

***MEDIA, PRACTICE AND POWER: OR,  
TWO DOGMAS OF PRAGMATISM***

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Let me start this response to Mark Hobart's comments by acknowledging my debt to him. In his chapter, he has greatly enriched the space of argument in which my original piece (Chapter 2) was situated. That piece, written in late 2003, was part polemic, part manifesto.<sup>1</sup> I wrote it fast, because the then recent adoption of practice theory in sociology - social theory and the sociology of consumption - seemed to have urgent implications for sociology of media, my field (as it has turned out, the piece has provoked more interest in media anthropology than in media sociology). So while I was well aware of the deep philosophical currents swirling around the term 'practice', I deliberately stepped across them for my immediate purpose of disrupting current schemas for analyzing 'media' and suggesting some new ones. Of course it is good to enlarge the discussion to include those philosophical issues, as Hobart has done. I want in this brief discussion to take up Hobart's challenge: this will enable me to explore the unstated philosophical hinterland of my original argument. Even better, it will force me to distinguish two rather different ways of introducing 'practice theory' to media research.

The first, 'radical' approach (Hobart's) adopts practice as its key philosophical concept to ground a complete reorientation of our epistemology of social analysis.<sup>2</sup> A second, 'instrumental' approach (my own) is more limited in scope and intent: it develops the analytical implications of certain debates and terms from 'practice theory' to dislodge old habits in the specific field of media analysis. It might sound as if the latter approach is philosophically lazy! Indeed that is what Hobart suggests. But this apparent laziness (and brazen instrumentalism) has the advantage of sidestepping the philosophical 'dogmas', as I shall call them, of the radical pragmatist approach - what, playfully, given Hobart's reference to Quine, I shall call the 'two dogmas of pragmatism'. This links incidentally to a wider divergence: between Laclau's social ontology/epistemology (applied to media research by Hobart) which gives most weight to discourse and interpretation and the rather different ontology/epistemology of critical realism (Roy Bhaskar) which is concerned above all with the material bases of power. I have explored the implications of that divergence for understanding media power elsewhere (Couldry forthcoming): it is this divergence I suspect that underpins the disagreement between Hobart and myself on 'practice'.

At the same time, there is a great deal on which Hobart and I agree: on the need to reorient media research and to internationalise it in a thoroughgoing way, and in our hostility to the survivals of functionalist explanation in media research.<sup>3</sup> It is these points of agreement of course that draw each of us to apply practice theory to media research.

Let me take that agreement as read - more productive here to focus on our disagreements, and so clarify some of the varied choices and priorities that the move to practice in media research opens up.

### *Structure and practice*

As already explained, Hobart's aim and my own are rather different. For Hobart, 'practice is not supplementary to notions such as system, structure, order or individuals but replaces them'; in this sense, at least, it deserves the title 'radical'. All my work, by contrast, has aimed to extend our understanding of what I would still prefer to call the 'structural' role that media do indeed have in social life (Couldry 2003); nor was my borrowing from practice theory an apostasy. Certainly, from his 'radical' perspective, Hobart is right to see my account of practice in media research as incomplete: I am guilty as charged! In response, I will argue that a 'radical' account depends on certain hidden assumptions and biases which compromise the openness of our understanding of practice in subtle ways and therefore are not necessarily radical. I agree with Daniel Taghioff that applying practice theory interlinks with political questions in a broad sense (Taghioff: [check date]), but will argue that in this respect the claim to radicalism of Hobart's approach is less secure than it first appears.

Rather than spend time on Hobart's critique of sources such as Swidler from whom I borrowed in my original piece, let me try to get to the root of our disagreements. What Hobart seems to object to above all in my use of practice theory (Schatzki), and certain social theory that has affinities with it (Swidler), is my primary interest in the question of how practice is organized. So when Hobart quotes me asking whether media might have a privileged role in organizing (let's put to one side Swidler's particular term 'anchoring') other practices, he responds: 'however privileging the media this way begs the question, because it anticipates how media practices relate to others'. Since in the passage Hobart is discussing I was merely asking, not judging, whether media have such an organizational role, Hobart must mean even to ask about such possibilities of ordering is to reject an ontology in which such ordering is excluded in advance. Indeed it is, but it is surely such an ontology that is question-begging; indeed this is the first sign of the a certain philosophical dogmatism that underlies Hobart's radical version of practice theory. Why after all believe in advance that what for convenience we might call a space of practices' (the various practices of a particular collection of individuals) is more likely to be unorganised than organized? The answer paradoxically lies, I shall suggest, in Hobart's at root idealist understanding of how practice might be organised.

Why is it that Hobart says he prefers the distinctive social ontology of a radical practice theory? (I'll leave aside the question of whether Hobart's picture of sociology is adequate. His passing comment on 'yet more surveys and questionnaires' suggests that he thinks that is all sociology has to offer to practice theory, which reduces the partial historical divergence of sociology and anthropology of methodological caricature; but the issues of ontology are, as he suggests, genuinely difficult and I will concentrate on them.) At one point, in discussing the issue of shared interpretation and understanding (clearly one possible feature of organization: whether it is necessary is something to which I

return), Hobart says: ‘how shared understandings are depends on how far away you are from the situatedness of daily argument’. So the analyst’s perception of shared understandings is, Hobart would have us believe, merely an illusion of distance, because there will always be details (graspable only close up) that override in significance the similarities graspable from afar. But this is to deny – in advance, by ontological fiat – the whole domain of convention and to give automatic priority instead to local interpretation. Convention, and more generally, habit are the broad terms we use to characterize a state of affairs where certain actions, understandings, and expressions get repeated without the causal input of interpretation, which is not to say that conventions or habits are not themselves open to further interpretation (of course they are).

A little later, Hobart raises the question of the analytic practice of academic – quite fairly – but his view of the consequences of academics seriously analyzing their own analytic practice in addition to just analyzing others’ practices is an odd one: ‘then we cannot speak unproblematically of hierarchies or authoritative representations – whose, where and when? . . . this in turn requires us to consider who does to *as* what, to whom, on what occasion, for what purpose.’ Hobart writes as if to register the possibility of a hierarchization in the practice of others is already to impose an interpretation on them, and worse, an interpretation which must overrides their own interpretations and representations. I would not for a moment deny that ‘representation *as*’ is an important domain of practice. But (outside a certain idealism) representation is not the only, or even the most important, mode of hierarchization. When in societies where class or gender are important organizational principles people enter buildings, sit or stand in locations, or hold their bodies in ways that associate them with members of the same class/gender and disassociate them from those of another class or gender, they may not (very often they are not) making representations about hierarchy or their place within it. But hierarchy will be reproduced if the different positions/ locations associated with those genders or classes are in turn associated with a further material condition, the regular and unequal distribution of resources, for example in relation to the resources of media production.

Suppose an academic came to the view that those conditions were met, and that a hierarchy was being reproduced here. The question of how that academic came to make that judgement would only disable such a judgment automatically if (a) s/he refused to reflect seriously on the possible implications of her/his position (in space and social organization) on her/his ability to observe such regularities or if (b) the inequality in question was such that it is negated always and automatically by a contrary interpretation by those allegedly subjected to hierarchy (for example a hierarchy of access to pleasure or a sense of happiness). Leaving aside (a) – clearly Hobart is right that judgements about hierarchy that make no such reflection are inadequate<sup>4</sup> - the real issue is (b): but many inequalities do not require for their existence agents’ positive sense of being unequally treated.

Hobart tends to rely on maxims about what we know from anthropology to block off further inquiry as to whether a particular space of practices might in particular ways be hierarchically ordered. For example he writes: ‘hierarchy and authoritative representations are the familiar language of approaches which privilege the knower above

the known', but this ignores the possibility of successfully identifying hierarchy among others under the conditions just discussed. Or (he writes) 'practices tend not to line up neatly': but (to echo Hobart) who says so? Is this always and everywhere? How any one field, such as anthropology, can establish such general conclusions is puzzling, unless it has the sanction of a certain philosophical authority that trumps any particular claims about observed hierarchy or organization.

### *How are practices organized?*

Perhaps however (leaving aside some comments about 'system' in general) Hobart's real concerns are narrowly focused on the question of how one practice can be established to be in a hierarchical relationship to another practice. It is here that Hobart questions the compatibility (with a radical theory of practice) of conventional notions of how practices are organized in relation to each other. The problem is interpretation. For if understanding the organization of a space of practices depends on (1) accurately translating the practices of others into the language of the analyst's practice and as well (2) establishing the intertranslatability of the languages of the two or more practices being analysed, then according to Hobart the whole enterprise is doomed. Sometimes, it is true, Hobart draws a more limited conclusion that 'a single coherent account of practice [that is, practice in general, NC] is doomed' – and quite plausibly, but this was never something I claimed to offer. When however Hobart uses philosophical arguments to cast doubt on the very possibility of organization within a space of practices, the wheels of his argument (to borrow his own metaphor) start to fall off!

Let me mention a few problems. First, Hobart suggests that the understanding of other people's practices that is required for claims of organization or ordering involves assuming 'a correspondence theory of reference, a universal hermeneutics and direct access to other minds'. But this is rather misleading. Wittgenstein's late philosophy (from which Schatzki developed his notion of practice) had already deconstructed, for many definitively, the pseudo-problem of 'other minds', through its account of the intersubjective basis of language and language games. Knowing 'other minds' according to Wittgenstein is not the impossible attempt to track the unreachable 'inner' experience of a separate consciousness, but instead the intersubjectively anchored and validated practice in language of registering what others say, and continually comparing what they say with what they do and subsequently say. Nor is any 'universal hermeneutics' needed to form adequate understandings of what others think and do in particular circumstances provided that there are some language games that analyst and analysand share in common (Hobart is right of course that sometimes that condition is not met, and that we must be careful here, but we do not require a universal hermeneutics to do so. Paradoxically, Hobart here is using the stick of a pre-Wittgensteinian view of the old 'problems' of language and other minds to beat applications of the (Wittgensteinian) notion of practice which happen not to fit with his own social ontology: this is to do philosophical history back to front.

Similarly on translation. Hobart mentions Quine's (1960) problem of 'radical translation' and the incommensurability between all but the most closely related natural languages.

But Donald Davidson's work has subsequently brought into question how radical such problems really are. Davidson (1984) argues in his essay 'On the Very Idea of a Conceptual Scheme' that the frequent claim of conceptual relativism – and the idea that many cultures are incommensurable – assumes in the background the misleading notion that each supposedly non-translatable language or culture has its separate and distinctive 'conceptual scheme', an implicit mentalism that Davidson wants to banish in impeccable Wittgensteinian style. The very idea of such a 'conceptual scheme', Davidson argues, itself would make no sense unless some translation between that scheme's associated language and other languages were possible, contradicting the claim of cultural relativism. From that (no doubt limited) translation between two very different languages, a broader area of common understanding can be built through the open-ended process of listening, explanation and comparing talk with action. Indeed, the apparently 'radical' position of scepticism about translatability – and the whole supposed problem of 'radical untranslatability' - is an illusion. It ignores the continual necessity to deal with cases of partial untranslatability which are resolved, as they must be, by what Davidson calls the principle of charity in interpretation. According to Davidson, 'charity is forced on us; whether we like it or not, if we want to understand others, we must count them right in most matters' (1984: 197). Indeed the perception of difference is not opposed to, but dependent on, the better grasping of what understandings are shared: 'we improve the clarity and bite of declarations of difference, whether of scheme or opinion, by enlarging the basis of shared (translatable) language or of shared opinion' (1984: 197). Davidson therefore bypasses the supposedly fundamental philosophical problem of analyzing others' practice that Hobart claims undermines my analysis.

The key term indeed here may be action. For the fundamental problem, I suspect, with Hobart's attempt to rule out any 'non-radical' notion of practice is his reliance, paradoxically, on a notion of understanding as the transmission of ideas between hermetically sealed individuals, rather than an inherently intersubjective practice that, so to speak, is 'out in the open', in the space where talk and action can usually be compared (and where talk itself is just another form of action). Of course the practice of interpreting others' practices remains difficult; it is unlikely to be easy. But it is not doomed in advance, either to failure or to a disabling charge of interpretative 'imperialism', any more than all mutual understanding and translation are doomed in advance by the pseudo-problems of 'other minds' or 'radical incommensurability'. It only muddles things further if, as Hobart does, we give such old philosophical objections a political spin. It is almost as if Hobart wants to offload all the problems of anthropology's 20<sup>th</sup> century history at the door of sociology. Hobart asks: 'what grounds do we have for assuming the understandings of the subjects of study are commensurate with the researcher's, when anthropological research suggests this is usually not the case'. But what without something like an anthropological 'imperialism' is there to convince a sociologist here (or a historian or economist for that matter) that their careful attempts to develop understandings of others (respecting, but often going beyond those others' self-reports) are doomed in advance by the 'lessons' of anthropology?

There there is Hobart's claim that any sociological adaptation of practice theory that seeks to preserve the empirical possibility (no more) that a space of practices is, at least

partly, hierarchically organized is tantamount to ‘hierarch[izing] and normal[izing]’ [its] subjects of study’. But what if, after talking with, listening to and observing particular ‘subjects’, an analyst concludes that, for example:

- when they refer to one type of action, they generally do give some priority to interpretations of it by a particular type of agent (say, a media source), or
- when they describe another type of action, they do generally relate it to statements from a media source, or
- that actors particularly associated with and shown in media are more frequently treated as reference-points in a particular domain than other types of actors?

Are we to rule out of court any such claim of pattern, order, even hierarchy? My point is not – and here Hobart’s critique is salutary – that establishing such claims is easy. Rather such paths of analysis should not be ruled out of court in advance by rhetorical gestures towards some general politics of representation.

### ***Two Dogmas of Pragmatism and Beyond***

I want to go a little further and suggest that underlying Hobart’s various arguments (negative and positive) for a ‘radical’ practice theory are two unarticulated (and unjustified) assumptions within Hobart’s own pragmatism. (Pragmatism is the term Hobart uses most often to characterize the philosophical positions he builds upon, so I repeat it here, without myself claiming to offer any commentary on the broader history of pragmatism.)

The first dogma is to see the social world as primarily and constitutively made up of *interpretations* (with no or little weight given to habit, the embodiment of prior interpretation in gesture or hexis, and so on). Of course, interpretations matter hugely, but their status is rarely divorced from action and, while actions themselves encode and enact, we are rarely left only with people’s interpretations of their world. Indeed interpretations are themselves, as Hobart would no doubt agree, themselves actions, whose regularity and patterning may have a significance that is not based on recourse to yet further interpretation on the part of the subject. That at least is the basis of discourse analysis which Hobart might be happy to jettison as a methodology, but which I am not.

The second dogma is to assume that the space of practices (if I may use that term, for convenience, to cover the infinite set of practice-spaces that might be analysed) is *naturally flat, that is, without organization, order or hierarchy*. A better starting-point, I suggest, is to assume neither that it is flat nor that it is ordered, but simply to allow that sometimes one practice is treated by those directly involved as providing the principal context for interpreting the significance of another practice. If so there, for certain purposes, there is order and perhaps even hierarchy. To deny this on the ground that, in the abstract, ‘this is the way things are’ prejudices the space of practices no less illegitimately than the prejudgements of which Hobart accuses other notions of practice.

Free of those two dogmas, we can apply the specific insights, and the interpretative adventurousness, of practice theory to register better the full variety of media-related practices around us. And here I am happy to adopt Hobart’s suggestion of the term

'media-related' instead of my original 'media-oriented', since it acknowledges (cf Couldry 2006: 13-15) the dangers of assuming everything in contemporary societies is oriented towards media – a great deal is not! But this insight need not come with an aversion to registering media's possible contributions to regularities of power - an unnecessarily high price to pay for the less media-centric language for media research that both Hobart and I want. The practices of mediated political spectacle, the interlocking of state and media practices, are in many countries too clear and insistent for us to ignore in the hope of achieving a 'radical' practice theory whose philosophical radicalism is itself, as I argued, open to challenge. Indeed there is a politics – a problematic politics – in blunting our tools for analyzing such tendencies towards order for the sake of a supposed philosophical radicalism.

If however we are freed from the unnecessary ontological limitations of the 'radical version' of practice theory, we can reflect openly on the responsibilities of interpreting media within a genuinely internationalized framework inspired not least by Hobart's own work on Bali.<sup>5</sup> All our concepts (including the term 'media rituals' which I have myself developed) need of course to be rigorously reviewed to see what transformations they must undergo to be useful for countries beyond those for which they were originally developed. This requires intense examination of what might, and might not, be comparable between how media-related practices are embedded in countries with diverse religious, political, economic and cultural norms. In all this, Hobart's call for caution and self-reflexivity before applying concepts beyond their horizon of usefulness is immensely helpful, and reinforces the interpretative decentring that follows from practice theory however applied. But this transformation of our analytic and conceptual languages surely requires us to be as open as possible to the varied ways in which practices are more or less organized and articulated to each other under various conditions. A modestly instrumental use of practice theory may guide us better, I suggest, here than a 'radical' pragmatism that relentlessly reads the web of practice in one particular, and paradoxically dogmatic, way.

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<sup>1</sup> That, for example, is why it uses the term 'paradigm' rhetorically, without much philosophical discussion.

<sup>2</sup> I use the word 'social' here and in what follows to encompass any attempts to understand the domain of human interactions: I am not making specific points about the concept of 'society' or 'the social', for example, which there is no room to debate here.

<sup>3</sup> See Couldry (2005), (2006: 178-183).

<sup>4</sup> See eg Bourdieu's 'reflexive sociology' (1998).

<sup>5</sup> See Mcmillin (2007: 169) discussing Hobart (2004).