

## ETHNOGRAPHIC FIELDWORK AND INTERPRETIVE POLITICAL SCIENCE\*

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## Introduction

Political anthropology is a minority sport and until recently there was little work by political scientists (Aronoff and Kubik 2013: 19; Schatz 2009a: 1). Auyer and Joseph (2007: 2) examined 1,000 articles published in the *American Journal of Political Science* and the *American Political Science Review* between 1996 and 2005. They found that ‘only one article relies on ethnography as a data-production technique’. There are no chapters on political science in the comprehensive surveys of ethnography by, for example, Atkinson et al. (2007) and Bryman (2001). So, there are no schools of thought about the theory or methods of political ethnography. Auyero and Joseph (2007: 2) conclude there is a ‘*double absence*: of politics in ethnographic literature and of ethnography in the study of politics’ (emphasis in the original).

In the 2000s, the interpretive approach became more prominent in political science notably in the fields of comparative politics (for surveys of the field see Aronoff and Kubik 2013; Schatz 2009; Wedeen 2010), and public policy analysis (for a survey of the field see Wagenaar 2011). It is not my task to describe and comment on substantive fieldwork reports. My concern is limited to ethnographic methods. The main fieldwork-based texts in comparative politics are discussed in Chapter 17 below. The main fieldwork-based texts in public policy and administration are discussed in Chapters 24 and 26 below. Elsewhere in Anglophone political science, there are only pockets of ethnographic work on, for example: parliament (Crewe 2005); party conferences (Faucher-King 2005); street-level bureaucrats (Maynard-Moody and Musheno 2003); and ministers and public servants (Rhodes 2011). In short, ethnography is conspicuous for its absence in political science.

If there are no major schools of thought in political ethnography, there are some defining debates in the literature and I organise my discussion around these debates. I begin by distinguishing between naturalist and interpretive ethnography. I also distinguish between studying-down and studying-up, providing an example each (see Figure 1.1)

INSERT FIGURE 1.1 ABOUT HERE

Figure 1.1 Varieties of ethnography

	<b>Naturalist</b>	<b>Interpretive</b>
<b>Studying-down</b>	Kaufman 1961	Maynard-Moodie and Musheno 2003
<b>Studying-up</b>	Fenno 1978 and 1990.	Rhodes 2011

Second, I review the shared toolkit; focusing on fieldwork, participant observation and ethnographic interviewing. Third, and at the heart of the chapter, I survey the defining debates surrounding ethnographic methods arising from the ‘culture wars’ of the 1980s in anthropology. Finally, I offer some comments on future trends in political ethnography, focusing on, for example, hit-and-run ethnography, and ‘new’ methods for recovering data.

### **Naturalist ethnography**

Naturalism refers to the idea that ‘The human sciences should strive to develop predictive and causal explanations akin to those found in the natural sciences’ (Bevir and Kedar 2008: 503; and Chapter 2 in this volume). Over the years, there has been some impressive naturalist political ethnography in political science. I give an example of studying-down and street-level bureaucrats, and of studying-up and governing elites.

The term ‘street-level bureaucrat’ was coined by Michael Lipsky (1980: xii) and refers to teachers, police officers and social workers and any other semi-profession in face-to-face contact with clients of state services. Although the term ‘street-level bureaucrat’ was not in common currency, Kaufman’s *The Forest Ranger* (1960) pioneered the topic. He studied forest rangers and their supervisors in five districts. He visited the first district for seven weeks and the other districts for one week each. There were also social visits to their families in the evening. He calls them ‘switchboards’, adapting general directives to specific conditions and areas. It is a pivotal position. Anyone who tries ‘to direct activities on a Ranger district without going through the Ranger can be sure of swift and vehement objection by the field officer’ (Kaufman 1960: 210). It is a classic example of the street-level bureaucrat, only they patrol trails, not streets.

For nearly eight years, Fenno (1978 and 1990) shadowed 18 US members of Congress in their Districts. He made 36 separate visits to the districts and, spent 110 working days with them. His visits varied from three to eleven days. In eleven cases, he supplemented the visits with ‘a lengthy interview’ in Washington. He seeks to answer two questions. What does an elected representative see when he or she sees a constituency? What are the consequences of these perceptions for his or her behaviour? What his fieldwork revealed was how each member of Congress developed their own ‘home style’ - a way of presenting themselves to their constituency – that helped them to achieve the three goals of re-election, power in Congress, and good public policy (Fenno 1990: 137). The presentation of self by members of Congress in their everyday constituency life was the surprise finding.

Naturalist ethnographies treat ethnography as a method for collecting data. The emphasis falls on systematic data collection, validating that data, avoiding observer bias, and writing up in the third-person (and see Werner and Schoeple 1987 for a detailed account of

how to achieve rigour in ‘ethno-science’). They also seek to test mainstream political science theories. For example, Kaufman explores the ideas of control and coordination from the public administration literature. Fenno (1978: xii-xiii) locates his study in the literature on representative-constituency relationships. Finally, for proponents of naturalist ethnography, the researcher’s role is that of detached observer (Fenno 1990: 79).

### **Interpretive ethnography**

Interpretive political studies draw on anti-naturalist philosophical thinking and emphasize the importance of meanings in the study of human life (see Bevir and Rhodes 2003 and chapter 1 above). It shifts analysis away from institutions, functions and roles to the actions and practices of interdependent actors towards an understanding of actions, and practices. We need to grasp the relevant meanings, the beliefs and preferences of the people involved and:

go beyond the bounds of a science based on verification to one which would study the inter-subjective and common meanings embedded in social reality ... this science would be hermeneutical in the sense that ... its most primitive data would be a reading of meanings (Taylor 1971: 45).

So, returning to street-level bureaucrats, Maynard-Moody and Musheno (2003: chapter 3 and 167-77) spent six to ten months in five research sites interviewing and observing cops, teachers and counsellors. They collected 157 everyday work stories from 48 street-level workers. Their narrative analysis showed that street-level bureaucrats ‘actually make policy choices rather than simply implement the decisions of elected officials’. Their

beliefs about clients fixed client identities, often stereotyping them, which, in turn fixed the beliefs of street-level bureaucrat about their occupational identity as, for example, bleeding heart or hard-nosed. Maynard-Moody and Musheno describe the practices of street-level bureaucrats in managing the ‘irreconcilable’ dilemmas posed by clients’ needs, administrative supervision (of rules and resources), and the exercise of state power.

Returning to elites, Rhodes (2011) observed the office of two British ministers and three permanent secretaries for two days each, totalling some 120 hours. He also shadowed two ministers and three permanent secretaries for five working days each, totalling some 300 hours. He conducted lengthy repeat interviews with: ten permanent, five secretaries of state and three ministers; and 20 other officials, totalling some 67 hours of interviews. He also had copies of speeches and public lectures; committee and other papers relevant to the meetings observed; newspaper reports; and published memoirs and diaries. Shadowing produced several surprises; for example, he found that a key task of civil servants and ministers was to steer other actors using storytelling. Storytelling organises dialogues, foster meanings, beliefs, and identities among the relevant actor. It seeks to influence what actors think and do, and foster shared narratives of continuity and change. It is about ‘willed ordinariness’ or continuities. It is about preserving the departmental philosophy and its everyday (or folk) theories. It is about shared languages that enable a retelling of yesterday to make sense of today. This portrait of a storytelling political-administrative elite, with beliefs and practices rooted in the nineteenth century Westminster constitution, that uses protocols and rituals to domesticate rude surprises and recurrent dilemmas, overturns the conventional portrait.

Interpretive ethnographies treat ethnography as a way of recovering meaning; that is, beliefs and practices. The researcher writes ‘our own construction of the other people’s constructions of what they and their compatriots are up to’ (Geertz 1973: 9). The knower and

the known are inseparable, interacting and influencing one another, leading to shared interpretations (Lincoln and Guba 1985). The emphasis falls on writing up fieldwork that has an ‘inherently story-like character’ and authors have ‘inevitable choices’ to make about how they will present their findings (Van Maanen 1988).

### **The toolkit**

Interpretivists will object to my using the toolkit metaphor as irredeemably naturalist. Rather, they see ethnographic methods as analogous to *bricolage*, quilt making, or montage:

The interpretive *bricoleur* produces a *bricolage*; that is a pieced together set of representations that are fitted to the specifics of a complex situation (Denzin and Lincoln 2011: 4).

However, the *bricoleur* also has a set of tools, so the questions stand: how do we recover the data? The specific practices of the *bricoleurs*’ trade are fieldwork, participant observation, and ethnographic interviewing.

### **Fieldwork or ‘being there’**

Any account of fieldwork starts with the puzzle of what do ethnographers do? For Hammersley and Atkinson (2007: 2), ‘ethnography does not have a standard, well-defined meaning’. Nonetheless, some words and phrases recur. The ethnographer studies people’s everyday lives. Such fieldwork is unstructured. The aim is to recover the meaning of their actions by deep immersion, whether looking at a Congressional district, a government department or a tribe in Africa. Historically, it meant going to another country, learning the

language and studying the everyday lives of the inhabitants of a village, tribe, or whatever unit of social organization had been selected. For the novice, it was the only way to become a cultural anthropologist; 'you can't teach fieldwork, you have to do it'. For Wood (2006: 123), it is 'research based on personal interaction with research subjects in their own setting', not in the laboratory, the library or one's office. It is deep hanging out or intensive immersion in the everyday lives of other people in their local environment normally for a substantial period of time.

Of course, fieldwork has various pen names such as the 'thick descriptions' (Geertz 1973: chapter 1) and 'the extended case study' (Aronoff and Kubik (2013: 56-7). On the face of it, there are affinities with the case studies common in political science which are in-depth studies of a single unit or event. The method was criticized often for being idiographic and not fostering generalizations. Latterly, political scientists have devoted much effort to assimilating the case method to naturalism and its language of variables and hypothesis testing. For example, Wood analysed five case studies of peasant support for insurgent groups explicitly 'sacrificing ethnographic depth of analysis for analytical traction through comparison of cases that vary in the extent of mobilization observed'. It was her way of overcoming 'the obstacles to making valid causal inferences based on field data' (Wood 2007: 132 and 142). So, case studies can be simply descriptions of specific subjects but political scientists are enjoined to use them to build theory, to test the validity of specific hypotheses, and to test theories by treating them as the equivalent of decisive experiments (see Eckstein 1975: 92-123; see also Yin 2008).

An interpretive approach to fieldwork is markedly different because it goes for ethnographic depth; for deep hanging out. Anthropologists would not refer to their fieldwork site as a 'case study' because it is not a 'case' of anything until they withdraw from the field

to analyse and write up their field notes. Indeed, interpretive ethnography is less concerned with generalizations (see 'Debates' below) than with raising new questions and 'shaking the bag'. The aim is edification; to find 'new, better, more interesting, more fruitful ways of speaking about' everyday life (Rorty 1980: 360). So, fieldwork provides detailed studies of social and political dramas. As Burawoy (1998: 5) suggests, it 'extracts the general from the unique, to move from the "micro" to the "macro"'. For example, Crewe's (2005: 240) study of the British House of Lords focuses on rituals, rules, symbols and hierarchies, especially 'the meaning of its rituals and symbols and how people use them to make sense of the past, present and future'. Her 'anthropological perspective' draws on the analysis of political ritual; of 'ritual as the process of politics itself, rather than as a servant to it' (Kertzner 1988). She was a participant observer for two years between 1998 and 2001. She had a staff pass and 'was able to take part actively in House of Lords' working life'. It was deep hanging out. She shows how the everyday rituals of an institution seen only as a dignified part of the constitution 'give the backbenchers the feeling that they are transcending their individual powerlessness to become important components of an influential whole.' As a result, the rituals ensure acquiescence to the dominance of the executive. What is the large issue that springs from small events? Political rituals are not 'trivial and backward looking' but 'key elements in the symbolism of which nations are made' (paraphrased from Crewe 2005: 229-35).

Fieldwork has several advantages over other methods in political science. As Wood (2007: 124 and 132) notes, it is a source of data not available elsewhere and is often the only way to identifying key individuals and core processes. It is well suited to giving voice to groups all too often ignored; to disaggregating organizations; to understanding 'the black box' or internal processes of groups and organizations; and, distinctively, to recovering the beliefs

and practices of actors. In addition, Rhodes et al (2007: chapter 9) argue ‘being there’ gets below and behind the surface of official accounts by providing texture, depth, and nuance, so our stories have richness as well as context. It lets interviewees explain the meaning of their actions, providing an authenticity that can only come from the main characters involved in the story. Crucially, the ethnographic approach admits of surprises, of moments of epiphany, which can open new research agendas. It accepts serendipity and happenstance. Finally, it helps us to see and analyse the symbolic dimensions of political action.

Ethnographic textbooks cover a standard list of techniques and procedures for collecting such fieldwork data. Such lists cover access, fieldwork roles, fieldwork relationships, fieldwork notes, interviewing, and leaving the field. The budding fieldworker is advised to consult the numerous available texts (my favourites are: Agar 1996 [1980]; Bryman 2001; Hammersley and Atkinson 2007 [1983]; and Wolcott 1995). The main tools for recovering meaning are participant observation and ethnographic interviewing.

### **Participant Observation**

As the label suggests, the researcher both observes and participates in everyday life. He or she needs to get to know the people being studied. You do not have to be friends. You do need to be accepted; to fit in. Commonly observations are recorded in a fieldwork notebook. The level of involvement can vary from being a bystander with little rapport, through a balance between ‘insider’ and ‘outsider;’ roles, to full involvement and the risk of ‘going native’ (see DeWalt et al. 1998).

The most striking fieldwork practice of ethnographers for a political scientist is the document known as the ‘fieldwork notebook’ (see: Bryman 2001, Volume 2, Part 7; Emerson et al. 2011; Sanjek 1990). It is simultaneously invisible and ever present, part of the tacit

knowledge of ethnographers. Ethnographers learn about the fieldwork notes on the job. There is no agreed definition of a fieldwork notebook. For some it includes note taking from documents. For others, it is mainly notes about what they have observed. Even then, ‘observation’ is a broad category, covering everyday activities, conversations, pen portraits of individuals, new ideas about how to do the research, the diary of the ethnographer recording personal impressions and feelings. Jottings can be made on the run and more substantial notes compiled at the end of the working day. The practices of the ethnographer are diverse and well captured by Jackson (1990: 33-34). She suggests field notes are a key symbol of professional identity and they ‘represent an individualistic, pioneering, approach to acquiring knowledge, at times even a maverick and rebellious one’. They symbolize the ‘ordeal by fire’ that is journeying to the field and the ‘uncertainty, mystique and ... ambivalence’ of that journey.

Much political ethnography is not only micro in the sense of studying the details of everyday actions but also in the sense of locale. We study-down; that is we visit villages, factories, schools and local communities. We talk to police officers, social workers, teachers, gays, drug users, and everyday people. As Shore and Nugent (2002: 11) comment, ‘Anthropology, by definition, is the study of the powerless “Others”’; it avoids the study of elites (see also Nader 1974: 289; and De Volo, chapter 17). I offer no criticism of studying down. Rather, I observe that a central concern of political scientists is who governs, and to answer that question we need to observe governing elites, so studying-up is a prime research strategy.

In studying governing elites, there are some obvious difficulties in ‘being there’. The most obvious game changer is that ‘the research participants are more powerful than the researchers’ (Shore and Nugent 2002: 11). They control access and exit. They end interviews,

refuse permission to quote interviews, and deny us documents. They can control what we see and hear. In practice, it means the researcher is involved in continuous negotiations over access and who can and cannot be seen. The researcher's role varies, at times with bewildering speed. One day you are the professional stranger walking the tightrope between insider and outsider. Next day you are the complete bystander, left behind in the office to twiddle your thumbs. They not only enforce the laws on secrecy but also decide what is secret. We are playing a game with a stacked deck of cards, and we are the punters.

There are also emotional stresses and strains. Participation calls for involvement. Observation calls for detachment. Endlessly balancing the two is a strain. It can be exacerbated by the researcher's biases. Rhodes (2011) found he was more comfortable with some of the inhabitants of the Whitehall village than with others. So, managing one's biases is important. Living away from family and home can lead to attacks of the blues. Like the rock star on the road, one can bewail another night in another cheap hotel, and in my case, another commute on the London underground as well.

Of course, the researcher strives for a Panglossian view of the world. There is no mileage in worrying about difficulties until they arise, and many do not. But the brute fact is that when problems do crop up, the elites win. Elites are different (and see Rhodes et al. 2007 and Gains 2011 for a more detailed discussion).

### **Ethnographic interviewing**

The common format for an elite interview is a recorded, one-hour conversation around a semi-structured questionnaire (see for example: Dexter 2006 [1970]; PS Symposium 2002). Of course, it can be revealing in the hands of a skilled interviewer but it courts the danger of becoming a confining ritual. Our conception of an elite interview can be too narrow. All elite

interviewers know the permanent secretary and minister who can negotiate such an encounter with ease and ‘talk for an hour without saying anything too interesting’ (Rawnsley 2001: xvii-xviii citing Robin Cooke, former British Foreign Secretary). There is another choice besides this format – intensive repeat interviews. I like to see them as a series of friendly conversations albeit conversations with an explicit purpose. Elites will be more open in such extended encounters because, as Rawnsley (2001: xi) observes ‘they have to tell an outsider because they are so worried about whether it makes sense or, indeed, whether they make sense’. Such interviews are still a negotiation. Their success depends on intangibles like trust and rapport. With trust and rapport comes far more information than can be obtained from working through a semi-structured questionnaire. And this information can be cross checked against observations - did they do what they said?

Fieldwork based on participant observation and intensive or ethnographic interviewing is the long-standing heart of ethnography not just political ethnography of whatever hue. It was not without critics among mainstream anthropologists. For example, Werner and Schoepfle (1987: 257-60) consider both participation and observation problematic with bias from class, language, gender and ethnicity ever present dangers. But such views are tepid compared to the heated ‘postmodern’ or ‘discursive’ challenge of the 1980s. It banished such basic ideas as deep immersion and participant observation. As a result, ethnography became a diverse and disparate set of practices.

## Debates

Political science may pay little attention to ethnography but the debates that took place in cultural anthropology in the 1980s continue to inform, even shape, ethnography in

the twenty-first century, and interpretive political ethnography is no exception. The best known text in these ‘culture wars’ is James Clifford and George Marcus, *Writing Culture: The Poetics and Politics of Ethnography* (1986). My starting point is their examination of the problems of representation, generalization, objectivity, explanation, and reflexivity. In each case, where possible, I provide political examples and indicate how the debate has moved on.

### **Representation**

Fieldwork’s claim to ethnographic authority in representing other cultures was a prime target for deconstruction. It was said to produce colonial, gendered and racist texts with a specious claim to objectivity that ignored power relations between observers and observed and failed to link the local to the global. The aspiration to represent a culture was rejected:

‘culture’ is not an object to be described, neither is it a unified corpus of symbols and meanings that be definitively interpreted. Culture is contested, temporal, and emergent’ (Clifford 1986: 19).

The aim was to deconstruct all essential concepts, all generalizations. So, we have:

‘a trend towards the specification of discourses in ethnography: who speaks? who writes? when and where? with or to whom under what institutional or historical constraints (Clifford 1986: 13)?

The classic intensive fieldwork study was challenged by hit-and-run ethnography. So, in its place, we ‘study-up’, and ‘follow through’ by conducting ‘yo-yo-research’ in ‘contact zones’ and multi-local sites. These several shorthand expressions can be explained easily. ‘Studying-up’ refers to the study of elites not police officers, social workers, and teachers. ‘Studying through’ refers to following events such as making a policy through the ‘webs and

relations between actors, institutions and discourses across time and space' (Shore and Wright 1997: 14). 'Yo-Yo research' refers to both regular movement in and out of the field and to participant observation in many local sites (Wulff 2002). A 'contact zone' is the "space", such as a museum, in which peoples geographically and historically separated come into contact with each other and establish ongoing relations', usually characterised by inequality and conflict (Clifford 1997: 6-7).

Ethnographic practice is no longer limited to participant observation, yet that rite of passage known as fieldwork or deep hanging out remains the historic heart of the discipline. The postmodern critics are seen as lacking in direction and a poor substitute for deep hanging out. Thus, Bunzl (2008: 58) sees 'anthropology collapsing into paralysis' from its inability to transcend a myriad analyses of specific discourses. Its practices have become 'baroque' (Marcus 2007). I incline to Fox's (2004: 4) practical and pragmatic assessment of fieldwork; it is a 'rather uneasy combination of involvement and detachment' but it 'is still the best method we have for exploring the complexities of human cultures, so it will have to do'. It may be the best method but it is not the only one. I return to the practices of hit-and-run ethnography below.

Whether we practice deep hanging out or hit-and-run fieldwork, we will confront that most stubborn of problems, finding a way to provide an authoritative account of the fieldwork. Van Maanen's (1998: 2, 8 and 14) aspiration is to find 'more, not fewer, ways to tell of culture', and he identifies several ways of telling: realist tales, confessional tales and impressionist tales. *Realist* accounts are dispassionate, third-person documentary accounts of everyday life in which the author has the final word, pronouncing on the meaning of the culture under study. The *confessional* account is an autobiographical, personalized story, which tells the tale from the field-worker's perspective; and aims for naturalness and getting

it right in the end. *Impressionist* tales ‘highlight the episodic, complex and ambivalent realities that are frozen and perhaps made too pat by realist or confessional conventions’. Their accounts are ‘as hesitant and open to contingency and interpretation as the concrete experiences on which they are based’ (Van Maanen, 1998: 119). There is no agreed way of reporting from the field. The craft of writing is paramount. Each way of telling the tale will reveal only a partial truth. So, political scientists need to become self-conscious practitioners of a literary craft that embraces literary experimentation (Hammersley and Atkinson 2007: chapter 9).

### **Generalization**

The idiographic character of ethnographic fieldwork is invariably seen as a weakness by political scientists. Critics claim that, it is not possible to deduce laws and predict outcomes from fieldwork; that is, it is not possible generalize. Of course, researchers can and do make general statements from a case. What they cannot do is make statistical generalizations and propound laws. Moreover, Shea-Schwartz and Yanow (2012: 26-34) suggest that the deductive logic of inquiry so common in political science are not relevant to interpretive research. They suggest that the logic of abduction is better suited.

Abductive reasoning is a:

Puzzling out process [in which] the researcher tacks continually, constantly, back and forth in an iterative-recursive fashion between what is puzzling and possible explanations for it.

A surprise or a puzzle occurs when ‘there is a misfit between experience and expectations’. The researcher is ‘grappling with the process of sensemaking; of coming up with an

interpretation that makes sense of the surprise'. The researcher is on an 'interpretive dance' as one discovery leads to another. If deduction reasons from its premises, abduction reasons from its puzzle. The researcher does not deduce law-like generalizations but infers the best explanation for the puzzle. So, the interpretive researcher does not ask if the findings are generalizable but whether 'it works in context' (Shea-Schwartz and Yanow 2012: 46-49). The aim of interpretive research is complex specificity in context, not generalizations.

For Lincoln and Guba (1985: 110), 'the only generalization is: there is no generalization. Indeed, as I have already noted, edification rather than generalization can be the name of the game. However, we can aspire to 'plausible conjectures'; that is, to making general statements which are plausible because they rest on good reasons and the reasons are good because they are inferred from relevant information (paraphrased from Bourdon 1993). We can derive plausible conjectures from intensive fieldwork.

### **Objectivity**

For the naturalist political scientist, ethnographic research fails to meet the standards posed by the logic of refutation. For the deconstructionist, there are only partial truths. For all qualitative researchers, there is the question of how do we evaluate the quality of research. We must start by accepting that the knowledge criteria of the naturalist ethnography - the logic of vindication and of refutation – are inappropriate. There is no point in trying to pretend the ethnographic approach and its distinctive research methods is just a 'soft' version of the naturalist approach with its penchant for 'hard' quantitative data. They are simply different in both the aims and the knowledge criteria they employ. Such notions as reliability, validity and generalization are not seen as relevant when the aim of research is 'complex specificness' (Geertz 1973: 23; Wolcott 1995: 174). So what are the relevant knowledge criteria?

There are many suggestions up for debate. For example, Roberts (2002: 6 and 37-40) suggests the relevant criteria include 'adequacy, aesthetic finality, accessibility, authenticity, credibility, explanatory power, persuasiveness, coherence, plausibility, trustworthiness, epistemological validity and verisimilitude' (see also Shea-Schwartz and Yanow 2012: 91-114). In many of these endeavours, aesthetic and other criteria associated with writing fiction are prominent.

In contrast, Bevir and Rhodes (2003: 38-40) recast the ambition for objective knowledge. They argue it arises from criticizing and comparing rival webs of interpretation; from the forensic interrogation of rival stories. The political science community's continuing debates define and redefine the criteria by which we judge the knowledge claims of individual members of that community. It is not self-referential because the knowledge claims can be 'reconfirmed' by encounters with practitioners and citizens. So, we translate abstract concepts into conversations in fieldwork. These encounters and their conversations produce data which we interpret to produce narratives which are then judged by evolving knowledge criteria of the relevant scholarly community. All debates are subject to the provisional rules of intellectual honesty such as established standards of evidence and reason; we prefer webs of interpretation that are accurate, comprehensive, and consistent. Reconfirmation is also integral to comparing webs of interpretation. It is an iterative process. Narratives are subjected to academic judgements; concepts are redefined; and again translated for new encounters and conversations in the field.

I have given these examples of different ways of comparing stories not to advocate any one approach. My objective is to show that the interpretive approach is different. Interpretive

approaches cannot approximate to a Popperian logic of refutation, nor should they even try, but they do need to be explicit about the criteria for comparing narratives.

### **Explanation**

A common misconception about interpretive ethnography is that it aims only to understand actions and practices, not explain them. A distinction is drawn between the nomothetic search for explanatory laws of the social sciences and idiographic understanding of the interpretive sciences. For Clifford (1986: 19), the task is ‘the specification of discourses’. Lincoln and Guba (1985: 151-2) want to replace the notion of causality with ‘mutual simultaneous shaping’ in which ‘Everything influences everything else ... But the interaction *has no directionality, no need to produce ‘that particular outcome’* ... it simply happened as a product of the interaction – the mutual shaping’. Wherever you look, it would seem that interpretive ethnography describes actions and practices, but it does not explain them.

It need not be so. The philosophical analysis of meaning in action that informs an interpretive approach suggests a distinctive form of explanation, which Bevir (1999: 304-306) refers to as narrative. Some care is necessary because the term narrative has become a ubiquitous term in the twenty-first century. It comes in many guises; for example, auto-ethnography, life history, oral history, memoirs, and storytelling (see Czarniawska 2004 for a survey of narratives in the social science). Here, I use narrative as a form of explanation.

For Bevir 1999 (chapters 4 and 7) a narrative unpacks the disparate and contingent beliefs and practices of individuals through which they construct their world to identify the recurrent patterns of actions and related beliefs. The resulting narrative is not just a chronological story. Narratives are the form theories take in the human sciences, and they

explain actions by reference to the beliefs and desires of actors. They explain actions and practices in narratives by specifying the beliefs and desires that caused the actions. People act for reasons, conscious and unconscious. A memoir or a story or a life history is a narrative if it explains actions by explicating beliefs.

So, interpretive ethnography is about explanation, not understanding. The natural and interpretive sciences use different concepts of causation, and the interpretive version of explanation differs from that often found among political scientists. Narratives are the way interpretive ethnography explain actions and practices.

### **Reflexivity**

As Hammersley and Atkinson (1983: 14-15) point out ‘the reflexive character of social research ... is not a matter of methodological commitment, it is an existential fact’. So, ‘rather than engaging in futile attempts to eliminate the effects of the researcher, we should set about understanding them’:

There is an obligation placed upon practitioners to scrutinize systematically the methodology by which findings, their own, and those of others, were produced, and, in particular, to consider how the activities of researcher may have shaped these findings (Hammersley and Atkinson (1983: 236).

There is no ‘may’ in later editions of the book. Reflexivity becomes the principle that runs through the third edition of their book (Hammersley and Atkinson 2007: 14-18, 191, 236). Critical self-awareness is essential but the danger is that the text becomes about the researcher; a diary of his or her involvement in the field. An excess of reflexivity spills over into a narcissism that is as unpalatable as it is boring. I have much sympathy with Watson’s

(1987) prayer, ‘make me reflexive - but not yet’ because the goal of remaining a ‘professional stranger’ balancing engagement, detachment and critical self-awareness is equivalent to searching for the Holy Grail – always out of reach. Yet, there is no alternative to trying – it’s life as we know it.

### **Chugging ahead by developing the craft**

Wedeen (2010: 264) is correct when she observes we continue to look back to the debates of the 1980s. She suggests ‘we might want to chug ahead to the anthropology of the 2000s’. What would chugging ahead involve? It involves dissolving the distinction between quantitative and qualitative methods; accepting the need for both deep hanging out and hit-and-run ethnography; and broadening the toolkit with a more eclectic choice of methods.

### **Quantitative and qualitative**

The distinction between ‘qualitative’ and ‘quantitative’ methods is unhelpful (Schwartz-Shea and Yanow 2002). It suggests, for example, that researchers do not interpret their quantitative data (on which see Stone 2001: chapter 10; and Chapter 11 below). Rather, an interpretive approach does not necessarily favour particular methods. It does not prescribe a particular toolkit for producing data but prescribes a particular way of treating data of any type. It should treat data as evidence of the meanings or beliefs embedded in actions. So, it is a mistake to equate an interpretive approach with only certain techniques of data generation such as reading texts and participant observation. It is wrong to exclude survey research and quantitative studies for the reach of interpretive analysis. Shore’s (2000: 7-11) is a true *bricoleur* because his cultural analysis of the beliefs and practices of European Union elites

uses participant observation, historical archives, textual analysis of official documents, biographies, oral histories, recorded interviews, and informal conversations as well as statistical and survey techniques.

### **The deep hanging out and hit-and-run ethnography**

The interpretive approach with its emphasis on recovering meaning does have implications for how we collect data. It leads to a much greater emphasis on qualitative methods than is common among naturalist political scientists. In short, deep hanging out is an obvious tool for grappling with complex specificity. However, for ease of exposition, I have used the dichotomy of intensive fieldwork or hit-and-run ethnography, among others. Although a useful narrative device, it runs the risk of turning an alternative into a mutually exclusive choice. The relevant skills include both deep immersion **and** hit-and-run ethnography. Especially when studying elites, who may refuse to be observed, we need to be adventurous in our choice of tools. The future is greater eclecticism. If fieldwork goes, then the discipline does **not** go with it'. So, when training future generations of ethnographers, we need to remember there are many ways of being there other than deep hanging out, and hit-and-run ethnography is another way of being there.

### **New tools (for political science)**

The craft of political science would benefit from having a more varied ethnographic toolkit than participant observation and ethnographic interviewing. As illustrations, I sketch briefly the possible contribution of focus groups and visual ethnography.

### Focus groups

Focus groups are widely used in electoral studies but they are not seen as a tool for political ethnographers. They involve getting a group of people together to discuss their beliefs and practices. The groups are interactive and group members are encouraged by a facilitator to talk to one another. For Morgan (1997: 2), the ‘hallmark’ of focus groups is ‘the explicit use of group interaction to produce data and insights’. Focus groups have some singular advantages. They provide a detailed understanding of the participants’ beliefs and experiences, and embrace a diversity of views. The method produces context-specific qualitative data on complex issues. For example, Rhodes and Tiernan (2014) ran two focus groups comprising the former Chiefs of Staff (CoS) of Australian prime ministers to discuss such questions as, how did each CoS approach the task of working with the prime minister? They conclude focus groups are a useful tool for recovering the beliefs and practices of governing elites but, second, they are not a stand-alone tool. They are part of a larger toolkit that encompasses intensive interviewing, official documents, biographies, memoirs and diaries, informal conversations, as well as observation. Finally, as Agar and MacDonald (1995: 85) also conclude, focus groups can take the ethnographic researcher into new territory when the conversation is located in broader folk theories, such as, in the example given here, the governmental traditions in which the participants work.

### Visual ethnography

There is a long history of filmmaking in anthropology (Brigard 1995). Here, I am interested in the opportunities opened by such new technology as: the lightweight digital camcorder. There is no longer any need for elaborate lighting, the conspicuous camera installations, specialist film teams, reams of film, and a heap of money (cf. Schaeffer 1995:

272-8). The camcorder has many specific uses for the political ethnographer besides recording an interview. For example, Thedvall (2007: 172-7) notes the challenge of observing committee meetings and trying to write everything down. You do not always understand the discussion, especially if they talk in acronyms. You can miss the opening comments of a speaker because you are busy writing what had been said before. You have to divide your time between what is spoken, how it is spoken, body language, and interactions between committee members. The camcorder provides a visual transcript of the committee meeting. Field notes can be compared with the visual record. Participants at the meeting can watch the recording and explain what they think is happening. Visual ethnography can resolve many of the problems identified by Thredvall (and for a more detailed discussion, see: Pink 2013). Whether we are studying local, regional or central governments, meetings are ubiquitous and the visual ethnography is an invaluable tool in the analysis of these multifarious committees.

## Conclusion

Ethnographers as *bricoleurs* employ a ragbag of tools; what works is best. Whatever the tools, interpretive political ethnographers are united by their quest to recover meaning. Ethnography exists in many forms and there are many ways to recover the beliefs and practices of everyday life. Fieldwork remains at the heart of the ethnographic enterprise and we can generalise because ‘small facts speak to large issues’. There may be no laws as in the natural sciences but we can still aspire to ‘plausible conjecture’. Fieldwork may uncover only partial truths but it will have to do as the best tool we have. We practice the logic of abduction rather than the logics of induction and deduction. Our knowledge may be provisional but it can be subjected to forensic debate guided by explicit, agreed criteria of merit. Objective knowledge may be out of reach but that does not mean we should not strive

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for narratives that meet established standards of evidence and reason, and are accurate, comprehensive, and consistent. As professional strangers, we need to ‘be aware’ of how we are shaping the research both in the field and as we write. For anthropologists and sociologists, there is little that is new in my discussion of ‘new’ ways to do political ethnography. However, political scientists can now explore the pros and cons of ‘being there’ in many more ways than they have used so far. As authors, we need to accept that we need to be self-conscious practitioners of a literary craft that encompasses many types of textual experimentation. We have not resolved the issues of the culture wars, but our answers are both more nuanced and reflexive

The great advantage of interpretive political ethnography is not just that we get to see what actors are thinking and doing but ‘being there’ offers the prospect of serendipity and surprise. It also induces more than a hint of apprehension. Negotiating and renegotiating elite access always carries the risk of refusal. Even after gaining access, doors can be shut in your face. There is that sinking feeling when they criticize your draft article. There is the suspense while they decide whether you can attribute a quote. The enterprise is not for the fainthearted. It keeps you on your toes. It adds zest to my life as I set off for my next fieldwork visit to confront my next surprise.

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## References

- Agar, M. (1996) [1980] *The Professional Stranger*, 2<sup>nd</sup> edition, San Diego: Academic Press.
- Agar, M. & MacDonald, J. (1995) 'Focus Groups and Ethnography.' *Human Organization*, 54, 78–86.
- Aronoff, M. J. and Kubik, J. (2013) *Anthropology and Political Science. A Convergent Approach*, New York: Berghahn Books.
- Atkinson, Paul, Sara Delamont, Amanda Coffey, John Lofland and Lyn Lofland (Eds.) (2007) *Handbook of Ethnography*, London: Sage.
- Bevir, M. (1999). *The Logic of the History of Ideas*. Cambridge: Cambridge University Press.
- Bevir, Mark and Asaf Kedar (2008) 'Concept Formation in Political Science: An Anti-Naturalist Critique of Qualitative Methodology', *Perspectives on Politics* 6 (3), 503-517.
- Bevir, M. and Rhodes, R. A. W. (2003) *Interpreting British Governance*, London: Routledge.
- Bourdon, R. 1993. 'Towards a Synthetic Theory of Rationality', *International Studies in the Philosophy of Science*, 7, 5-19.
- Brigard, Emilie de (1995) 'The History of Ethnographic Film', in P. Hockings (Ed.) *Principles of Visual Anthropology*, 2nd edition, Berlin: de Gruyter, 13-43.
- Bryman, A. (Ed.) (2001) *Ethnography*. Four volumes. London: Sage Benchmarks in Social Research Methods.

Bunzl, M. (2008) 'The Quest for Anthropological Relevance: Borgesian Maps and Epistemological Pitfalls', *American Anthropologist*, 110, 53–60.

Burawoy, A. (1998) 'The Extended Case Method', *Sociological Theory*, 16, 4-33.

Clifford, J. (1986) 'Introduction: Partial Truths', in Clifford, J. and Marcus G. E. (Eds.) *Writing Culture: The Poetics and Politics of Ethnography*, Berkeley: University of California Press, 1-26.

Clifford, J. (1988) 'On Ethnographic Authority', in his *The Predicament of Culture. Twentieth Century Ethnography, Literature, and Art*, Cambridge, Mass: Harvard University Press, 21-54.

Clifford, James (1997) 'Four Northwest Coast Museums: Travel Reflections', in his *Routes: Travel and Translation in the late Twentieth Century*, Cambridge: Harvard University Press, 107-46.

Crewe, E. (2005) *Lords of Parliament. Manners, Rituals and Politics*, Manchester: Manchester University Press.

Czarniawska, B. (2004) *Narratives in social science research*. London: Sage.

Denzin, N. K. and Lincoln, Y. S. (2011) [1994] 'Introduction: the discipline and practice of qualitative research', in N. K. Denzin, and Y. S. Lincoln (Eds.) *Handbook of Qualitative Research*, 4th edition, London: Sage, 1-19.

Dexter, L. (2006) [1970] *Elite and Specialized Interviewing*, Colchester, Essex: European Consortium for Political Research.

DeWalt, K. M., DeWalt, B. R., and Wayland, C. B. (1998). 'Participant observation', in H. R. Bernard (Ed.), *Handbook of Methods in Cultural Anthropology*. Walnut Creek, CA: AltaMira Press, 259-299.

Eckstein H (1974), 'Case Study and Theory in Political Science' in F I Greenstein and N Polsby (Eds.), *Handbook of Political Science Volume 7 Strategies of Inquiry* Reading, Mass.: Addison-Wesley: 79-137.

Emerson, Robert M., Fretz, Rachel I. and Shaw, Linda L. (2011) [1995] *Writing ethnographic fieldnotes*, 2<sup>nd</sup> edition, Chicago: University of Chicago Press.

Faucher-King, F. (2005) *Changing Parties: An Anthropology of British Political Conferences*, Houndmills, Basingstoke: Palgrave-Macmillan.

Fenno, R. E. (1978) *Home Style: House Members in their Districts*, Boston: Little, Brown.

Fenno, R. E. (1990) *Watching politicians: essays on participant observation*, Berkeley: Institute of Governmental Studies, University of California.

Fox, K. (2004) *Watching the English. The Hidden Rules of English Behaviour*, London: Hodder and Stoughton.

Gains, F. (2011) 'Elite ethnographies: potential, pitfalls and prospects for "getting up close and personal"', *Public Administration* 89, 156–66.

Geertz, C. (1993) [1973] 'Thick Description: Toward an Interpretive Theory of Culture', in his *The Interpretation of Cultures*, London: Fontana, 3-30.

Hammersley, M. and Atkinson, P. (2007) [1983] *Ethnography: principles in practice*, 3<sup>rd</sup> edition. London: Routledge.

Jackson, Jean, E. (1990) “I Am a Fieldnote”: Fieldnotes as a Symbol of Professional Identity’, in Sanjek, R. (Ed.) *Fieldnotes: the Making of Anthropology*, Ithaca: Cornell University Press, 3-33.

Joseph, L. Mahler, M. and Auyero, J. (Eds.) (2007) *New Perspectives on Political Ethnography*, New York: Springer.

Kaufman, H. 1960) *The Forest Ranger: A Study In Administrative Behavior*, Baltimore, Maryland: Johns Hopkins Press.

Kertzer, D (1988) *Ritual Politics and Power*. New Haven: Yale University Press.

Lincoln, Y. S. and Guba, E. G. (1985) *Naturalistic Inquiry*, Newbury Park, CA: Sage.

Lipsky, M. (1980) *Street-level Bureaucracy: Dilemmas of the Individual in Public Services* New York: Russell Sage Foundation.

Marcus, George E. (2007) ‘Ethnography Two Decades after Writing Culture: From the Experimental to the Baroque’, *Anthropological Quarterly* 80 (4): 1127-1145

Maynard-Moody, S. and Musheno, M. (2003) *Cops, Teachers, Counselors: Stories from the Front Lines of Public Service*, Ann Arbor, Michigan: The University of Michigan Press.

Morgan, D. (1997) *Focus Groups as Qualitative Research*, 2nd edition, London: Sage Publications.

Nader, L. (1974) 'Up the Anthropologist -Perspectives Gained From Studying Up', in D. Hymes (Ed), *Reinventing Anthropology* New York: Vintage Books: 284-311.

Pink, Sarah (2013) *Doing Visual Ethnography*, 3<sup>rd</sup> edition. London: Sage

PS Symposium (2002) 'Symposium: Interview Methods in Political Science', *PS: Political Science and Politics* 35 (4): 663-76.

Rawnsley, A. (2001) *Servants of the People: The Inside Story of New Labour*, revised edition, London: Penguin Books.

Rhodes, R. A. W. (2011) *Everyday Life in British Government*, Oxford: Oxford University Press.

Rhodes, R. A. W., Paul 't Hart and M. Noordegraaf (Eds.) (2007) *Observing Government Elites: Up Close and Personal*, Houndmills, Basingstoke: Palgrave-Macmillan.

Rhodes, R. A. W. and A Tiernan (2014) *Lessons of Governing: A Profile of Prime Ministers' Chiefs Of Staff*, Melbourne: Melbourne University Press.

Roberts, B. (2002) *Biographical research*, Buckingham: Open University Press.

Rorty, R. (1980) *Philosophy and the Mirror of Nature* Oxford: Blackwell.

Sanjek, R (1990) *Fieldnotes: the Making of Anthropology*, Ithaca: Cornell University Press.

Schaeffer, Joseph H. (1995) 'Videotape: New Techniques of Observation and Analysis in Anthropology', in P. Hockings (Ed.) *Principles of Visual Anthropology*, 2nd edition, Berlin: de Gruyter, 255-84.

Schatz, E. (Ed.) (2009) *Political Ethnography: What Immersion Contributes to the Study of Power*, Chicago: University of Chicago Press.

Schwartz-Shea, Peregrine, and Yanow, Dvora (2002) “Reading” “Methods” “Texts”: How Research Methods Texts Construct Political Science’ *Political Research Quarterly*, 55 (2), 457-486.

Schwartz-Shea, Peregrine, and Yanow, Dvora (2012) *Interpretive Research Design: Concepts and Processes*. London: Routledge.

Shore, C. (2000) *Building Europe: The Cultural Politics of European Integration*. London: Routledge.

Shore, C. and Nugent, S. (2002) *Elite Cultures. Anthropological Perspectives*, ASA Monographs 38, London: Routledge.

Shore, C. and Wright, S. (Eds.) (1997) *The Anthropology of Policy. Critical Perspectives on Governance and Power*. London: Routledge.

Stone, D. (2001) *Policy Paradox. The Art of Political Decision Making*. 2<sup>nd</sup> revised edition. New York: Norton.

Taylor, C. (1971). ‘Interpretation and the sciences of man’, *Review of Metaphysics*, 25(1): 3-51.

Taylor, C. (1985) *Philosophical Paper. Volumes 1 and 2*. Cambridge: Cambridge University Press.

Thedvall, R. (2007) ‘The EU’s Nomads: National Eurocrats in European Policy-Making’, in Rhodes, R. A. W., Paul ’t Hart and M. Noordegraaf (Eds) *Observing*

*Government Elites: Up Close and Personal*, Houndmills, Basingstoke: Palgrave-Macmillan, 160-179.

Van Maanen, J. (1988) *Tales of the Field. On Writing Ethnography*, Chicago: University of Chicago Press.

Wagenaar, H. (2011) *Meaning in action: interpretation and dialogue in policy analysis*. New York: M. E. Sharpe.

Watson, G. (1986) 'Make me reflexive—But not yet: Strategies for managing essential reflexivity in ethnographic discourse', *Journal of Anthropological Research*, 43 (1): 29-41.

Wedeen, L. (2010) 'Reflections on Ethnographic Work in Political Science', *Annual Review of Political Science* 13, 255-72.

Werner, O. and Schoepfle, G. M. (1987) *Systematic Fieldwork*. Two volumes. London: Sage.

Wolcott, H. F. (1995) *The Art of Fieldwork* Walnut Creek, CA: Altamira Press.

Wood, Elizabeth J. (2007) 'Field Research' in Carles Boix and Susan C. Stokes (Eds.), *The Oxford Handbook of Comparative Politics* Oxford: Oxford University Press 123-46.

Wulff, H. (2002). 'Yo-Yo fieldwork: mobility and time in multi-local study of dance in Ireland', *Anthropological Journal of European Cultures* 11: 117-36.

Yin, Robert K (2008) *Case Study Research: Design and Methods*, 4<sup>th</sup> edition, London: Sage.

For longer term ethnographic fieldwork, the field site itself can be very complicated in terms of social relationships and social organization. Can be an entire community, neighbourhood or even city (scale and scope) → Can be "multi-sited" - involves doing fieldwork in different institutions, with different groups, and maybe even different cities; can even be focus on "flows" of knowledge untethered to any physical space → Can be "virtual" (i.e. online) - using Facebook/Twitter, online forums, and online games → Also based on the self - i.e. auto-ethnography. Usin