Science

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'Science' features twice in anthropology. On the one hand, science is an object of anthropological enquiry, in much the same way as 'kinship', 'religion', or 'nationalism'. Anthropologists have studied scientific practices and practitioners ethnographically, and have traced the effects of scientific knowledge in other spheres of human activity. Alongside other scholars in 'science and technology studies,' anthropologists have raised questions such as: is scientific knowledge 'socially constructed'? Does the 'culture' of scientists matter? What is objectivity? Is science a distinct kind of activity or domain? Are scientists in the business of describing the world, or transforming it? And is science 'western'? In a number of these cases, anthropologists' answers have been distinctive.

On the other hand, for much of its history anthropology itself was understood as a science of society or culture - and continues to be so understood by some of its practitioners today. An anthropological look at science thus also involves turning the lens back onto anthropology itself, and examining it with the same tools we are using to inspect other scientific practices: how are the methods and concepts of anthropological knowledge production (culture, society, ethnography, the site, comparison) themselves put together? And how does applying these terms and methods to the strange object that is 'science' distort and transform them?

A science of non-science?

As noted above, anthropologists are only one voice in the broad chorus of social science and humanities disciplines which have taken 'science' as their objects. Philosophers and historians have been studying science for nearly as long as such a thing has been thought to exist. Sociologists joined the conversation in the twentieth century with quite far-reaching effects. Anthropology was a relative latecomer to the study of science, and there was no self-defined 'anthropology of science' until the late 1970s.

The main reason for this is that for much of its history, the discipline of anthropology was imagined both by its practitioners and by others as a 'science of non-science' (Viveiros de Castro; see also Nader 1996). In other words, anthropologists tended to assume both that their methods and approaches were part of a unitary project they thought of as Science, which belonged properly to the modern West, and that their object of study was made up of alternatives to this project: non-scientific or not-quite scientific ways of thinking and being amongst non-western peoples. When nineteenth century evolutionists and early twentieth century functionalists argued about magic, 'animism', witchcraft or religion, they often framed these, implicitly or explicitly, as the non-western 'others' of western science - including the western science of anthropology.

Another approach involved the study of what came to be called 'ethno-science'. This line of enquiry was launched by Bronislav Malinowski's (1884-1942) essay 'Magic, science and religion' (Malinowski 1925). Here Malinowski argued that, in fact, scientific and non-scientific ways of thinking existed alongside each other in all human cultures, 'primitive' as well as 'modern'. Malinowski concluded that

> If by science be understood a body of rules and conceptions, based on experience and derived from it by logical inference, embodied in material achievements and in a fixed form of tradition and carried on by some sort of social organization – then there is no doubt that even the lowest savage communities have the beginnings of science, however rudimentary. (Malinowski 1925: 34)

Anthropologists took up Malinowski's point, to develop an interest in what came to be known as 'ethnoscience'. The patronising language and the evolutionary assumptions were progressively abandoned, and studies of ethnoscience came to document sophisticated non-western cultural knowledge about the natural world, which contemporary western botanists or biologists might indeed seek to learn from. And yet, the very need to qualify these non-western beliefs and practices as *ethno*science intrinsically carries with it the assumption of a distinction between this and 'proper' – read: Western – science. Once the comparison has been set up in this way, it is hard to avoid the conclusion that ethnoscience is a more rudimentary, or more practical or limited version of something which in its full form is the prerogative of the West. A more radical point was just around the corner, namely that all science (including Western science) was an 'ethnoscience'. But, as we shall see below, it took some time - and some help from other disciplines – for the full effects of this realization to sink in.

It is precisely against this portrayal of non-western people's knowledge as a more practically oriented, rudimentary version of Western science, that Claude Levi-Strauss (1908-2009) built his theses about 'the savage mind' (Lévi-Strauss 1996). The point here was, as for Malinowski, to show that scientific and non-scientific ways of thinking co-existed in all human societies. But whereas Malinowski tried to argue that even the technologically 'simplest' peoples mix in a good dose of science with their rituals and beliefs, Levi-Strauss took a different tack. He started from a description of the incredible complexity of the symbolic systems through which many non-western peoples classify the natural world around them, to dispel the sense that this might be reduced to the mere satisfaction of their immediate practical needs. Rather, for Levi-Strauss, this 'untamed thought' which exists everywhere, but is particularly prevalent in 'simpler' societies, is a fully fledged intellectual pursuit, different but equal in sophistication to scientific thinking. It is a 'logic of the concrete' in which natural objects are combined and recombined into a complex symbolic language for thinking about social and existential problems.

From Science to sciences

The nudge to think ethnographically about western science itself, however, came from outside anthropology, as sociologists and historians started to rethink western science as an object of study. Many

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of these works, however, themselves drew on the findings and ideas that anthropologists had been developing in their studies of science's 'others'. Eventually, anthropologists joined the science studies party in their own right, and their contributions were distinctive.

Taking his cue from Durkheim's sociology of knowledge, as well as Weber's writings on science and ethics, sociologist of science Robert K. Merton (1910-2003) investigated science as a functionally integrated social institution whose role was 'the extension of certified knowledge' (Merton 1973). This institution operated through the production of a 'complex of values and norms which is held to be binding on the man of science.' (Merton 1973). A number of sociologists later took issue with Merton's account of norms, noting in particular that these seemed to be mainly honoured in the breach by practising scientists (e.g. Mitroff 1974). More profoundly, what many later sociologists of science found lacking in Merton was the explicit way in which he cordoned off his account of the structure and norms of science from the positive *content* of science - its actual facts and findings. Sociology might explain failures or perversions of scientific knowledge and might give clues to the general conduct that would permit such perversions to be avoided. But it had little to say about the successes of science - its established facts and currently powerful theories. Paradoxically, while Merton's account does suggest that the effective pursuit of scientific knowledge requires particular social and cultural factors, the nature of his 'norms' means that in most cases, what this structure requires is precisely that the interference of historical, sociological, and personal factors be eliminated. Ultimately, we are left with a picture in which, as in classic histories of science and in accounts of scientific practice by many scientists themselves, socio-cultural, historical, and personal factors could explain the context of science, always, but its content only in the case of scientific error. As for scientific success, it remained, presumably, a sign of the fact that scientists had managed to get in touch with reality and that extraneous social, cultural, and personal factors had been kept at bay. Merton just highlighted the idea that such keeping at bay was itself a social and cultural process - a thought to which later historians and anthropologists would return.

A more profound challenge, however, was under way. Long-standing assumptions about science as a broadly unitary method for moving from individual facts to general claims in a rational, value-free way - the sort of picture of science which remained at the core of Merton's account and underpinned anthropologists' own ideas about their own discipline – had started to be challenged from the early twentieth century onwards. Doctor and historian of science Ludwig Fleck pointed out that in tracing the history of a particular scientific object - syphilis – one did not find the expected history of the systematic application of a standard method, of rigorous hypothesis testing leading to a progressive history of discovery (Fleck 2012 [1934]). Science, for Fleck, could not be understood without a study of the particular communities of scholars and the 'thought-styles' which they developed and passed on through training. These thought-styles, and not simply evidence, reasoning, or logic, shaped what would count as an interesting question or an acceptable answer at any particular historical moment.

Years later, Thomas Kuhn expanded and popularised this notion through his discussion of 'paradigms' (1962). In a strong, and much debated statement, Kuhn claimed that paradigms represented 'incommensurable ways of seeing the world and of practising science in it.' (Kuhn 1962). This view of science exploded the idea of a single project with a continuous, progressive history. Instead, historians and sociologists were offered a new object of study: the rich tapestry of multiple scientific paradigms, each carried by a human community with its own internal rules, forms of transmission and structure - much like the 'cultures' or 'societies' which anthropologists had been investigating.

Studying scientists in their labs: two examples

By the later 1970s and early 80s, sociologists of science had begun detailed ethnographic studies of particular laboratories seeking to demonstrate the social construction of scientific knowledge in particular concrete settings. They showed how collective cultural assumptions, pragmatic negotiations between individuals, and the use of particular methods, tools and techniques rather than others, all came together to build a finished product which would later be packaged as a 'mind-independent fact'. These sociologists opened up the practice of science to scrutiny.

A classic of the genre was Latour and Woolgar's *Laboratory life: the social construction of scientific facts* (1979), based on fieldwork and interviews undertaken in a biology lab – the Salk institute in La Jolla California. The authors – of whom one, (Woolgar) was a sociologist influenced by Garfinkel's 'ethnomethodology' (cf. McDonald 2012) – highlight that their aim is to treat scientific practice as if it were as unfamiliar and in need of explanation as the subjects usually tackled by anthropologists. They give a deadpan and minute description of the spaces of the lab, the kinds of people present there and their daily activities - all as if the endpoint of this bustling were mysterious and unknown. On the face of it, it seems that enormous amount of time and money, masses of physical materials (frogs, mice, paper, electricity, pipettes, etc.) are being expended to produce a seemingly rather slim result: some papers, published in scientific journals. These papers contain statements about the world, cast with more or less qualification. The less qualified a statement is, the closer it is to an indisputable fact. By the time papers are published, the scientists themselves, like the broader public, talk of the facts they contained as if they were merely abstract statements made of an 'external reality'. Facts become independent of the process of production described above. The laboratory is thus rather like a factory - a factory for producing standalone objects called scientific facts.

As another prominent author in this tradition commented, the core point of sociological lab studies was to show that "scientific products are 'occasioned' by the circumstances of their production" (Knorr-Cetina 1983). That is to say, the facts cannot stand alone. The circumstances of their production are not just an external 'context' – they are what constitutes these facts. In sum, laboratory studies continued the philosophical discussions about the nature of scientific knowledge, by making knowledge empirical through and through.

Early anthropological studies of scientists at work had a slightly different flavour, and different concerns. In one of the earliest ethnographies of Western science, *Beamtimes and lifetimes* (1988), Sharon Traweek studied high energy physicists in America and Japan to elucidate their shared and contrasting cultural constructions of their subject matter and themselves. Traweek, like Latour and Woolgar, explicitly played up the strangeness of treating physicists as if they were an alien cultural and social form. She described their spaces and the tools and techniques they used in detail, as well as the social arrangements which tied their scientific communities together and the hierarchies and training trajectories that crosscut them, and built up a detailed and convincing ethnographic picture of the ways in which these scientists understood their world.

Unlike Latour and Woolgar, Traweek was not interested in the construction of particular facts in physics. Rather, one main takeaway of Traweek's ethnography – beyond the rich description itself – was that social organization structures scientists' perception of nature, and vice versa. Traweek noted, for instance, that core gendered metaphors about nature as a 'female' realm to be investigated and unveiled by forceful 'masculine' scientists both stemmed from and reinforced broader gender stereotypes and assumptions in scientists' own careers and lives. In these respects, Traweek's project was strictly social constructionist: it related the ways in which these researchers understood nature to the social structures within which they operated, such as their gender relations, or the structures of authority and training which characterised their scientific communities.

Three key lessons

The anthropology of science today is a complex and diverse field, which is not easy to systematize or order into 'schools'. However, one might point to a number of key debates which arose over the past twenty or so years since the beginnings of the anthropology of sciences, and key lessons which contemporary anthropologists have drawn from them.

Beyond social construction: don't forget the things!

The contrasts between the methods and approach of Latour and Woolgar on the one hand, and Traweek, on the other, is instructive. All took as their object scientists and their daily practices. All began with careful and methodologically 'naive' descriptions of the spaces, practices, and social organisation of scientific activity. But there the similarities mostly ended.

While Traweek's aim remained fairly classically – to demonstrate that understandings of nature were socially and culturally constructed – Latour and Woolgar's book actually drove the first nail into the coffin of this popular kind of explanation. As Latour noted in a review of Traweek's book (Latour 1990), to

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describe scientists as socially constructing ‘nature’ on the basis of their existing cultural and social arrangements was to write past the fact that their work produced specific realities which would later impact the actual worlds they lived in (and not just western culture’s ideas about nature, but also westerners’ and others’ daily lives).

The broader point was that ‘social construction’ itself was in fact a misnomer, if one took it to mean that the solid facts of science could be explained away by pointing at ‘social factors’ lying behind them. To understand the construction of scientific facts, Latour argued, one had to attend not only to the activity of humans, as sociologists typically did, but also to the activity and effects of the non-human materials in the lab: the machines, which enabled particular stabilizations and inscriptions, and the biological entities which ‘behaved’ in particular ways. Both Traweek and Latour and Woolgar had paid attention to the machines and objects which enabled scientists to construct their explanations and seek to encounter nature. But Traweek’s interest, ultimately, was in the ways the scientists understood and symbolised these machines (reflecting for instance, on the gendered imagery of huge expensive machines with names like ‘SPEAR’ etc.). In Latour and Woolgar’s account, the actual activity of these machines, the ways in which they transformed phenomena, was a core element in the explanation. Humans, their account suggested, are not the only agents. Rather, action is distributed, and swathes of human and nonhuman actants have to be aligned to produce effects in the world. This may be construction, but there is nothing straightforwardly ‘social’ about it. From this point of view, to claim that scientific facts or ‘nature’ are mere social constructs becomes as absurd as arguing that a chair or an apartment block is a mere social construct.

This philosophically counterintuitive point of view was eventually articulated more broadly as ‘Actor-Network theory’ (Latour 2005). The point was general. It did not imply a return to the earlier position that western science was not socially constructed, but that other knowledges were. Rather, for Latour and the actor-network theorists, classical sociological approaches always failed when they sought to explain phenomena away as social constructions – it’s just that the sciences (and scientists) to whom they did this were rather more frequently in the position to speak back loudly enough to prove them wrong. Sociologists and anthropologists of science writing today, while they might object to particular elements of Latour’s approach and assumptions and want to retain a more traditionally critical stance, still hold on to the core lesson: never forget the effects of materials!

Science beyond the lab: no need to stay put!

But there were gaps in Latour’s picture too, as anthropologists observed (Martin 1998). Comparing once more Traweek and Latour and Woolgar’s books, one obvious gap in the latter is how little the scientists’ own understandings and perspectives - their words, even - featured. We will return to this below. Another was the rather myopic attention to one particular setting, the lab, to the exclusion of broader extensions and connections. This echoes a broader distinction. Where sociologists of science more generally had

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focused in on particular laboratories and research programmes, the tradition of comparative and holistic thinking in anthropology drove anthropologists to ask broader questions about the ways in which purportedly scientific and non-scientific ways of encountering the world relate, differ, or cut across each other. Note, for instance, the fact that Traweek's work focused on physicists in Japan and the US, thus introducing sophisticated questions from the start about the notion of cultural 'context' and what is or is not shared. Anthropologists of science pursued these complexities by asking how scientific knowledge and practices travelled beyond laboratory settings. This brought to center stage questions of history, power, and culture.

One could move beyond the lab by relating scientific and non-scientific knowledges within western societies themselves. A classic example of this approach is Emily Martin's influential book *Flexible bodies* (1994). There, the author traces the changing ways in which Americans imagine immunity. Drawing on the history of immunology and on popular media representations of the body, and moving backwards and forwards between researchers in immunological laboratories and interviews with a wide selection of laypersons in Baltimore, Martin shows the complex interplay between changing scientific and popular conceptions within the broad cultural setting of late-twentieth century America.

Crucially, Martin's point is not that initially correct scientific understandings are 'dumbed down' in popular portrayals. Nor is she arguing that scientific facts are the straightforward effects of social structure or a stable 'cultural context'. Rather, she notes that there is a constant interplay whereby scientists themselves draw on changing popular conceptions and metaphors to think through their research questions and findings, and that these findings in turn shape and transform popular conceptions. Scientific facts and representations travel and change as they move beyond the lab. The question of the cultural construction of scientific facts thereby opens up onto the broader question of how 'American culture' itself is in part constructed by reference to certain popular understandings of scientific facts. More broadly, Martin's work involved a critical reflection (responding to the 'crisis of representation' of the 80s) about the formerly rather static and bounded ways in which anthropologists had conceptualised culture. This also involved new and creative ways of re-imagining anthropological fieldwork stretching over multiple places and times (Marcus 1995). Anthropology's 'holistic' imaginary was thus both challenged and reconfigured (Candea 2007).

The other way in which anthropologists moved beyond the lab was by explicitly challenging their earlier distinctions between 'Science' and 'ethnoscience' (see above). A fairly straightforward point was that all science, including western science, is after all an ethnoscience - each can only be understood in context, and none can act as a privileged vantage-point from which others can be judged. These comparisons demote western science from its unique and exceptional position (Nader 1996). But to leave it there might suggest that each (ethno-)science operates in a self-contained world, rather like Kuhn's 'incommensurable' paradigms. The more challenging task is to trace the multiple power-laden interactions between these

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various ethnosciences.

An example of a convincing attempt to do this can be found in Roberto Gonzalez's *Zapotec science* (2001). Gonzalez argues that while Zapotec farming practices are grounded in a range of beliefs which western scientists might dismiss (such as a humoral theory, or the belief in animate supernatural beings), they also involve the key elements of scientific thinking:

> Like agricultural scientists, Talean campesinos conduct experiments, formulate hypotheses, mold their results to theoretical frameworks, and disseminate their findings, from campesino to campesino, and from parent to child. (González 2001)

So far, Gonzalez's argument sounds rather like Malinowski's regarding Trobriand gardening. But, as we saw above, where Malinowski portrays Trobriand science as a pragmatic and rudimentary version of 'proper' contemporary science, Gonzalez portrays the two as equally theoretical and equally complex. The difference is historical and political: Zapotec science is a 'local' science, whereas western science – Gonzalez calls it 'cosmopolitan science' – is an ethnoscience which has gone global, partly through the effects of colonial and capitalist expansion. Gonzalez traces the historical process whereby Zapotec and cosmopolitan sciences have historically borrowed knowledges and techniques from each other. In sum, Gonzalez shows us how anthropology can take us beyond relativism by putting different (ethno)sciences in historical relation - not only different views *on* the world, but different and unequally powerful views *in* the same world.

Science, norms and ethics: take scientists seriously!

Throughout these developments, an increasing distance crept in between the way most anthropologists and sociologists thought about science, and the way many self-defined scientists did. For many of the latter, as for much of the Western public at large, science remains, despite its occasional failings, a unique and broadly successful attempt to establish truths about nature.

By the late 1980s and early 1990s, the so-called 'science wars' (Ross 1996; Parsons 2003) erupted as a number of scientists struck back at what they read as anti-realist and politically motivated attacks on science from the humanities and social sciences. At the margins of this occasionally rather unedifying debate, a number of more interesting positions emerged within the anthropology of science. These voices asked again what it would mean to really take science and scientists seriously. This question has particular traction in anthropology - after all, one of the discipline’s core commitment had always been to take seriously the people with whom anthropologists work. If anthropologists’ accounts persistently irritate and offend the people they are describing, then surely something must be wrong? Again, one can distinguish two main approaches to dealing with this question.

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We have seen the outline of one of these approaches in Latour's comments about science's power to talk back, which were made precisely in the context of the science wars. His far-reaching philosophical reconfiguration of science was cast as a partial response to these concerns. However, Latour's radically performative view of science takes a very different turn when combined with the more engaged political stance stemming initially from feminist critiques of science. If science is a process of active world-making, rather than merely the discovery of truths about the world, then this recasts the question of how one might do science for 'better' or 'worse'. Science becomes political through and through, not simply because it provides legitimising narratives for this or that political practice or social arrangement, but more potently and directly because it can build the world in different ways. For example, a world in which humans are understood as behavioural machines of the type described by some forms of psychology is a world in which voting, advertising, education, and taxation, will all take a certain form. Actual humans will be shaped and transformed in important ways by these various offshoots of scientific understandings built in particular labs, and will in turn live to confirm the value and reality of these understandings. Other paradigms in psychology might lead to different understandings of the human – different policies, different humans. In other words, the frontlines of the 'science wars' are not between science and non-science, or between science and the humanities who critique 'it' from the outside. The frontlines are within science itself.

One of the most influential exponents of this position is Donna Haraway. In her painstakingly detailed history of primatology (Haraway 1989), Haraway draws together Marxism, feminism, cultural anthropology, actor-network theory, and her own experiences as a trained biologist. Haraway shows how particular research programmes emerged out of a mix of assumptions, techniques, and human and non-human actors differently situated and positioned, and how these scientific practices and their results fed into and fed off of popular imaginaries. At every juncture, different sciences and different politics were possible.

What is crucially at stake here is a challenge to the ability of any one commentator to speak for 'Science'. The sciences are multiple, contest-riven, and political. Anthropologists and other scholars in the social sciences and humanities learnt from Haraway to attend to the many voices within scientific debates, and occasionally to shed their reserve and enter debates 'behind the lines', forming alliances with particular scientists against others, rather than sniping at 'Science' from a self-definedly external position. In sum, a 'feminist technoscience' approach such as Haraway's confronts the question 'are you taking scientists seriously' with another question: 'which scientists?'. In some respects there can be no more serious engagement with science that to get stuck in and argue within it.

Nevertheless, one might argue that, as with actor-network theory, the alliances proposed by this approach paper over some deep philosophical divergences. At its core lies a radical assumption that one can only properly engage with scientists once a general narrative about the aims, norms, and duties of 'Science' (as an objective, value-free, method-bound quest for knowledge about the world detached from any particular standpoint) has been replaced with one that depicts sciences as this-worldly, inherently political, and grounded in multiple standpoints. While it may encourage engagement with scientists on particular issues and projects, this approach comes, in other words, with a strong 'top-level' sense of what science is and how it should be done, one which is intentionally and forcefully at odds with the way scientists themselves have usually imagined those broadest aims and meanings of their practice.

There is, however, a very different way of tackling the question of ethics and of taking scientists seriously. This way traces how these commitments are lived in practice - to return, in other words, to the question of science as a vocation, as launched by Max Weber, and developed by Merton (see above). As anthropologist Paul Rabinow noted,

> Although each component of Merton's picture of science has been subjected to historical, sociological and philosophical reevaluation, it is fair to say that many scientists believe that these norms guide their practice. Hence, a major gap has developed today between scientists' self-representation and the representations of scientists by those who study them. (Rabinow 1996)

These norms were mostly denounced as ideological cover by an early generation of social constructionists, and ignored by those who chose to focus on the practice of scientists in their labs rather than their accounts of what they did. Finally, 'performative' approaches such as those of Haraway sought to engage them head-on by articulating specific counter-norms (for instance that of the scientist as 'modest witness' (Haraway 1997). Rabinow called, instead, for anthropologists to study them, as they would study any other social practice which exists in tension with particular ideals. That means accepting that norms may never be completely and coherently instantiated, but they can nevertheless guide practice and inform scientists sense of what they are up to and their judgment of each other. Despite the mention of Merton, Michel Foucault's late interest in the subject of ethics and self-formation was perhaps more of a key conceptual influence and guide here.

This interest in norms had strong roots, too, in the history of science. Shapin and Schaffer's account of the controversy between Boyle and Hobbes over the nature of scientific knowledge, *Leviathan and the air-pump* (Shapin, Schaffer, & Hobbes 1985), for instance, gave an account of the way proper scientific experimental procedure was articulated, from the start, in moral (as well as gendered and classed) terms as going hand in hand with a particular 'gentlemanly' ethos of honour and trustworthiness. Later, Shapin returned to the subject – with explicit reference to Merton – to trace the transformations in scientific norms which came with the increasing professionalization of science and the increasingly strong links which developed during the twentieth century between science and industry (Shapin 2008). Daston and Galison's monumental history of changing understandings and practices of scientific objectivity (Daston & Galison 2007), traces the effects of changing instrumentation, new scientific problems, and historical contexts. At its heart, however, the book approaches objectivity precisely as an 'epistemic virtue' - something scientists

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genuinely strive for, although its content may change.

In anthropology, this interest in scientific virtues was bolstered by the broader consolidation of the anthropology of ethics as a field of study (Laidlaw 2014). It became easier to think of scientists at work as – much like persons everywhere – pursuing particular kinds of ethical projects and undergoing particular practices of self-formation. One could point out that scientists were not unique in this respect, and yet do justice to their sense that the aims, goals, and ascetic practices they underwent were distinctive (see e.g. Candea 2010).

In sum, from the diverse and interwoven strands and debates above, emerged three fairly strong elements of advice to the aspiring anthropologist of science: 1) *Don't forget the things*: Pay attention to the power and effects of non-human entities; 2*) Don't stay put*: think about sciences (in the plural) and other knowledges as they interact and intersect in power-laden ways in the world beyond the lab; 3) *Take scientists seriously*: keep in view the real politics of scientific world-building and scientists' own sense of themselves as engaged in particular ethical projects. The best anthropology of science today does all of the above.

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