

WHAT DO 'WE' KNOW THAT 'THEY' DON'T? SOCIOLOGISTS' VERSUS NONSOCIOLOGISTS' KNOWLEDGE

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Abstract. This paper clarifies and repositions some of the controversies generated by Burawoy's defense of public sociology and his vision of the mutually stimulating relationship between the different forms of sociology. Before arguing if, why, and how, sociology should or could be more "public," it is useful to reflect upon what we think we, as sociologists, know that "lay people" do not. This paper thus explores the public sociology debate's epistemological core, the issue of the relationship between sociologists' and nonsociologists' knowledge of the social world. Four positions regarding the status of sociologists' knowledge versus lay people's knowledge are explored: superiority (sociologists' knowledge of the social world is more accurate, objective, and reflexive than lay people's knowledge, thanks to science's methods and norms); homology (explicit lay theories about the social world often parallel social scientists' theories); complementarity (lay people's and social scientists' knowledge complement one another; the former's local, embedded knowledge is essential to the latter's general, disembedded knowledge); and circularity (sociologists' knowledge continuously infuses commonsensical knowledge, and scientific knowledge about the social world is itself rooted in common sense knowledge; each form of knowledge feeds the other). For each of these positions, implications are drawn regarding the terms, possibilities, and conditions of a dialogue between sociologists and their publics, especially if we take the circularity thesis seriously. Conclusions point to the accountability we face towards the people we study, and to the idea that sociology is always performative, a point that has, to some extent, been obscured by Burawoy's distinctions between professional, critical, policy, and public sociologies.

Key words: epistemology; common sense; scientific knowledge; public sociology

Résumé. Cet article vise à clarifier et à repositionner certaines des controverses générées par la notion de « sociologie publique » et par la défense qu'en fait Burawoy, à partir de sa conception des relations vertueuses entre quatre formes de sociologie — professionnelle, publique, critique et de politique publique. Cet

effort de clarification repose sur l'exploration de la question de définir ce que les sociologues « savent » que les non-sociologues ne savent pas. Il s'agit donc de revenir au noyau épistémologique du débat. Nous explorons quatre positions possibles relatives au statut des connaissances des sociologues par rapport à celles des « gens ordinaires » : la supériorité (les connaissances des sociologues au sujet du monde social sont plus justes, plus objectives ou plus réflexives que celles des non-sociologues, en vertu de la méthode et des normes de production propres à la science); l'homologie (lorsque elles sont explicitées, les théories « ordinaires » au sujet du monde social rejoignent les théories scientifiques); la complémentarité (les théories scientifiques et les théories « ordinaires » se complètent : les connaissances locales « enchâssées » des secondes sont essentielles au caractère général, « déenchâssé » des premières); et la circularité (les connaissances des sociologues imprègnent continuellement les connaissances de sens commun; et les connaissances scientifiques sont elles-mêmes enracinées dans les connaissances de sens commun). Pour chacune de ces quatre positions, nous tirons les implications concernant les possibilités et les conditions d'un dialogue entre les sociologues et leurs « publics ». Une attention accrue est portée à la quatrième position, à savoir la circularité. Les conclusions portent sur la responsabilité des sociologues face aux personnes qu'ils étudient, ainsi que sur le caractère « performatif » de la sociologie. Ces éléments, dans une certaine mesure, ont été occultés par le découpage en quatre quadrants que fait Burawoy de la sociologie.

Mots-clé: épistémologie; sens commun; connaissances scientifiques; sociologie publique.

I am a sociologist performing sociology in a business school. I teach sociology but also management courses to students that include managers. My research interests in the field of management imply that I study people in organizations, often managers and executives, who also constitute a primary “public” I try to reach through my research. In terms of Burawoy’s division of sociological labour, I am not sure in which one of the four quadrants I belong. I suspect any pretense I might have to contribute to a critical sociology would be met by suspicion since little “moral vision” can be expected from someone working in an institution so enmeshed in “market tyranny” (Burawoy 2005: 24). Some might doubt that professional sociology can be produced in this context, for “the management literature is full of pop sociology ..., much of it so poor that every six months yet another new analysis becomes a brief best-seller” (Gans 1989:5). That leaves me with the prospect of policy and public sociologies. Most sociologists would likely see what I do as some version of policy sociology, in the sense that I work “in the service of a goal defined by a client” (Burawoy 2005:9), the client being, in this case, the firms that management scientists usually study, rather than the state. My possible attempts towards “public sociology” would also be

closely examined in light of the likeliness that I would be “hostage to outside forces” and “tempted to pander and flatter” my publics (Burawoy 2005:17).

Yet I believe that this particular position as a sociologist is an interesting vantage point from which to reflect on the current place of sociology in today’s society, and to engage critically with the debate, recently renewed by Burawoy, about the relationship between sociology and its “publics.” The “outside-the-box” attribute of such a position, to some extent, eases the grip of the “vested interests in disciplinary structures” (Braithwaite 2005:351).

Doing sociology in a business school context forces one to reconsider on a more or less continuous basis some of the issues which are at the core of the public sociology debate. For example, the issue of the status of our publics, and about their power and resources, takes on a renewed significance when we are talking about managers in small or big corporations, who are far from being the “underdogs” that sociologists are used to studying (Barnes 1979:34; Barrett 1984:4). The issue of whether sociologists should be concerned by the uses and usefulness of scientific knowledge is also cast in different terms in the field of management studies, which seems to be under higher pressure than sociology to produce useful and usable knowledge, defined here in a clearly instrumental way. The competition that we face, as social scientists, for the representation and elucidation of the corporate world is also more intense in management studies than in other sociological subfields and the “pop management” literature is indeed very alive (Mazza and Alvarez 2000).

Finally, the dissemination of academic knowledge in the “public sphere” has a very dynamic character in the field of management studies, which forces us to reflect upon the many ways by which knowledge issued from scientific research is incorporated into the practice of management. In this regard, the debate in management studies about knowledge “transfer” between management scientists and management practitioners has been continuous for years (Baldrige et al. 2004; Rynes et al. 2001) and interesting parallels can be made with the uses of sociology’s instrumental knowledge (Hodgkinson, Herriot, and Anderson 2001).

To clarify or reposition some of the controversies generated by Burawoy’s defense of public sociology and his vision of the mutually stimulating relationship between the different forms of sociology, my aim in this paper is to go back to the debate’s epistemological core — the nature of sociologists’ and nonsociologists’ respective knowledges of the social world, and the relationship between them. I explore four positions regarding the status of sociologists’ knowledge versus lay people’s knowledge: superiority, homology, complementarity, and circularity (Table 1).

Table 1. Four conceptions of the distinction between sociologists' and non-sociologists' knowledge of the social world

<i>Approach</i>	<i>Basic point</i>	<i>Exemplifying quote from major theorists</i>
1. Superiority	Sociologists' knowledge of the social world is more accurate, and/or more objective, and/or more reflexive, than lay people's knowledge, thanks to sociologists' scientific or professional methods and norms.	Sociology's central topics are aspects of everyday life for which common sense has already provided workable explanations and socially appropriate action guidance. Perhaps no other science faces such formidable pseudoscientific competition. (Lange 1987:25)
2. Homology	When they are made explicit, lay theories about the social world often parallel social scientists' theories.	Lay people can, and do, formulate theories that are explicit, coherent, and falsifiable, as do "scientists" who are frequently far from infallible in their own model-building.... All the different academic theories of crime and delinquency are to be found in common-sense formulations. (Furnham 1988:7; 27)
3. Complementarity	Lay people's and social scientists' knowledge complement one another. The former's local, embedded knowledge is essential to the latter's general, disembedded knowledge, and <i>vice versa</i> .	The aim of the sociology we call "institutional ethnography" is to reorganize the social relations of knowledge of the social so that people can take that knowledge up as an extension of our ordinary knowledge of the local actualities of our lives.... Institutional ethnography recognizes the authority of the experienter to inform the ethnographer's ignorance. (Smith 2005:29; 138)
4. Circularity	Sociologists' knowledge continuously infuses commonsensical knowledge and scientific knowledge about the social world is itself rooted in common sense knowledge. Each form of knowledge feeds the other.	All social science is irretrievably hermeneutic in the sense that to be able to describe "what someone is doing" in any given context means knowing what the agent or agents themselves know, and apply, in the constitution of their activities.... Original ideas and findings in social science tend to "disappear" to the degree to which they are incorporated within the familiar components of practical activities. (Giddens 1993:13; 15)

For each of these positions, implications will be drawn regarding the terms, possibilities and conditions of a dialogue between sociologists and their publics. Travelling, as it were, from one position to another, sheds a new light on two issues that have been restricted to discussions about that portion of sociological work that Burawoy calls “public sociology”: first, the issue of sociologists’ accountability to lay people and, second, the issue of the disruptive and performative character of sociological knowledge. These two issues, I suggest, tend to dissolve Burawoy’s divisions of sociological labour and, by the same token, recast the debate about public sociology in more general and fundamental terms.

In the course of the discussion, I will venture into the subfield of sociology of scientific knowledge. The enormous — and well deserved — attention placed on the significance of technoscientific knowledge can be contrasted with the — much less celebrated — significance of social science knowledge in today’s society. I draw in particular upon the field of management studies which offers another interesting point of comparison for sociologists.

SUPERIORITY

It is very natural for sociologists to assume that their knowledge is, somehow, superior to nonsociologists’ knowledge, for reasons that have to do with the fact that sociologists’ knowledge is the product of systematic, professional, or scientific research. Most sociologists would likely resist the term superiority for fear of arrogance, but I will show in the four variants that follow that many sociologists, including some that glorify lay knowledge, maintain a sharp distinction between their knowledge and lay people’s knowledge, the former being superior by being more accurate, and/or more objective, and/or more reflexive.

False vs. True

Sociological research can pinpoint or uncover social facts that contradict common sense knowledge. The break with common sense remains one of the hallmarks of social science according to many sociologists who partly view their job as identifying and correcting the inaccuracy, imprecision, or blunt senselessness of public knowledge regarding the social phenomena they study. A standard ploy of sociology textbooks is to show how commonsensical knowledge claims about particular phenomena are contradicted by scientific knowledge produced by sociologists about these phenomena. For example, Joseph’s *Sociology for Everyone* (1986) demonstrates how commonsensical assumptions such as “class

is less important now that it was fifty or hundred years ago” (1986:71) should be seriously reconsidered in light of sociological knowledge. In light of the public’s perception that sociology is a “pseudo-science that expends a great deal of effort describing or proving what we already know” (Furnham 1983:105), reasserting the break with common sense almost seems a matter of the discipline’s life or death.

In the public sociology debate, this argument that sociologists have the means to reveal “true” facts about the social world is also the reason why many argue that sociology should be more public. Sociologists should diffuse truths that have the potential to settle or advance many of the public debates about particular social phenomena. In this view, sociology’s strength and authoritative claim “is in the data” (Best 2004).

Neutral vs. Value-laden

Another way to assert the superiority of sociologists’ versus nonsociologists’ knowledge is to insist not on the solidity of knowledge produced by sociological research, but its disinterested, neutral, objective, value-free character. In fact, many sociologists are prompt to acknowledge the likely falsification of their current knowledge and refuse to found the superiority of their knowledge on its truthfulness. It is precisely on the basis of that argument that Tittle, for example, denounces the

arrogance of public sociology ... what we think we know today may prove contrary to what we learn tomorrow and that, consequently, sociologists cannot assume that they ‘now have sufficient knowledge to share with the public.’ (Tittle 2004:3)

Yet for Tittle and others, sociologists’ knowledge is superior to the public’s knowledge in another sense which has to do with its disinterested, unbiased character; sociologists are motivated to find the truth whereas “publics rarely want to find the truth in the sense of looking at the full array of positive and negative evidence” (Tittle 2004:5). It is not the role of sociologists to hold a particular position within a debate, especially since there generally is scientific evidence on both sides of a debate to substantiate various interested positions (Stacey 2004).

The mainstream critique of Burawoy’s vision of public sociology is based on this ideal of “disinterested scholarly inquiry” (McLaughlin et al. 2005) and so-called “norms of scientific-scholarly objectivity” (Nielsen 2004). Only by sticking to these norms can sociologists maintain the relevance of their discipline and their hallmark as a form of knowledge about the social world.

Reflexive vs. Nonreflexive

Another variant of the superiority thesis is the one that focuses on the much debated concept of reflexivity. Sociologists' knowledge is superior because sociologists are reflexive in a way that lay people are not, or the knowledge produced by sociological research is reflexive in a way that common sense is not. The many ways in which sociologists and other social scientists have used the concept of reflexivity makes it difficult to establish just what is being claimed here (Lynch 2000; Mesny 1998b). What is often referred to is "the process of identifying, and critically examining, the basic, preanalytic assumptions that frame knowledge-commitments" (Wynne 1993:324). In this sense, reflexivity is supposed to be a "methodological virtue and source of superior insight, perspicacity or awareness" (Lynch 2000:26) that accounts for the fundamental difference between sociologists' knowledge and lay people's knowledge.

When used to characterize social science or scientists, reflexivity thus refers to an unusual capacity for critical reflection, a capacity to hold a detached position that involves a "stepping back from full engagement in cultural activity" (Lynch 2000:30). This capacity is the core of

the modernist Enlightenment vision of science as the epitome of open thought, endemically self-correcting through its intrinsic ability and moral drive to apply scepticism to all its own commitments.... [Science is] the ultimate repository of reflexivity in that it is thought to expose its own founding assumptions and to be able to reflect critically upon them. (Wynne 1993:323)

Sociologists need to be aware of their own presuppositions and, more particularly, of the social roots of their scientific dispositions. It is this conception of reflexivity, as inspired by Bachelard's "epistemological vigilance," that Bourdieu, for example, has developed, arguing that a "sociology of sociology" is an inescapable component of the sociological method (Bourdieu 1990).

In contrast to social scientists, lay people

are usually seen as unreflexive cultural dupes who are tradition-bound and incapable of critical reflection upon epistemic issues and their own relationship to knowledge. (Wynne 1993:325)

Their knowledge is seen as nonreflexive in the sense that it is necessarily engaged in the routine accomplishment of day-to-day life and is not ordinarily reflected upon or questioned explicitly. It is this practical, unreflective and taken-for-granted character which enables people to go on with their lives in an efficient way. This characterization of lay people's

knowledge is exemplified by Bourdieu's notion of *sens pratique*, a form of knowledge that implies immediate comprehension, but a comprehension which is "unaware of itself" (Bourdieu 1980).

The opposition between reflexive sociologists and nonreflexive lay people has been under severe scrutiny on both fronts, namely the supposed nonreflexivity of lay people on one hand, and the supposed reflexivity of social scientists on the other hand. Regarding lay people, ethnomethodology has made a major contribution in showing that lay agents are reflexive in the sense that they have insight into the normative background of their own actions and continuously use "seen but unnoticed procedures for accomplishing, producing and reproducing 'perceivedly normal' courses of action" (Heritage 1984:118). People are not Parsonian "cultural dopes" enmeshed in a normative system that operates behind their backs. This ethnomethodological reflexivity, which stresses an "ubiquitous and unremarkable" form of reflexivity (Lynch 2000) does not, however, fundamentally endanger the more epistemic reflexivity which is at core of the distinction between social scientists' and lay people's knowledge.

To find a real questioning of this epistemic reflexivity, one needs to turn to studies that opened "the black box of scientific rationality" (Fuchs 1992) and, for example, to the field of public understanding of natural or technoscientific "hard" science. Wynne, for example, demonstrates not only that "lay publics enjoy a much greater capacity for reflexivity in relation to science than is usually recognized," but also that "modern science exhibits much less reflexive capacity to problematize its own founding commitments than is supposed" (1993:334).

The thesis that lay people lack reflexivity has also been questioned from another perspective which argues that lay people's knowledgeable ability increasingly displays features that are close to sociologists' postures. To use Bourdieu's terminology, the argument is that, in contemporary society, lay people's knowledgeable ability seems to fit less and less an unreflexive *sens pratique* since lay people routinely develop theoretical and reflexive postures in the course of their day-to-day lives (Corcuff 2003; Mesny 2002). Lahire's work (1998) about people's daily writing practices, or Kaufmann's (2001) analysis of new forms of balance, in the life of contemporary couples, between *sens pratique* and reflexivity, illustrate the point.

One of the key theoretical assumptions of Burawoy's picture of sociology is that only certain types of sociology — critical and public sociologies — are reflexive, whereas the two other types — professional and policy sociologies — are not. What Burawoy means by reflexive is more limited than the epistemic reflexivity presented above, since it

simply refers to value discussion and “a dialogue about ends” (Burawoy 2004:4; 2005:11). Since, in his view, policy and professional sociologies are geared to predetermined ends, they produce instrumental rather than reflexive knowledge. This view has been criticized precisely because “there is nothing in public sociology that is, by definition, reflexive,” and “reflexivity is integral to the practice of sociology, regardless of the audience” (McLaughlin et al. 2005:142).

The superiority thesis has important consequences for the relationship between sociology and its publics. Broadly speaking, it is consistent with what Callon (1999a) calls the public education model¹ which, in a nutshell, aims at the education of a scientifically illiterate public, and which largely depends on the trust that lay people have in scientists. Thus, thanks to the professional way sociologists study society, they can identify mistaken assumptions and educate their publics in order to correct their mistaken views about particular social phenomena. In this perspective, public sociology can be seen as sociologists' willingness to accept the responsibility that follows from their exclusive capacity to produce truths — albeit sometimes provisional ones — about the social world. We shall see later in the paper how this public's education project can fit the circularity thesis about the continuous infusion of common sense by knowledge issued from sociological research.

HOMOLOGY

As natural and taken-for-granted as it may seem, the superiority thesis is not the only way we can conceive of the relationship between sociologists' and lay people's knowledge. A second conception draws upon the idea that lay people's knowledge about social phenomena qualifies as “lay theories,”² and stresses the similarity or homology between scientific and lay theories. Whereas the superiority thesis focuses on social “facts,” the homology thesis focuses more on explanations, interpretations, or theories. In order to make sense of their world and of the phenomena salient to their lives, lay people produce “lay theories” which can be made explicit and then compared, as it were, to scientific theories about these phenomena (Furnham 1988). Instead of insisting on the dif-

1. Callon explores three modes of participation by nonspecialists in scientific and technological debates: the public education model, the public debate model, and the coproduction knowledge model. My argument is that what Callon says about technoscientific knowledge largely applies to social science knowledge.
2. In the sense that lay people's knowledge about particular social phenomena, when rendered explicit with the proper methodological tools, has the characteristics of theory: “concepts are identified, relationships between concepts are specified and most of these concepts are measurable” (Calori 2000:1042).

ferences and incommensurability of scientific and lay theories, proponents of the homology thesis stress their similarities and comparability. The argumentational structure of lay theories, for example, is found to be similar to that of scientific theories, although the former generally remains implicit in the normal course of day-to-day life (Semin and Gergen 1990). This homology between lay and scientific theories has been established regarding issues such as poverty, wealth, unemployment, alcoholism, crime, and delinquency (Furnham 1988), personality theory (Semin et al. 1981), or, in the field of management studies, the international dynamics of industries (Calori 2000) or business-level contingency theories (Priem and Rosenstein 2000). Regarding crime and delinquency, it has been stressed that

lay people have as many, and as complex, theories as criminologists, which can be variously classified into strain theories (delinquency is the result of socially induced tension), labeling or control theories (certain people and acts are processed as deviant by agencies of social control), and drift theories (people drift into delinquency by the suspension of ordinarily accepted moral and legal obligations). (Furnham 1988:170)

The homology thesis has important implications regarding the relationship between sociologists and their “publics.” It means that one of sociologists’ tasks is to uncover lay people’s practical knowledge about the phenomena sociologists take as their object of study, expose the richness of these lay theories, and compare them to their own “scientific” theories. In contrast to the superiority thesis, the homology thesis implies the need to work very closely with the people directly concerned with the phenomena under study. Their concern for the public starts with the production phase of sociological knowledge, rather than the diffusion phase.

The homology thesis, however, can parallel the superiority thesis when discrepancies are found between scientific and lay theories that lead social scientists to find proper explanations for these discrepancies. For example, although Furnham (1988) is eager to stress lay theories’ richness and sophistication, he also stresses major differences between scientific and lay theories, in particular the fact that lay people generally underestimate the importance of external factors. He then explains this “defect” by attribution theorists’ well-known argument about the “psychological function” played by lay theories which implies that lay people regularly commit attribution errors in order to maintain a sense of control about their lives (Jaspars et al. 1983). Social scientists, apparently, are immune to attribution errors.

In management studies, Priem and Rosenstein (2000) set out to answer the question “Is organization theory obvious to practitioners?” by

testing whether business level contingency theory confirms relationships that are already well understood by experienced managers who have not been exposed to the theory. In their own words, they aimed to test the degree to which this theory “meets or exceeds the complexity of common sense theory already used by a practitioner” (2000:512). Their conclusion is that business-level contingency theory is not obvious for all practitioners and that “academics may be wise to increase efforts to disseminate their theories among practitioners” (2000:521). The gaps they found between academics’ and practitioners’ theories imply, in their view, that “there should be a prescriptive role for organization studies in the world of affairs” (2000:520) and that “a compelling case can be made against practitioners’ disregard when it is shown that managers are not applying useful theories” (2000:521). Here the true/false version of the superiority thesis is supplanted by does work/does not work dichotomy, that is, by a criterion of pragmatic success: a “useful” theory is one which “works” which means, in that case, a theory that enables practitioners to take appropriate action to ensure their company’s performance. What is worth noting in Priem and Rosenstein’s argument is that, in contrast to the superiority thesis, social scientists’ knowledge is not *in principle* considered as being more accurate than practitioners’ knowledge. It is a matter of empirical investigation to determine in what cases practitioners’ knowledge is more accurate or useful than social scientists’ knowledge, and *vice versa*.

What is interesting in the homology thesis is that there is no intrinsic or essential difference between sociologists’ and laypeople’s knowledge-abilities, although there might be differences in their knowledge claims. Homology implies commensurability, rather than incommensurability as in the superiority thesis. Also interesting are the two opposite implications that can be drawn from the differences between scientific and lay theories, once they are placed on the same epistemological ground: either trying to modify lay theories in light of scientific theories, or amend scientific theories in light of lay knowledge. The second option is explored further in what follows, since it suggests that lay knowledge can contribute to scientific knowledge.

COMPLEMENTARITY

The third conception of the relationship between social science knowledge and lay people’s knowledge focuses on the complementary character of both forms of knowledge. The complementarity thesis can be a natural development of the homology thesis. In that case the argument

is that social scientists can learn from lay theories and enrich, or even modify, their own scientific theories in light of nonscientists' knowledge. For example, when comparing management practitioners' knowledge and scientific theories about the dynamics of international competition, Calori points to a number of deviations of nonscientists' knowledge from orthodox theories. In his view, what should follow is *not* the discarding of nonscientists' knowledge because of these deviations but, rather, the amending of scientific theories in light of what practitioners' knowledge has revealed. In his own words: "We propose to listen to practitioners and recognize them as co-authors, tap their practical knowledge and transform it into scientific knowledge" (Calori 2000:1031). There is a complementarity between practitioners' "knowledge of acquaintance," based on action and experience, and scientists' "knowledge about" which is the result of systematic thought:

The distinction between 'knowledge of acquaintance' possessed by practitioners and the 'knowledge about' mainly possessed by professional researchers, shows the complementarity and the necessary co-authorship of managers and researchers in building management theories. The type of knowledge possessed by each category is never pure: practitioners learn from action and experience, but also from journals and business schools, and researchers are influenced by stories told by practitioners. However, their relative specialization in a particular form of knowledge calls for a number of coordination and translation mechanisms. (Calori 2000:1033).

Smith's feminist sociology (1987), which later became a sociology "for people" (2005:1) rather than for women only, can be seen as another potent version of the complementarity thesis. At first sight, Smith's institutional ethnography might be seen as pertaining to an inferiority thesis, asserting that social scientific knowledge is somehow *inferior* to lay knowledge. Lay people's "work knowledges"³ constitute the sociologist's data, and "there is always much more to learn from people's experiences than the researcher can cope with. For the institutional ethnographer, what he or she does not know and what the informant can teach her or him is central to the research project" (Smith 2005:141). This sociology recognizes "the authority of the experienter to inform the ethnographer's ignorance" (2005:138) and, as far as their experience is concerned, lay people are the "experts" (2005:24). The sociologist's role is that of "an acute, thoughtful, and probing listener who is learning from the informant or observational setting" (2005:138).

3. *Work* is used by Smith in a "generous sense to extend to anything done by people that takes time and effort, that they mean to do, that is done under definite conditions and with whatever means and tools, and that they may have to think about" (Smith 2005:151-152).

The sociologist, however, must go beyond people's experience to create "maps of how things work beyond the scope of our everyday knowledge" (2005:206). For that reason, Smith's institutional ethnography clearly belongs to the complementarity thesis, rather than to an inferiority thesis. The knowledge produced through institutional ethnography is "an extension of the ordinary ways in which we know our everyday worlds into regions we have not been to, and perhaps could not go to, without the explorer's interests and cartographic skills" (2005:2). These "maps," produced collaboratively by the sociologist and his or her informants, concern the "ruling relations" that are largely not observable from where people are in the course of their day-to-day lives (Smith 1987:127–40).

There is at least one fundamental difference, however, between Smith's conception of the sociologist as a cartographer of ruling relations, and other conceptions — for example, that of Bourdieu, which clearly pertains to the superiority thesis — of the sociologist who "makes the social visible" for the — forever deprived of sociological imagination — lay person. The knowledge produced through institutional ethnography aims at *extending*, rather than *negating*, or *supplanting*, lay knowledge. It extends lay knowledge to the social relations and institutional orders in which people participate (Smith 1987:43). The informants always remain subjects rather than objects, and they remain "the knowers, or potential knowers of what institutional ethnography discovers" (1987:52). More precisely: although

the work of discovery sometimes calls for research that is technical and conceptually outside the everyday language of experience ... once the institutional ethnography is completed, it becomes a resource that can be translated into people's everyday work knowledge. Hence it becomes a means of expanding people's own knowledge rather than substituting the expert's knowledge for our own. (Smith 1987:1)

The knowledge produced in institutional ethnography is, almost by definition, used by, and useful for, the people who worked collaboratively with the researcher(s). Beyond this restricted circle of people, however, institutional ethnography's "publics" can be as difficult to reach as any other public. In principle, institutional ethnography can be used "to locate sites of institutional change within the reach of local practitioners" (1987:221). To illustrate this point, Smith cites Pence's work about the judicial processes regarding domestic abuse cases, which has been used "to locate a number of places where it has been possible to make changes that contribute to increasing the safety of women who are subject to violence from their spouses" (1987:221).

For those who embrace the complementary thesis, differentiated publics possess specific, local knowledge about social issues which can be mobilized, debated, and extended, and which can enhance sociologists' knowledge of these issues. Only "organic" — rather than traditional — sociology (Burawoy 2005:7–8) seems to be consistent with the complementary thesis, since it implies a close connection between the sociologists and a "thick" public through a process of mutual education (2005:7–8).

CIRCULARITY

The circularity thesis consists in stressing the circular — or "spiral-like" — relationship between sociologists' and lay people's knowledges. Lay knowledge is partly infused by sociological research, and sociologists' knowledge always draws upon commonsensical knowledge. This process of interpenetration is based on social science's fundamental reflexivity, as expressed in various — positive or negative — ways by numerous scholars. For example, in Easthope's negative terms:

Men, unlike physical objects, are self-aware. This self-awareness means that the predictions and descriptions of sociologists of the social world are not separate from that social world but form part of it. This implies that there can never be any recurrent situations to study in sociology because study of a situation changes that situation. Thus sociology can never be a science and must always cause change. (1974:2)

Or, in Taylor's more neutral terms: "Social theories do not bear upon an independent object; [t]he objects they bear upon are not resistant to the alterations in self-understanding which these theories bring" (1983:85).

A logical way to think about the circularity thesis is to see it as a continuation of the superiority thesis, particularly in its "true-versus-false" version. Whereas the focus of the superiority thesis is on that part of the process when sociologists produce social facts or interpretations which contradict common sense, the circularity thesis focuses on a later part of the process that could logically follow, namely the appropriation of sociological knowledge by lay people who, in particular circumstances, are ready to revise their commonsensical knowledge in light of new authoritative knowledge. In that way, knowledge produced by sociologists helps to contribute to common sense.

What is almost immediately brought to the fore when discussing the circularity thesis is the empirical challenge that it poses. Once incorporated into common sense, knowledge issued from sociological research

has generally lost the very attribute that allowed us to identify it as scientific knowledge, what Abrams called, in his study of the uses of British sociology, the "Cheshire Cat problem":

insights, concepts, language which began life as sociology filter into the world of taken-for-granted common-sense and common discourse and to the extent that they are indeed used in that world are no longer perceived as sociology ... what is seen as sociology is likely to be that which has not yet been found useful. When demystification fails the demystifier is an irritant; when it succeeds the demystifier is redundant. (Abrams 1985:202)

A few social scientists have tried to tackle this problem and found ways to trace social scientific knowledge which has been incorporated into common sense. Wrong's (1990:19) attempt to "identify broadly concepts and notions originating in academic sociology that have entered the awareness, or at least the vocabulary, of Americans" is well-known, as is Merton and Wolfe's study (1995:15) of "the degree to which sociological concepts have been incorporated into the vernacular of society." Because psychoanalytic vocabulary seems easier to trace than sociological vocabulary, social scientists have tried to assess the incorporation of social psychoanalytic knowledge into common sense or social representations (Berger 1965; Moscovici 1961; Farr 1993). Tracing specific concepts in commonsensical knowledge does not imply, however, that the scientific knowledge tied up to the concepts has been incorporated into common sense: "terms in the vernacular take on a life of their own, [and] their meaning can wander far from any original sociological purpose associated with the development of such terms" (Merton and Wolfe 1995:21).

As is patently obvious in the public sociology debate, sociologists who acknowledge the significance of the process whereby knowledge issued from sociological research enters common sense often have mixed feelings about this process's blessings. On one side, the process is a huge comfort for sociologists, who have had to face a great deal of criticism over the years regarding what the public perceives as the uselessness of sociological knowledge for society. By arguing that a "lay sociological imagination" (Mesny 1998b) has diffused throughout society, that we live in a "sociological culture" (Merton and Wolfe 1995) and that a "lay sociology" is spreading (Gans 1989), sociologists can take comfort in the fact that, although the process is uncontrollable and hardly researchable, sociology goes public on a continuous and pervasive basis, and contributes an important part to society's institutional reflexivity (Giddens 1990). On the other side, precisely because sociologists have very little control over this process, they lament the fact that any knowledge claim about the social world, if properly packaged, can become com-

nonsensical knowledge, including knowledge from “nonprofessional sociologists,” or knowledge which, although originating in “serious” sociological knowledge, becomes distorted in the popularization process (Shinn and Whitley 1985; Weiss and Singer 1988).

These mixed feelings about the incorporation of social scientific knowledge into common sense are echoed in the way sociologists address the issue of the mass media. It has long been acknowledged that the knowledge originated in sociological knowledge cannot become public and influence commonsensical knowledge through traditional academic channels. Even policymakers, a distinctive category among the public, do not have access to social scientific knowledge primarily through research reports or academic publications (Heller 1986). Rather, a large part of the knowledge produced by sociologists might reach lay people through the mass media. Thus, regular calls are made for sociologists to be more active in disseminating their work through the mass media (Bulmer 1986; Gans 1989). At the same time, equally frequent warnings are issued regarding the vicious, uncontrollable, paradoxical, and counterproductive aspects of this mass diffusion of sociological knowledge (Best 2004; Ewer 1979; Stacey 2004; Weiss and Singer 1988).

The thrust of this argument is that the mass media have a logic of their own, which has important effects on the way sociological research is reported. First, the vast majority of social research goes unreported and the research that does is overwhelmingly quantitative (Weiss and Singer 1988). Second, the process of mass diffusion tends to reinforce a positivist epistemology and to strip reported knowledge of complexity, nuance, ambiguity, or uncertainty (Stacey 2004). Third, sociologists are portrayed as advocates for a particular set of prescriptions (Ewer 1979). All in all, the conclusion is that “the presentation of research and theory to a broader public often leads to diluted and counterproductive debate” (Brady 2004:4) and that mass media produce a “virtual social science” that “involves the repetition and reification of selective representations of social science findings, often based on misleading statistical claims, as scientific truth” (Stacey 2004:132).

This conclusion extends to the numerous ways research results findings are put to use by various lay agents in society. They tend to distort scientific knowledge in order to fit their own interests. The argument that public uses of sociology amount to an abuse of sociological knowledge has been a major argument against the call, made by Burawoy and others before him, to promote a public sociology. Cases abound that illustrate the ways in which sociologists have lost control of the uses of the knowledge they have produced: Stacey’s (2004) bitter observation that her work about lesbian/gay parenthood, much to her dismay, has

contributed to pro-marriage ideology; Tittle's (2004) account of the legislators who totally ignored the work done by him and other social scientists and the testimony they provided vis-à-vis the death penalty; or Tumin's (1970) even more disturbing report that his research, which was prompted by fundamentally antiracist beliefs, was eventually accused by Black people of being a racist document. Burawoy's call for more public sociology has therefore been met with hostility by those who share the long-stated view that "far from being non-applied, the social sciences are too easily, and too loosely applied" and that social scientists should be "more concerned about the misuse of [their] theories and techniques than about their non-use" (Lévy-Leboyer 1986:24, 26).

Two main responses have been offered to account for what sociologists see as unpredictable, and sometimes counterproductive, uses of sociological research by the public: one is a protectionist reaction which consists in standing "as sentries at the door of our ideas and techniques" (Lévy-Leboyer 1986:26), in refusing to practice any form of public sociology, and in sticking to a traditional scientific ethos according to which popularization equals pollution, and belongs essentially to the realm of nonscience (Shinn and Whitley 1985). The contrary response is for sociologists to influence and direct as much as they can the uses made of their research. This involves becoming familiar with the logic of mass media (Best 2004) and skilled in lessening the frustrating features of the going-public process, even at the cost of the innocence that one might have had about "the progressive potential of public sociology" (Stacey 2004:134). It also implies that "good sociology is unashamed of its advocacy" (Agger 2000) and that sociologists who choose to enter the public debate are *de facto* endorsing the advocate's role. Advocating a particular position in public debate implies that sociologists become "just another interest group in competition with the legions of interest groups already out there" (Tittle 2004:3), a position that is very uncomfortable for those who consider that science should be apart from society, whatever that is supposed to mean.

As long as the focus is exclusively on this chancy process of trying to transform lay knowledge, common sense, or social representations through social scientific knowledge, the circularity thesis is only a corollary of the superiority thesis, aimed at those public sociologists who feel adventurous enough to play that game or, to use Nielsen's (2004) terms, who feel "temperamentally compatible" with that conception of public sociology. There is another way to conceive of the circularity thesis, one which fully acknowledges the circular character of the relationship between social science and common sense, without assuming that a clear distinction can be made between the two forms of knowledge and that,

consequently, degrees of “contamination” of one by the other, can be assessed.

This alternative conception of the circularity thesis springs more from the complementarity than from the superiority thesis. It revokes the assumption that sociologists produce knowledge which can *then* be used by lay people. To put it differently, the classical distinction between the production and the diffusion of knowledge as two distinct moments and processes has, in my view, obscured some of the discussions about public sociology. So has the assumption that producing “good” or “professional” sociology is an issue that can be distinguished from that of producing a useful sociology, or a sociology that “matters.”

By this I do not mean that all sociology should be participatory, intervention, or advocacy research, nor that our research should always be based on extensive case studies. Sociology, almost by definition, is concerned with issues that are relevant and meaningful for at least a portion of the population, if not the population in general. These concerned groups, who have their own views about the issues at hand — which, on a strictly scientific basis, cannot be ignored by the sociologist⁴ — can play a more active role in the production of knowledge than by simply being our informants. They also have an interest, and the resources, to comprehend, and occasionally appropriate the knowledge produced by sociologists.

Sociological inquiries that take lay people’s, or the concerned groups’ knowledge seriously, that is, not as “prenotions,” or “nonreflexive” or “ideological” knowledge, all tend to illuminate, first, the meaningfulness of sociological research for these people and, second, the enrichment and betterment of scientific knowledge that can result from the consideration of this “lay” knowledge. Wynne’s (1993) research with radiation workers at the Sellafield nuclear reprocessing plant or with Cumbrian farmers exposed to radiation, and Callon’s (1999a) study of the coproduction of knowledge by experts and patients, are cases in point. In fact, the conclusion that Wynne draws about the way the public understanding of technoscience has been misconceived and underestimated by scholars could also apply to the way sociologists have conceived lay people’s understanding of sociology.

The circularity thesis is, in my view, most useful when it leads to a “dialogical approach” between sociologists and lay people, that is, a form of sociology that allows for other voices than those of sociologists (Flyvbjerg 2001). Sociology based in that conception is always public as sociologists become a part of the phenomena they study, “without ne-

4. Since the study of social phenomena can only be “as stable as the self-interpretations of the individuals studied” (Flyvbjerg 2001:33).

cessarily 'going native' or the project becoming simple action research" (Flyvbjerg 2001:132).

CONCLUSIONS

The positioning of this paper regarding the public sociology debate can be simply stated: before arguing if, why, and how, sociology should or could be more "public," we need to reflect upon what it is we, as sociologists, are able to know that "lay people" are not. Supporting what I called in this paper the superiority thesis, that is, the belief that sociologists are able to produce knowledge which is "superior" to lay people's knowledge because it is more accurate, neutral or reflexive, leads to sharply different implications regarding public sociology than when one supports the idea that both forms of knowledge are complementary or feed one another in a circular relationship.

Let me sum up my argument in this paper by putting on the table two propositions which follow from the above discussion of the various conceptions of the distinction between sociologists' and nonsociologists' knowledge. These two propositions, in my view, deserve to be discussed with respect to the public sociology debate.

1. We are Accountable to the People We Study

In his 1988 ASA presidential address, Gans stated that "unlike practitioners, our research does not need to be accountable to nonsociological kinds of validity, so that we are not open to, and thus do not receive corrections from the people we have studied" (1989:11). I believe the exact opposite is true. We are accountable to the people we study, and increasingly so, for various structural and normative reasons. First, the people we study are the people who provide the funding for our research. In management studies, as in some other areas of social science, research is increasingly funded by private rather than public sources, and the organizations that we study are also our research sponsors. We may lament this situation, but it certainly increases the pressure on us to be accountable.

Second, the ethics of empirical research mean that we need the fully informed consent of the people we want to study directly. As many social scientists have experienced in the last decades, particularly anthropologists, people increasingly resist being subjects of inquiry, especially for purposes not their own (Hymes 1974) and tend to impose conditions before agreeing to be studied, in order to exercise control over the inquiries made about themselves (Barnes 1979). Here again, we can lament the

fact, but being accountable to the people we study now seems a necessary condition for conducting empirically grounded social research.

Finally, and more importantly, the people we study directly or indirectly “talk back” to us more than ever, because they are more educated and they have easier access to our research than before. What Barnes reported in 1967, namely that “Islanders now know of Malinowski’s books and one of them has reported that Malinowski did not understand their system of clans and chiefs” (Barnes 1967:205), hardly seems an extraordinary occurrence today. Assuming a break, epistemological or otherwise, between social science and common sense, at best amounts to generalizing what has now become a very particular case. Higher education levels, better access to information and research findings through Internet and the mass media, and the parallel development of institutional and individual forms of reflexivity, imply that most aspects of society and of people’s lives are susceptible to “chronic revision in the light of new information or knowledge” (Giddens 1991:20). Including, thus, social science knowledge.

This more intense dialogue with the people we study implies that we should systematically seek the feed-back of our “subjects” regarding the knowledge we have produced about them, rather than discover accidentally, as in Malinowski’s case above — if we “got it right” from their point of view. In this sense, being accountable to the people we study does not mean producing knowledge that necessarily “suits” them, far from it. Rather, it means considering the process of sharing our knowledge with our subjects and of being alert to their reactions to it, an essential part of the research process.

2. Sociology, Whether Professional, Critical, Public, or Policy, is always Performative

Sociology is always public in the sense that it is always performative (Callon 1999b), which means that our research makes visible or sheds lights on groups, entities, or phenomena that did not exist, or were less visible before we studied them. We are necessarily the “representatives” of what we helped to put to the open. In this sense, the conception of sociology as “neutral” seems entirely misplaced. Sociology, and social science more generally, are inherently subversive (Barrett 1984). There is no such thing as neutral sociological knowledge, and this has nothing to do with sociologists’ capacities or incapacities for reflexivity. Following Flyvbjerg, we could see social science as

a practical, intellectual activity aimed at clarifying the problems, risks, and possibilities we face as humans and societies, and at contributing to

social and political praxis ... in full knowledge that we cannot find ultimate answers to these questions or even a single version of what the questions are. (2001:4; 140)

In that perspective we should also restore the idea that sociology is about values through and through, and that sociologists' activities, whether they want it or not, are completely entangled in power relations. At the core of these relations lies, for example, the "difficult conciliation between the defense of minorities, whose identity depends to a large degree on the knowledge produced, and the achievement of a common good which is not carved up by particular interests" (Callon 1999a:93). There is no easy way to achieve this difficult conciliation, and Burawoy's vision of a division of labour between professional, policy, critical, and public sociologists does not really move us closer to resolving this dilemma.

Going back to where I began, that is, to the kind of sociology I can perform in a Canadian business school environment, I certainly am "into a conversation (Burawoy 2005:7) with the "publics" of my research, even more so as some of my students — executives or MBA students — are directly concerned with this research and perfectly able to discuss and criticize it. If what I do is "public sociology," it differs in important ways, however, with the public sociology Burawoy has apparently in mind. First, it is a public sociology that does not spring from an anticapitalist perspective, or from the conviction that the "interest of humanity" lies in the maintenance of "civil society" as Burawoy (2004:14) defines it. Second, it is a public sociology that is critical through and through, as the questions "knowledge for whom" and "knowledge for what" are always at stake in the conversations I have with the managers and employees concerned with management research. When, for example, I try to explore the foundations and implications of the managerial precept that "the client is always right," the issue of accountability immediately arises according to whether I define my public as the population in general (the clients), the company's stakeholders and executives, or the employees who have to work along that precept. Finally, the circular relationship between social scientists' and practitioners' knowledge is especially clear in the field of management. It implies that two positions are equally dubious: one consists in viewing public sociologists as the "translators" of professional sociology for an invisible and passive public (what Burawoy calls "traditional public sociology"); the other consists in a kind of "glorification" of practitioners' knowledge that often characterizes "organic public sociology." Between epistemological distance and empathetic glorification, I suggest that there is room for other

conceptions of the relationship between sociologists' and practitioners' knowledge and, thus, other ways to practice a "public sociology."

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