms* into a system of *rules* by creating an explicit social contract. Such working rules have the potential to make explicit many of the issues in Figure 19.1. Easterby-Smith and Malina (1999) describe the process of relying heavily on norms based on the trust that individual collaborators develop through experience.

Relying on a strong group leader can also prove useful. Many cross-cultural projects are known by the name of the person who coordinated them. The GLOBE project (House et al. 2004) provides an example of how to coordinate a project based on a clear *leader* and carefully thought out *explicit hierarchy* of roles and responsibilities. Hanges et al. (2005) describe a number of challenges faced by the GLOBE project, and how the senior leader and leadership group addressed them, in part by relying on norms about how to do good social science, rules in the form of social contracts, and other social structures such as collaborators chosen to fit the purposes of the project.

International research without questionnaires

This discussion has centred on survey research, principally because that is the dominant approach to cross-cultural organizational research. There are other extensive international literatures that deal with explicit systems of rules, laws, and economic indicators that do not evoke the concept of culture. These draw from literatures of neoinstitutional theory (Kostova and Roth, 2002), national business systems (Whitley, 1999), and sovereignty (Krasner, 1988). One challenge in such work is to contrast laws in nations where they tend to be followed and enforced, with laws in nations where they are public expressions of ideals. Another challenge is to know how to interpret economic data where those providing information about, say, wages paid or received, operate in a system that rewards distorting the information reported. Collaboration with local researchers with a through knowledge of the local business context is likely to be particularly useful in these cases.

A certain amount of qualitative ethnographic analysis is built into survey-based projects when the modes of decentring methods described in the preceding section are followed (Morris et al., 1999). Occasionally, distinct ethnographic projects also appear in the cross-cultural management literature. To date, these have tended to be conducted by relatively small groups and represent only a few nations (Baba et al., 2004; Farh et al., 2004; Maznevski and Chudoba, 2000). Easterby-Smith and Malina (1999) describe the social aspects of their research process in some depth. One larger and more loosely coordinated international comparative analysis of psychological contracts relied on a combination of historical analysis, ethnography, and survey research (Rousseau and Schalk, 2000). The collaborators in that case worked from a core of basic constructs and a loose analytical framework, but the researchers from each nation conducted their analyses in individualized ways. This project merges with a larger literature in which a researcher edits a book that includes chapters

on a particular theme, including contributions from multiple nations.

A challenge for research relying heavily on local concepts is to find a basis for comparison. To the extent that much survey research is introduced or informed by historical and ethnographic analysis, survey studies implicitly provide this sort of comparative framing. If a large number of ethnographic projects conducted separately in multiple nations were to produce the sort of fragmentation that characterized anthropology in the mid-twentieth century, the *post hoc* coordination provided by the Human Relations Area File (Murdock, 1940) would be useful for establishing an accessible index and facilitating comparisons.

CONCLUSIONS

International collaborative research has a technical side, a methodological and theoretical orientation side, and a social process side. The purpose of this chapter has been to identify issues and literatures that can help to think through each of these. International collaborative studies bring together researchers whose cultural backgrounds and intercultural experiences lead them to make different assumptions about how the group will operate. It also brings together people who have different degrees of skill and different views about technical issues, who are likely to have different perspectives about methodological and theoretical issues, and who bring unique personal research interests, career plans, and resources. A review of international collaborative projects suggests that those which produce influential research seldom met all of the ideals of any normative model about how they should operate (Peterson, 2001). A clear sequence of moving back and forth between prior theory and new experience is rarely followed, but both inputs are typically included in some way. All collaborators rarely contribute equally to the research design or execution, most typically because many have other interests and other demands on their time apart from the interests of a core group.

Human nature can be helpful in international research collaboration. People

are able to use different theoretical and methodological tools at different times for different purposes. We are able to use a map containing representations of roads even when such maps do not recognize trees and houses. We can also refer to a photograph without worrying whether or not map making is incommensurate with photography, and vice versa. Similarly, social scientists are frequently able to see analogies between quite different ways of analysing cultural characteristics and understanding societies. Whereas theories can be incommensurate such that one does not recognize the existence of central elements of another, people collaborating in international teams are likely to be more successful if they seek elements of complementarity. Ideologies and attitudes about how research *should be* conducted tend to give way to pragmatic considerations of personal necessity and resource limitations about how research *can be* conducted.

In order to become involved in an international project in a way that is likely to be satisfactory to all concerned, the considerations identified here suggest that one should look for a project having several characteristics. It should have:

- Members with adequate technical skills
- A constructive way to develop technical agreements
- Basic methodological and theoretical orientations where differences are viewed as complementary rather than incommensurable
- Compatible purposes
- A working consensus about the norms, rules, and roles that will guide the research process

To the extent that these qualities are lacking, as they always will be to some degree, efforts to improve the project group need to attend to all of these, and not just to a preferred subset.

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