Elephants in the rooms where publics encounter "science"?: A response to Darrin Durant, "Accounting for expertise: Wynne and the autonomy of the lay public"

Brian Wynne

Public Understanding of Science 2008; 17; 21
DOI: 10.1177/0963662507085162

Public Understand. Sci. **17** (2008) 21–33

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1. Introduction

Flattered vanities aside, it is slightly alarming to be invited to respond to a hitherto unknown analysis of my work over many years on publics and science. While "the public" and "its" mysterious doings and imaginations relating to "science" have become the intense focus of interest in recent times, I feel that we have not yet approached the central questions. In this sense I feel almost as if I have involuntarily distracted Darrin Durant into paying my work too much attention. Here, two programmatic opening points are worth making.

Firstly, our field should draw more fully on wider and more historical work in political philosophy, not only but including Dewey's ([1927] 1991) response to Lippman's ([1927] 2002) dismissal of the public as merely "a phantom." However, this would be much more than just a (constipated) "political philosophy of risk" which is how some scholars have imagined it (e.g. Kusch, 2007).[1]

Connected with this—but also remedying a lacuna shared by that self-proclaimed political philosophy, as well as by too much of the wider social scientific work on "public understanding of (or engagement with) science"—the supposed object of such public meanings and responses, "science" (risk) itself, needs to be more thoroughly problematized, differentiated, and taken apart.[2] The "scientific" knowledge-cultures which pervade, shape and perhaps cloud these relationships have been insufficiently examined, especially in their subtle and several forms of inter-construction with public worlds of policy, controversy, economics, politics and "non-politics." We cannot properly conduct relevant research on publics in relation to science, unless we also critically examine the elephant in the room—what is the "science" which we are supposing that people experience and sense in each of these situations?

The critical tone of these opening observations is not a comment on Durant, but more a necessary contextual prologue. I attempt to address these broader points, inevitably only indicatively here, in responding to his more specific observations on my work.

Since this body of work has arisen from a variety of unconnected arenas, issues and cases covering a diverse range of immediate aims and audiences, Durant has taken on a complex task. What coherence or consistency, let alone worthwhile takeaway insights, can we expect or discern in such a composition? He has done an admirable job of digesting at least thirteen of my publications on public interactions with science over that period, indicating considerable reserves of stamina and patience, quite apart from salient analytical capacity and constructive critical spirit. However, some, though not all, of his reading of my work in this

 ISSN 0963-6625 DOI: 10.1177/0963662507085162

domain shares some important misconceptions with other widely cited critical discussions of Wynne (e.g. Collins and Evans, 2002, 2003; Kerr, 2004; Kusch, 2007). Thus while paying due respect to Durant's individual thoughts, I also make some more general points in part-response to those other readings of my critique of the so-called "public deficit model" explanations of public dissent.[3]

2. What is at stake—knowledge-expertise; or concerns and meanings?

The work which Durant discussed reflects my developing understandings of the understandings—by social scientists, natural scientists and other policy actors—of the "publics" who feature in the fields of public understanding of science, science and policy, or science and society. Of course, this has been done in fields of public interactions with scientific expertise; but *only one* aspect of this has meant asking what forms of "expertise" can publics of various kinds be deemed to have, and for which they have rights of recognition? Like others (e.g. Collins and Evans, 2002, 2003; Kusch, 2007), Durant seems to take for granted that this is the essential question. I do not share this premise. My own work has started from a different place from what we might call the mainstream in this domain, in always assuming *relationality* as the ontological ground of being and knowing.[4] One implication of this is already stated above—we cannot approach the challenge of "understanding publics" in relation to science, without at the same time addressing the ambiguous question of "science" (and its normative constructions of publics) too, as an empirical, theoretical, and normative matter. To me, this remains the main issue. As I explain below, Durant and these others seem to have invisibly squeezed my wider issues into their own unquestioned premises, when it is just those premises which I have been trying to challenge.

For example, as historians such as Shapin and Schaffer (1985) have shown, science has in its epistemic-cultural practices both reflected and performed particular imagined publics, often tacitly, since its earliest natural and experimental philosophy days. I have therefore always assumed statements made about public knowledge-abilities, public concerns, and independent collective citizen hermeneutic capacities (making sense, making meanings), to be made in relation to dominant presumptive institutional scientific assertions. People are not responding to science as *we* understand it. They are working with *their own* (collective) meanings, not ours. This is true for example, of my accounts of the trust question logically underlying risk and so-called attitudes to risk; that so-called public *risk* concerns are always also public concerns about their social relations of dependency—and rationally so. Thus the predominant assumption that public issues *involving* risks are *risk issues*, is a serious mistake with far-reaching ramifications; and one perpetrated not only by scientific and policy institutional powers, but also by too many social scientists.

This ontologically founded emphasis on the need to recognize hermeneutical *differences* (of meanings, and concerns; thus of framing of "the issue(s)") is also reflected in what I have said about public concerns and knowledge-capacities in relation to scientific interventions in public domains. Thus most of the complaints, which include Durant, albeit in a particular way, about my alleged romanticization of public knowledge-abilities seem to be founded on misunderstanding of this fundamentally different starting point I have taken. Here the main point of contention I have with such critics as Collins and Evans (2002, 2003), Kusch (2007), or partial critics such as Kerr (2004; and maybe Durant?), is not as they seem to assume, about the extents of "lay expertise." It is more fundamentally about what they assume to be the *basis* of public divergences from scientific expert views, when these occur. These are not divergences of propositional knowledge-claim, or anyway, not that alone; but they are divergences of an

ontological kind—about meanings, concerns, relationships and forms of life. Whereas these colleagues wish to reduce the public issues to knowledge and thus "expertise" or its lack, I want to insist upon dimensions of contested meanings, and contested concerns (and thus, of what is deemed relevant), and the institutional-scientific denial of legitimate difference here.[5]

I have therefore said that those public concerns *are not predominantly concerns about being illegitimately disqualified and excluded from expert debate and decision, on a propositional knowledge-question such as "what are the risks?,"*[6] which Collins and Evans presume like most policy and scientific institutions themselves, to be the public policy issue. They are more about the presumptive hegemonic imposition of what the salient concerns thus salient knowledge-questions, thus salient knowledges, are recognized to be in the first place, as the public frame of meaning of the issue at stake. This normally involves a reduction of the complex multidimensional questions involved in assessing technological-social innovations, to ones of scientific risk.[7] Whereas Collins and Evans seem to wish to separate these framing and public meanings issues from the knowledge issues, as "political" issues for other more democratic institutions to deal with, this kind of formal schematizing must address the observable complexities, such as that the "scientific" and the "policy" dimensions are never so clear-cut, and instead are seen to be mutually established and constituted in practice. Moreover, crucially, they are then presented in public as if purely "science," so inevitably the "science" in play as public authority here then invariably carries unaccountable normative commitments. That is, it is an "unpolitical" politics.[8] This "science" is imbued with, and shaped by, imaginations of publics as vacuous, and threatening, which are unstated and thus also surreptitiously normative.

For example, to call public issues about new technologies which involve risk but which also involve many other issues, "risk issues," is to claim: i) that public concerns and meanings are exclusively about risk; and ii) that public dissent from expert pronouncements thereupon must therefore be due to public rejection or ignorance of the risk science. Thus as often as this public deficit explanation of public dissent on technically intensive issues involving risk is abandoned, it has been reinvented in revised forms because this imposed premise as to public meaning allows no other logical conclusion for explaining public divergence (Wynne, 2006a, 2007). So, the deficit model is dead—Long live the deficit model!

3. Publics (and scientists) as reflexive agents, or dupes? Why "either-or"?

Given my own situational and relational ontological starting-points, I am quite relieved that Durant did not find, or was too polite to note, more inconsistencies and self-contradictions in a body of work which has developed through various contexts and cases over a period of about twenty-five years. An important one which he does, however, identify, is the tension between "public" viewed: on one hand as a collective of relentlessly reflexive individual subjects; and on the other as vacuous cultural dupes, enacting in behavioristic fashion the dictates of whatever surrounding social-cultural norms prevail. My apparent oscillation between these "under-socialized" and "over-socialized" models of public actors is indeed salient, as Durant notes.

While one elephant—the public—is clearly in the room, the other is left unquestioned by this elephantine unspeakability. As Durant indicates, but only obliquely (and thus does not pursue), the same questions about reflexivity or "dupedom" pertain to science as an institution, and scientists or experts as individual actors. Thus the very idea of "[expert-]institutional reflexivity" which I called for in Wynne (1993), while recognizing it as strictly speaking a contradiction-in-terms, also comes into focus as a question mark. Durant says, plaintively it

seems, that I successfully deconstructed an established epistemic divide between lay publics and experts, only to replace this with an essential ontological difference (of a particular kind). Although I have indeed always believed such conflicts to be primarily ontological—about forms of life and relations—beneath their represented epistemic differences, my ontological difference is not at all as Durant reads it.

For a start, of course there are epistemic differences—of epistemic power, that is—between expert scientists and non-scientists,[9] on technical questions salient to that scientific training. But a second less obvious epistemic difference is a function of more basic ontological (and hermeneutic) difference too, and here unlike in the first case the epistemic issues are not "closed" and determined by an established frame of whichever expert culture has been given authority. They are instead (or rather should be) open—about what *are* the salient concerns, thus questions, and thus knowledges? And what imagined social purposes and priorities thus help define the going criteria for valid knowledge? For example, long-term environmental sustainability if given priority, might demand wholistic and comprehensive epistemic principles, at the inevitable expense of precision and (thus) exclusion and (artificial) control. In most public conflicts involving science, these are the substantive epistemic—*and* tacitly ontological (*and* hermeneutic)—differences, about *what questions knowledge should be addressing, and thus, what (combinations of) knowledge should be in play*.[10]

Thus ambiguously, knowledge might be deemed valid or non-valid according either to its propositional-evidentiary status, or to its deemed salience to the recognized issue (or both). In public issues involving science, these are typically seamlessly conflated, or (as with Collins and Evans (2002, 2003) too, the latter reduced to the former), so that real ontological-normative difference is thus silently deleted in favor of dominant commitments. Science is implicated in this politics, because this silent politics occurs in science's acquiescent name.

It ought to be clear by now that I do not understand why Durant makes a song-and-dance about my "erect[ing] an ontological" difference between experts and publics—except that his idea of this ontological difference is totally different from mine. His version centralizes what he takes to be my essentialist "ontological" claim, that experts are intrinsically unreflexive, while publics are intrinsically and relentlessly reflexive (Durant, 2008: 10). As he also noted, but complains (Durant, 2008: 10) that I "buried" it in a footnote, I suggested the inverse law of reflexivity, that it is inversely proportional to power. In the situations where I have analyzed public–science interactions, in all of which cases science was being enacted as attempted but contested public authority, over far more than scientific propositions alone, the relative extent of self-reflexivity was as I described it—much greater for the powerless publics on the receiving end, than it was for the scientists embedded as they were as agents in the institutional nexus of policy, science advice, political economy, and power. But this does not mean this difference was a reflection of essential qualities of their subjects. Just to clarify, I would continue to assert this difference, but not remotely as the claim which Durant takes it to be, a claim of *intrinsic ontological* difference of reflexivity between scientists and publics. Instead I cleave to just what I stated it to be in the "buried" footnote: actors' reflexivity as a function of their situational power and related social-institutional conditions. I am not really interested in categorizing the putative knowledge-differences, or "ontological" reflexivity-differences, between lay and expert actors. The differences are contingent; but this does not make them easily revised, or non-substantial; not at all. My situational analytical perspective can also be upheld with respect to the vexed issue of the "self-reflexivity" of science. Here like too many others, Durant fails to distinguish between "science" as research scientific knowledge-culture, and "science" as aspirant public authority knowledge.[11]

It is worth also noting that Durant's otherwise excellent discussion and connection of the reflexivity debate in science studies between for example Bloor and Lynch (and more

recently, between Bloor and Latour), with mine in the public encounters with science domain, nevertheless falls foul of just that lack of problematization I opened with, about what we mean by "science" in such domains. The definitive science studies exchanges on this have always been focused on science as research and specialist knowledge-production activities, whereas mine have always been about a very different "science," namely that being deployed as attempted public authority. There is no need for any falsely essentialist claims nor readings here, and I apologize if my footnote "burial" of this proposed reflexivity relationship allowed that to occur. I confess I thought this conditional statement was clear enough.

4. Social relations and identity as explanatory factors: contingent differences

Durant takes issue with how I appear to oscillate between the use of "social relations" on one hand as an explanatory factor for the formation of lay public beliefs in relation to science, and "social identity" on the other. As with the reflexivity issue, he sees my distinction here as an essentialist one, when I don't recognize this at all. I have been quite explicit in saying that for me, social identity has to be understood as a function of social relations (even if sometimes for some people, these relations may be so stable that one can identify a relatively unambiguous and stable "social identity," multivalent and always emergent as one would also like to recognize). The case of the Cumbrian sheep-farmer from near the Sellafield nuclear reprocessing plant after Chernobyl's radioactive fallout, is illustrative here. This fallout on Cumbrian fells and sheep from a distant source was blamed by the UK authorities for causing the scientifically managed restrictions which badly affected sheep-farming; but several factors led to widespread beliefs about Sellafield's previously hidden *local* responsibility for the contamination. This farmer's brother, like other neighbors in the valley, worked at the reprocessing plant, as well as helping him on the farm. Contrary to familiar romantic representations of hill sheep-farming, social relations and identities were multiple and heterogeneous. As this farmer explicitly and tentatively reflected to me in interview, to believe that Sellafield was at least partly to blame—as many farmers did—was potentially to undermine local commitment to the plant and its central role in the local economy and society. This would threaten (some of) his own valued social relations and identity. Yet his farming contacts from further away—also centrally part of his social relations and identity—believed in this local cause (and said so to me) even if they were too careful to say so more openly and directly in local circles where it mattered. He was experiencing a problem of contradictory identities and beliefs, rooted in his simultaneous practice of incompatible social relations, in different networks of significant others; and he was articulately self-reflexive in his ambivalence about this. So also were those others who believed for good reasons in Sellafield's part-responsibility, but did not want to disrupt local relations by openly disputing the official scientific and policy line of Sellafield's total innocence. These relatively powerless actors could or would not do what powerful institutional science and policy is often able to do with such contradictions, which is to externalize them on to others through forms of routine denial. Nowhere here can I recognize a clear distinction between "social identity" and "social relations." Whichever might be emphasized is I believe a contingent matter.[12]

5. Reimagining the ends of PUS research

Likewise the "reflexivity-distribution" issue aired above is contingent. Thus for me, the key element of the Lynch–Bloor exchanges over how we should understand the knowledge actor-subject—reflexive, or dupe?—is Bloor's point that, as Durant puts it (2008: 12):

> Some kind of judgmental dope is required: intentional social facts (such as acts of self-reference) rest on non-intentional dispositions (i.e.: habits, custom, biological nature) (Bloor, 2004: 596–7). We can be "blindly conscientious" because "automaticity" is always embedded within socialization processes.

This is the whole point about institutions, as Bloor has also said. They and at least some "dupedom" are essential as collective forms of social economy which make social life viable. We cannot go back to first principles all the time. But in any given situation these are not essentialist properties either way, reflexive or dupe. The reflexive subjects of Lynch's preference are not wiped out by this institutional habit of inducing "dupedom," or in Bloor's terms automaticity. The extents and distributions of either quality are functions of social, cultural and institutional conditions. Thus not all institutional behavior—and we can include science here—is that of dupes. As I said (Wynne, 1993), reflexivity is always in tension with this essential social character of routinization, and no "degree of reflexivity" should ever be essentialized, even if it can be approximately observed in given situations to be greater or lesser.

Durant criticizes (p. 11) my silence on this reflexivity debate as I had referred to it in 1996, in respect of my critical exchanges with Collins and Evans in 2003, and suggests "this is an instructive silence, as this debate prefigured an implicit feature of the 'Third Wave' [Collins and Evans vs Wynne, et al.] debate: models of the actor." I confess that the silence was more likely simply the pernicious work of my defective memory's failure to engage, and I think Durant has done a fine job of identifying these connections. But the point is that, just as defining what is "expert" about "lay" was never my prime concern, so too "models of the actor" were never for me the central point of the exchange with Collins and Evans, nor I think of any other work I have done, so I did not recognize the connection with the Lynch–Bloor exchange as salient. Durant deserves credit for making those insightful connections, which rightly or wrongly just were not my interest. My issue with Collins–Evans was consistent with what it has most often been for me, but usually with those institutions themselves and not with sociological friends. This is about their unwillingness to acknowledge the salience of the inability of dominant institutions operating in the name of "science," including scientific bodies, to acknowledge that the contested policy issues *involving* science, are not therefore as they assume, *scientific issues* like "risk" alone. They are *public* issues, which means that identifying and addressing different public concerns and meanings should be a responsibility of the institutions involved. These are not simply "political" thus allocable to other domains and institutional agents, as Collins would tidy them away from the scene. This categorical demarcation cannot be justified since many such concerns are *about* science, for example, about: how it has been unaccountably established as a key agent of a political economy based on non-factual scientific promises; how it is set up to exaggerate what it can control and predict; and probably also about the authoritarian dismissal, by (politics operating with implicit scientific acceptance in the name of) "science," of those different, relational public concerns and meanings as non-existent or vacuous.[13]

6. Romanticizing publics? Or mobilizing politics and difference

The problem of the insistent dictatorial imposition of this scientistic "unpolitical" political frame upon public life and publics is utterly obscured by that academic scholarship which insistently focuses attention to the side issues of categorizing expertise, and asking whether publics or scientists know better. At least by default, this absurd scholarly preoccupation reinforces the scientistic diktat of "unpolitics" and imposed public meanings. This imposition as

a form of politics, and the role of science in this, has always been my central concern. The accusations, which I reject, that I have constructed a romantic account of the lay expert who knows better than scientists, also fall into this trap, because they reflect and reinforce the dominant de facto normative stance, that public meaning in those issues involving science is not a civic issue, but is properly given by science.

Take for example Kusch's (2007) misrepresentation and misuse of my clearly qualified statement to the effect that *every democratic citizen* is legitimately an actor in contestation with expert institutions. He represents this as Wynne's assertion of the qualification of every such citizen to be involved in expert deliberation over propositional issues of policy such as what are the environmental and health risks from GM (genetically modified) crops, or the likelihood of a nuclear power plant explosion? Yet I was clearly referring to the qualification of every citizen to be involved in the negotiation of (dominant) public meaning, of what are the specific issues to be addressed, when "a public issue" emerges? This misrepresentation reflects his presumptive imposition of the same old scientistic frame of meaning on such issues, that it is experts' meanings—that is, scientific questions only—which then define the public issues and concerns to be addressed. Yet this statement by Wynne never involved such a claim. I was making explicit reference (Wynne, 2003) for example in response to Collins and Evans (2002), to my familiar point that public concerns and meanings legitimately differ from expert ones (and amongst themselves very often, too), and that *every such citizen* is in principle a legitimate participant in what should be the deliberative negotiation of such public meanings.

This, the framing responsibility, is not a specialist issue. Yet, blind to this open public meanings issue, Kusch instead takes this to be an assertion by Wynne that every democratic citizen has at least the same (or greater) knowledge as the expert! Ergo, Wynne is a romanticizing populist! Kusch has just presumptively imposed his own object-reference to my statement about public qualification, deleting my own utterly different one in so doing. Then he has judged my statement against his own arbitrarily imposed yardstick! Kerr (2004) performs a similar albeit more temperate and more surprising distortion in arriving at a similar critique of my supposed romanticizing of public knowledge-abilities. Hers is all the more surprising since she fully recognizes the point about the concerns of ordinary citizens (in relation to genomics innovations in her own work) which scientific and policy institutions too often seamlessly ignore. Thus she also implicitly recognizes the ontological and hermeneutical difference issue which I have stressed.

These misreadings presuppose the very thing I was explicitly challenging, namely the assertion that the only public issue at stake is the propositional one to which the salient expertise can be addressed. Once one sees that the more fundamental issue is how multiple different public concerns and meanings in addition to scientific risk questions alone, are elements of legitimate concern which the recognized public issue should include, or at least recognize, negotiate with, and only exclude with accountable reasons, then of course every democratic citizen is in principle qualified to be a legitimate participant in such collective negotiations—which is what I asserted, and will continue to assert. The point is that the qualifications and the negotiations are over what is recognized as collective public meaning and thus what questions and knowledges are salient, not just about any propositional question itself.

7. Conclusions

Durant's critique of my alleged romanticizing of the public refers to my supposed assertion of its greater *essential* reflexivity than that of scientists. I hope I have explained why I do not accept this. If so then the romanticization complaint evaporates, at least in his case. In the case

of supposed knowledge itself, contrary to the claims of such as Kusch (2007) and even Kerr (2004: 138–42), I have never expressed any idealized romantic belief that "publics know better than scientists." This false account of my position only reinforces the institutional-scientific normative reduction of the multiplicity of ontological-epistemic collectives in play in society at large—what Irwin and Michael (2003) call "ethno-epistemic assemblages," after Rabinow (1996)—to effectively a hegemonistic scientistic one. Civic collective capability, multiplexity and difference as an independent agency has been deleted in this process, and subordinated to the tacit agency of prevailing political-economic knowledge-power. My point has not been about who has better knowledge, but instead has been at a different level, about how in public issues *multiple* knowledges, reflecting their *different* sets of priority concerns, are usually salient and need to be respected, accountably validated as far as possible, and if necessary (but unity or consensus should not be presumed) negotiated together as such.

Thus my assertions were never unqualified and decontextualized; that "publics know better than you [scientists and related experts]." Instead they were that "publics know better than you *think they do*" (that is, relationality, and otherness); and, that "they know *some salient things* better than you" (that is, ontological difference). It was also, as explained before, about in principle universal civic qualification to be involved in collective democratic negotiation of such public meanings, which should not be confused as Durant and others have done, with any supposed but spurious claim of universal civic qualification to negotiate propositional specialist knowledges themselves.[14] It is difficult to understand how serious scholars could read my various interpreted case studies and their emphatic, explicitly *relational* analysis as if they were unconditional assertions of idealized public knowledge-superiority over experts, in one dimension.

The only way I can understand this persistent misrepresentation, in which I do not include Durant, is by inferring that these authors have not understood, or have decided to reject, this absolutely fundamental relational, ontologically (and epistemically) weighted point. They have instead reproduced the deeply entrenched dominant institutional cultural ideology individualized, reductionist, difference denying, effectively anti cosmopolitan, and obsessed with presenting an image of control, however deeply incredible this becomes in practice. This implicit assertion of dominant ontological commitments also exerts an unstated politics in the name of 'science', namely that only instrumental scientistic definitions of public meaning are to be recognised as legitimate public meanings and concerns, and that other, different legitimate social articulations of collective meanings and concerns do not exist. In my view this is a further example, extending the public deficit model rationalisations of public dissent, of what Laclau (2005) was referring to when he talked about "The 'people' and the discursive production of emptiness". In these historical processes, 'the public' is a construct, imagined and indirectly performed by science and policy. These performances project a substantive normative form of 'public' which reflects those dominant institutional concerns and insecurities (including insecurities about having to address genuine ontological human difference, epistemic difference in the form of ignorance and uncertainty, and thus acknowledge lack of control). These qualities of the dominant institutional science policy commerce culture which systematically deny legitimate relational others, are what the academic field of 'public understanding of science' should be helping to illuminate, as the neglected prior factors which shape public reactions to what is called science.

Therefore this deletion of the relational even as a question, is itself not merely an epistemic, but an ontological-normative commitment. If we do not recognize as a matter of principle, the autonomous ability of citizens collectively to construct independent public meanings, which of course requires recognition also of their independently framed intellectual abilities (is there any form of competent practice that is not also in some respect epistemic?), then democratic citizenship and civil society have effectively been deleted.[15] It is a contradiction-in-terms

to talk of democracy with respect to science, but to subsume citizenship to collective compliance with public meanings which are externally imposed, in a dictatorial manner, in the name of science and "risk." Thus the implicit condition for citizens' recognition by science-informed policy institutions, is that they comply with the reductionist issue-framings and meanings imposed by those policy institutions and their experts. This would mean for example, accepting what Collins and Evans (2002) assert, that a public issue like nuclear power is "only" a question of whether it is safe (and thus accepting their absurd proposition that parliament has already decided democratically all the other non-technical issues, and also the framing of what count as the technical issues). The same applies to GM crops, which has been insistently defined by policy expert institutions as only a scientific risk issue.[16]

This kind of condition for recognition remains an effective deletion of a democratic politics constituted by negotiation between free agents of civil society, democratic legislative and related political institutions, state institutions, and other legitimate bodies. Recognizing and addressing such difference appears to be a threat to dominant science-encultured policy institutions, who implicitly see "the public" and the tacit difference it portends, as an emergency in the form of fundamental threat of disorder. I have argued elsewhere that this deletion, which resonates somewhat with the notion of the "State of Exception" discussed by Agamben (2005), and the deep attenuation of politics described by Arendt (2005), could be ascribed to the unnoticed but powerful change in science's role since the 1950s, from one which *informs* public policy, to one which also now, by default, *provides public policy with its meanings*. By this I mean that, first, techno-scientific imaginations of innovations in a widening range of areas of social life, have become the imagined ends of public policy, to create the conditions of innovation for these end-points; and more specifically, when such prospective innovations encounter social questions, almost the only public form of concern, thus public issue, recognized by policy institutions, is that of "risk." This therefore, inadvertently or not, becomes the public meaning by which such issues are defined *as public issues*. I would suggest that social sciences, especially those claiming to deal with publics and science, have a responsibility to challenge this development, rather than to reproduce and reinforce it. With due respect, I think Durant, by default, sails too close to this sorry whirlpool. He is not alone.

It seems especially ironic that the institution most compellingly associated historically with a cosmopolitan ethos of openness, that is science, should find itself implicated increasingly deeply in the opposite. That typical members of the public have always and for good reason wished to understand science in terms of its institutional realities, of its forms of ownership, control, driving imaginations, and directions, and not only or even primarily in terms of its technical contents alone, has been recognized in our field for at least sixteen years (Wynne, 1991). These dimensions of "science," and their versions of "the public" including "public interest" or "public good," continue to beg for attention.

Notes

1 The papers by such authors as Dario Gamboni (pp.162–95), Simon Schaffer (pp.196–202), and Noortje Marres (pp. 208–17), in Latour and Weibel (2005), do provide the beginnings of such a fresh perspective. But it is striking just how systematically most of the mainstream texts in political philosophy and deliberative democracy, despite their important insights in other respects, fail to address the implications for their topic, of historical changes in the political and cultural roles of science. For just two typical examples, see Benhabib (1996) and Bohmann and Lutz-Bachmann (1997).

2 Hereafter in this paper, for simplicity I refer in the text to "science" without the continued use of inverted commas, but fully maintaining the questioning intent as to what might be meant by this common reference.

3 Faced with the curious paradox of repeated official abandonment of such forms of explanation of public dissent, followed almost in the same breath by invention of new versions, I have suggested, consistent with the perspective of this paper, that this public deficit model obsession on the part of scientific, policy and industrial bodies

must be seen as a diagnostic clue to other deeper issues in those institutional cultures which perpetrate this apparent self-contradiction (see e.g., Wynne, 2006a, 2007). It is not Durant's fault that he has not seen these more recent papers, and anyway the deficit model issue is not his focus.

4 Durant recognizes this, though tends to over-read the difference between "social relations," and "identity" in my work, as explained later. A further distinct element of this ontological starting point is also what can be seen as taken from symbolic interactionism, and Gofmann (1971) as social theory. This is the axiom that in social interaction, individuals or organizations are incessantly imagining in anticipatory manner the views and expectations of significant others, and behaving in relation to these imagined expectations and responses.

5 Here I just flag an issue for development elsewhere. This is the intrinsically dictatorial nature of this institutional cultural process of denial of those citizen concerns which do not easily correspond with dominant interests and concerns—dictatorial because presumptively imposed on populations by governments, usually in the name of science, with not the slightest element of recognition, negotiation or accountability over what are the concerns which should be addressed, for example in regulatory processes which are organized as "risk assessment" only. Here political and legal philosophers such as Agamben (2005), and earlier Walter Benjamin and Carl Schmitt, have described a "state of emergency" condition of modern politics, in which normal forms of political contract between state and citizenry, in which constitutional guarantees of basic citizen rights, responsibilities and freedoms, are suspended by reference to exceptional conditions—emergency, and incipient disorder—which are said to prevail. The philosophical debate hinges on whether nowadays, this "emergency" state of insecurity on the part of institutional authorities is any longer to be seen as temporary, thus always performed in the context of guaranteed ways of restoring normal democratic constitutionalism, or whether for various reasons it has now become the norm, and permanent. Agamben uses the concept of "the camp" as in Nazi Jewish concentration camps, for his exemplar of what he argues to be this general state of dissolution of civic rights and identities—of *human* recognition (as distinct from bare biological survival). At first sight this seems extreme, yet what is striking in the present context is just how fully such philosophical discussion neglects to examine the ways in which techno-science as modern public culture, order and authority, especially since the mid-1950s and the arrival of the nuclear weapons age (see Weart, 1988), has installed a de facto, of course not explicitly formalized in law, denial of the legitimacy, indeed capacities, of citizens collectively to articulate independent concerns and public meanings which cannot be domesticated and controlled by scientific forms of representation. These differences are fundamentally political, about what forms of society, social need, and social relations we want to struggle for and against. Such conflict and difference, subsumed as it is by science as the dominant public discourse, is thus left to be conducted indirectly, in the name of science; and the profound difference and otherness which citizen responses indicate, but which is threatening to authorities imbued with expressly scientific cultural reflexes, is thus denied rather than addressed for what it is. Risk discourses as pervasively institutionalized for so-called "public risk issues," are the definitive form of this authoritarian denial and deletion, not only of the concerns but of the autonomous capacities and legitimate rights of ordinary citizens. As political philosophers (Gutmann, 2003; Benhabib, 1996) have expressed it, fundamentally the issues underlying so-called "risk issues" and controversies are about (non-)recognition—of those different citizen concerns, capacities (and thus also, but derivatively, knowledges) which are deleted in the name of "science." This reduction of issues to the unpolitical, and citizens and their differences from the science-based culture to the incapable thus un-present, cannot be deemed to be as brutally deliberate, violent or total as the Nazi concentration camps. Nevertheless, one can see some fundamental elements of identity in terms of dictatorially presumed and imposed refusal of moral recognition, this time obscured by scientific discourses and mystifications.

6 This is the origin of Collins and Evans' (2002) "extension problem," in which they assert, proposals like mine, to recognize lay knowledge-ability, potentially expand lay participation in techno-scientific deliberations to a ridiculous degree. Yet I was addressing a different issue which they refuse to recognize, which is about recognizing the legitimate but often different hermeneutic and ontological bases of those citizen-knowledges and concerns, and the concerns which such citizen groups often therefore have about institutional-expert lack of recognition of their realities. This is not lack of expert recognition of their putative expertise.

7 Durant appears to be imagining the same when he refers (Durant, 2008: 10) to "Wynne's solution [to the public alienation which I see as caused by expert institutions' lack of recognition] is to increase fulfillment via the expansion of expert systems … into a forum for culture, not just knowledge." It is hard to see how one could approach what I have in mind through "the expansion of expert systems," which seems still to be constrained within the reimposition of a monolithic frame of collective meaning that I argue is the central problem.

8 See for example, Wynne and Felt (2007: ch. 4), and Wynne (2007). This is just where the lack of problematization of (public) "science," referred to above, bites hard—and this disappointingly here by leading science studies analysts like Collins.

9 This should not need to be said, but apparently despite having said it often enough, I am thought to believe differently, so there it is—as obviously superfluous as it may seem.

10 Such conflicts are often in play within scientific conflicts too, but are misunderstood as deterministic and thus resolvable solely by better evidence or calculation.

11 The latter of course trades on the public image of the former, for its authority; and the conflation of the two plays into this politics: thus all the more reason to emphasize the distinction, and all the more disappointing when science studies work fails to do this. For "science" assumed to be research science, I would adopt Barnes and Bloor's (1982; Barnes et al., 1996) approach, and recognize that there is a great deal more collective self-reflexivity to be found here. This is manifested as assumptions and commitments which might otherwise remain unchallenged and taken for granted as the normative principles of functional tendencies towards dogmatism of scientific disciplinary cultures (Kuhn, 1962), become subject instead to challenge and contestation. However, as Barnes and Bloor noted, this is not just because as Popper laid it out, scientists are intrinsically self-challenging and uncompromisingly provisional as a collective. Using inter alia Kuhn's detailed empirical work on science, they argued against the conventional wisdom that to the significant extent to which research science does show such collective self-challenge and thus intellectual openness, it is a contingent function of three main structural features of science: i) its very extensive and dynamic differentiation into many scientific subcultures; ii) its very high mobility of scientific practitioners between these different knowledge-communities; and iii) the extremely competitive nature of scientific culture overall.
Thus the idea of an essential difference on reflexivity as a *defining* difference between lay and scientific expert cultures, is not something that I would ever have recognized. I take the relationality point seriously.

12 Here I can do little better than to quote, in full support, Bonnie Honig (1996: 275): "The social dimensions of the self's formation as a subject-citizen require and generate an openness to its continual renegotiation of its boundaries and affiliations in relation to a variety of (often incommensurable) groups, networks, discourses, and ideologies." Obviously, such social networks and relations are not random, but neither are they totally and deterministically subject-selected. The point is the interwoven, mutual quality of "self-identity" and "social relations," and their multiplex fluidities at least in modern complex societies.

13 I do not think academics should apologize for, though we should be clear about, the unavoidable politics which this kind of research agenda involves, as I explain briefly in Wynne (2006b).

14 Collins and Evans (2002, 2003) have dismissed my emphasis of this dimension of lay qualification—I do not use the term lay expertise as I think it inappropriate here—as simply the general human quality of "sociality." This would be fine if it were not that the scope of the civic exercise of such possible sociality has been radically attenuated by the subsumption of much of the domain of such negotiation into presumptively framed public scientific discourses which are by definition exclusive, not only with respect to the propositional questions themselves, but also more problematically, with respect to negotiating the proper sovereignty of such knowledge over arenas of legitimate social contestation and difference. See e.g., Young (1996).

15 See Szerszynski and MacGregor (2006), who consistent with Arendt discuss the inauthenticity of notions of citizen agency framed and imposed from outside those citizen-collectivities themselves.

16 In this case, an ethical dimension of public concerns was also later recognized in principle (Gaskell et al., 1997; Gaskell and Bauer, 2006), but significantly in ways which only reinforce my thesis. Thus the ethical issue of GMOs was recognized, but deleted as a *public, collective* issue, in two connected ways: first, ethical concerns such as the "playing god" issue have been defined as private individual issues only, thus tractable through labeling and private individual free choice in the marketplace; second, public ethical concerns about institutional exaggeration of control and of knowledge of future consequences have been reduced to risk assessment and handed back to scientists, thus in effect fueling the very concern it was supposed to address; a third form of public concern, about lack of benefits and lack of social debate about benefits issues, has just been ignored (but see UK ACRE, 2007) because of the capitalist regulatory tradition that any product brought forward by any promoter for regulatory decision, by definition equals prospective social benefit because that free entrepreneur has defined it as such. For further discussion see Wynne (2001).

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Author

Brian Wynne is Professor of Science Studies and Associate Director of the UK ESRC (Economic and Social Research Council) Centre for Economic and Social Aspects of Genomics, CESAGen, at Lancaster University. Inter alia he recently chaired a STS "expert group" report for the European Commission, D-G Research: *Science and Governance: Taking European Knowledge Society Seriously* (January 2007). His 1982 British Society for the History of Science book, *Rationality and Ritual: The Windscale Inquiry and Nuclear Decisions in Britain*, will be republished by Earthscan with an updated Introduction, in 2008. Correspondence: CESAGen, Lancaster University, Institute for Advanced Studies, County College South, Lancaster LA1 4YD, UK; e-mail: b.wynne@lancaster.ac.uk