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HOW TO DO THINGS WITH KNOWLEDGE: AN INTERVIEW WITH SHEILA JASANOFF

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Sheila Jasanoff, Pforzheimer Professor of Science and Technology Studies at Harvard Kennedy School, is one of the most original, influential and productive researchers currently working in the field of STS. Jasanoff's work starts from the constructivist view that knowledge is situated in social and/or political contexts, and that researching the practices that define those contexts is crucial for understanding the production of knowledge, science and technology. However, reflecting her training in law and policy, she shows that the reverse is true as well. Her detailed research on topics as diverse as biotechnology, the British BSE crisis and the Bhopal disaster demonstrates that in our contemporary 'knowledge societies' the meanings of democracy, ethics, law and accountability are defined in part by *knowledge* practices. If we want to understand these meanings fully, we need to investigate not only the social and political, but also the 'knowledge contexts' in which these concepts play out.

Jasanoff's reversal of the familiar directions in STS research parallels to some extent the study of scientific controversies that became popular among European STS scholars in the 1980s. In order to gain access to knowledge practices that otherwise stay hidden, researchers such as Harry Collins (1985), Steven Shapin and Simon Schaffer (1985) studied debates between scientists. As Thomas Kuhn (1970) famously argued in *The Structure of Scientific Revolutions,* these debates teach us much about the logic of science, even though this logic might be a product of its time and situated in a particular context. In her early works, *Controlling Chemicals* (co-authored, 1985), *The Fifth Branch* (1990) and *Science at the Bar* (1995), Jasanoff by contrast focuses her research on practices of regulatory policy-making and law, where controversies often display an epistemic dimension. *The Fifth Branch* examines the legitimating work of scientific advisory committees, whose findings are frequently contested but which are indispensable for policy-makers who are responsible for technical decisions. In *Science at the Bar* the 'turbulent confrontations' between science and law in the courtroom are central. Both books scrutinize a "new" domain of social action, showing how, outside the context of the laboratory or the clinic familiar to most STS researchers, controversies involving science are also intimately linked to the production of social norms.

Among the many books and articles that Jasanoff has written and/or edited, the volume *States of Knowledge* (2004c) is a key source for understanding how her oeuvre can be seen as a coherent whole. The book consists of thirteen chapters written by established social scientists from fields such as political theory, sociology and anthropology, each addressing an issue that lies at the interface of science and social order. The stories described in these chapters take place against the background of modern society's sweeping efforts to rationalize the organization of political, societal and juridical practices. But like Bruno Latour's stories in *We Have Never Been Modern,* the chapters in *States of Knowledge* read almost like Greek tragedies. However hard we try to separate politics from science, facts from values, and knowledge from the world itself, that project of modernity is bound to fail. The world that Jasanoff and her co-authors depict is in this respect similar to the one inhabited by Latour's 'proliferating hybrids' (Latour 1993).

But the differences are crucial and profound. In the introduction and first chapter of the book Jasanoff frames the issues by means of the 'idiom of co-production', which rests on the premise that we cannot fundamentally distinguish 'the ways in which we know and represent the world (both nature and society)' from 'the ways in which we choose to live in it' (Jasanoff 2004c: 2). Importantly, then, the two sides of the modern distinction, nature and politics, are not demarcated *a priori*, but are 'co-produced' in the effort to secure order: the same processes by which we epistemically represent and organize the world also produce the social and political worlds we wish to inhabit.

Although the widely-used idiom of co-production was not introduced solely by Jasanoff, it is fair to say that she is its 'central impresario' (Winickoff 2012). She includes in her horizon of research the strand of co-production that is grounded in the 'epistemologically oriented Edinburgh School of Sociology of Knowledge' (i.e. Steven Shapin and Simon Schaffer (2004b: 28)). Instead of focusing exclusively on the emergence and stabilization of what Latour calls the modern constitution, her 'interactional co-production' looks at 'knowledge conflicts within worlds that have already been demarcated into the natural and the social' (2004b). For Jasanoff, it is not the purification of nature and society that is the key constitutional move of modernity. Rather, it is the continual production and reproduction of epistemic, material and normative hybrids that are constitutive of worlds that make sense and hold together.

In order to clarify this distinction, let us briefly look at the example of Dolly, the Scottish sheep that was cloned in 1996. Both Latour and Jasanoff would argue against the understanding of such an artefact of technological intervention as either a neutral instrument or an event that will determine our future. Nevertheless, their analyses would differ. Latour would focus on the ontological aspects: making visible the actor network that stabilizes Dolly as a technoscientific object, separating it from Dolly as an object of political or ethical deliberation. Jasanoff, on the other hand, would emphasize precisely those latter dimensions, using the normative discourses on Dolly as a vehicle for exploring why particular sociotechnical constellations take the forms they do. From this perspective, the birth of Dolly is a disruptive event that revealed a range of already existing frames within which social actors think and act. Focusing on the effects of such transformative events can bring more clearly into view salient differences between the political cultures of different countries and societies.

A second unifying theme in Jasanoff's oeuvre is public reason. What is it that 'societies do in practice, when they claim to be reasoning in public interest?' Or: 'how do democratic governments construct public reason' (2012). Different societies have differing political cultures reflected in specific modes of public knowing. Governmental officials and citizens of one country tend to frame technoscientific issues differently from citizens and officials of another. Questions such as what or who should be held accountable after a technoscientific disaster or what constitutes responsible political behavior will be answered differently depending on the country of concern. These differing answers are linked to what Jasanoff calls 'civic epistemologies': 'institutionalized practices by which members of a given society test and deploy knowledge claims used as a basis for making collective decisions' (2005: 255). Convictions that are part of civic epistemologies manifest themselves in specific moments of controversy, as well as of emergence, stabilization and cross-boundary transport. Such moments are especially interesting, because it is in them that the characteristics of a nation's civic epistemology and its modes of public reasoning come into view.

One of the reasons why Jasanoff is inspiring to many scholars is that she is strongly committed not only to describing and explaining issues on the interface of science and society, but also to a more normative agenda. Jasanoff's eye is directed to the moments in which new orders emerge or when existing orders clash. These moments, when everything is in flux, are especially suitable for formulating critique and 'setting the stage for future development' (2004a: 278). In the interview, Jasanoff talks about the different ways in which Science and Technology Studies in general, and her work in particular, not only describe the world, but can also help change practices in the political as well as the scientific domain. Moving from one example to another, she explains the possibilities and limitations of her field to influence or improve the world that is a significant object of her studies.

One of the important claims in your work is that contemporary society is a knowledge society. Do you think contemporary politicians know enough about the practice of science to function well as politicians in these 'knowledge societies'?

That politicians may not know science, that is really not the point. We can expect that politicians should have a clearer understanding of the differences between an exact representation of nature and the kinds of imperfect representations that must be relied on when exact replicas are not available. They should understand that there is no 'truth to nature' in a strict sense. It is judgment all the way down. But which judgments should be trusted more than others? And what are the standard types of errors that creep into expert assessments? That is the kind of understanding that Science and Technology Studies can generalize about and that can be communicated to policy-makers. This is not only about the organization of science but also about the interpretation and uptake of science. I think that Science and Technology Studies does have many things to say about the organization of scientific activity, but it also has crucial things to say about the interpretation of knowledge, about the circulation of knowledge and about the uptake and use of knowledge. It is important for a literate society to understand something about the place of science in society, and also for politicians and policy-makers, because they are representing the rest of society.

From reading your work it seems that for an STS scholar your aims are considerably normative. Do you think the insights of Science and Technology Studies could be informative for the practice of science?

Unlike Philosophy of Science, which has sometimes claimed that it can inform and improve the practices of science, I do not make that claim about Science and Technology Studies. I do think that the position of science in society would be improved if everybody, including scientists, understood some of the basic findings of Science and Technology Studies. For instance, when the Hubble telescope was launched, the initial pictures that were sent back showed that the lens had been manufactured wrong. The pictures were blurry. When you investigate what exactly was the matter with the production of the Hubble telescope, you find that it was a very trivial error, but an error of perfect theory bumping up against imperfect practice of the sort that STS scholars are very familiar with. In order to direct a laser-light beam in a particular way, the lens makers had to use a non-reflecting film in which there was just one pinpoint through which the light could pass. But at that point some of the film had broken away, so the light actually reflected back from the wrong point and produced a distortion in the lens. So this multibillion-dollar exercise produced a lens that was a little off-centred.

Fortunately the Hubble engineers were able to correct it using highly sophisticated technology. I am not sure if knowing more Science and Technology Studies would have kept that kind of error from happening; I do think STS-trained engineers might have looked a little more carefully when tests of the lens produced inconsistent results. Science and Technology Studies is not a predictive science, but it might attune you to the moments at which you have to pay particular attention. When you are using a new instrument or process for instance, or when you are engaged in an interface activity, in which you are moving from the theoretical physics of light into the material properties of the equivalent of photographic film. Any STS scholar would tell you that at those kinds of moments an error can creep in through the messiness of practice. Interface problems can arise at many points in a technological system. Maybe it would be useful for managers of large technological systems just to be more aware of those kinds of situations.

Once you understand the limitations of all knowledge-production systems, then it makes you more humble about the kinds of things that are claimed [by scientists]. I think some of what has been said in Science and Technology Studies has actually seeped into people's consciousness. When you talk about models, for instance, there is willingness, among modellers at least, to admit that these are not exact representations of the real world. They are representations of a simplified version of the real

world. But I think outside of modelling communities that kind of subtlety is not always very well understood.

More generally, in my work on co-production I explicitly address the connections between description, analysis and criticism. I think that for any social-science field, all of those aims are necessary. You need to have good descriptions to get anywhere. You need data, but you also need some form of critical analytic apparatus to make sense of what you are describing. In that sense prescription comes with the territory of STS. You don't have to bifurcate the field by saying: 'Now, I am doing description, and now I am doing the prescription that comes from it'. I've always argued that built into the very project of STS is a critical perspective. In the same way that philosophers believe observation is theory-laden, I would say from my point of view, that observation is normativity-laden, because it always presumes answers to the question why are we observing those things, in what ways and for what purpose. This is in effect the Jasanoffian law. It is an explicit theme of mine; it is not a claim that STS scholars make in general.

In one of your essays in the Public Reason *collection you relate the question of whether and how much societies can learn due to their underlying civic epistemologies (Jasanoff: 2005). Do you think political theory is focused too much on change and too little on stability?*

In that article on technological disasters, but also in other places, I ask what big breakdowns can teach us about the way things hold together, that is, about the constitution of modernity. Stability and change are two faces of the same coin, of the same enterprise. Controversy studies have had a lot of play in Science and Technology Studies as a methodological device. If you were an outsider to Science and Technology Studies and you asked 'does the field have any methods?' the answer might be, 'Yes, we use standard social-science methods like participant observation.' But you might also say that we have specific methods like lab studies and controversy studies. But we also have what I might call 'stabilization studies'. All the STS work on standardization, for instance, can be read as a contribution not only to the question 'how did a contingent claim become a fact?' but also 'how did it last, why did it endure?' STS importantly addresses the temporality question: 'Why do things last over time? When does it become part of culture?' Anthropologists often say that culture is not a stable thing. Nevertheless there is stability in social systems, and I think that Science and Technology Studies can and does contribute to answering questions about how properties of a culture get handed on across generations. How micro-practices become macro-institutions and so on.

I would not say that social and political theories do not pay enough attention to continuity or do not pay enough attention to the dynamics of change. But the dynamics of change itself has more than one face. It is partly about why things do not change. What is the inertia in systems all about? It is particularly interesting for studying sciences and technologies because these are represented as continually changing. If that is so, why is the rest of the social order not changing? That is a theoretically interesting question. Scientists always want to talk about the other institutions lagging behind, as if only science progresses. In some of my work - as you may know- I criticize this idea of the lag, in particular the law lag. But in that piece you mentioned I was in a sense tackling both instability and stability at the same time. At moments of extreme disorder, people wonder: 'Is our government really capable of doing the things that we entrust the government with doing?' I was suggesting that even in those moments of radical disorder you actually end up uncovering what gives a system stability over time, and I found part of the explanation in my other work on civic epistemologies.

In the above-mentioned article you relate civic epistemology to questions of causality or responsibility. Do you think that sometimes blaming is unproductive for learning processes in times of disaster?

My interest in the article was to make the point that in order to get a socially robust answer to the question 'what went wrong and who is to blame?' you have to shut out some pieces of the causal puzzle and act as if

those things do not matter. The easiest case to understand in this respect is the British BSE crisis. In this case study I said that British political culture takes pains to prevent individuals from being blamed for what went wrong, because in the British state it is important to maintain the legitimacy of the virtuous public servant. This is a well-established feature of the way that the British government is constructed. It is not a highly legalistic system, so governmental virtue is not secured by saying, 'You shall follow the Constitution and the laws'. That might be more of a German approach. Instead in Britain public servants have to demonstrate their ability to handle public affairs responsibly. The system depends on trust, and so it produces a crisis if any of the people who are carefully trained and selected to look after the public interest suddenly fail to live up to the public's trust. It is not surprising, against that backdrop, that inquiries seem to reframe the problems to some extent as problems of carelessness or problems of honest empirical error, but not as failures of virtue. Criticizing the public servant for having failed in virtue would really pull away one of the fundamental beliefs of the British system - that it is possible to train public servants who will be virtuous in the name of the people.

So I was articulating ways in which non-learning happens. Even the most overwhelming event is interpreted in the light of particular traditions of interpretation. These allow you to draw certain conclusions and make it hard to draw other ones because they would be too destabilizing. Learning is imperfect because cultural tendencies, such as civic epistemologies, place outer limits on the degree of self-reflection that an institution is capable of achieving.

Can awareness of a country's civic epistemology be of help for democratizing the public involvement in science?

Civic epistemology for me is operating in a different place [than democratizing public involvement of science]. It is part of my effort to theorize political culture. This is a concept that has completely fallen out of regular analysis in political science. I think Science and Technology Studies can do a lot to animate it. For me political culture is a very important component of how people know things collectively, as part of a political system. In turn, the concept of civic epistemology helps me to understand fairly important divergences that I see across political cultures. So for instance why is there far more contestation over scientific claims as foundations for public policy in America than anywhere else? Why is there the least amount of such contestation in Germany? The economist's answer might be: 'Because the public has no preference for it. They do not create a demand for it.' But why do publics have some preferences and not others?

The specific civic epistemology of a country offers an answer to that set of questions. A simple way to state the point is that, out of all the possible styles of producing and debating evidence for public policy in a nation state or in any stable political community, some styles are preferred, institutionally cultivated, performed and maintained. That is what I call civic epistemology. And it is worth studying those styles. This is not a claim that civic etymologies are unchanging. It is not a claim that civic epistemologies are the one and only thing that is alive in a culture. It is about the value of understanding the institutionalized preferences for styles of evidence and argumentation that are sustained in a durable political culture.

You ask if knowing this would help for organizing more democratic procedures of public involvement in science. I would say not necessarily. When you are organizing a public engagement process, you are operating most of the time with tacit knowledge of your culture's civic epistemologies. My work, the work of the social theorist, is to make explicit what is implicit. It is an open question whether, once you recognize that, you choose to do things in a different way or not. I get to my theoretical positions by seeing how people are behaving and generalizing from those observations.

Once you make the tacit theories explicit, it is a separate and interesting question whether people will change their ways in consequence. It is not possible to change long-held collective assumptions and practices all that easily. But if people were aware that civic epistemologies exist and matter, then they might not go around using universalizing terms like 'evidence-

based policy' that easily. What counts as evidence in public reasoning is a cultural product. It depends on national civic epistemologies. The term 'evidence-based policy' implies that evidence is a-cultural or transcultural. If you are reflexively aware how the processes of collective knowing work in your community, or in your country, maybe you can start tinkering to see what alternative processes are possible, and then you can try to borrow from other systems. But such borrowing does not always work.

One of the illustrative cases I talk about concerns America's observation in the 1980s that European consensual processes produce less contested evidence for policy. American regulatory agencies therefore began to experiment with European-style negotiation. It became enough of a practice to get its own little abbreviation. It is 'regulatory negotiation' and people called it 'reg neg'. So 'reg neg' became a preferred institutional practice for a while until it became obvious that actors participated in regulatory negotiation but then they fought the results anyway. So the American style of contested knowledge production resurfaced and it did not matter that 'reg neg' had been layered on top of it. It is a nice little example of how civic epistemology will come back in play at the end of the day.

Being aware of a political culture's prevailing civic epistemologies is not democratizing in and of itself, because it does not tell us whose voices have been marginalized or silenced. Again, there may be a more or less democratic way to build knowledge on a certain issue, but that has nothing to do with whether the issue itself was framed democratically in the first place. You know, if most people in a society are worried about poverty, and the decision-makers keep on talking about climate change, then they are not even in the same ball park as their fellow citizens. If you say: 'Don't talk to me about poverty until we first address climate change', that is politically not democratic because you are telling people to alter their priorities. If you say, 'Okay, I will talk about poverty but only as a consequence of climate change, as an issue about climate refugees or displacement and poverty', then people still may reasonably say: 'I don't want to go through the eye of the needle of climate change before I get to discuss my issue, which is poverty'. This is the sort of democratization that I address in a recent article.[1] It is about why and when we need to delegate the framing of public knowledge problems down to citizens and not try to establish the frames at a centralized, supranational or universal level.

Do you think the field of Science and Technology Studies has influenced the field of Philosophy of Science?

Thus far it is much easier to say how Science and Technology Studies, as a relatively new field, has borrowed ideas from existing disciplines. Those disciplines have not been particularly quick to take theoretical ideas from Science and Technology Studies on board. This is partly because disciplinary theories are tied to the kinds of questions that people are used to asking and the discourses in which they are asking them. So it is going to take a while before Philosophy, an ancient and well-formed discipline, decides that its questions could be enriched by History and Sociology of Science and Science and Technology Studies. In essence, these are diffusion problems. I don't have a grand theory of disciplinary diffusion, but I have observed that my work diffuses much more easily into other relatively new fields of study, and less easily into the more self-contained fields that have built their own high social and disciplinary walls around them. It is not necessary for philosophers to go out and listen to STS scholars. It is almost necessary for STS scholars to go out and listen to other disciplines, including Philosophy.

I have sometimes noticed that in the Netherlands, philosophers who have close ties with Science and Technology Studies are not considered real philosophers. Why do you think that is the case?

I think that kind of questioning can actually be quite useful if one does not allow oneself to get annoyed by it. When somebody tells you that 'what you are doing is not such and such', you can be attentive to it as a piece of mini-fieldwork. You can immediately recognize what is happening as boundary work. In turn, recognizing that something is boundary work is a starting point for asking 'why this boundary?' First of all, what

does it mean to the person drawing the boundary and what are the stakes in creating and upholding that boundary? And how can you bridge the boundary and overcome it? One has to learn to be charitable enough to say, okay, *you* might be trying to keep *me* out, but you are also showing me that there is something here that I need to overcome to make my own knowledge travel further. And then it almost becomes an exercise in political epistemology. It reveals the politics underlying the competing epistemological position. In that way, one can actually tackle and deal with what at first looks like an alien perspective in a productive, proactive way.

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[1] The article Jasanoff refers to is 'Epistemic Subsidiarity: Coexistence, Cosmopolitanism, Constitutionalism' *European Journal of Risk Regulation*, Vol. 2 (2013), 133-141.