How Much and How Not to Explain: Gestural Referencing and Conceptual Misappropriations

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Introduction

> Don't bring a rival company's product to a job interview. It shows the hiring manager that you haven't researched the company, their history, or their competitors. (WorkopolisTM advertisement, Toronto Star, 7 April 2015)

This essay reflects on issues of intellectual community, citation and reference practices, and the nature of research infrastructure. Like any active reviewer of grant applications, book and article manuscripts, or any reader of a reasonably wide range of scholarly literature, I have often been struck and irritated by writers making dubious appeals to 'what everyone knows' and by citation practices that are truncated, gestural, or, at times, thoroughly mistaken.

What one fellow reviewer recently called 'theoretical hand-waving' is common, both in grant applications and in published work. Writers will begin with a brief theoretical statement, citing some authors or concepts they claim are important to their projects, but will not use the concepts invoked. In grant applications, the tactic is used by established scholars trying to travel on their reputations, or too busy to do the (outrageous) amount of work needed to write a serious application. Beginners, too, may lay claim to intellectual capital through the citation of lengthy lists of works that are decorative, because never put to use. Although sociologists are far from immune to the practice, many narrative historians seek to 'get the theory out of the way' so they can get on with storytelling, with the result that they often reproduce in an acritical fashion common sense or 'practice' categories rather than analytic ones (for the

distinction, Cooper and Brubaker, 2000). Other writers avoid sustained interrogation of the phenomena they study by working to normalize them: when I read 'it is not surprising that x...' I wonder if the writer has abdicated the responsibility of taking distance from the everyday. Or again, one finds attempts to bolster claims, often ones weakly supported by evidence, through the use of such affirmations as 'it is obvious that,' 'clearly,' 'certainly,' and so forth.

And there are the formulaic ways of presenting problems and issues. Guetzkow et al's study of how American grant reviewers assess originality and innovation cites a panellist who immediately disqualifies any applicant who seeks to 'fill a gap in the literature' (2004: 197). The stock phrase seems to flag a lack of curiosity. At the same time, panellists saw applicants who worked in their field or used their framework to be more upright and more innovative than others. The first finding points to the dangers to applicants of following a formula; the second points to the strategic value of 'hand-waving.' Worst, of course, is the simple mis-appropriation, mis-application, or mis-attribution of concepts (about which more below).

On the other side of this coin is the fact that any speech community has to share a common language with generally-agreed upon concepts, categories, and ways of making claims and observations: what Lorna Weir (2008: 378) calls 'mundane formulae of truth.' Members of a community might treat their taken-for-granted practices as objects of scrutiny, and sociologists, whose objects of investigation necessarily include their own subjectivity, must do so. But *only* to scrutinize practices is to be unable to make use of them for investigative purposes. Thus, some things go without saying and many things have to go without being said at length for any community to go about its business. What can be taken for granted in communities which are internally diverse and rapidly changing is quite different from what can be assumed in tightly-knit and internally homogeneous communities. The amorphous and rapidly-changing (or cyclical) nature of sociological concept-formation and analysis means that the taken-for-granted is often fleeting. It also means that members of sub-disciplines may not share a common conceptual repertoire. If you are reading this, I predict that you would find a three-page explication of 'the panopticon' to be tedious and unnecessary. If you went to graduate school after 1990, however, I predict that I could not assume that you would immediately recognize the difference between 'relative' and 'absolute surplus-value,' understand immediately 'the degradation of labour,' or have heard of 'the problem with no name.'

Two Further Provocations

This essay was also provoked, first, by my reviewing an earlier version of Nob Doran's fascinating contribution, in the current issue, on his relation with Michel Foucault's late work. I enjoyed the piece thoroughly, and I was intrigued when he mentioned in a note that my *Politics of Population* (2001) had been squarely in the theoretical frame of Philip Corrigan and Derek Sayer's (1985) *The Great Arch*: *English State Formation as Cultural Revolution*, but my *Ruling by Schooling Quebec* (2012) barely even mentioned that work (Doran 2015: note18). Instead, it was a work of governmentality. I am not about to offer a *mea culpa* nor critical commentary on Doran's piece, but the remark made me think about sociological memory, and it suggested that *Great Arch* is another of those works which has been forgotten by those with whom I often wish to speak .

About the same time, I noticed that Derek Sayer had made *Great Arch* available online, noting that it was long out of print and used copies are very expensive (see http://coastsofbohemia.com/2013/08/24/corrigansayer-the-great-arch-online/). Sayer restated the book's main argument in a post and suggested some minor modifications to it, although he did not respond to critiques of the book's tendency to assimilate all forms of regulation to state regulation. Still, as he points out, *Great Arch* identifies state formation as an ongoing and continually problematic process, stretching across the *longue durée*. The process is bound up practices that both totalize and individualize political subjects. Projects of state formation typically mobilize a moral ethos, both to anchor legitimacy and to shape the character of political subjection. Those struggling in, over and through the state system seek legitimacy for politically organized subjection in various forms of moral regulation. Rule and domination are performative relations that mobilize ritual and symbolic resources (largely displaced in the then-current Foucauldian literature by preoccupations with routine and rationalization). State formation projects attempt to overcome class and other forms of difference through the fabrication of an illusory community. Thus, in part, *Great Arch* appealed to the dynamic of individualization and totalization that Foucault portrayed as the essence of pastoral power. Pastoral power, in turn, is an immediate precursor of governmentality (Foucault 1983; 1994a, b, c; 2007). To my reading, Corrigan and Sayer also foreshadowed Jacques Rancière's related juxtaposition of 'the people' and 'population' as central categories in liberal government (1999; 2005).

The arguments of *Great Arch* were taken up widely across the social sciences (e.g, in Glasbeek 2006; de Coninck-Smith 2001), but they were

also subjected to rough handling in the 1990s academic conflicts over Michel Foucault's bones, especially those that fore grounded the early Anglo-Australian appropriations of Foucault's work (Burchell 1991; Dean 1994; Valverde 1994). Those conflicts coincided with the dissolution of the Soviet empire, the decline of interest in academic Marxism, the political triumphs of varieties of neo-liberalism, and the mirroring of the latter's anti-statist discourse in some readings that presented Foucault's governmentality as itself an anti-statist discourse. While *Great Arch's* 'moral regulation,' is of Durkheimian origin, and concerns the formation of self-governing political subjects, such readings dismissed it as a crude form of social control (but not Hunt 1998). And critics forgot or cared not to notice that 'the state' in *Great Arch* was meant to be thought in keeping with Philip Abrams' 'Notes on the difficulty of studying the state' (1988): that is, not as an actor, or an agent, or an apparatus, or a unitary entity, but as a violent political abstraction.

Many, if not most entering graduate students in English Canadian sociology who were exposed to governmentality over the last couple of decades have come at it through Anglo-Australian renditions, although William Walter's *Governmentality* (2012) may change things. Interestingly, governmentality has not been widely picked up in French or French-Canadian debate, but writers who have done so have seen the work as a contribution to state theory (e.g., Labourier and Lascoumes 2005). While the approach elaborated in *Great Arch* has faded from most of the Canadian landscape, the analysis remains prominent in French because of Pierre Bourdieu's embrace and extension of it in his Collège de France lectures on the state (2014).

Much of *Great Arch's* theoretical stance and many of its analytic strategies remain elements in the infrastructure of my own work and thinking. The book came at Foucault's work as I did, from classical social theory, from English cultural Marxism, and from gritty historical sociological investigations of politics and government. The research I did on attempts to rule Quebec by schooling its population was centred on a core proposition from *Great Arch*: that projects of state formation are necessarily projects for the formation of political subjects. Rule is a *relation* between rulers and ruled, and one must study it in relational terms. Schooling lends itself particularly well to the investigation of individualization and totalization and of attempts to anchor political rule in the selves of the ruled. In the Quebec case, projects for schooling population led to pioneering social inquiries, in which intellectuals and state servants grappled with practical techniques of investigation. These are some of the neglected roots of Canadian social science. And, given the hegemony of the Catholic church and the seigneurial system, the case

involved a direct confrontation between rule based on pious pastoral ig-
norance and rule based on a cultivated intelligence and responsible self-
government (i.e. between pastoral and liberal modes of government).

Nob Doran's remark that Corrigan and Sayer's approach was absent
from my work thus startled me, because it is apt—I don't cite *Great Arch*
in *Ruling*, and the book has 'governmentality' and not 'state formation'
in the title—and inapt, since I think its approach is in the book's foun-
dations. Doran and Sayer's activities make we wonder: What can one
assume in doing historical sociological work in Canada—or elsewhere?
What does one need to explain and what can one take for granted before
proceeding to analysis? Moreover, how do people point to what they as-
sume is taken for granted? What are some of the difficult or unacceptable
ways of pointing and taking for granted? Is there a conceptual repertoire
that we (me and you, my readers) share? I engage with these and related
questions in what follows.

Community Research Infrastructure

All practice communities share basic epistemological, ontological, and
methodological assumptions. For tightly-knit communities of research-
ers, writers or speakers, lengthy conceptual explication is redundant, and
in the course of their work participants can usually content themselves
with gestures to a common stock of knowledge. Matters are more compli-
cated for loosely-knit communities, of which academic sociology is one.
The discipline's internal diversity allows it to come at the most various
questions imaginable from many different points of view, a strength that
deepens insight. The same diversity makes a strong and consistent scien-
tific consensus impossible. As Pierre Bourdieu pointed out, a peculiarity
of the academic discipline is that individuals or groups can advance in
it by denouncing even those positions which are relatively widely held,
something quite unlikely in one of the natural sciences (2001: 60-3).

Susan Leigh Star observed that practice communities share infra-
structures which are 'learned as part of membership. The taken-for-
grantedness of artifacts and organizational arrangements is a sine qua
non of membership in a community of practice.' On the other hand,
'strangers and outsiders encounter infrastructure as a target object to be
learned about. New participants acquire a naturalized familiarity with its
objects as they become members' (Star 2010: 612). Yet new sociologists
are often socialized only into regions of the discipline, and many come
into the discipline in graduate school with assumptions from an under-
graduate education in one of its offspring, such as Women's Studies,

Communication Studies, or Criminology. Diversity poses challenges for sociologists seeking to speak a common conceptual language.

Learning key concepts is part of disciplinary socialization, but foundational concepts once learned tend to disappear into the background. Michael Lynch made a related point in explaining key concepts of ethnomethodology to beginners in science studies. For instance, the concept 'indexicality' emerged initially out of early experiments at computer-based language translations. Early programs could not make sense of pronouns, such as 'he' she' and 'it'; deictic expressions, such as 'here', 'this,' or 'over there;' auxiliary verbs; and 'anaphoric uses,' in which a term's meanings vary in different clauses of a sentence. These terms only make sense in reference to particular contexts, and resist attempts at any extra-local specification. ("It's hot enough now" can mean the egg is cooked, the ice is melting, or it's time to turn off the furnace. 31 degrees Celsius at 50 degrees of latitude on 4 July doesn't work.)

As Lynch put it, "once it is agreed that all utterances and activities are indexical, then it no longer makes sense to suppose that a system of context-free and standardized meanings can apply to all occasions of natural language use. Less obviously, however, it no longer makes sense to treat the *unrealizable possibility* of such a context-free system as a general backdrop for analyzing situated practices" (1983: 22). Lynch described 'indexicality' as one of the tickets that give access to the 'ethnomethodological theatre.' Once one is inside, it is no longer necessary to go on purchasing it.

A complication for sociologists is that there are a number of concepts that circulate under the same name, but whose meanings are quite different. As Christian Caron (2013) has shown, the concept 'reflexivity' burst into common sociology-speak from the 1990s, but 'reflexivity' works in radically different ways in different strains of sociological theorizing. In its most obnoxious 'narcissistic' variety, it denies the very possibility of a common stock of sociological discourse. No one using 'reflexivity' in Canadian sociology can assume readers will immediately share a common understanding of their particular usage. Arpad Szakolczai's own reflexive historical sociology (1998 a and b; 2000a and b) goes further by suggesting the inadequacy of invoking labels such as 'Marxist,' 'Durkheimian,' or 'Weberian' to locate one's work or that of others. Szakolczai's individual and collective theoretical biographies point to periods of liminality and to formative 'reading experiences' for social theorists, in consequence of which fundamental elements in understanding and analysis may change. It is thus often erroneous to assume strong coherence across a body of work over the course of intellectual biography, or to treat any individual work of scholarship as representative of a whole

corpus. To read Max Weber as the same scholar before and after he en-
countered Nietzsche and Freud, or before and after his visit to America;
to read the Marx of the *Economic and Philosophic Manuscripts* and of
Grundrisse; to read C.L.R. James before and after he went to England;
Dorothy Smith before and after 'institutional ethnography'; or Michel
Foucault before and after 1970, simply as the same thinker is to miss
the developments, discoveries, changes of position, and altered concepts
that appear in the works of any intellectual actively engaged in research
work and social practice.

Concepts developed using one set of assumptions to address a par-
ticular set of issues or problems may reappear in a different interpret-
ive framework. There is an accompanying risk of conceptual distortion
or confusion. For instance, Erving Goffman's early work is commonly
treated as a form of 'dramaturgical analysis' located in a broad frame
work that sociologists call symbolic interactionism (to many, one can
simply say 'SI'). As others have done, I have commonly used such terms
to teach Goffman's *Presentation of Self* (1959) and *Asylums* (1961) as
critiques of Talcott Parsons' structural functionalist account of individ-
uals as 'personality systems' organized in response to Society's needs
(1969: 34-57).

It is striking then to read Goffman's own account of the location and
assumptions of his work. He reported that 'symbolic interactionism' was
a label applied to some Chicago-style sociology by Herbert Blumer in
a passing footnote, but Blumer was rarely around Chicago when Goff-
man and his cohort were studying and his work had little impact on their
actual research. The label was promoted by people calling themselves
'ethnomethodologists' as a means of taking professional distance from
some of their colleagues, which Goffman deplored. 'Dramaturgy' also
identified no inherent characteristic of his work to Goffman. Instead, he
argued, such labels became current after some sociologists read Kuhn's
Structure of Scientific Revolutions (1970) and set about looking for soci-
ology's paradigms. Goffman thought that effort promoted a conservative
conception of science, and bore little relation to his experience of how
sociology had developed. His own account of himself was that he was
in large part 'a structural functionalist in the traditional sense' (in Verho-
even 1993: 317).

Goffman's concepts have been taken up in ways he did not intend
and in ways to which he was opposed. Ian Hacking, to take one example,
altered Goffman's concept of the 'looping effect' in keeping with his
social constructionist interest in 'making up people.' Goffman was con-
cerned with the ways in which attempts by institutionalized individuals
to resist or deny official definitions of themselves were seen by insti-

tutions as proof of the very correctness of those definitions. Hacking, by contrast, gave a reading in which looping was first and foremost a phenomenon of classification (Hacking 1986; 1999: 160). Classifications created new ways of being, and classified individuals could come to conduct themselves in keeping with the categories in which they were placed ('secondary adjustments' for Goffman, only one of several possible strategies).

In another essay, Hacking went further to argue that there was a complementary relation between the analysis conducted by Goffman and that of Michel Foucault. Both were described as concerned with discourse, but Hacking called Goffman's approach "bottom-up" because we start with individual face-to-face exchanges, and develop an account of how such exchanges constitute lives. Foucault, on the other hand, used an approach called 'top-down because he starts with a mass of sentences at a time and place, dissociated from the human beings who spoke them, and uses them as the data upon which to characterize a system of thought, or rather, its verbal incarnation, a discursive formation' (2004: 278).

Goffman had earlier denied the label 'social constructionist' in the strong sense. Every sociologist is something of a constructionist, he noted, since we are aware of cultural and historical relativism in human affairs. 'But where I differ from social constructionists is that I don't think the individual himself or herself does much of the constructing. He rather comes to a world, already in some sense or other, established. I am ... closer to the structural functionalists, like Parsons or [Robert] Merton. Just as they were closer to initial functionalist anthropology.' He also explicitly rejected the notion that his work was 'bottom up' in Hacking's sense: 'I don't think you can go from the individual to society. Given society, society has got to make use of individuals or constitute individuals in such a fashion that social organization can be sustained' (in Verhoeven 1993: 318; 323-4).

In the interesting interview from which I have been quoting, Goffman also affirmed his belief that practitioners are by no means the best judges of their own practice. As he put it, 'what an individual says he does, or what he likes that he does, has very little bearing very often on what he actually does. It seems to me that you can't get a picture of anyone's work by asking them what they do or by reading explicit statements in their texts about what they do. Because that's by and large all doctrine and ideology' (1993: 322). Goffman rarely made statements about theoretical orientations. Before he became concerned with 'frame analysis', his published work focussed directly on face-to-face relations and, even in his ASA presidential address he remained committed to specifying the outlines of an 'interaction order' (1983). Hacking may

have better understood Goffman's position than did Goffman himself, but it is remarkable that a great many intellectuals have found little or no trace of a structural-functionalist grounding in his work.

My 'looping' example is meant to show that concepts can be detached from the conditions under which they emerged and can be put to uses that those who developed them did not intend. In some cases such detachment is not a problem; indeed rigid conceptual orthodoxy can impede research work. In other cases detachment does damage to the concepts in question or encourages usages that are imprecise.

Detachment from the context of emergence is perhaps easier with 'sensitizing' or 'intuitive' concepts: concepts that point to phenomena worthy of study or to questions interesting to pose. Such concepts may aid in work of analysis, but they may also serve as placeholders to fill problem spaces that we know to exist, but for which we have no strong analytic resources. Such concepts typically resist specification or disintegrate when the attempt is made to specify them exactly. This characteristic makes them flexible, but it also encourages gestural usage and mis-appropriation or distortion.

Take, for instance, the convoluted career of Raymond Williams' concept 'structure of feeling.' Williams developed the concept in a cultural critique of Marxist economism in order to do two kinds of work: to focus analysis on the *substance* of works of cultural production, and to encourage investigation of the cultural roots of critical consciousness. Thus, rather than taking the class location of cultural producers as proof of the class content of their products, Williams investigated the internal economies of texts (especially literary texts) as such. 'The notion of a structure of feeling,' as he put it, 'was designed to focus a mode of historical and social relations which was yet quite internal to the work, rather than deducible from it or supplied by some external placing or classification' (1979: 164). An analyst could detect structure of feeling 'in a pattern of impulses, restraints, tones, for which the best evidence was often the actual conventions of literary or dramatic writing' (1979: 159).

On the other hand, while rejecting the concept of 'false consciousness,' Williams wanted 'structure of feeling' to give access to an organized disjuncture between the lived experience of subordinate groups or classes and the dominant forms and practices of cultural expression available in society. In his Marxist understanding, class exploitation and domination are lived viscerally and the means of expression for them in hegemonic cultures are inadequate. The existence of a subordinate structure of feeling is made manifest when 'an experience which is really very wide suddenly finds a semantic figure which articulates it' (1979: 164). Williams insisted that lived popular or working class 'experience' was

a material reality with the potential to burst the bounds of the 'selective tradition' of hegemonic culture. It was anchored in a deeply felt solidarity, manifest in 'features of the inflexion and timbre of familiar speech which carries what is already known, and which need not always be articulated more elaborately' (quoted in Filmer 2003: 205).

And yet, the concept 'structure of feeling' lost much of its bite as Williams both deepened his engagement with a Gramscian analysis of hegemony and grappled with the popularity of Thatcherite politics. He came to argue that there were dominant and subordinate structures of feeling and that the available cultural articulations might constrain opposition or mystify understanding. As he put it, 'a dominant set of forms or conventions—and in that sense structures of feeling—can represent a profound blockage for subordinate groups in society, above all an oppressed class. In these cases, it is very dangerous to presume that an articulate structure of feeling is necessarily equivalent to inarticulate experience' (Williams 1979: 164). The promise that the lived culture of domination might find spontaneous and oppositional expression in new, critical semantic figures receded in the face of working class Thatcherism: hegemonic cultural practice more deeply invaded common sense. If popular common sense embodied hegemonic culture, the grounds for assigning it critical insight were undermined, and with them, perhaps, the promise of 'structure of feeling'

Nonetheless, 'structure of feeling' spread rapidly and widely through the new field of cultural studies and into culturally-oriented enquiry in other domains. Many of these uses are not framed by the cultural Marxism which the concept presumed. Authors now invoke and explain it in greater or lesser detail and with various kinds of emphasis in the anthropology of state formation and nationalism, in curriculum studies in education, in the investigation of television content, in studies of popular emotional outpourings, in political theory, in the sociology of music, and beyond (for some examples, Alonso 1994; Best 2012; Filmer 2003; Grossberg 1998; Harding and Pribram 2002; Jordan 2011; Kirk 1999; Simpson 1992; Zembylas 2002).

The concept has acquired such currency in social science discourse that some people invoke it, or gesture towards it, without citing Williams any longer and with no attempt at explication. Perhaps this is a common fate of popular concepts, but they may become denatured in the process. Thus Reed and Alexander can write, in an account of the development of American social theory in the 1960s and 1970s, 'there was also a deep change of mood, a shift in the structure of feeling of sociologists, a vague yet powerful sense that the time for crisis and renewal had passed, that the hopes and dreams of theory belonged to a different time' (2011: 24).

Or Lois McNay, writing on Foucault's anticipation of the 'enterprise self', suggests that this political subject presents ethical possibility for openness and difference, and then comments that, it remains difficult nonetheless to explain 'how a generalized structure of feeling has sufficient force to amount to a 'refusal'' of neo-liberalism (2009: 69). Neither paper cites Williams and, although McNay is close to Williams' political concerns, for Reed and Alexander, as for many other casual users, 'structure of feeling' is simply a synonym for the equally elusive 'mood' or 'atmosphere.' Here it works as a placeholder, pointing to but not grappling with a real analytic problem.

This kind of gestural referencing or hand-waving is masterfully dissected in Keith Sawyer's 'A Discourse on Discourse' (2002). Sawyer shows that much English-language scholarship points offhandedly towards the work of Michel Foucault whenever the concept 'discourse' is invoked. People point to various of Foucault's works, especially the *Power/Knowledge* (1980) collection and *Discipline and Punish* (1979), typically without providing page references. In fact, the relevant work is *Archaeology of Knowledge* (1975), and Foucault's usage is quite different from that typically ascribed to him. His operative concept was not 'discourse', but rather 'discursive formation.' After the failure of the *Archaeology* project, he rarely invoked the concept and never focussed on it again. In fact, Sawyer shows, the concept's lineage was from Jacques Lacan to Roland Barthes and Louis Althusser. It was refined by Michel Pêcheux and came into English cultural studies in the 1970s. The first generation did not attribute it to Foucault. A more influential source was Althusser's 'Ideology and Ideological State Apparatuses' (1984), but later, jumbled-up readings of Foucault brought 'discourse' in the large sense into the cultural studies mainstream where it was attributed it to him. That Foucault is the source of 'discourse' has become such a banal belief in English-language sociology that people rarely enquire into the matter. Earlier intellectual struggles that pitted 'ideology' against 'discourse' have faded from much disciplinary memory (Purvis and Hunt 1993).

The (mis-)appropriation of concepts is facilitated as they migrate from one domain of investigation and from one theoretical perspective to another. Such migrations are often fruitful by pushing local scholarship beyond its limitations but, at times, at the cost of precision in use. In the sociology of music, for instance, essentialist conceptions of 'music' as a thing in itself possessed of some intrinsic qualities were undermined in the late 1980s and 1990s by the work of Christopher Small (e.g. 1987; 1998; 1999). Small insisted that music is not a thing, but rather an ac-

tivity, and he coined or revived the concept 'musicking' to capture the phenomenon. As he put it in one formulation,

> It is quite simple. To music is to take part, in any capacity, in a musical performance. That means not only to perform but also to listen, to provide material for performance (what we call composing), to prepare for a performance (what we call practising or rehearsing), or to take part in any activity that can affect the nature of that style of human encounter which is a musical performance. We should certainly include dancing...(1998: 4).

This definition does not solve all difficulties, of course, since there is an assumption that 'to music' is different, in ways that are not specified, from 'to have sex' or 'to write a sociology paper.'

Still, moving towards performance opened new avenues of research, including Tia DeNora's very influential *Music and Everyday Life* (2000), which reported on interviews where people were asked when, why and under what conditions they listened to music. The book reproduced as one of its chapters the author's earlier article 'Music as a technology of the self' (1999). Thanks mainly to her work, the notion of music as a technology of the self has acquired broad currency in this sub-discipline. In Roy and Dowd's authoritative overview of the sociology of music literature, 'technology of the self' is attributed to DeNora (2010: 189-90). DeNora has since worked extensively on the therapeutic dimensions of the consumption of music. In one of her many later pieces, with a colleague she examined the role of music in practices of 'self-care' (Batt-Rawden and DeNora 2005).

It seems to pass unnoticed in the music sociology literature that 'technologies of the self' and 'care of the self' are concepts developed by Michel Foucault. There is no attribution to his work in DeNora's pieces, although the 1999 article's bibliography listed *The Order of Things* (1970) and *The Birth of the Clinic* (1973). 'Techniques of the self' seems to have appeared first in Foucault's 1981 lecture 'Sexualité et solitude' (1994a), while the variation 'technologies of the self' was used in his 1980-1 Collège de France course (1994b). As far I can tell, the latter concept did not appear in Foucault's work in English until 1988. For Foucault, of course, 'technologies of the self' was not intended to address only practices of consumption. As one of four social technologies, alongside those of signs and symbols (meaning-making), of production, and of power or domination, this concept was meant to capture the general practices of self-formation effected by individuals themselves. Individuals do work of self-fashioning in pursuit of some end, making use of instruments, techniques, plans, procedures, expert and vulgar knowledge, and so on, usually with the aid of experts. Moreover, these four

technologies are articulated in Foucault's analysis, and the intersection of technologies of domination and of the self is Foucault's last definition of 'governmentality' (Foucault 1988: 17-19). Pulling the concept out of its articulation and treating is as limited to consumption does violence to it.

Vigilance

Bourdieu's version of reflexivity enjoins a kind of 'epistemological vigilance' on working sociologists (2001: 173; Curtis 2014: 48-9). He claims that we must strive to treat both the object of research and the subject of research—ourselves—in the same manner. This double exercise of objectivation demands that we attempt systematically to make ourselves aware of our own research biographies, our location in the field of research and our spontaneously preferred strategies: those things we commonly take for granted. Bourdieu was not being naive in this matter: perfect self-knowledge is impossible; there is always more going on than the subject can know is going on. But as Szakolczai also points out (1998b: 217), there is also nothing subjective about the history of subjectivity.

Bourdieu's project involves developing an awareness of how it comes to be that we engage in our research strategies and tactics. It means being attentive to the use of concepts and categories, especially to the relations between conceptual choices and the figuration of the objects of research. To my reading, the stance is not an invitation to a confessional mode of writing nor a prohibition on flagging our theoretical orientation and sources of inspiration. (Doran's footnote about my work makes me notice that some flagging would have been in order.) Encouragement to be aware of formulae does not simply mean abandoning the conventions of research in a scholarly community or in the face of the norms of a funding regime. We need to identify our audiences and write in consequence.

As Kevin Haggerty pointed out in a comment on this essay, citation practices may be active attempts by writers to enlist readers in a practice community, and I agree. Yet to avoid the kind of gestural references that denature concepts, and that may remove their practical purchase on research problems, we should be attentive to the context of emergence of the concepts we use and we should attend carefully to the baggage that may come with them.

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