Journal of Organizational Ethnography

Who are we trying to impress?: Reflections on navigating political science, ethnography and interpretation

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Received 15 August 2014
Revised 15 January 2015
26 February 2015
Accepted 10 March 2015

Abstract

Purpose – Turning laborious ethnographic research into stylized argumentative prose for academic consumption is a painstaking craft. The purpose of this paper is to revisit this perennial issue, and extend a claim the authors have made elsewhere about the inevitably impressionistic, rather than the oft-claimed "systematic", nature of this task.

Design/methodology/approach – The authors draw and reflect on their own experiences of conducting and navigating across political science, ethnography and interpretation in order to justify and uphold the benefits of impressionism.

Findings – The authors argue that the impressionistic account of writing up fieldwork has important implications for these diverse disciplinary terrains.

Originality/value – The authors develop an argument as to how and why an appreciation of this craft's impressionistic nature can affect how the authors go about creating, evaluating and ultimately thinking about ethnographic research in foreign disciplines like political science.

Keywords Ethnography, Reflexivity, Political science, Interpretation, Systematic

Paper type Research paper

Introduction

How to turn laboriously collected, fine grained and nuanced qualitative material into stylized argumentative prose suitable for publication in disciplinary journals is a question that confronts all ethnographers. In a recent article, we reflected on the significant dissonance we felt as young scholars writing up our PhD research between the experience of this process and a rigorously "systematic" social science (Corbett and Boswell, 2015). We came at the argument from the disciplinary perspective of political science, a relative latecomer to questions of this nature. The conclusion we came to is that interpreting fieldwork can legitimately be (and for us, certainly felt) impressionistic rather than systematic; the interpretations we communicate represent illuminating and interesting, but also stylized, partial and fleeting, impressions of the phenomena under investigation. Rather than suppressing this aspect of doing research, we maintain that researchers should acknowledge, embrace and incorporate it in our practice.

In presenting this argument to a range of academic audiences, either at conferences, via the peer review process, or in casual conversation, it quickly became clear that it met with firm, but conflicting, opposition. For many in the mainstream of political science, the discipline in which we are both situated, appropriating the label impressionism, especially by way of an analogy to the art world, seemed farfetched and career limiting. For some scholars associated with the "interpretive turn" in the study of politics, for whom ethnography is a methodological label employed to critique the dominance of quantitative methods in America in particular, our argument was read as a hostile critique bordering on betrayal. In contrast, anthropologists or scholars whose

Journal of Organizational Ethnography
Vol. 4 No. 2, 2015
pp. 223-235

2046-6749
DOI 10.1108/JOE-08-2014-0030

work falls under the label cultural studies were not so much shocked or angry as underwhelmed. For them, the claims we were making about the limits and problems of writing up "thick description" seemed so obvious as to be banal.

Unperturbed by these (somewhat caricatured) receptions, we seek here to situate our argument in and across three communities of academic practice – mainstream political science, ethnography and interpretive theory – in order to understand why our argument has provoked such discordant reactions. We reconcile these responses by delving into the relationships between these three communities, and reflecting on our own experiences in navigating across them. In particular, given the focus of the special issue on political science, we seek to show that far from being banal (or indeed outlandish), our argument about the impressionistic nature of ethnographic research can open up a timely debate about how this method is understood, used, and judged in and across different disciplines.

This paper has three parts. First, we offer a brief description of Corbett's (2015) research and the different ways it has been labeled and received by each of our three chosen audiences to ground the discussion. Our discussion relies on our impressions – the reception of our work at conferences, by reviewers and in casual conversation – as review articles will take time to appear. Second, we unpack these labels and situate each reaction in the methodological debates that infuse these communities of practice. In doing so we outline how our version of "impressionism" differs from the influential work of John van Maanen. Third, we conclude by exploring how a consciously impressionistic orientation can reinforce and revise ideas about ethnography as a research tradition used across disciplinary boundaries.

Being impressionistic: studying leadership and democracy in the Pacific Islands

Corbett's PhD training was spilt between a public policy school and a history/anthropology school at the Australian National University. In its initial design, his project was method driven; Corbett read work from the interpretive canon, specifically as it related to politicians elsewhere (Rhodes, 2011; Reeher, 2006; Tiernan and Weller, 2010), and sought to replicate this approach in the Pacific Islands. He wanted to use it to counter the popular perception of them as corrupt, power-hungry and self-interested and instead privilege and provide space for the politicians' voice. Studies of political leadership in the Pacific have predominantly focused on chiefs and traditional leaders rather than politicians and so, to push the impressionism metaphor, his research paints onto a relatively blank canvas, thus partly explaining why it was initially methods driven. Increasingly, however, the final product became self-consciously theoretical. The intricate detail and nuance of his painstakingly compiled account is unavoidably truncated and stylized by the need to situate his work in the broader body of scholarship about politicians in order to get published in political science journals. Quoting Weller (2001, p. 183), Corbett called the account a collective portrait: an "amalgam of views, a majority voice constructed by the author as a representation of the spectrum of opinions" expressed across a group of lives.

The portrait was constructed using the standard sources prescribed in the interpretivist canon: public documents (more than 40 published biographies and autobiographies); in-depth interviews (more than 110); and observation-based research. This raised few questions when Corbett proposed it in an Australian setting, either because interpretive research is accepted as legitimate in this setting (see Boswell and Corbett, 2014) or, as one American-trained quantitative researcher observed at his confirmation seminar, the project was "like Fenno", a reference to a classic piece of ethnography in political science discussed further below. In contrast, anthropologists were skeptical of the use of the label ethnographic. This latter view bears some explanation given its relevance to our overall argument.

Anthropology is the dominant discipline in Pacific studies. Since Malinowski, the Pacific has become a veritable Mecca of social anthropology due to the unique diversity of languages and cultures (more than one-quarter of the world's spoken languages can be found in the Melanesian region alone). So pervasive is the influence of Pacific anthropology that "New Melanesian Ethnography" is an established school of research in its own right (for review see Mosko, 2012). Despite the appropriateness of Malinowski-style fieldwork having since been questioned within the discipline (e.g. Marcus, 2000, p. 2013), scholars working in this tradition are somewhat skeptical of the types of observational research Corbett undertook (pejoratively "airport lounge" ethnography), despite the fact that they fell within the conventions established by Fenno (1978) and others (e.g. Rhodes, 2011) in political science. For many Pacific anthropologists, anything less than a year's fieldwork is insufficient to fully "immerse" oneself in a given context (among these scholars it is common practice to list the amount of months spent in the field on their CVs).

In contrast, Corbett's "yo-yo" (Rhodes, 2014) observation took him across 11 countries, often for as little as a week. He spent time "looking over the shoulder" of politicians on the campaign trail, in their constituency, at parliament, and during regional meetings. Corbett felt comfortable claiming to have "seen some of their world" (Reeher, 2006) and that "being there" had enriched his account. And yet, he remained mindful of the views of anthropologist colleagues and their belief that ethnography should aim to "see the world as they see it". As a result, Corbett avoids using the label ethnographic to describe his work. But, by genuflecting to his anthropological colleagues interested in the Pacific and shunning the label "ethnography", Corbett found it difficult to describe his work to mainstream political scientists; if it is not ethnography, what is it and how were conclusions reached?

Aspects of Corbett's method could be described as systematic and thus be passed off as naturalist in orientation – he certainly had a system for thematically sorting mountains of interview data and his desire to provide an account that politicians deemed authentic led him to send draft chapters to willing interviewees – but generating the final product was far more dynamic and uncertain. Ultimately, while Corbett took care to account for the full range of interpretations that emerged from his data, the literature he was contributing to dictated which stories would make the greatest impression: he privileged the parts of his work that enabled him to "see new aspects of cases". No amount of additional time spent in the field would alter this. And yet, Corbett remains paranoid about the inevitable anthropological critique that he has missed crucial context-specific nuances (Corbett, 2014), and so at no stage has he stopped doing interviews, reading new literature or revising his findings. Corbett simply started publishing his impressions.

Doing so, however, meant that he was confronted by the various norms and beliefs of each respective audience. While journal reviewers' responses emphasize various weaknesses of papers, a selection that focus on method bears these differences out. Some mainstream political science journals were, predictably, hostile to the approach with variations of the phrase "the interpretive method is no method at all" a feature of reviewer comments. They noted that findings were not tested numerically, that the presentation of evidence was clumsy and insufficiently analytic due to the reliance on the reproduction of direct quotations, and that the interview sample was not representative. That is not to tar all reviewers or journals with the same brush – some scholars applaud the emphasis on description (e.g. Gerring, 2012) and Corbett and his collaborators have published self-labeled interpretive work in political science journals that might be considered mainstream (e.g. Corbett, 2013a; Corbett and Liki, 2015) – but a more productive strategy when engaging with this audience has been to downplay the methodological aspects of his data collection and focus instead on disciplinary debates (e.g. Corbett, 2013b; Corbett and Wood, 2013; Veenendaal and Corbett, 2015).

We might expect anthropologists and anthropology journals to be more receptive to this type of material but this was not necessarily the case either. As one response to a proposed special issue contribution indicates, this audience felt the work was "insufficiently grounded in long-term field research". As a result, he has avoided sending articles to anthropology journals. By contrast, interdisciplinary journals – be they policy, development or area studies focused – have on the whole been more receptive to his approach (e.g. Corbett, 2013c; Corbett and Ng Shiu, 2014). How should we make sense of these differences?

Relating political science, interpretation and ethnography

As we have seen, ethnographic methods have many advantages but one of the least heralded is the capacity of painstakingly compiled "thick descriptions" to uncover data relevant to a range of research topics and questions. The same phenomenon can, quite plausibly, be constructed to serve a seemingly endless variety of theoretical insights that are rarely confined to one discipline or stream of academic inquiry. Indeed, some of the most acclaimed scholarship in political anthropology over the last two decades has been compiled by reinterpreting the "thick description" of other researchers in service of a meta-narrative (e.g. Scott, 1998, 2009). But, even putting meta-narratives aside, the variety of theoretical implications that arise from any "thick description" presents practitioners engaging in ethnographic research with a number of conundrums. Most obviously, they have to choose which audience to target and in doing so be conscious of the different traditions and beliefs that actors hold in each. We will not endeavour to present any sort of criteria for making these choices, as any rationale is likely to be unique to the goals, aspirations and interests of the researcher, as the Corbett example illustrates. Corbett was more willing to persist with political science journals than anthropological equivalents, despite numerous rejections, because that is the discipline he most strongly identified with. Rather, building on the clues outlined in the above discussion, this essay reflects on our experience as early career researchers about the way different audiences treat this type of work. To do so, we first unpack the way these different audiences "see" ethnography.

The orthodox view of mainstream political science is of a discipline increasingly dominated by behavioralism, naturalism and positivism. What these labels collectively describe is an approach to studying politics which draws on inspiration from, and mimics as far as possible, the natural sciences. Such an approach is associated with a preference for parsimony over complexity, generalizability over idiosyncrasy and though much less uniformly, quantitative measurement over qualitative assessment. The general perception is that these preferences have broadened and strengthened across the discipline in the last two or three decades, as marked by trends in publishing, grants and postgraduate training. As we discuss further below, ethnographic research has been employed in service of a naturalist political science (for review see Rhodes, 2014), but this is uncommon.

As part of the critique of this strengthening hegemony – which came to something of a head in the early part of the last decade through the Perestroika movement for greater methodological pluralism (see the contributions to Monroe 2005) – an oppositional paradigm has built momentum, albeit still in the margins. This has occurred in the form of the "interpretive turn" in the study of the political[1]. A significant minority of politics researchers, albeit in far greater numbers in the more stubbornly plural subfields of policy studies and public administration than those of political behaviour or institutions [2], have begun moving away from orthodox explanations of the political world as an objective entity, and towards investigating the ways in which actors make sense of this world and how their perceptions relate to political practices. This approach has consciously looked outside contemporary political science for theoretical and methodological inspiration.

Particularly important in the context of this discussion is that many have drawn from anthropology and more or less explicitly the tools of ethnography to design and conduct their studies. Indeed, for many political scientists, interpretive research is synonymous with ethnography. A number of attempts have been made to dispel this perception (e.g. Stone, 2002) but the association remains. The elective affinity between the two stems from the typical focus of ethnographic research on the actions and practices of individuals rather than the means of production, the flow of capital or formal institutions, for example. In particular, the work of Geertz (1973), Clifford and Marcus (1986) and others have provided interpretivist political scientists and public administration scholars with a repertoire of terms and ideas upon which they have made claims about the rigour of their approach and the validity of their findings to mainstream scholars. Dvora Yanow (2014, p. 131), for example, asserts that ethnographic research reveals insights that the established toolkit of positivist social scientists cannot: that "being there" is a better way of doing research on complex phenomena. But as well as turning to anthropology for inspiration, some interpretive scholars also point to the established canon of ethnographic classics in political science when explaining the use of this approach to their disciplinary colleagues (e.g. Rhodes, 2014). On calling on the noted works of Kaufman (1960) and Fenno (1978), for example, and recalling a time when the discipline as a whole was more plural and open, they seek to bolster claims that this type of work is both rigorous and systematic.

In fact, we suggest that appealing to these sources presents awkward questions for interpretive researchers (Boswell and Corbett, 2015). Most obviously, since these political science classics hold to naturalist ontological and epistemological assumptions (Rhodes, 2014), we question the extent to which interpretive researchers can really claim to be continuing an old tradition into the present. This is tied to problematic assertions of "systemacity" and the attempt to reclaim epistemic authority in opposition to the mainstream – and we come back to this below.

The broader point we want to stress here, however, is that although these affinities provide interpretive researchers with precedents and philosophical suppositions to support their approach, many still bristle at mainstream assumptions about its (lack of) validity. Indeed, they remain acutely alert to criticism that their methodological choices do not meet the rigorous standards of positivist political science. But in doing so they, too, cling to what we argue are equally flimsy claims about systemacity – in order to understand this critique, however, we first need to briefly outline what those claims are.

Systemacity, impressionism and ethnography

Systemacity, in mainstream political science, derives from objective theory-testing, conducted increasingly (though not exclusively) via sophisticated quantitative methods borrowed from economics, psychology and statistics. Our focus, though, is on interpretivist claims to systemacity, which are quite distinct influenced as they are by anthropology and continental philosophy (see especially Bevir and Kedar, 2008). Obviously, interpretivists are deeply skeptical about many of the key tenets of "the scientific method" in political research and generally argue that the conventional standards of mainstream political science do not apply to interpretive research (Yanow, 2006, pp. 100-101). Different standards, more befitting the philosophical presuppositions of interpretivism, take their place. The authors who have advanced these the furthest are Ospina and Dodge (see Dodge *et al.*, 2005; Ospina and Dodge, 2005a, b), Bevir and Rhodes (2010), Wagenaar (2011), Yanow (2006, 2007, 2009), and Schwartz-Shea and Yanow (2012). Though these authors all deploy their own idiosyncratic terminology, each broadly accord with the same set of principles or criteria by which interpretive research is produced – immersion in the field, deep familiarity with academic literature, abductive linking of data and theory, and so on. They claim that, taken together, these principles comprise an equally systematic process by which the researcher accumulates insights. We beg to differ.

As noted in the introduction, we lay out our argument for impressionism in detail elsewhere. Here, we simply seek to extract the essence of this claim – that although we concur with the key principles and practices outlined by these pioneering scholars of interpretive political and policy studies, we do not see them as contributing to a process that is in any way systematic. Interpretive research, and especially the experience of writing up and recording findings, is for us inherently impressionistic – it involves a deliberate and at times painful abandonment of any attempt to capture the full complexity of the phenomenon under investigation, and instead involves embarking on more stylized reflections, inflected by and expressed via memorable impressions, in order to say something interesting but also coherent and digestible to scholarly and practitioner audiences.

As we have laboured to background in these early sections, and as we suggested explicitly at the outset, this argument has been met with some hostility among the interpretive research community (and general bemusement by those in mainstream political science). Yet, of course, for many readers of this journal it may seem completely uncontroversial. Indeed, in anthropology and cultural studies, where such methods are mainstream, such an orientation to writing up research is well grounded, most obviously in John van Maanen's (1988) influential and well-cited account of writing up ethnographic research from a quarter of a century ago. For van Maanen, impressionism (or, more precisely, the crafting of "impressionistic tales") was a key part of the ethnographer's repertoire, allowing him or her to startle readers with new insights or enliven established phenomena in new ways. There are clear affinities here with what we are arguing. However, we also see there as being two key interrelated differences in our argument to his, which we suggest have important implications for scholars who use ethnography in any discipline.

The first difference is that our argument assumes a much broader relevance for impressionism than van Maanen's insistence that such work "remains very much a sub-genre of ethnographic writing" which for him is imparted through a reflexive, first-person, experiential narrative of the field. The way we use the term "impressionism" is intended less to capture a rare, purposeful mode of narration which researchers can switch on and off, and more to describe a defensible way of thinking about research more generally; we are not just talking about first-person narratives – such accounts are virtually unknown in political science, even among the most committed interpretivists.

The second difference indicated by this, on which this first point turns, is the disciplinary context in which we are working – and it perhaps instructive here to bring the analogy to impressionist art back in. While the accounts that van Maanen has in mind differ only in accent and degree from the dominant anthropological tradition, the accounts we have in mind, just like impressionist artworks in the context of preceding trends and traditions, represent a much more significant break from the dominant tradition in political science. All ethnographic work is impressionistic in this disciplinary context; the precise tone and style of representation simply depends on what sort of impression the author is seeking to make. And it is for this reason we suggest that our account has important implications for how we think about ethnography in relatively alien territory like political science.

The implications of embracing impressionism

Corbett's experience highlights some of the difficulties of doing and communicating interpretive research, and defends a consciously impressionistic rather than defiantly systematic approach to doing so. As we have already acknowledged, such a focus on the challenges of writing up is not new to interpretivism (see Law, 2004; Yanow, 2009) nor of course ethnography more broadly (van Maanen, 1988; Wolf, 1992). But our claims here for a defensibly impressionistic orientation push further than political scholars have thus far been willing to go, with potentially important implications. One is that pushing this analogy further suggests a need to rethink and make more explicit how scholars judge the quality of ethnographic work (whether self-labelling or not). Two, and the more fundamental note on which we conclude, is that it also has key implications for how scholars, especially those working across disciplines or outside the mainstays of cultural studies and anthropology, think about and use ethnography in their work.

How might we judge political ethnography

To be clear, we are not advocating that ethnographic research be conducted in a manner akin to the everyday connotation of impressionism. Random streaks on a canvas did not an impressionist make; the community of practice surrounding the impressionist movement had explicit and implicit criteria by which to decipher good from bad impressionist art. Likewise, anything does not go when interpreting fieldwork. There are conscious or unconscious criteria that we apply in developing and reflecting on our own work and in reviewing the work of peers, and a consciously impressionistic orientation, we suggest, provides a lot of purchase for understanding and unpacking these. In further extending the analogy here, we make explicit three such criteria that we feel typically apply to ethnographic research in politics, and which are consistent with our impressionistic orientation. Though we are wary of presenting a reified checklist of the type we have critiqued above, it is incumbent on anyone advocating an alternative to the mainstream to clarify what is being promoted in its place as effective, interesting or appropriate practice (see Ospina and Dodge, 2005a; Bevir and Rhodes, 2010; also Boswell and Corbett, 2015). And to be clear, we see the process of developing and judging interpretive research as a dynamic, open-ended and evolving one. As such, our contribution is intended to open up and inspire debate about what good ethnographic research in politics and policy entails.

Aesthetics. Just as scholars in and before the "interpretive turn" have already done much to emphasize the importance of writing to how researchers interpret fieldwork (see Laws, 2004; Yanow, 2009), so too is there growing reflection on the importance of prose for the consumption of such work. The argument that quality of writing matters could sensibly be made about all scholarly work, but again, the analogy here holds because interpreting fieldwork presents particular and (most would agree) more acute challenges in this regard. Where writing up research in the tradition of hard sciences, which positivist political science and public administration typically ape, involves adhering by and large to strict formulae, interpretive, ethnographic researchers have many more choices to make. As Yanow (2014) describes, the things that ethnographers of politics and policy observe and study have a "context specificity" that necessarily renders their meaning ambiguous in any broader sense; the task of the analyst is to make them intelligible in a convincing and appealing manner. The aesthetics of the final composition – how the narrative flows, how it moves between theory and practice, how it imparts "flavour" by drawing on data from the field – matter for how we make and judge the work. van Maanen (1988, p. 34), for instance, explains that "artful ethnography is evocative in addition to being factual and truthful" and attention to "literary style and writing quality in general is heightened among ethnographers". And so it should be.

Impact. Perhaps more important than aesthetics is that any interpretation of fieldwork must have impact. We mean this not in the sense of the creeping "impact agenda" in academia which UK readers in particular will be familiar with (we address the issue of relevance in the concluding section), but in the sense of leaving a mark on the reader[3].

In fleshing out this criterion, it is again useful to dwell again on van Maanen's (1988, pp. 101-102) account of writing up ethnographic fieldwork. He defines an "impressionist tale" as one in which authors deliberately try to set out to unsettle their readers:

> The impressionists of ethnography are also out to startle their audience. But striking stories, not luminous paintings, are their stock-in-trade.

In fact, this accords with the orientation already clearly adopted by some pioneers of the interpretive turn in political science. Colin Hay (2011), for instance, argues that a key facet of the "interpretation" in such research involves the author trying to work out what of his or her work is of relevance for particular audiences and how it may be presented to best achieve such impact. Mark Bevir (2011, p. 191), too, is explicit about this:

> Public administration is less about discovering general rules, than about seeing new aspects of cases and relating them to our more abstract concepts.

After all, the point of the impressionist orientation in art was to arrest the viewer by capturing something novel or startling in its insight. Are not typical questions we ask ourselves when we develop our own work and read that of our peers: is it interesting? Is it memorable? Does it make me think about an old question in a new way?

Resonance. But the criteria described above do not entail an abandonment of "reality" in service of a more beautiful or startling account. Resonance with pre-existing understandings of how politics operates is essential to the aesthetic appeal and potential impact of any interpretation. Here the analogy holds again – the impressionists took "nature as their model", and were motivated by the desire to better capture such "reality" (Merleau-Ponty, 1964). What makes the composition attractive and memorable or revealing is that in any case it also rings true.

Indeed, we agree with Bevir and Rhodes (2010, p. 207) who make clear that that plausibility as governed by intersubjective agreement must remain an important criterion of good interpretive research. We also agree with Ospina and Dodge's (2005a, b) that such agreement should involve both the internal audience (of specialists in the substantive area) and the external audience (of politics, policy and public administration scholars). We simply suggest that a more consciously impressionistic approach to interpretation might advance or enhance these forms of resonance.

One avenue is through its potential to strengthen relationships with the political actors that are the subject of interpretive research. A consciously impressionistic orientation can confront the messy, partial nature of conducting research and therefore invite practitioners into discussion of claims and findings on a more equal footing at the "back end" of the process. Acknowledging that researchers and practitioners simply have impressions of different types can help realize co-production and embed relevance as core features of the research process (see Ospina and Dodge, 2005b).

Two is in its recognition that the internal audience of scholars is not always self-evident or homogenous – this is the lesson from the above example of research in the Pacific Islands. As a result, we hold, such resonance need and usually does not rely on fine-grained accuracy; there is a trade-off in detail and accuracy to be made in unveiling and communicating broader "truths" to different academic audiences.

How might we understand and use political ethnography

We have, in the earlier iteration of our account, reflected on the implications of our argument for interpretivism and its relationship to mainstream political science. Again, we do not want to retread this ground and instead want to focus on the implications for the likely readers of this special issue – ethnographers of political, public and other organizations. We conclude by suggesting how a consciously "impressionistic" method can affect the way we think about and use ethnography as an approach and as a label.

Ethnography, as a loose set of research techniques, has never been the domain of one discipline but it is most commonly associated with anthropology nonetheless. Despite this, as we outlined at the outset, the elective affinity between political scientists doing interpretive research and ethnographic methods is such that many mainstream scholars see them as one and the same. And yet, our own work and thinking on this topic receives a mixed reception from those who are more familiar with this method than political scientists. For this audience, we are guilty for both having not "been there" long enough to justify claims to deep immersion and for privileging a stylized and somewhat parsimonious rendering of our fieldwork in service of broader theoretical arguments. There are numerous labels and philosophic bases for this type of ethnography (for review see Rhodes, 2014) but the important point is that the emphasis here is on getting the descriptive story "right". This idea is captured neatly by Wolf (1992), where in the opening lines of her acclaimed *Thrice-Told Tale* she makes clear that it is written for anthropologists and social scientists who, like her, are:

> […] just a little more interested in the content of the ethnographies we read and write than in the ethnographers' epistemologies […] Like most anthropologists, I remain more interested in why Chinese peasants do what they do, and, as Graham Watson puts it, in "getting the news out".

On the other hand, as we have seen, many anthropologists and cultural studies scholars, while sympathetic to our argument, find our claims somewhat banal; we have heard all of this before, they say. How should we account for this discrepancy? While it may not be new, we argue that this disjuncture remains an important topic for scholars of ethnography, regardless of discipline. Questions about epistemology may sometimes generate more heat than light but they remain at the heart of what we do. This is the lesson from political science where those working with ethnographic methods increasingly feel marginalized, especially in the USA.

More importantly, for political scientists like us undertaking ethnographic research in an interpretive orientation, this disjuncture presents awkward questions that, first and foremost, relate to the notion of "systemacity". We venture that interpretivists might be better served by treating ethnography as a research "tradition" – a set of living understandings or orthodoxies that we all receive during socialization (Bevir and Rhodes, 2010) – rather than an essential idea that can be judged in a taxonomic or checklist manner. As this special issue demonstrates, there are lots of ways of doing ethnography and any attempt to pin down prerequisites of the craft risk setting up the type of reification that interpretivists have been at pains to critique in mainstream political science. When scholars choose to employ (or avoid in Corbett's case) the label "ethnography" to describe their work, they are seeking to invoke a series of intersubjectively held meanings and beliefs about what they are doing and what they think their research is about. When assessing this type of work the important question is not whether it justifies the label ethnographic but why is the label being employed (or avoided) and how does that serve the impression the author is trying to create: who, exactly, are they trying to impress?

Notes

1. For overviews, see Yanow and Schwartz-Shea (2006), especially their introductory chapter, as well as the various perspectives in Finlayson *et al.* (2004) and the more recent special issue to mark the end of RAW Rhodes' editorship of Public Administration.
2. This is best embodied through the legacy of Harold Lasswell (see Lasswell and Lerner, 1951) and his commitment, as the pioneer of modern policy sciences, to a plural discipline.
3. A fuller discussion of the relationship between our criteria and theory building and concept formation is necessary to flesh this point out but it is beyond our scope here (see Bevir and Kedar, 2008 for further discussion on interpretive research and concept formation in political science).

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