



The Value of Strident Agnosticism: Dorothy Pawluch and the Endurance of Ontological Gerrymandering

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Abstract

This paper reflects on the origins and subsequent reception of the paper “Ontological Gerrymandering: The anatomy of social problems explanations”, published in 1985. It describes the circumstances of my turning up at McGill University as a Visiting Professor in Sociology and meeting Dorothy, then a graduate student and the TA assigned to an undergraduate course on Social Problems which I was asked to teach. The paper reflects on the twin benefits: of an interloper, from Europe and from Science and Technology Studies (STS), entering the exotic and heady fray of North American social problems; and of Dorothy’s steady and resolute guidance in introducing me to a new field. The paper suggests some reasons for the endurance of the paper’s arguments, more than 35 years after its publication, drawing on some parallel developments in Social Problems and STS. It asks why has there been rather little mutual interaction between these disciplines, given their common concern with questions, among others, about values, effects and interventions in academic scholarship. The paper concludes that many more of us might have done well to pursue the path of strident agnosticism.

Keywords Ontological gerrymandering · Science and technology studies · Social constructionism · Provocation · Agnosticism

Introduction

I am delighted to be invited to contribute to this special issue/festschrift for Dorothy Pawluch. I don’t consider myself a scholar in social problems. I have not tried to keep up with the field. So I was surprised to discover recently that the paper “Ontological Gerrymandering: the anatomy of social problems explanations” (Woolgar & Pawluch, 1985a) has had such an impact. It has been reprinted in several collections and been the subject of extensive comment and debate (see Ibarra & Adorjan, 2018

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for a recent overview). With all due caution in using quantitative indicators, I note that the paper has received fully 935 citations¹ to date. The aim of this short piece is to reflect on the endurance of ontological gerrymandering. By which I mean not just the enduring prominence of the published paper, but also of the concept and the explanatory practices which the term connotes.

The paper begins by describing the circumstances which gave rise to the publication of ontological gerrymandering, romanticising in particular the significance of my own arrival on the distant shores of North America. This is the basis for discussing, in the second section, some key similarities and differences between notions of social construction worked out in Social Problems and in Science and Technology Studies. Finally, the third section suggests some reasons for the endurance of the problem of ontological gerrymandering and for the absence of mutual interaction between social problems and STS scholarship.

An important preliminary word of caution is necessary. I do not attempt a definitive account of the state of social problems nor of science and technology studies (STS) scholarship. A comprehensive overview is well beyond the scope of this brief paper. Instead, I prioritise personal and idiosyncratic reflections. More interestingly perhaps, this feature of my account arises from key characteristics of the phenomenon it is trying to describe. For, as we shall see in more detail, to take on the task of analysing others' (usually scientists') knowledge practices affords the possibility of drawing on resonances with one's own knowledge practice².

British Invader

I'm not really sure why I responded to the advertisement for a Visiting Professor in Sociology at McGill University. Maybe it was the feeling, widespread at the time, that North America was the continent of opportunity for aspiring academics, even for those already with a permanent position in England. So I became an international interloper for the academic years 1979–81. My duties included teaching a graduate course on Sociology of Science, and an undergraduate course on Social Problems while Malcolm Spector was on sabbatical leave³. At that time I had no idea what a course on social problems should look like. "Social problems" was not (and is not still) an established organizing category in social science teaching and scholarship in the UK and Europe. I recall that the class seemed huge compared to what I was used to in my British university.

For a junior lecturer living and working in North America for the first time, this was an exotic experience. In my first days at McGill I was surprised to witness lines

¹ Or should this be "has received only a mere 935 citations to date"? (And more now thanks to this paper). On the artful deployment of "fully-only" debates in the articulation of social problems see Spector and Kitsuse (1977).

² For an alternative, albeit somewhat protracted, solution to this dilemma, see (Woolgar et al., 2009), in which the exposition is split in two: a "smooth" (or straight) narrative which purports to depict the actual facts of the matter and a "rough" narrative which reveals and reflects upon the contentions and disagreements involved in the production of the smooth narrative.

³ My memory of the details is hazy. Unfortunately, if they even exist, records of the teaching arrangements during this period are inaccessible during the pandemic.

of students queueing up outside faculty offices to complain about the grades they had been given in a recent exam. This, it was explained to me, was a routine practice known as “chiselling”. I was astonished to see a note fixed to the office door of my next door colleague: “All students add 7% to your grade for this course”. In my opening lecture on the course I spent some time extolling the intellectual virtues of the topic, hinting at the intriguing twists and turns that would characterize the academic journey in prospect, generally bigging up the learning experience to come. The students were clearly unimpressed: there was fidgeting and shuffling of feet. The air of general dis-attention and irritation was broken by a prosaic inquiry of startling instrumentalism: “Professor, how is this course assessed?” Not long after arrival I was delighted to receive a series of free copies of thick textbooks from publishers. These seemed to be mainly problem oriented titles covering topics such as abortion, crime, alcohol and drug use, prostitution, health care and so on. All things to do with social problems. Invariably these texts started with an accepted definition of the problem and discussed possible solutions for dealing with them. But nonetheless, getting all those books for free! The privileged life of a Visiting Professor in North America! I felt like Philip Swallow in *Changing Places* (Lodge, 1975). How to cope with all this exoticism? I was lucky to have Dorothy assigned as the TA to the course. She proved a source of steady reassurance amidst this turbulent exoticism.

In the 1970s and 1980s sociological studies of science were developing fast. Earlier work in the sociology of science, especially focusing on the careers of scientists and their institutional arrangements, had given way to an interest in sociological analyses of the content of scientific knowledge. This development characterized a marked distinction between the largely Mertonian inspired North American sociology of science, focusing on science as a social institution, and European and, especially, British sociology of scientific knowledge, with a much greater focus on epistemology: what explained the character of the scientific knowledge that scientists generated? British sociologists of scientific knowledge were drawn into heated debates with US-based philosophers of science about the legitimacy of claiming a social basis for the very content of scientific knowledge.

I arrived at McGill in 1979, when social constructivism was very much on the rise⁴. The first edition of *Laboratory Life* (Latour & Woolgar, 1979) had just been published, but not (yet) at all to much acclaim. Early reviews were not very complimentary. One complained that our argument was a “very bumpy ride over familiar terrain”; another reviewer (a philosopher) claimed that our constructivist analysis unwittingly confirmed everything that objectivist philosophy of science had always said about the scientific process. Yet social constructivism was on the rise. Indeed, it

⁴ For the purposes of this paper I make no big distinction between constructivism and constructionism, except perhaps by reference to the Gershwin’s (Gershwin & Gershwin, 1937) well known comment on transatlantic language differences:

You say eether and I say eyether; You say neether and I say nyther.

Eether, eyether, neether, nyther; Let’s call the whole thing off!

You like potato and I like potahto; You like tomato and I like tomahto.

Potato, potahto, tomato, tomahto; Let’s call the whole thing off!

already seemed to me that by that time the use of social constructivism was rapidly becoming formulaic. This was especially evident for example from proposals for the wholesale application of the constructivist argument to technology. So the sociology of scientific knowledge was followed in quick succession by the social construction of technology, and the institutionalization of their formulaic approaches was enshrined in their respective acronyms: SSK and SCOT.

In a paper written around that time (subsequently published in 1983), I worried that analyses which followed the social constructivist formula deployed a form of static or instrumental irony. That is, they relied upon an implicit distinction between the way things are and the ways things appear to be (Woolgar, 1983). In ways reminiscent of labelling theory the (presumably) same entity could be labelled differently. Thus, for example, it could be argued that the astrophysical phenomenon of pulsars was socially constructed in the precise sense that what they are could have turned out differently⁵. A particular constellation of social interests and arrangements lead to their discovery. With a different set of circumstances pulsars would not have been discovered or might have turned out to be something different. One possible alternative construction briefly considered was signals from an extra terrestrial civilization: the graphical records of the signals bear the labels “LGM1, LGM2...” etc. designating “Little Green Men” (Woolgar, 1978).

In working through the then emerging literature on the social construction of social problems, as part of my preparations for teaching, I could see that many of the virtues and pitfalls of social constructivism were common to social problems (SP) and the sociology of scientific knowledge (SSK)⁶. I saw the opportunity to use the constructivist literature in social problems to help my development of a substantial critique of social constructivism in SSK. A preliminary articulation of this critique was published towards the end of my stay at McGill (Woolgar, 1981). My paper criticizes the use of social interests as an explanatory resource in the “strong programme” (Bloor, 1976) approach to the social study of scientific knowledge. Specifically, it points out that while in this approach knowledge products and scientific events of all kinds are construed as socially constructed representations, interests are not. In effect my argument called out early STS for wanting its ontological cake and eating it too, for holding one element objectively constant while simultaneously subjectively problematizing others. Here then it is possible to discern the antecedent or germ of the ontological gerrymandering argument. I notice that this 1981 paper includes my thanks to Malcolm Spector for his help in its preparation, perhaps signalling the possibility of a linked collaboration between disciplines in a radical critique of constructivism⁷.

⁵ Note that this is a stronger sense of social construction than the claim that pulsars could have been differently *perceived to be* what they are. The former implies ontological construction whereas the latter implies a merely epistemological sense of construction.

⁶ Some theories champion the role of foreigners and newcomers as an important source of innovation (e.g., Akcigit et al., 2017).

⁷ My thanks to an anonymous referee for the helpful formulations in this paragraph.

A common feature of social constructivism in SP and SSK centred on the relation between an entity and its representation. This is of course a very longstanding preoccupation across a wide range of social sciences and humanities scholarship, often articulated in terms of a concern with reality-appearance puzzles (for a recent example see Coopmans, 2021). In the 1980s this concern featured in the context of highly charged disputes about the nature of scientific knowledge which all to some degree centred around differing notions of the extent to which the existence of the external world can be taken for granted. Objectivist philosophers of science were outraged by the proposition that scientific facts are socially constructed since this seemed to admit an irresponsible relativism⁸.

The key analytic logic in the social construction of social problems is the declaration that since a condition (or substance or behaviour) had not changed over a period of time, and yet the response had changed, this proved that the representation was socially contingent. For example, attitudes to marijuana smoking changed across a period of several years. Since, it is claimed, the nature of marijuana had not changed, this demonstrates the contingency of the apprehension (definition, construction)⁹. This contingency is then explained in traditional terms, through the invocation of antecedent circumstances, typically as social forces, interests and so on. The logic is straightforwardly analogous to labelling theory. The ‘same’ behaviour is labelled differently by different parties, in different circumstances etc. So it is the labelling not the behaviour which is socially contingent.

It is this core assumption, that the condition or behaviour did not change, which was the focus of our challenge in ontological gerrymandering.

Beyond Construction

In retrospect it is clear that social constructivism was very important in opening up scientific knowledge and technological capacity to sociological scrutiny. But as the sociology of scientific knowledge developed, a number of key problems with the constructivist formula became evident. Take for example the classic case of the social construction of the bicycle. The argument was that the development of the bicycle was a long process over time, characterized by a series of historical events and the involvement of various social interests and social groups. For example, at the cessation of the Franco Prussian war, manufacturers of metal tubes, suppliers of rifles and other armaments, found themselves without a market. But they managed instead to reposition themselves as suppliers of frames for the emerging bicycle market. Metal framed bicycles were thus “socially constructed”.

⁸ I recall a seminar in Oxford at about this time when the philosopher Bill Newton Smith asked how much he would need to pay the sociologist David Bloor to make him believe that a fact was true.

⁹ This example is also found in Spector and Kitsuse (1977). The general analytic point deriving from this particular example is somewhat weakened by claims that the material characteristics of marijuana have changed (for example through hydroponics technology, better/more intense highs) since the 1970s. My thanks to Michael Adorjan for this point.

Among several problems with this approach is the implication that the entity in question (the bicycle) stabilizes and becomes fixed at the end of the process. Yet other scholars made the point that interpretation does not cease at this point, the “interpretive flexibility” which characterizes the development process does not end. What the bicycle is and what it is for remains an issue of interactional interpretation. There can be marked ambivalence over this and/or determinations of what the bicycle is can be deferred or delayed, accountability for which is perhaps passed on to others in the community. By analogy, a social problem rarely becomes fixed, settled or established, but is always open to further (re)interpretation.

The more important post constructivist development was the rise of critical attention to the very notion of the “social”. It became clear that “the social” is not an independent explanans, to be used to make sense of the construction of science and technology. “The social” is instead more productively understood as the upshot of complex practices, one result of which is to establish a distinction between domains variously articulated as “science”, “technology” and “society”. In other words, the social is as much a construction as is science and technology. This development is indexed by the morphing of SSK and SCOT into science and technology studies (STS). STS is currently a vast multidiscipline comprising a range of disciplines such as anthropology, sociology, media studies, psychology, history and philosophy. It embodies a variety of intellectual currents such as constructivism, posthumanism and feminism. The terms “sociology” in SSK and “social” in SCOT do not feature in the later acronym STS¹⁰. The dissatisfaction with the use of “social” as a ready-to-hand explanans is also signalled by the modified subtitle of the 1986 edition of *Laboratory Life*, changed from “the social construction of scientific facts” (Latour & Woolgar, 1979) to “the construction of scientific facts” (Latour & Woolgar, 1986). Again, the significant point is that “social” is the upshot of constructivist practices not a given, preexisting determinant of those practices (cf. Latour, 2005).

The field of STS has since gone through many twists and turns. Some advocate “turning” as a healthy dynamic of the field (Woolgar & Lezaun, 2013). Others have criticized the notion of turn (Vasileva, 2015) or have flatly denounced what they see as repetitive (Guggenheim & Nowotny, 2003). But the most interesting and, arguably, the most productive feature of STS is its capacity to identify new challenges, in particular, to explore new ways of contributing to, and pushing forward, a long-standing series of conversations about the relations between appearance and reality. These conversations occur across the social sciences and humanities. In STS alone they take the form of a succession of different, perhaps increasingly bold, ways of doubting, and of different targets of doubt, stretching from Merton (1973) and Kuhn (1962) to ontological politics and beyond, via the strong programme (Bloor, 1976), the Sociology of Scientific Knowledge (Collins, 1992; Star, 1991), Actor Network Theory (Callon & Latour, 1981; Latour, 2005; Law, 1994), reflexivity (Ashmore, 1987) and multiplicity (Mol, 2003), to name but a few. The long history of these moves can be understood as a history of different modes of ‘otherwising’: successive

¹⁰ For similar reasons, “science and technology studies” is generally preferred to “science, technology and society” as the decode for STS.

demonstrations of the indeterminacy of a wide range of increasingly recalcitrant phenomena.

In the light of this trajectory of different STS perspectives and approaches we can see the importance of constructivism was not so much in its “explanation” of the ways in which a social problem is defined as in the provocation provided by the initial assertion of contingency. The organizing dynamic of constructivism was the provocation that it could be otherwise. And as is demonstrated by the recent history of STS, constructivism is just one of many ways of pressing the case that It Could Be Otherwise. The formulae for dealing with - we can say explaining away or closing down - indeterminacy are rather less interesting and important than the effort to keep alive the provocative drive behind these resolutions. This was one aspect of [the] ontological gerrymandering critique: in explaining (away) one aspect of a social problem one needs always to be alert to those aspects of the situation which one is overlooking or backgrounding.

Of course, the contention that keeping the provocative drive alive is more interesting and important than attempts to resolve indeterminacy, is itself a provocation. It invites criticism that investigations of indeterminacy fail to address “real world problems” - the environmental crises, patriarchy, neoliberalism and so on. Against this - and in light of conventional sociology’s abject failure to solve any of these problems to date - we need much better to understand indeterminacy as a key component of the genesis and apprehension of just these problems. As is argued in recent work on the phenomenon of imposters, indeterminacy is the engine of social (dis)order (Woolgar et al., 2021).

So STS can no longer be simply identified with constructivism. But the proliferation of different ways of exploring It Could Be Otherwise in STS means that no single alternative -ism has come to dominate and no consensus has formed around a single perspective. This is generally regarded as healthy. A multidiscipline which is at war with itself is more generative in finding further ways of investigating it could be otherwise. Lynch (2000) even went as far as declaring that at the point when STS agrees upon a topic, canon and method, then the field is effectively dead.

Partly for this reason, STS practitioners tend to speak in terms, not of the correct topic, phenomenon, method or perspective, but in terms of sensibilities. Some key sensibilities of STS might be itemized as follows: (1) a propensity to cause trouble, provoke, be awkward; (2) a tendency to work through difficult conceptual issues in relation to specific empirical cases, deflating grandiose theoretical concepts and claims (and even some ordinary ones); (3) an emphasis on the local, specific and contingent in relation to the genesis and use of science and technology; (4) caution about the unreflexive adoption and deployment of standard social science lexicons (e.g. power, culture, meaning, value); (5) reflexive attention to our (frequently unexplicated) notions of our audiences, our values and the utility of our actions. It is characteristic of this approach to take revered and standardized ideas and concepts—science, technology, the law, the market—and convert them into objects of study. This can be done by recasting ideas and concepts so as to stress the processual, situated and contingent bases for the terms: for example, ethics becomes ethicizing; futures becomes futuring; governance becomes governancing; evidence becomes

evidencing¹¹. It can also be done by construing (purportedly robust or recalcitrant) concepts as objects of ethnographic study, whereby technology gives rise to technography, epistemology to epistemography (Dear, 2001) and ontology to ontography (Lynch, 2013). Thus STS, in its more interesting modes, follows the aspirations of good ethnography¹². Technology, epistemology and ontology are subject to the sceptical lens of the curious anthropologist. It means recasting these honorific concepts as ordinary mundane practices, demystifying and bringing these weighty notions down to earth, working them through as everyday practices. In the present case, where the target analytic object is academic research into social problems, our critical inquiry might be appropriately designated socialproblemsography¹³.

Consistent with the premise that our notions of our users are performed, enacted, and configured (Woolgar, 1991), for a whole range of cultural artefacts, this style of STS maintains an active interest in the transposition of social science research across sometimes challenging social-organizational boundaries. This we construe as a radical intellectual challenge, not merely a political preference or a practical obligation.

Over the evolution of the multidiscipline, symmetry and impartiality have been central to the core sensibilities of STS. For example, this initially meant that the same kinds of social explanation should be used to account for the genesis of scientific facts whether or not they were deemed to be true. In a later version, notably Actor Network Theory (ANT), the symmetry principle meant that both humans and nonhumans should feature equally in any explanation (Callon, 1984; Latour, 2005).

Yet perhaps the most important feature of all the STS sensibilities is provocation¹⁴. Provocation – to draw on its etymological roots: pro vox – is to give voice. And it means to give voice to things, people, situations and conditions. Especially to give voice to those entities which are neglected, overlooked or just reckoned to be mundane (that is, from latin *mundus*, those entities which have all the appearance of being of the world, that seem just the way they are). More especially perhaps, provocation means resisting the voices already associated with things, people, situations and conditions. Of always asking whether other alternative voices are possible.

¹¹ More generally, all nouns might best be treated as verbs. Nouns should thereby be understood as “chameleons of objectivity” (Adorjan, 2020).

¹² For a related description of STS sensibilities compare Law and Lin (n.d.): “Good research grows out of concerns and an iterative process in which sensibilities and concerns are educated and adapted to generate researchable questions and topics. Since STS is qualitative, working through empirical case studies, if you want to cultivate STS sensibilities you need to read its cases. It also means that for STS the extent to which it is possible to design research is limited. Yes, there are specific skills to be acquired. But research practices unfold more or less uncertainly, and a sensibility to those uncertainties is a core STS sensibility. Others we have touched on include sensitivities to: materials; normativities; the webs of association; the fragility of relations and objects; the multiplicity of logics running through webs; the situated character of our narratives; their performativity (political and otherwise); the possibility of alternatives; otherness; and the fact that insensibilities go along with sensibilities.”

¹³ Admittedly, a slightly less catchy designation than those mentioned above.

¹⁴ Interestingly, Spector and Kitsuse (1977) much quoted original comment on social problems research, in the opening lines of *Constructing Social Problems*, “that there has never been a sociology of social problems” is often described as “provocative”.

It is this form of provocation which captures the essential features of agnosticism. Agnosticism can be understood as a determined refusal to accept any of the particular voices (definitions, constructions, apprehensions and so on) associated with an entity. It instead seeks to keep open the possibility of other, alternative voices. The dominance of any particular voice (definition etc.) is to be resisted. And this is a principled resistance, rather than an argument in favour of some particular alternative voice (definition, construction...). That it could always be otherwise is the guiding principle here.

The main advantage of agnosticism is that it keeps open the possibility of more and different questions about the social problem in view. And “strident agnosticism” – which I think nicely characterizes Dorothy’s *modus operandi* – signals that whenever agnosticism rubs up against fixed or accepted definitions it asks the question what and how. It would be a mistake to think of agnosticism as an easy option: as a way of claiming a neutral position and hence to somehow dodge the difficulty of committing politically. I think this is entirely wrong. Agnosticism is not an easy option to embrace purity. It is, rather, to invoke Douglas’ (1966) famous contrast, fraught with danger. To refuse acceptance (of the status quo, of one or other alternative interpretation or definition) requires courage.

In particular, it is important to recognize that following the path of “strident agnosticism” is an aspiration rather than a methodological formula. Just as good ethnography depends on candid and reflexive reporting of the pitfalls, setbacks and, indeed, dangers involved, so does the aspiration to agnosticism. The struggles to make sense of the “exotic” are more fruitfully understood as providing insights into our own assumptions and taken for granted practices than as part of a path towards objective knowledge about the other. Similarly, agnosticism is much better understood as a provocative sensibility than as a routinised method or formula with a guaranteed outcome. Agnosticism is not about method, it runs deeper than method, it is about sensibility. As Dorothy herself has eloquently discussed, this is not at all an easy path (Pawluch, 2019).

Conclusion

The popularity of social problems constructionism has waned since the disputes of the 1980s, as has the popularity of STS constructivism. One of the editors of this special issue suggested that the post-ontological gerrymandering debates peaked in the late 80 s and mid-1990s, produced some interesting edited collections (e.g., Holstein, 2003), including edgy papers (e.g., Best, 1993), “but folks ultimately got on with things and produced good work in the years since” (Adorjan, 2020). By contrast, as we have seen, STS has evolved analytically in many different directions. I am not at all sure its practitioners would be content with the idea that they “ultimately got on with things”. That makes ontological gerrymandering seem like a temporary, and inconsequential, distraction from the main business. By contrast, as I have tried to argue, the problems and possibilities of provocation, the relentless pursuit of agnosticism with all its difficulties, is the main business.

Are the subsequent directions taken by Social Problems and by STS because of - or in spite of - the ontological gerrymandering critique? My own view is that ontological gerrymandering is always with us, but it is too difficult a phenomenon to which to fashion a “solution” in conventional terms. Indeed, the quest for a solution is an inappropriate response. Ontological gerrymandering is an unavoidable and omnipresent feature of sociological analysis because it involves the selective application of the sceptical gaze. Instead of seeking a solution we should better focus on what we make of it and what to do with it. Importantly, this makes clear that the point of the ontological gerrymandering critique was not simply to chastise social problems authors for error (Woolgar & Pawluch, 1985b). To do so would be to imply that there is a better way.

The advantages of being stridently agnostic towards the social problems one examines lie in the provocative possibilities of thinking differently. Because we inhabit a contemporary world largely populated by binary choices and commitments, it is easy to mistake this provocation for a retreat to political quietism. But, as I have tried to show, agnosticism is emphatically not about adopting a ‘sideline neutral’ approach. We should instead understand that ‘sideline neutral’ is actually productively provocative. It encourages us to step out of the conventionally defined political choices and posing deeper questions. Ultimately, agnosticism is far more radical than mere (conventional) politics¹⁵.

I am immensely grateful to Dorothy for assisting my introduction to the exotic world of North American scholarship on social problems. It helped clarify the possibility of finding different ways to challenge traditional ways of doing sociology. And it helped bring down to earth the kinds of rarified examples often treated in STS at the time, to show that the provocation “it could be otherwise” applies as much to ordinary objects and circumstances as to scientific knowledge, and does so with significant consequences (Woolgar & Neyland, 2014; Woolgar & Neyland, forthcoming).

Declarations

Conflict of Interest On behalf of all authors, the corresponding author states that there is no conflict of interest.

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¹⁵ See Mol (1999, 2003) on ontological politics as one important alternative to “mere politics”. In short, Mol argues that the categories, identities, nature and attributes of political players are all the upshot of practices, not their precondition.

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