

***Doing and Knowing in the L'Aquila Case***  
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In bringing multiple ontology theory to bear on the question of expertise and the conduct of sociotechnical decision making, Danielle DeVasto (2016) stages the conversation between two bodies of theory with different origins, purposes and affordances. This is a daunting task and makes for a delicate coupling rather like the docking procedures of space ships from two different technologies in a science fiction novel (summer reading will out). There are multiple points of contact and misfit between the two, but the general purpose and direction of the project is productive.

**The Practice of Expertise**

DeVasto identifies a crucial issue in studies of expertise and in the process of decision making when she argues that “[a]pproaching expertise more broadly, as practise-based, not only makes room for but also *requires evaluation of claims to expertise* such as Giuliani’s [the non-certified expert in the L’Aquila case under consideration]” (385, emphasis added). In concentrating on the question of how to evaluate claims to expertise, DeVasto supplies a useful amendment to Collins and Evans’s argument. In their response to Ari Rip’s commentary on their original article, Collins and Evans (2003) note that:

He [Rip] points out that we do not show exactly how to recognize expertise, how to recognize whether a specialism has continuity with Western science, and so forth. He is right; this is the kind of problem that besets every new theoretical scheme and we would not expect to be able to settle them in the absence of case-by-case study that builds up a level of understanding of the issues (443).

DeVasto’s argues that the performative orientation of multiple ontology theory offers just such a practical method for evaluating claims to expertise and who might best participate in a deliberation. Further, DeVasto recognizes that in many such situations time is pressing and decisions not only about what should be done, but also about who should participate must be made relatively quickly. To that end, she suggests that interpreting studies in expertise and experience through multiple ontology theory can help develop “pre-established inclusion *management* procedures, which, in turn, can provide guidance about who should or should not be included (385, original emphasis).

Collins and Evans (2002) say that deciding the role of science in decision making and policy is the “pressing intellectual problem of our age” (236) and Latour pithily argues that the Greeks gave us one gift too many, both democracy and mathematics. In response to these programmatic manifestos, DeVasto’s argument moves our discipline a little further toward drawing practical conclusions from our complex theories. This is certainly a pressing opportunity for rhetoric of science, technology and medicine scholars interested in engaging scientific policy deliberations. This is one reason why DeVasto’s argument provides such rich material for RSTM.

## **From Postmodernity to Materiality**

If every new theoretical scheme is beset with unanswered questions as Collins and Evans say of their own work, I think there are productive questions to be asked about DeVasto's scheme. In examining the L'Aquila case, DeVasto invokes Annmarie Mol's (2002) discussion of how different medical practices "do" arteriosclerosis and enact multiple diseases. This marks the shift she makes from postmodern questions about the epistemic grounds of disagreement to the materialist question of acting and ontologizing differently. As DeVasto demonstrates, "doing" is everywhere in the L'Aquila case. Armed with home-made radon detectors, Giampaolo Giuliani "does" earthquakes differently than the seismologists and engineers on the National Commission for the Prediction and Prevention of Major Risks or CGR. Both groups do earthquakes differently than the residents of L'Aquila. Beyond that, DeVasto argues that "[s]imilarly, I propose that the types of expertise put forth by Collins and Evans are actually distinct ontologies. As Collins and Evans rightly note, expertise is multiple. However, these expertises are not simply different perspectives *but different ways of doing and practicing expertise*" (383, emphasis added).

The use of "doing" as a flat, all encompassing notion fits with the performative nature of multiple ontologies nicely, but it can become like "paradigm" in Kuhn's *Structure* book; a useful analytic term but one that might need more distinctions between different types of doings if we are to tie ontology to expertise. I think this is what DeVasto might be working toward when she suggests in her conclusion that it is questionable whether or not the L'Aquilani have expertise. Not all "doings" are expertise. Collins and Evans (2002) make this point in their original argument. After arguing for the existence of experience-based expertise, they recognize that, "[e]xperience, however, cannot be the defining criterion of expertise. It may be necessary to have experience in order to have experienced-based expertise, but it is not sufficient" (251). I think the same applies to doings.

Are all patterns of doing also ontological practices, and can they all constitute expertise? How far down do we want to push ontological practice of the everyday as we study things like medicine, earthquake prediction and living in earthquake zones? The practices Mol describes as ontological doings are all highly disciplined, trained clinical practices with explicit methodologies, epistemological regimes and discursive and institutional regulation. How readily can we transfer Mol's argument about the performance of disease in a hospital to less formal, less regulated practices? Since expertise connects everyday experience to formal knowledge claims in something like the way genre connects the micro level and the structural level in Miller's analysis of genre as social action, is an ontological practice like a genre? Does it have a minimal discursive and material regularity and self-consistency? Without some normative sense of doing, we are back at the problem of extension again only through a different doorway.

While I am not sure how to answer these questions, I think there are useful ways to frame it. The motive for Collins and Evans' work is to provide a normative theory of expertise so as to avoid the problem of too much democracy. Mol, Collins and Evans, and DeVasto

want to push back and recuperate some of the authority of “experts” in sociotechnical deliberations. This is the point of DeVasto’s call to evaluate claims to expertise and build a mechanism for doing so that can be institutionalized in advance of crisis. What we need is something like the principles of rarefaction Foucault develops in the *Archaeology of Knowledge* as he describes the regulation of discursive regimes. What principles of rarefaction can flexibly regulate what counts as practice-based expertise in sociotechnical policy deliberations?

One candidate for such a principle of rarefaction is something like Keith and Madjik’s (2011) proposal that expertise can be understood as expert argumentation. They propose that “[f]rom this perspective, experts are people who can make arguments about things that best respond to a particular problem, and who possess an expertise consisting in their ability to make a case for a particular definition of problem or solution” (374). In making this argument, they resist the dominant understanding of expertise as merely the possession of knowledge, what Bryan Wynne (2003) calls the “propositional hegemony,” and argue for a notion of expert judgment which requires specific argumentative skills. Had Giuliani managed this requirement, he might well have been more successful in gaining entry to the deliberation. Put otherwise, making an argumentative case for a particular judgment based on alternative practices and community values is a practical necessity for constituting expertise that can engage in these sorts of deliberations. Thus, an expertise of doing includes a rhetorical and argumentative element that distinguishes between experts and others.

Another possible way to separate the sheep of ontological doing from the ontological goats might lie in the notion of practice that is always linked to the doing or enacting of disease or earthquakes. Following Mol, DeVasto talks about “practice-based orientation” and about “patterns of practices” in which doing is embedded. When Bourdieu replaced a notion of rule governed social structures with a tacit “sense” of appropriate action inculcated through practices, his practices were still collective, highly patterned and directive. The rules of structural explanation give way to the sense of practice and practical knowledge, but however fuzzy and porous, this is still a bounded activity. For Althusser, the practices that constitute the ideological state apparatuses are institutionalized and highly regularized. That is, when we are thinking about what constitutes expertise and how it is authorized, not all patterns of activity rise to the level of practice. The practice of teaching is substantively different than the practice of recreational bike riding.

### **The Case Itself**

A second set of questions in DeVasto’s argument concern the nature of the L’Aquila case itself. DeVasto correctly points out that each case and problem is unique and the kinds of expertise they call for vary. The L’Aquila case is fascinating and troubling, but I think the uniqueness of the case limits the argument DeVasto can make about expertise. The practical if not the legal and theoretical question involved was binary: is there going to be an earthquake or is there not? Certainly Giuliani and the CGR made predictions with varying degrees of hedging, but in light of the earthquake, the political and institutional

decision about whether Giuliani's warning should have been heeded seems straightforward. He was right. They were wrong. Certainly the argument goes beyond this to questions of policy processes. Nonetheless, I wonder how differently this issue and the question of expertise would look if the question were an environmental policy question for which there could never be a clear answer, for which an answer such as it is might be 20 years in coming. More importantly, the deliberation in L'Aquila seems mostly about epistemic issues rather than values. Whose prediction and methodology is better? Certainly the CGR argued that frightening the populace unnecessarily was a bad idea, but this is not the same sort of value clash typically involved in many sociotechnical policy deliberations over environmental risk, community development, sustainability. This is not a wicked problem.

In the case of wicked problems, as Wynne and Keith and Madjik argue, we cannot separate knowledge and values in the neat way Collins and Evans do. As Wynne argues, Collins and Evans frame expertise in public policy deliberations as only propositional, a matter of knowledge not values. This reflects their prior commitment to a fixed notion of the social and the natural, reinforcing the modernist separation of nature and culture that critics like Latour have worked so hard to avoid. As Wynne points out, part of our cultural epistemology is a propositional hegemony in which science determines the meaning and salience of decisions and events, often to the exclusion of other cultural positions and formulations of salience. Or, as Latour says, when Nature speaks, the agora empties.

The science of risk formulates specific technical and quantifiable criteria and patterns of reasoning that dominate the public imaginary. It is possible that combining the theory of expertise with the ineluctable multiplicity of multiple ontologies might ameliorate the bias toward modernism implicit in Collins and Evan's theory and their repeated invocation of western science as we know it. The constitutive role of value in deliberations seems minimized in the L'Aquila case framed as it is as a forensic analysis of failure. That is not an error on DeVasto's part, but an artifact of the case itself. What would multiple ontology theory bring to less circumscribed cases or wicked problems where no decision is clearly correct even in retrospect? This seems to me a much less certain but potentially rewarding question we might now ask.

### **Regarding Interactional Expertise**

My last response to DeVasto's argument concerns her turn toward interactional expertise in her conclusion. DeVasto argues that interactional expertise is missing in the L'Aquila case, and she certainly seems correct. DeVasto frames the deliberative problem nicely in terms of the ontological theory when she writes that: "There are a series of mediations that need to happen in order for reality to hang together across multiple sites of practice, which indicates the very important function that interactional expertise plays" (392). In Mol's discussion of the hospital, the various sites of practice, e.g. internal medicine and surgery, operate in separate but coordinated institutional spaces. As Mol points out, the distance between them is important to their peaceful coexistence. When they do interact in the case of an individual patient, Mol uses the metaphor of "layering" to describe the

way practitioners can build alliances across different practices and their ontological theaters.

The L'Aquila case is different from Mol's analysis in suggestive ways. Hospital practitioners are of relatively equal status and authority; all have medical degrees, work for the same institution and are subject to a shared institutional organization. This is not the case in L'Aquila where Giuliani is a retired technician, not a member of the institution of the CGR. Also, this is a new interaction, the first time Giuliani has tried to insert his enactment into the decision-making. Where the doctors have had time and tradition to help them negotiate their layering, the citizens and scientists have not. This is probably true of almost all policy or decision-making interactions between scientists and citizens.

So how do we negotiate expertise, as DeVasto points out, "in a timely fashion" when there is not a pattern of previous interaction? It is perhaps a small point, but DeVasto's analysis suggests that we redefine interactional expertise more precisely. Collins and Evans had left it vague, a matter of interacting in interesting ways with science. If layering is the mechanism through which Mol's institution manages the multiplicity of ontological doing and calibration is the master trope in Graham's analysis of the politics of pain management, then perhaps interactional expertise involves the ability to layer and calibrate the ontologies at play in a policy process. This will certainly involve familiarity with the knowledge and methodology of scientific practices as Collins and Evans suggest. But it will also involve mediating the different values, actants and the practices which produce them in deliberative conflicts. Here I use "mediation" in Latour's privileged sense in which mediation changes all parties to an event and creates a new entity and set of actants. In a new materialist strain, interactional expertise will even include mediating among the human and non-human things involved in policy deliberations.

As I have tried to show, there are a lot of open questions about the intersection of these two bodies of theory. That said, combining studies of expertise with multiple ontology theory opens a productive conversation not only about expertise, but also about what rhetoricians might themselves do in these policy theaters. It is perhaps self-serving, on my part if not on DeVasto's, but refiguring interactional expertise as a form of mediation offers rhetoricians a fascinating place at the table.

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