



The promises of practice

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Abstract

Practice has become a topic of increasing empirical and conceptual concern within sociology and neighbouring fields. 'Practice' can refer to a location or it can refer to action. It is possible to be 'in practice', to 'have a practice' or to be 'constituted by practice'. Practice can be a cause, an effect or an explanation. Within science and technology studies (STS), the practice orientation is simultaneously analytical – in the form of various practice theories – and empirical, in that research objects are often defined as 'practices'. Focusing on a range of examples, especially ethnomethodological, this paper examines some implications and problems that follow when practice slides unnoticed between empirical and conceptual registers. Arguing that a reconsideration of practice thinking is important in order to retain its vigour, we outline a view of practice as a 'factish', at once conceptual and empirical, which facilitates analyses of practical ontologies and their transformations. This informs a final discussion of the politics and promises of practice.

Keywords: factish, politics, practical ontologies, practice, STS

Introduction

Practice has become a topic of increasing empirical and conceptual concern within sociology and neighbouring fields (Schatzki *et al.*, 2001; Turner, 1994). For better and worse 'practice' has a very wide range of connotations. It can refer to a location, it can locate actors in a context, and it can refer to action, or to construction. It is possible to be 'in practice', to 'have a practice', or to be 'constituted by practice'. Practice can be a cause, an effect, or an explanation. Given the widespread use of practice terminologies with conflicting definitions and analytical tendencies, practice seems to beg for disentanglement.

Our aim in this paper is to examine some implications of taking practice as both an analytical starting point and an empirical focus for social inquiry. We focus on the case of science and technology studies (STS) and especially on some ethnomethodological considerations of what practice entails. The case of ethnomethodology in STS is particularly interesting because it is one of the most prominent approaches in the field, because of its strong focus on situated practical action and, not least, due to its insistence that its job is confined to

elucidating ethnomethods. Though this scope is itself fairly narrow, we nevertheless believe that the discussion is illuminating for sociological theory more broadly.

Now it is by no means the case that ethnomethodology has a stranglehold on STS. Indeed, as John Law writes in *On Sociology and STS* (2008), the field is both heterogeneous and multifaceted. It is, however, the case that practice has emerged as a very central, if not paradigmatic, area of interest within the field. This intellectual orientation is simultaneously analytical – as exemplified by the proliferation of various practice theories and perspectives (not least ethnomethodology) – and empirical, in that research objects are often defined as ‘practices’.

On the one hand, practice has become something of an unproblematic *empirical* category. Practices, in this view, are simply that of which the world consists, so as a matter of course one can find them wherever one looks. This premise often leads to a fairly narrow orientation, like examining the concrete activities through which a group of scientists or technologists work towards some particular goal. On the other hand, practice also designates an *analytical* approach the scope and meaning of which is rarely explicated. Thus, practice approaches are slippery: they can slide easily between empirical and conceptual registers, without at any point losing their aura of common sense.

Arguably such slipperiness is both unavoidable and potentially productive (Jensen, 2013). Yet, it causes a variety of problems if this predicament is not recognized, and more if it is actively denied. Such denial occurs, for example, when ‘practices’ are purified by insisting that they are simply empirically found or by disavowing that they are also conceptually constructed. As Bruno Latour has written, this makes it appear ‘as if scientific practice, technical practice, and political practice lead into entirely different realms than those of theory of science, theory of techniques, theory of politics’ (1999: 266). His alternative to such purifications is to emphasize the inseparability of the conceptual and the empirical, the mutual constitution of which he captures with the term ‘factish’. Neither a ‘fact’ nor a ‘fetish’, the factish bypasses discussion of whether practices can be ‘just found’ (and are thus real) or are necessarily ‘analytically fabricated’ (and are thus purely cognitive, and possibly unreal). It does so by insisting that fabrication is not the anti-thesis of reality, but rather its condition. While Latour uses the notion of factishes to relocate scientific ‘facts’ in between the registers of realism and constructivism, we argue here that ‘practice’ itself can be seen as a sociological factish.

We begin with a brief genealogy of the emergence of practice as a central focus within STS and pinpoint certain conceptual challenges with the current practice orientation. We proceed to argue that a reconsideration of important tenets of practice approaches is central in order to retain their vigour. Finally, we discuss the relations between practice approaches, social theory and the politics of social science.

Why practise in STS?

The intellectual roots and forms of STS are so varied that it is difficult to specify analytical commonalities between the positions that currently make up the inter-discipline (Biagioli, 1999; Law, 2008). But if we were to choose one term, widely seen as analytically, methodically or theoretically valuable, it would be practice. The contemporary pervasiveness of practice in STS piques our curiosity. Why and how has practice become such a central concern?

Following a period of intellectual predominance, the logical positivism affiliated with the Vienna School from the late 1920s onwards, exemplified by the works of the philosophers Otto von Neurath and Rudolf Carnap, were challenged by various new positions. These included the later Wittgenstein (in his move from the *Tractatus* (1921) to *Philosophical Investigations* (1953)), Karl Popper (1968) and Imre Lakatos (1976), and also thinkers inspired by Marxism, phenomenology and hermeneutics. Nevertheless, we can point to Thomas Kuhn's *The Structure of Scientific Revolutions* (1962) as the volume that really energized new discussions about science. Kuhn argued that scientific development cannot be understood as progressive and cannot be explained rationally (cf. Law, 2008). Kuhn's analyses of paradigm changes thus opened a space, wherein a variety of factors other than scientific method and truth – for example, metaphysical presuppositions, political views, habits and institutional rules – could be seen as active ingredients in the construction of scientific facts and reality. Social constructivists like Barry Barnes (1983) quickly realized that Kuhn had paved the way for a new sociology of science. Along with David Bloor, he developed the formative strong programme in the sociology of scientific knowledge (SSK) (Bloor, 1976).

The analytical questions of SSK were radically different from the traditional philosophy of science. For example, the philosopher Hans Reichenbach (1938) operated with a distinction between the scientific *context of discovery* and the *context of justification*. But SSK was able to reject just that distinction since, *pace* Kuhn, there is no separate context of justification. The same argument, pitched somewhat differently, also elucidates how SSK distanced itself from the more traditional sociology of science. For while Robert Merton (1973) insisted that institutional settings were of crucial importance for the sociology of knowledge, he maintained that the *content* of science was outside its analytical scope.

SSK argued that the sciences construct reality. These analyses showed scientific facts as resulting from classifications of reality based on negotiations between social groups with different interests and forms of knowledge. Given that nature can be classified in multiple ways, this was a relativist stance that rejected classical philosophical virtues like objectivity and neutrality. One consequence was that scientific knowledge lost its special epistemological privilege, vis-à-vis other forms of knowledge, heretofore considered 'irrational' or 'practical'. However, the central analytical focus in SSK remained

knowledge, and the main battles in which SSK researchers engaged were indeed with philosophers of science interested in epistemological issues (eg Hollis and Lukes, 1982).

Many other approaches also contributed to the development of a practice orientation in STS. In particular, we highlight Harold Garfinkel's (1967) ethnomethodological programme, which, for the practice-turn, in STS, as in sociology at large, would turn out to be central. Ethnomethodology was developed as an alternative to Talcott Parsons' (1949) structural-functionalist sociology, and entailed a focus on local, reflexively ordered action (Linstead, 2006). As early as 1981, Garfinkel and his students Eric Livingston and Michael Lynch wrote on the centrality of scientific work (Garfinkel *et al.*, 1981).

Around the same time, laboratory studies emerged as a novel form of inquiry in the sociology of science. Affiliated with Bruno Latour and Steve Woolgar (1979), and Karin Knorr-Cetina (1981), these studies aligned with the ethnomethodological preoccupation with work, rather than with SSK's focus on knowledge. Thus, Latour and Woolgar aimed to demonstrate how scientific practices constitute natural facts. Reminiscent of ethnomethodological arguments (Garfinkel, 1967), they advocated that attention be paid to scientific work at the laboratory bench rather than scientists' models and rationalizations. It is not unlikely that showing the extent to which practices mattered *even in science*, putatively the epitome of cognitive, knowledge-based activities, contributed to the sense that the study of practice would be important everywhere.

Laboratory studies were the immediate precursor of actor-network theory (ANT), which Latour and Michel Callon developed in collaboration with John Law (eg Callon and Latour, 1981; Callon and Law, 1982). The analytical signature of ANT is its insistence that the role of non-human actors in scientific knowledge production is equally or, sometimes, more important than that of scientists (cf. Schillmeier, 2008). 'Practical action', in this view, was not confined to the human realm, but also encompassed the 'activities' of technologies and scientific objects.

Although SSK also saw scientific facts and, indeed, nature itself as constructed, social constructivists nevertheless viewed ANT's interest in non-human agency as a regression to the same kind of realism that they had spent so much energy refuting (Collins and Yearley, 1992; Bloor, 1999). For ANT, however, the fabrications were not exclusively social, since humans, technologies and 'natural objects' all constructed each other, and reality, in the same 'practical' process.

In 1995, Andrew Pickering had already described practice as a generic term for a 'work of cultural extension' (1995: 3), one of the central qualities of which was its propensity to continuously take new forms. Pickering's definition of practice was thus very different from an idea of practice as a relatively static, bounded domain – a 'local practice' – *in which* activities take place. Hence, it was no coincidence that he gradually came to replace the notion of practice

with a terminology of ontological transformation. Later, Annemarie Mol (2002) would define a similar line of inquiry by insisting that realities in the plural are enacted through a wide range of practices, comprised by multiple forms of agents.

Similar to actor-network theory and Pickering's emphasis on ontological transformation, Mol's empirical philosophy extended the empirical and conceptual rubrics of practice studies. In this work, ontology spoke to the mutual shaping of people and things that cross, or rewire, 'the local' and 'the global' as much as the 'empirical' and the 'conceptual'. The emphasis on ontological transformation pointed to changes in the very composition of the world, accomplished through all kinds of means, including some that might at first glance seem quite un- or impractical.

As these genealogies suggest, there is no neutral pathway to practices *an sich*. Indeed, practice is an elastic word, which can be stretched or tightened depending on interest and orientation (Pickering, 1995; Sewell, 1992). In the following, we designate the ontologically oriented approaches of Latour, Mol and Pickering with the umbrella term 'studies of practical ontologies'. Its guiding impetus is to enrich practice studies through sensitization to the existence of a wealth of forces, effects and actors. Just as importantly, these studies are also explicit about their particular *conceptual* orientations. The importance of the latter observation will become clearer as we look more closely at practice studies in some of their current guises.

A practice orientation

The analytical power (and original provocation) of ANT was based in the argument that neither *nature* nor *society* could provide the basis for any general theory of science, since both are historically variable categories, mutually constituted in scientific laboratories and other social arenas. In Latour's (1994) formulation, essence is existence and existence is action. And since essence thus *is* action, practice becomes a natural point of analytical and empirical attention. Andrew Pickering's edited volume *Science as Practice and Culture* from 1992, testifies to the gradual strengthening and intensification of this emphasis in STS, both as a theoretical strategy and empirical focus.

In *The Practice Turn in Contemporary Theory*, Theodor Schatzki offers the following characterization of the status of 'practice' in the 2000s in the social sciences at large:

Thinkers once spoke of 'structures', 'systems', 'meaning', 'life-world', 'events', and 'actions' when naming the primary generic social thing. Today, many theorists would accord 'practices' a comparable honor. Varied references to practices await the contemporary academician in diverse disciplines, from philosophy, cultural theory, and history to sociology, anthropology and science and technology studies. (Schatzki, 2001: 1)

Schatzki notes that the focus on practice has not caused the emergence of a ‘unified practice approach’ (2001: 2). He suggests, though this is in fact also dubious, that all practice theorists will subscribe to the minimal assumption that practice entails ‘the dependence on activity on shared skills or understandings (which are typically viewed as embodied)’. Meanwhile, disagreement reigns concerning ‘what, if anything, beyond shared understandings is necessary to explain practices’ (2001: 3).

This disagreement can be illustrated by contrasting prominent sociological positions: a *theoretical* interest in practice is found in work of Pierre Bourdieu, who contrasts practice theory with *positivist materialism* (cf. Schinkel, 2007). Practice theory, Bourdieu says, insists that ‘objects of knowledge are constructed, not passively recorded’ (Bourdieu, 1990: 52). Bourdieu carefully distinguishes practice theory from what he refers to as *intellectualist idealism*, referencing his notion of the *habitus*, which is always constituted in practice, and is always oriented towards ‘practical functions’ (1990: 52). As Nick Turnbull and Nikó Antalfy note, this is a project that entails that

sociology must always aim at understanding the world as it appears to the actors in that world. In seeking to uncover the reality of social action as closely as possible, he differentiates academic interpretations of the world which deploy abstract, ‘scholastic’ reason, from the ‘practical’ reason of social actors’ own ways of being and acting. It is the task of sociology to use the former to uncover the latter. (Turnbull and Antalfy, 2009: 550)

Thus Bourdieu posits a distinction between two forms of reasoning, ‘academic’ and ‘practical’, but while his project emphasizes the importance of accounting for the latter, this project is itself determinedly theoretical, or, in his own terminology, ‘scholastic’.

Meanwhile ethnomethodology defined its interest in diametrical opposition to just such a theoretical interest. Defining ethnomethodology as the study of ‘practical action and practical reasoning’, Michael Lynch, a key figure in STS from the 80s to the present, further describes this approach as an *alternative* to any interpretation that views practice as ‘parts of society, social structures, cultural systems’ or which relies on a distinction between the micro and the macro (Lynch, 2001: 131).

According to Stephen Linstead, ethnomethodology offers ‘a powerful challenge to programmatic views of sociology that sought to determine stable law as underpinning social order’ and sets ‘out an alternative programme to reveal social order as a dynamic, contingent “ongoing accomplishment”’ (2006: 399). Ethnomethodology thus centres on ‘situated accomplishments by the parties whose local practice “assemble” the recurrent scenes of action that make up a stable society’ (Lynch, 2001: 131). This orientation neatly aligns with ANT’s interest in practically constituted assemblages of heterogeneous actors.

The differences between Bourdieu's sociological practice theory and the ethnomethodological search for 'situated accomplishments' indicate the breadth of what counts as a practice approach in STS and in sociology at large. It is beyond the scope of the present paper to discuss all these approaches, but they are numerous. Centrally, however, whereas Bourdieu is a major figure in sociology, ethnomethodology being rather more marginal, these positions are reversed in STS. Whereas one can certainly find studies inspired by Bourdieu, they fade in comparison with the massive presence of studies focused on ordinary action in situated practices.¹ One consequence of this situation, we suggest, is that practice has become a slippery term that has the propensity to slide unnoticed between empirical and analytical registers.

We can draw nearer to this issue by way of analogy to a case of concept development in bioinformatics. In her study of this field, the sociologist of science Joan Fujimura discussed the ambivalent status of homology. Homology, Fujimura explained, works as 'an interpretive resource shared by biologists' (Fujimura, 1999, 59), which has undergone a gradual semantic shift. Whereas for Darwin and his contemporaries, homologous structures were seen as *evidence* of evolution, later 'the existence of shared ancestry . . . became part of the definition of homology' (1999: 60). Homology, that is, changed from being a notion used to explain the *consequences* of a common genetic history, to defining what shared history *is*. 'This transformation of explanation to definition', Fujimura wrote, 'created a "semantic confusion"', which haunts bioinformatics to this day (1999: 61).

Likewise, within STS, we suggest, scientific and technological practice has gradually changed from being viewed as *what results from* a varied history of heterogeneous interactions to being posited as what uniquely characterizes these domains, or indeed, as the empirical orientation of the field has expanded, *any* domain. Further, this situation has also been critically interpreted from the point of view of the 'semantic confusion' it has generated (Turner, 1994). Even so, this 'confusion' can be seen as both unavoidable and potentially productive. Rather than searching for ways of avoiding it, we are thus concerned with elucidating the consequences of taking the slipperiness of practice seriously. An examination of such slipperiness, we suggest, holds potential for opening up new forms of practice inquiry. It may also facilitate a rethinking of fruitful engagements between sociologist and their interlocutors.

The anthropologist Michael J. Fischer has critically observed that certain STS concepts seem to operate akin to 'objects' in object-oriented programming languages:

In computer programming, object-oriented languages allow the programmer to drag and drop convenient objects that are already pretranslated into machine language and thus ease the programming. It is a kind of black boxing, but may be more productively thought of as a creation of concepts

or vocabulary that others can use without having to fully rederive and reargue their utility, meaning and justification. (2007: 555)

Though this is not a fair characterization of STS in general, it offers a quite lucid metaphor for what happens to practice when the term comes to be taken as a 'natural' focus of inquiry. Indeed, it is fair to say that, practice vocabularies in STS can currently be applied without much argument concerning their 'utility, meaning and justification'. Mimicking Foucault, this risks turning practice into an empirico-conceptual 'charm', which appears as a guarantor of insight, yet is 'for the most part, more magic than real' (Foucault in Davidson, 1997: 10).² Such 'magical qualities', we suggest, are exhibited most vividly when conceptualization of practice is assumed subservient to empirically found practices.

We can stretch the analogy to bioinformatics a bit further. Michael J. Donoghue argues that 'the history of the word homology can be interpreted as a series of responses to challenges brought on by underlying conceptual challenges' (cited in Fujimura, 1999: 76). Seen thus, conceptual change is always part of processes of disciplinary contestation. As Donoghue notes, such changes are invested with power, since they are always 'a means of forcing other scientists to pay closer attention to what one thinks is most important' (cited in Fujimura, 1999, 76).

Of course, paying close attention to practice is not a bad idea. Yet, inasmuch as STS research is presently extremely attentive to practice, it seems important to look more closely at the costs and benefits of this preoccupation. To avoid turning practice into a magical charm, it must be 'denaturalized' and this entails recognizing that practice is never simply 'found' but is always fabricated. This would transform practice into a factish: part made, part found – and real and consequential, just for that reason (Latour, 1999).

Practice pitfalls

In *The Social Theory of Practices*, the philosopher Steven Turner identifies the central 'semantic confusion' of practice thinking as the ascription to practice of the status of a kind of object; a social substance with unanalysed, but implied, causal powers (1994: 11).

According to Turner, the concept of practice, along with several analogous terms including tacit knowledge, social rules and tradition, is often used in an essentializing manner, in spite of their overt anti-essentialism. Turner argues that these concepts refer to something undefined and *hidden* that is accorded with the power of explaining what we share and how we act:

Each of them is a name for an analogical object: something like spectacles, but 'behind the eyes', with the effect of conventions but without explicit agreement, like knowledge in some ways, unlike knowledge in that it cannot be articulated. (Turner, 1994: 11)

Practice and its kin terms, Turner says, give rise to a sort of practice relativism, since *everything can and should* be related to the question of practice or how something 'is practised'. Apparently this imbues practice with quite significant powers of explanation. Yet, according to Turner, the actual effect is more akin to analytical paralysis. His book is thus a sustained problematization of the concept of practice and the obviousness with which it has come to be treated in social theory. In the end, Turner argues that practice is more than anything a *promissory* concept. Turner offers a reading of practice as a kind of *ersatz*-theory. In this view, one of the consequences is the aforementioned causal reversal and attendant naturalization whereby practice, previously in need of philosophical or other theoretical justifications, now comes to be what fuels inquiry and, indeed, legitimizes it:

the diversity of human practices has become a place-holder or filler in the slot formerly occupied by the traditional 'foundationalist' notions of truth, validity and interpretive correctness. Truth, validity and correctness are held to be *practice-relative* rather than *practice-justifying* notions. (Turner, 1994: 9)

This is a powerful critique. Turner's solution to the problem, to replace the notion of practice with a concept of individual habits, however, appears to take as much for granted as the approaches he criticizes. It ascribes to habits and the individuals purported to 'have them' an explanatory power, that is arguably just as problematic as that of practice. For what is left of habits, if they cannot be taken for granted either? And where do habits come from?

Even so, Turner's critique indicates that the very elasticity of practice makes conceptual clarification difficult if not impossible: a situation that is exacerbated because practice seems both to be able to explain many things and to exist everywhere. In the context of STS, we surmise that this is an effect of the mixed ancestry of the concept, which was defined it in opposition to rationalism, positivism, individualism, *teoria*, knowledge, and much else. As Barry Barnes has argued, however, one of the most important sources of the current elevation of practice is the general decline over the last decades of approaches analytically predisposed to Marxism and oriented toward the macro level:

an unkind account of this development might regard 'practice' as part of the debris produced by the disintegration of Marxism, a concept carried by refugee theorists who have found home in various 'post-Marxist' forms of sociology and social theory. (Barnes, 2001: 18)

In Barnes's view, one effect of this 'retreat' to practice is that it can presently appear as if *there is nothing else to study*. In a resonant argument, Katie Vann and Geof Bowker (2001) suggested that practice approaches to work (or 'labour'), though usually humble in their emphasis on the everyday and

mundane, and often eschewing grand theory, may yet have some striking ‘imperialist’ tendencies. In particular, they argued, one reason why practice has gained such momentum after the decline of Marxism is that the concept seems to obviate the need for explicit political analysis since politics is presumed to be directly ‘findable’ in practice (see also Vann, 2004). In this sense, practice studies can be assumed to disclose unmediated truths about society. According to Vann and Bowker, such studies gain leverage from the implicit way in which they collapse empirical descriptions of ‘actual practice’ and theoretically informed political critique.

Through such processes of condensation and conflation, practice becomes endowed with capacity to account for a *mass* of things, as in Barnes’s list: ‘power, theory, competence, knowledge, experience, goal directed, informed enactment’ (2001: 19). It is just this overly comprehensive mandate, argued Barnes, which makes analysis of the limitations of practice increasingly necessary. In particular, he suggested, something extra-practical is always needed to account for how practices are produced and reproduced (2001: 20). In contrast, the assumption that practices are self-delimiting, self-reproducing or self-explaining is what tends to endow practice with magical properties.

One problem with the encompassing mandate of practice in contemporary STS is that it simply reverses the excessive emphasis on theory and epistemology found in earlier philosophies of science. The result is a new favouritism, no longer in favour of rationality, but of skills and embodiment (Barnes, 2001: 21). Additionally, insofar as the focus on practical action orients analysts to what people do at the cost of elucidating their many different reasons for doing it, practice approaches risk failing to describe people as agents. Paradoxically, we can interpret Barnes’s criticism as indicating that a practice orientation can lead *to* a view of scientists as ‘cultural dopes’ rather than *away* from just that view (Garfinkel, 1967; cf. Ratner, 2012: 152 on ‘practice dopes’).³

Ethnomethodological practices

As mentioned, Turner shows practice thinking to often rely on the implicit notion of an underlying social substance, which can be accessed ‘through practice’. In this sense, to state that something ‘depends on practice’ is analogous to the common argument for context-dependence. Though statements to this effect are by no means necessarily wrong, they are obviously of limited analytical power insofar as practice is not itself further specified. Thus, we agree with Turner, Barnes and Pickering that, whatever the boundary of a practice is, it is not auto-determined. The implication is that studies, which take little or no interest in what they perceive to ‘fall outside’ practice are most prone to the problems we have identified.

Perhaps the clearest example of this tendency within STS is ethnomethodology. For ethnomethodology, anything whatsoever is viewed as

a matter of practice and, accordingly, situated accomplishments can be found anywhere. This expansive view has been a major source of inspiration for actor-network theory and for studies of practical ontologies in the more general sense defined above.

Even so, the focus of specific ethnomethodological studies is usually 'local practice', and what is not part of the ordinary situated action that makes up a local practice, falls outside the scope of interest. Thus, whereas ethnomethodologists will insist that the intricate ways in which scientists relate to an optical pulsar (Lynch, 1993), a mathematical proof, or even a game of checkers (Livingston, 2006) are fascinating empirical topics, they will rarely view scientists' own *concepts* of science, or their rationalizing and universalizing discourses, as similarly worthy of attention. Given the focus on ordinary action – that is, scientists are primarily described as acting practitioners rather than imaginative (and sometimes scheming) thinkers – scientists risk being depicted as relatively detached from, or naïve about, the broader significance and implications of the work in which they engage *qua* practitioners.

Of course, ethnomethodologists also notice the existence of scientific imaginaries and political discourses. However, such manifestations are dealt with in two characteristic ways, both of which tend to minimize their implications. One is to consider scientific visions and discourses as *post-hoc* legitimations of 'real' situated practice, which means that they fall outside the scope of analysis. The other is to embed such discourses so firmly *in* local practice that their consequences elsewhere can be deliberately ignored (see Harper, 1998).

For example, Michael Lynch and David Bogen's fascinating *The Spectacle of History* (1996) investigates the Iran-Contra hearings as a case of how history is produced in practice. Though historical, their purpose is not the same as that of the traditional historian, who would unravel what really happened 'behind the scenes'. Instead they are concerned with the social production of history, and thus their interest lies 'in the infrastructure of that construction, the practical methods through which the event was assembled, contested and stabilised' (1996: 7). For the same reason, their examination of the Iran-Contra case does not lead to an interest in developing an encompassing social theoretical explanation. This is in contrast, with 'deconstruction', they say (rather dubiously) and with 'social constructivism'. Instead their descriptions 'are organized around and take many of their initiatives from the complexities and circumstances of the case at hand' (1996: 15).⁴

Though the resulting study is both rich and fascinating, the Iran-Contra hearings nevertheless also made abundantly clear that the involved people engaged in activities and discourses that extended significantly beyond this locally organized setting. Indeed, the activities and discourses which made up the hearings and the histories produced in its course, were both irremediably entangled with discourses and histories produced in other places, and

themselves *meant* to travel far and wide and engage in all manners of constructive or deconstructive history making *elsewhere*.

Returning to studies of scientific practice more specifically, it may well be that the general reluctance to take scientists' own theories of science seriously relates to the fact that STS was to an important extent *constituted by the ambition to offer alternatives to just these kinds of theories*. Presently, however, this unwillingness creates its own problems (Bowker, 2009). Not least, it routinely leads to an implicit privileging of practice thinkers' *own* forms of reflections and modes of thinking, veiled in the 'descriptive' language of practical action. As this happens, practice begins to morph into an all-purpose solution: the aforementioned sorcerer's formula.⁵

As noted, the alternative provided by 'the factish' is not premised on a reversal, whereby 'empirical' practice turns out to be a purely 'conceptual construct'. Instead, the premise is that 'the difference between theory and practice is no more a given than the difference between context and content, nature and society. It is a divide that has been *made*' (Latour, 1999: 267). Avoiding the sorcerer's formula requires that constant vigilance be directed simultaneously at 'the one who justifies practice by theory or theory by practice' (Spivak, 1996: 156), and it is just such vigilance that the factish enables.

Practice against theory?

Stephen Linstead characterizes ethnomethodology as a 'rigorous non-positivistic micro-study of natural social interaction' capable of illuminating 'the most fundamental sociological issues' (2006: 403). He further argues that ethnomethodology is 'making advances with regard to its perceived institutional relevance for the study of organization and administration, especially in the study of managerial processes such as decision-making, negotiation, policy and strategy making' (2006: 403).

It is perhaps testimony to the tensions within ethnomethodology that these claims for theoretical and practical relevance run directly counter to Michael Lynch's explanation of what follows from ethnomethodology's programme (for a third position see Shove *et al.*, 2012). Noticing, like Linstead, the currently widespread preoccupation with the question of whom and for what purpose social inquiry is 'usable' (Lynch, 2001: 142, cf. Jensen, 2007), Lynch has been emphatic that ethnomethodological studies of practice have no practical use.

This is an argument one might also come across in other university domains, typically those affiliated with 'basic science' (like physics) or 'high theory' (like cultural or literary theory). In contrast to these endeavours, however, Lynch's rejection of utility is not premised on the claim that what ethnomethodology offers is of a theoretical rather than practical nature. For he is also adamant that ethnomethodology offers no 'general theoretical solutions' of the kinds

sought after by social theorists. Indeed, he insists that ethnomethodology is not a theory at all. What ethnomethodology produces is simply a corpus of studies, which ‘appeals to a relatively few of us who are interested in pursuing studies of locally reflexive orders of action’ (2000: 47). As an approach to social inquiry it is unique in suffering no ‘grand theoretical delusion’ (2001: 146).

It is nevertheless the case that practice studies *have* garnered institutional attention for what is taken to be their practical utility. In the case of ethnomethodology, this is perhaps most vividly the case for studies in the areas of information systems and participatory design, where researchers deliver implications for technology design based on their uniquely detailed case studies (eg Hartswood *et al.*, 2002; Luff *et al.*, 2000). More generally, Katie Vann and Geoff Bowker (2001) have examined the concept of communities of practice, a concept that originally entailed a critique of formal learning theory (Lave, 1988) but which was gradually adopted as a resource for business innovation, and instrumentalized in corporate efforts to ‘build COPs’.

It might be considered obvious, and not in itself scandalous, that something happens to any set of ideas or concepts that moves into the world, encounters new interests, and is adopted for new ends. Indeed, in more recent work, Lynch is explicit that this is a frequent occurrence (2009). Even so, however, he delimits the relevance of ethnomethodological practice studies to those (‘relatively few’) who care about the study of locally reflexive orders of action (2000: 47). For everyone else, he insists, ethnomethodology offers simply ‘a valuable form of therapy’ (2001: 146). This apparently humble position may perhaps be seen as a rhetorical measure, taken to protect ethnomethodology against being co-opted as instrumental business advice in the sense that befell communities of practice. However, it could also be seen as indicative of the problems that arise from overly narrow definitions of practice.

In a 2009 article, which discussed the relation between social science and business, Lynch noted that ‘in the social sciences, “doing your own job” often, and perhaps *always*, requires some form of engagement with the jobs of others’ (Lynch, 2009: 105). Even so, his own explicitly ‘non-applied’ and ‘a-theoretical’ stance makes it difficult to make sense of the multiple ways in which practice research in fact intervenes in the world (through description, analysis, even prescription, and other means). This stance follows naturally from the claim to simply elucidate ethno-methods. From it follows also the rejection that (ethnomethodological) practice studies can or should lead to any kind of political project.

Now our aim is not to prevent Lynch or other ethnomethodologists from conducting their, often fascinating, studies of practical, ordinary action. We, too, have learned much from them. Further, we agree with Lynch when he argues that some social scientists show far too much faith in the ability of theory to change the world (Lynch, 2009; see also Fish, 2004). Yet, in our view, his defensiveness on behalf of (ethnomethodological) practice is both unnecessary and counter-productive. For if practice studies do not improve the

conceptual vocabularies of social science, and also do not pave the way for any kind of practical improvement *or* political change, what *do they* do?

In Lynch's view, 'the social analyst is not a manager aiming to improve the efficiency or effectiveness of the work under her purview; instead, the proximal "payoff" of the analysis, if there is one, is delivered in the academic field's seminars and publication outlets' (Lynch, 2009: 106). But as we have seen, the payoffs *delivered in those seminars and outlets are also narrowed*, since the study of practical action offers no more than therapeutic correctives to the grandiose theoretical, practical or political aspirations of other researchers (2001: 146). In this sense, the proximal payoffs to which Lynch refers are defined negatively and purely reactively (cf. Deleuze, 1983: 111). They centre on limiting practice studies' spheres of action rather than on enhancing their inventive potentials. When politically inclined social scientists have learned the ethnomethodological lesson, Lynch tells us, 'hopes for enlightenment and political emancipation would . . . return to the streets where they belong' (2000: 47).

But why accept this narrow scope and therapeutic function? Why not instead encourage the development of a wider and stronger array of practice approaches, with a more variegated set of aims and ambitions? Instead of policing the boundaries of practice, why not strive to produce new and interesting factishes?

Practices as factishes

Like Lynch, we are also sceptical towards the idea that practice can be rendered in the form of a general explanation of science, technology or, indeed, anything else. Further, we agree with ethnomethodologists that everything in the world can be seen as situated accomplishments and that everything can therefore be studied as practice. However, ethnomethodology tends to focus exclusively on localized ethno-methods, and routinely show scant interest in the imaginations and conceptualizations of the actors whose actions they describe. It also tends to display little curiosity concerning how practices are distributed, how they extend, or how they interact. When Lynch claims that ethnomethodology engages in neither theory nor politics, he both naturalizes and valorizes these limitations. In fact, his claim is a double one: Ethnomethodology *does not want to engage in politics and theory* and it is *also capable of avoiding it*.

While the former is surely correct (at least in Lynch's version of ethnomethodology), the latter is clearly wrong. Indeed, it is obvious that ethnomethodologists relentlessly engage in sophisticated forms of conceptualization (concept-notions like reflexive action and situated accomplishments spring to mind). At the same time, the disavowal of ethnomethodological concepts *as concepts* exhibits a curious asymmetry, and, indeed, purism. After all it is well known that *everyone* uses concepts to come to terms with the

world and its problems. Why should practice researchers be the only ones obliged and able to abstain from conceptualization? 'In practice', this aspiration is indeed impossible. We might add, that the claim to 'simply describe' seems oddly similar to the post-hoc rationalizations that scientists might offer of their own work.

Consequentially for our present argument, the disavowal of conceptualization deprives social scientists of opportunities for learning: both from the concepts and dreams of *our various informants* and from those of other *scholars*. Indeed, one of the central benefits of reconceiving practice as a (set of) factishes is that this creates room for practice studies to roam and experiment with diverse resources for thinking. Practice studies may (and will) indeed take many forms. But practice can never be purified and this means that the social scientist will always have to take responsibility for his or her 'fabrications'.

To illustrate what it can mean to make practice into a factish, we briefly consider François Cooren's (2009) contribution to the study of organizational communicative practice, which elegantly *combines* ethnomethodology and deconstruction, thus redefining both.⁶ Inspired by ethnomethodology's focus on situated accomplishments, Cooren's research contributes to understandings of how practices are reproduced. However, insisting, with Derrida, that all practices leak and bleed into other practices, thus creating contexts where ideas and actions mutually infect one another, his study also offers a novel approach to the analysis of interrelations between practices (which, as we have noted, are routinely left unspecified by those that study 'a local practice').

'Faced with a limitless context', writes Cooren, 'and an infinite number of markers to recognize, interactants (and analysts) must therefore often negotiate what this world is made of' (Cooren, 2009: 52). Attending to detailed negotiations about what practice is, which takes place *both* among his empirical interlocutors and his academic ones (Garfinkel and Derrida), Cooren offers an analysis of how organizational ontologies are accomplished, which is at once empirically sensitive and theoretically inventive. At the same time (or, indeed, for the same reason), he collapses the dualist 'distinction between a world "out there" and a palace of ideas, imagination, fancies, and distortions "in here"' (Latour, 1999: 283). For, at each step, his elicitation of these organizational worlds entails a *mingling* of what is 'found in the world' and what in his 'palace of ideas'.

Previously, we pointed to the 'semantic confusion' to which, sliding unnoticed between empirical and conceptual registers, practice often gives rise. In Cooren's case, too, practice is indeed semantically confusing. Using a Derridean refrain, we might even say that it is *undecideable* where 'empirical practice' stops and 'conceptual practice' begins. But this is not a weakness but rather the main strength of Cooren's study, since it allows him to elicit organizational communicative practice in a very interesting new form. In other words, it is *because* his analysis is explicitly and experimentally situated between the conceptual and the empirical, that it is capable of eliciting 'prac-

tice' as a factish. The result is a risky fabrication that is open to critical re-examination and future transformation. For the same reason it is very far from a magical charm that auto-repels critique. Studies of practical ontology, we argue, thrive by making such factishes.

A concluding reflection: the politics of practice

The possibilities and limitations of practice thinking are entwined with a discussion of its political implications and engagements. In the heated exchanges of the science wars, critics ascribed odious political motives and dangerous implications to the study of scientific practice (cf. Turnbull and Antalfy, 2009). According to critics (eg Gross and Levitt, 1997), there was little doubt that STS studies *had* important consequences, and they were, to put it mildly, not viewed as benign. Not only was STS seen to imply relativism and to undermine scientific authority; the field was also claimed to abnegate responsibility for supporting the sciences against the onslaughts of Christian creationists and others. Although these polemic interpretations were often based on misreading (wilful or otherwise) (Smith, 1997), the *Science Wars* exchanges probably helped to consolidate the practice focus within STS.

Michael Lynch's criticism of the political hope social scientists invest in theory can be read in part as a reaction to the kinds of accusations that faced the STS community during this period. His previously mentioned argument that political action should be located on *the streets where it belongs*, largely replicates Stanley Fish's argument that intellectuals should 'do their jobs', refrain from politicizing and 'save the world on their own time' (Fish, 2008). Recognizing the limits of this view, however, Lynch also notes that 'If a university were to follow through on his [Fish's] words of wisdom, it would have to purge itself of substantial amounts of research and teaching that do not respect a hard-line distinction between what is proper for the academic profession and what is beyond the purview of its practitioners' (Lynch, 2009: 104).

If Lynch is nevertheless in broad agreement with Fish's anti-political argument, it is plausible to interpret this alignment as part of an effort to render ethnomethodological practice studies immune to criticism. If, as Lynch say, such studies have no applied, theoretical or political consequences, there really is not much for critics to bite into (except, as we do here, the lack of bite itself). Even so, STS studies, including ethnomethodological ones, are often read by critics as offering versions of an argument, according to which science is *merely* another way of doing politics (eg Boghossian, 2002).

In the case of ANT's symmetrical anthropology, the central reason for this rather strange misinterpretation is that actor-network analyses focus on the mutual translations between people and things. When Pasteur succeeds in 'enrolling' the microbes in Latour's famous story (1988), new knowledge was made, but new power was made in the same process. Pasteur became influ-

ential due to his ability to control the microbes. But since knowledge, power and nature were generated in the same process, the microbes, in their way, also managed Pasteur. Pasteur and the microbes thus *ended up* engaging in French politics, and this happened irrespective of whether it was Pasteur's intention.

While Pasteur and the microbes no doubt engaged in situated accomplishments, together they also produced a new practical metaphysics (Latour, 1999: 287); one that subsequently extended far and wide. We are here in the realm of an *ontological* politics (Mol, 1999), for the outcome was the transformation of the natural world, no less than of French society.⁷

Yet, even if we trace practical ontologies *and* their politics, surely it does not follow that practices studies *themselves* must necessarily have any political or practical agenda? As we have seen, Lynch was adamantly opposed to such involvement in the early 2000s. In 2009, however, his point 'is not that academics should withdraw from worldly engagement. They are already engaged in worldly affairs – 'business' is not limited to public testimonies and private consultancies that exceed or contradict the performance of academic 'jobs'. Instead, the lesson is that engagements in public organizational affairs can be full of surprises or, worse, subject to unintended reversals' (2009: 113–114, cf. Cole, 2009). We are now in a position to *agree – both* with the 'early Lynch's' insistence that there can be no general normative agenda, for either ethnomethodology, STS, or sociology at large – *and* with the 'later Lynch's' emphasis on the surprises likely to occur when studies of practice loop back into practice, and their practical ontologies begin to entwine.

For the same reason, however, we also emphasize that while studies of practice 'should' have no general political or practice agenda, it is very likely that specific studies will occasionally have political or practical implications. Further, this can happen whether intended or not (it is possible to have political impacts not of one's choosing, as much as it is possible to be practically irrelevant in spite of one's best efforts). Indeed, the practical or political effects of practice studies *are no more and no less contingent* than the actions of our informants. They, as we know too well, routinely change tacks, move into different arenas, or argue for courses of action they had not foreseen, or which they had previously opposed. Once again, we are thus pulled into the realm of equivocal factishes, unstable practical ontologies and unpredictable transformations, rather than clear-cut separations between the empirical and the conceptual, thinking and action, or the descriptive and the political. In these situations, nobody is able to keep actors within the confines of a locally bounded practice.

In his adventures with the microbes, Pasteur moved across science, agriculture, medicine, politics and civil society and, in consequence, the very boundaries between these domains shifted. Though most sociologists and STS researchers will never have any influence on the scale of Pasteur, they might learn from him that it is indeed occasionally *the immoderate indifference to established boundaries* that facilitate some of the most consequential changes

of worlds (whether exhilarating or catastrophic). Just sometimes, the practical ontologies we study can also turn into practices we want to change.

Invariably, this takes us into experimental and risky territory. However, since there is neither any principled reason nor any failsafe method for escaping such risky situations, the relevant question seems to be how to make interesting factishes that may illuminate or, just possibly, better our worlds.

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Notes

- 1 In the context of STS research, a search in the two flagship journals (*Science, Technology and Human Values* and *Social Studies of Science*) shows that in the period 2007–2011, respectively 90 per cent and 88 per cent of the papers refer to ‘practice’. In contrast the numbers were 62 per cent and 13 per cent for the year 1991. We have not gone deeper into an analysis of these numbers, but we view them as indicative of the general trend we describe.
- 2 Foucault himself does not refer to practice, but to concepts such as change, interests and ideology.
- 3 Following Barnes, we may add that this is also the problem with Turner’s solution, since his emphasis on habit focuses attention on routine, if not, mindless, action.
- 4 For example, though Lynch and Bogen (1996) take an interest in deconstruction and undecidability, this is not because they have learned from Jacques Derrida but because, they suggest, deconstruction is practically accomplished by Oliver North and his lawyers in the hearings.
- 5 This critique of *descriptivism* can also be levelled at Latour’s methodological dicta, prominently the insistence that one ‘follows the actors’ (Krarup and Blok, 2011). Yet, the similarity quickly breaks down, because no one, including Latour, would deny that his descriptions are part and parcel of a more general reconceptualization (Gad and Jensen, 2010) of the ‘amodern’ world. See Latour (1996) for an illuminating discussion of the differences between ANT and ethnomethodology.
- 6 In contrast to Lynch and Bogen (1996), whose interest in deconstruction was explicitly limited to ‘finding it’ as a practical resource deployed by actors.
- 7 Contrary to Lynch, this ontological politics certainly cannot be shipped back to the streets where it belongs (which streets precisely? All those French streets named after Pasteur?).

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