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The anthropology of science as an anthropology of ethics (and vice versa): elements, problems and possibilities

The first explicit call to approach the anthropology of science from the perspective of ethical self-formation and virtuous conduct (Rabinow 1996a) predated and anticipated the turn to the ethical in anthropology more broadly (Faubion 2001; e.g. Laidlaw 2002) – while in the history of science, the importance of what would later come to be called 'moral economies' and 'epistemic virtues' emerged as early as the 1980s (Daston 1995; Shapin & Schaffer 1985). Yet it is fair to say that despite some important contributions (Fortun & Fortun 2005; e.g. Rabinow 1996b, 1999) the anthropology of science more broadly has not to date seen any systematic or sustained turn to the ethical. As the anthropology of ethics has matured as a field over the past decade and produced a number of increasingly focused and incisive problematics, analytics and debates of the kind exemplified in this volume, this chapter argues that anthropologists of science could benefit from taking a closer and more systematic look at the anthropology of ethics. Conversely it suggests that anthropologists of ethics might gain from a closer consideration of some of the specificities of Euro-American science as an (internally and externally contested, multiple and perhaps part fictitious) ethical tradition. The first half of the chapter explores the (rather fitful) genealogy of an interest in the ethical in the history, sociology and anthropology of science, and points to some of the elements from which the anthropology of science as an anthropology of ethics might be (re)built. The second half focuses in on one extended instance of what such a (re)building might produce, and suggests some of the ways in which anthropology's exploration of both science and ethics might benefit.

A false Friend? Ethics, research ethics and the anethical nature of science

As a starting-point, it is important to disambiguate the kind of "ethics" which are typically the focus for an anthropology of ethics, from the way in which "ethics" are invoked by scientists themselves and laypersons writing or speaking about science, as a separate sphere of concern or activity which must be integrated with or applied to science from the outside – most obviously, for instance, in talk of ethics committees, ethics procedures, 'bioethics' (Rabinow 2003: p.20), and so forth. This vision of ethics – let us call it, for ease of recognition, 'research ethics' – often implies a series of procedural concerns and constraints, a blue-print of proper conduct amenable to bureaucratic regulation. Research ethics in this sense, sound rather more like a species of what Michel Foucault or Bernard Williams have termed morality, by opposition to ethics in the sense of deliberative conduct and reflexive self-formation aimed at certain virtues (Laidlaw 2014: 110–119).[1] One might thus be tempted to dismiss 'research ethics' as a *faux ami* – a topic to the side of

[1] Much depends here on what one takes Foucault in particular to have meant by ethics, and what genuine

the real subject of what an anthropologist of ethics might study in scientific practice. It is certainly true that formal assessments of 'research ethics' typically miss the bulk of what, as we shall see below, an anthropologist might recognise as ethics in the daily life and practice of scientists. Furthermore, a structural implication of research ethics is precisely that the rest of scientific practice is something other than ethical (Rabinow 2011: 175–176). This view of science as "anethical" (Faubion 2011: 94) is directly inimical to the core insight of the anthropology of ethics as applied to science, which is precisely that scientific practices, like all other aspects of human life, are pervasively shot through with ethical considerations. To imagine science as 'anethical' because it is sometimes described as such, including by some practitioners, would be just as much of a category error as describing science as 'ungendered' for the same reason[2].

And yet it would be short-sighted to dismiss research ethics from the frame altogether. After all, the anthropology of ethics has been animated by important debates surrounding the lived relation between ethical conduct and moral rules (Heywood 2015; Laidlaw 2014: 111–119; Zigon 2007). The fact that scientists encounter formalised ethical regimes raises a host of empirical questions: how do different scientists in different settings come to understand and live the relationship between, on the one hand, their own senses of the good, the disciplinary pedagogies they have undergone and the various epistemic virtues they have learnt to orient themselves towards, and on the other hand, the formalised research ethics to which their practice is perforce subjected. Where and when are these ethical and moral planes in tension, and when are they mutually supporting?

Consider for instance the following scene from my fieldwork with animal behaviour scientists in the late 2000s. As we visited the complex of bird cages in her research lab, a PhD researcher complained that the official ethical regulations concerning how birds should be kept, and how often they should be moved between collective and individual cages, had been drawn up based on what she saw as insufficiently fine-grained knowledge of different species' social behaviour, such that to adhere to these rules precisely would actually create distress and discomfort amongst the particular birds she worked with and cared for. Here questions of scientific authority and ethical care were interwoven in complex ways. Ethical regulations of the kind she mentioned were cast as external constraints upon the putatively anethical desires of scientists whose main concerns were thus assumed to be for knowledge production at the animals' expense.[3] In return this particular researcher challenged the epistemic basis from which the particular set of rules she encountered were drawn up, in the name of her own ethical concern for her research subjects.

Indeed, the fact that research ethics seems to imply that science is in and of itself 'anethical' is perhaps what raises the most fascinating questions for an anthropology of ethics. How, to what extent and with what effects have some practising scientists come to experience 'ethics' as an external set of rules objectively detachable from and outside – possibly even an impediment

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measure of freedom and reflexive action Foucault's account is read as providing – a live debate within the anthropology of ethics (Laidlaw 2014; Mattingly 2012 see Heywood, this volume). Peter Pels, for instance, reading Foucault's discussion of ethics as a critique of neoliberal emphases on self-management, finds in it an apt description of research ethics regimes also (Pels et al. 1999). It is probably fair to say that most research ethics regimes operate in practice across this tension between formulaic rules and expectations of self-examination (Strathern 2000)

[2] On the comparison between anthropology's attention to gender and anthropology's attention to ethics, see Laidlaw (2014). The literature on science and gender is huge - some key references are included below.

[3] Thus, for instance, an influential animal behaviour study manual introduces ethical concerns as a negative element in a calculus about whether a particular piece of research ought to be done at all (Martin & Bateson 2007).

to – their 'properly scientific' practice? To ask this is, in effect, to ask how it comes to be the case that science itself came in some quarters to be understood as inherently value-free – and what it might mean to inhabit such a practice as a scientist.

Stephen Shapin has addressed something like the former, historical, question in his magisterial account of the ways in which the moral ordinariness of scientists and the amorality of science rose to prominence as powerful discourses during the course of the 20th century (Shapin 2008). Shapin argues that it was commonplace to assume, into the early 20th century, that science was a calling which required of or bestowed upon its practitioners some extraordinary moral qualities: the contemplation of the works of God, or later of Nature, was widely understood to lend to the scientist a kind of moral grandeur, selflessness, and benevolence, a view shored up by, amongst other things, the un-remunerative nature of scientific work (Shapin 2008: 21–46). By the mid to late 20th century, the "moral equivalence" of the scientist – the thought that a career in science neither required nor bespoke any particular moral qualities, had become equally commonplace. To radically foreshorten Shapin's complex historical narrative, one might point to philosophical shifts in understandings of the nature of truth, the increasing integration of the natural sciences into government and commercial apparatuses during world war two and the Cold war (Candea et al. 2015; see also Rabinow 2011: 102–105), and the invidious critiques levelled at natural scientists by commentators in the social sciences and humanities whose disciplines had not undergone similar transformations (Shapin 2008: 47–92). By the end of the 20th century, "[t]here were just no grounds in the nature of science […] or in the make-up of the scientist […] to expect expertise in the natural order to translate into virtue in the moral order." (Ibid. 13).

Shapin's account historicises what might otherwise seem like a series of obvious banalities about the value-free nature of science and the merely technical and pragmatic nature of scientific practice. While he doesn't connect the rise of these discourses explicitly to the emergence of research ethics regimes, there is a clear correlation between the thought that scientists are no more nor less virtuous than other people, and the suspicion that their conduct ought to be regulated externally.

For Shapin, however, the main aim of this historical account is as a backdrop for a study of the ways in which, despite these commonplace assumptions and despite fundamental transformations in the practice of the natural sciences over the course of the 20th century, practising scientists today are still as fundamentally invested in questions of personal virtue and character as ever they were. The story of a progressive demoralisation of science in which virtuous individuals were replaced by impersonal, institutionalised processes is just that: a story, however influential. Shapin draws on a combination of historical and ethnographic sources and on his own interviews, to examine the new types of character and forms of virtue which lie at the heart of the contemporary science-industry-government nexus. Research managers praise in each other qualities of persuasiveness, trustworthiness and leadership; they pride themselves on recognising and fostering "integrity", "autonomy" and "inspiration" in the research workers they manage {}; venture capitalists look for "commitment", "vision", and a balance between prudence and passion in the scientific entrepreneurs they invest in, and describe themselves as embodying similar virtues (Shapin 2008: 289–303). Crucially, these roles are shifting and mobile, as people and virtues move across the increasingly fluid boundaries between science, industry and government. And yet, Shapin argues, one element of the picture is stable: today, as in the 18th century, the production of authoritative knowledge is inseparable from an economy of personal relations, trust and assumptions about virtue and character. The much-trumpeted move from

personal virtues to institutionalised, impersonal norms has never happened.

Shapin's corrective is important and his book is amongst the clearest manifestos for the study of the ethical in science – even though it does raise some questions concerning the demarcation of epistemic virtues from other virtues, to which I return in the conclusion. For now, the point is that Shapin's account gives a clear sense of why the ethical in his (or Foucault or Laidlaw's) sense is a category which bears little to no relation to the 'research ethics' we began from in this section – and also of why the former version of the ethical may have been overlooked in popular accounts of science in the 20th century. It might be interesting, however, as I suggested above, to look more closely than Shapin does at such "indigenous" invocations of the category of the ethical amongst practising scientists – ethics committees, ethics forms, ethics procedures, etc. – which contribute to the vision of science as an inherently 'anethical' practice to which 'ethics' has to be added as an external constraint.

After Science: Mertonian norms and their critics

The pervasive 20th century discourses documented by Shapin, about the relationship between science and ethics shared one assumption, namely that science could broadly be treated as a unitary object. Debates raged over what would happen to the ethics of scientists as "science" became entangled with industry and government, about the effects of changing university structures on "science" and the like. Shapin's own arguments, by contrast, chime in with the broad consensus in contemporary science studies according to which little can be usefully said on this scale of generality about "science". The thought that science might be a unitary enterprise in ethical terms now seems as unconvincing, in the light of this literature, as the thought that science might be reducible to a single set of epistemic principles or a single transposable method. Few contemporary social students of science would disagree with Thomas Kuhn's forceful claim that "[i]nstructed to examine electrical or chemical phenomena, the man who is ignorant of these fields but who knows what it is to be scientific may reach any number of incompatible conclusions" (Kuhn 1970). Whether one is interested in ethics or in epistemology, the action, contemporary science studies suggests, is in the study of particular disciplinary and sub-disciplinary settings in particular periods.

This is a sound methodological principle, although it suggests a caveat analogous to the one I raised above for "research ethics". The unity of science as an enterprise may have fallen away as an analytical presupposition, but it remains an important source of ethical commitment for some practitioners, and should thus remain in the ethnographic frame. From that perspective it might be worth briefly looking back to a time when the sociology of science imagined the normativity of science in the singular, and tracing how and why this way of asking the question then fell away.

Taking his cue from Durkheim's sociology of knowledge, as well as from Weber's writings on science as a vocation, sociologist of science Robert K. Merton 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). Merton identified five key norms - niftily summarised as CUDOS: Communism (the substantive findings of science belonged to all, not to their discoverers); Universalism (the need for knowledge to be validated through impersonal means, i.e. without reference to the authority of particular individuals or

social groups); Disinterestedness (Scientists had to disengage their personal interests from the reporting of results); and organised Skepticism (new claims were to be systematically challenged). In a classic functionalist vein, Merton's account of science explicitly focused on the relationship between this particular institution and the broader social structure. He noted in particular that

> "the institution of science is part of a larger social structure with which it is not always integrated. When the larger culture opposes universalism, the ethos of science is subjected to serious strain. […] Particularly in times of international conflict, when the dominant definition of the situation is such as to emphasize national loyalties, the man of science is subjected to the conflicting imperatives of scientific universalism and of ethnocentric particularism." (Merton 1973)

An obvious example of this for Merton, a Jewish American writing in the 1930s and 40s, was the perversion of "Aryan science" in Nazi Germany. Ultimately, Merton's sociology of science made a strong claim that a democratic, liberal political structure represents the best (perhaps the only) context for the proper development of science as an institution. It thus simultaneously relativized science, by treating it as just another social institution, and yet ultimately retained both the sense of science as a single, integrated project, and its special status as the prerogative of the liberal West.

A number of sociologists later took issue with Merton's early account of norms. For one thing, as Merton himself acknowledged in later work, scientists in their daily work also clearly valued goods which seemed to be in direct conflict with the five 'norms' he had identified. Thus Ian Mitroff (1974), based on interviews with Apollo moon scientists, argued that while, in their assessments of their colleagues, they often recognised and upheld many of the norms outlined by Merton, they also seemed, at other times, to praise behaviour which ran directly counter to such norms, such as a single-minded attachment to a particular hypothesis in the face of opposition, a steadfast pursuit of personal fame, or a reliance upon, rather than undue scepticism about, established findings. Mitroff sought to extend, rather than challenge Merton's functionalist paradigm, by positing the existence of what he termed "counter-norms" such as "self-interestedness", "emotional commitment", or "dogmatism", which stood in a productive tension with the Mertonian norms. Each set, Mitroff suggested, played a functional role depending on the particular kinds of problems scientists were dealing with: "Whereas the conventional norms of science are dominant for well-structured problems, the counter-norms proposed here appear to be dominant for ill-structured problems."(1974: 594).

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 (Bloor 1999: 82; Shapin 1992). Sociology might explain failures or perversions of scientific knowledge (as in the Nazi case, or that of Lysenko in the USSR), 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. In this sense Merton's picture chimed extremely well with Popper's attempt to frame the distinctiveness of Science as a matter of good method (e.g. Popper 1959). 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.

By the 1970s, however, the social study of science had moved decisively against this way of framing the problem. The so-called "Strong Programme" in the sociology of scientific knowledge was built on an attack against what they termed "asymmetrical" approaches to scientific knowledge: approaches which sought to explain correct findings in relation to nature and incorrect ones in relation to social factors, such as ideology or personal interest. For scholars writing in this vein the task of sociology was to treat knowledge production "symmetrically" by showing the interweaving of social factors in *any* kind of scientific knowledge practice - in the "correct" theories of Mendel, as much as in the "false" theories of Lysenko (Bloor 1999). This approach chimed with the broader turn to Marxist, Feminist and (early-)Foucaultian inspired critical investigation into the social and cultural construction of scientific facts[4]. Much of the early anthropological attention to science shared in these sensibilities and concerns. The optic shifted from attempts to characterise the moral structure of (good) science, imagined as a "system", towards an attention to the ways in which particular scientific endeavours, practices and findings were continuous with and perhaps occasioned by, as well as feeding into 'naturalising', broader sociological realities, such as gender, race or class (Haraway 1989; Martin 1991, 1994; see for instance Traweek 1988). These critical accounts shifted attention away from, when they didn't represent a direct attack on, the kind of ethico-epistemic generalities about "Science" which Mertonian sociology had foregrounded. Mertonian sociology had assumed that science was epistemologically unique, and sought to explain this uniqueness in terms of its moral and institutional structure (Collins 1982). Social constructionists, relativist and other critical theories started by bracketing this assumption of uniqueness, producing instead "diverse studies of the local practices of science [which] have sought (with some success) to lower-case the abstractions of Science, Reason, Truth and Society" (Rabinow 1992: 7).

Actor-Network Theory, the great contestant of these social constructionist approaches, which also coalesced during the 1980s, drew attention away from Mertonian norms in a radically different way, by foregrounding the importance of non-human actants in explaining the distinctive power of scientific knowledge making (e.g. Latour 1987). Despite the increasingly clear differences which developed between ANT and critical social constructionist accounts (Bloor 1999; for some of the contentions and tensions, see Latour 1990; Martin 1998), they shared one important feature: neither had a particular *ethnographic* interest in norms or ethics (Candea 2018a; Rabinow 1996b). The critical social constructionist literature – particularly the strand which, partly recombined with insights drawn from ANT, came to be known as 'feminist technoscience' – was itself of course intensely normative, actively invested not only in critiquing the shortcomings of classic forms of scientism, but also in proposing alternative 'ethical' repertoires for doing science, which would be more in tune with the commitments of "antiracist, feminist, multicultural, and radical science movements" (Barad 2007; Despret 2004; Haraway 1997: 267; Latour 2004; see also Stengers 2000). While these explorations led to occasional

[4] Latour and Woolgar's celebrated *Laboratory Life (1979)* was arguably an instance of this approach, despite Latour's later critiques of Strong Programme Sociology (see below).

strategic alliances with the ethical visions of some individual scientists who espoused similar concerns, they were in the main aiming to challenge and transform the ethics which inform the conduct of the majority of practising scientists, rather than to document them in the way anthropologists typically seek to document the commitments and perspectives of the people they study (Candea 2013a, 2017).

Ethnographies of science and histories of epistemic virtue: the elements of an anthropology of science as anthropology of ethics

While they drew attention away from the study of scientific norms, these developments did lay the grounds for an anthropology of ethics approach in one important sense, by forcefully shifting the optic from generalities about Science, to an attention to the particular modes of operation of particular scientific disciplines and controversies in scientific settings. The sociology, history and anthropology of science in the 1970s, 80s and 90s was increasingly populated with accounts of the practices of physicists, biologists, primatologists, immunologists, computer scientists and the like. The increasingly fine-grained documentation of the sheer diversity of practices, material settings, epistemic commitments and effects of scientific endeavours provided a canvas upon which an account that went beyond Durkheimian visions of functional norms, might begin to be articulated. In order to do this, however, something of the spirit of Merton's initial agenda had to be rescued from the critical consensus. Paul Rabinow articulated this point forcefully in the introduction to his book *Making PCR (1996b).* Reviewing the state of science studies at that moment, Rabinow noted that while a wealth of critical insight and close description of scientific work was now on offer, the loss of the old Mertonian questions had led to an important kind of ethnographic deficit:

> "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 1996b: 17)

By asking the question of how practising scientists themselves envisaged the goods to which their practices tended, Rabinow anticipated some of the ways in which anthropologists of ethics would later point to the high-handedness of critical accounts that thought fit to leave out people's own sense of why they did what they did, and explain their actions instead by reference to other forces and factors (e.g. Laidlaw 2002, 2014). In essays published the same year (1996a), Rabinow explicitly invoked Foucault's four-part scheme for the study of ethics, suggesting that a reconfiguration of Merton's problematics today would actually ask about the ontology, deontology, ascetics and teleology of particular scientific practices.

It is perhaps not surprising that Rabinow, one of anthropology's most prominent Foucault commentators, called for what was effectively an anthropology of ethics some years before this was proposed as such by others.[5] Yet Rabinow's own instantiation of his programme did not look much like the rich ethnographic accounts of ethical self-formation and pedagogy which the

[5] Rabinow was also drawing on work in the history of science (e.g. Shapin 1994) to which I return below

anthropology of ethics would later produce. Rather, Rabinow's work took the form of close and rather densely written accounts of the career of particular scientific projects, techniques and individuals (Rabinow 1996b, 1999, 2012) in which the question of the ethical was threaded throughout more than it was systematically teased out. A number of insightful and illuminating observations were made along the way, albeit often – as is the case of much good history and ethnography – without a very easy summative take-home point. Partly, the difficulty and also the richness of this work comes from Rabinow's consistently reflexive commitment to keeping anthropology's own knowledge practices in view alongside the practices he uses anthropology to account for, an increasingly prominent theme in his later writing {}. Thus some of the clearest instances of what his proposed anthropology of science as anthropology of ethics might look like come from systematic comparisons between *his own* ethical orientations and those of one of his key scientific interlocutors (Rabinow 1996a: 162–187) or between his and Bourdieu's respective "ethical styles" (ibid. 16-25).

It is perhaps fair to say that, as a result, the influence of Rabinow's calls for a study of the ethical on the anthropology of science has been diffuse, rather than focused. The ethnography of science, still described as an "emergent" field a decade later (Fortun & Fortun 2005), did not experience anything like a full-scale turn to the ethical, being more directly influenced by the theoretical echoes of feminist technoscience and Actor-Network Theory.

Historians of science, on the other hand, engaged with these questions earlier and in a more sustained way. In an important article written a year before Rabinow's book, Lorraine Daston had already reviewed a range of works in the history of science which had been tending towards an account of what she proposed to term the "moral economies" integral to science, "to its sources of inspiration, its choice of subject matter and procedures, its sifting of evidence, and its standards of explanation." (Daston 1995: 6). The rise of quantification, for instance, was interwoven with appeals to particular "mathematical virtues" such as impartiality, precision and communicability, which also drew on and helped to consolidate particular kinds of scientific collectivities (Daston 1995: 8–12; Porter 1992); empiricism made epistemic virtues of the trust, civility and curiosity embedded in 17th century gentlemanly culture (Daston 1995: 12–18; Shapin 1994; Shapin & Schaffer 1985). As for objectivity – a moral economy in it own right – Daston began in this article to detail its diverse incarnations, a project which she expanded upon systematically with Peter Galison in *Objectivity (2007)*, a book that traces the historical transformations of this epistemic virtue – whose various forms included trained judgement, mechanical distantiation, or a yearning for a perspective of no perspective – and the changing ways in which scientific selves came to be shaped around it.[6]

In the meantime, while anthropological ethnographies of science did not, pace Rabinow, take up the ethical as a core concern, what they did do, however, is accumulate increasingly rich accounts of scientific practices in which some of the later concerns of the anthropology of ethics were prefigured. Tellingly, in one of the few papers on the anthropology of science in which the ethical is elevated to title status, Fortun & Fortun (2005) while they mention Rabinow, trace the roots of their interest in self-formation (alongside Foucault) to an earlier ethnography: Sharon Traweek's comparative ethnography of American and Japanese particle physicists in *Beamtimes and Lifetimes (1988)*. While Traweek did not herself mention the ethical as a focus for analysis,

[6] In Daston's 1995 article, Foucault's work on self-discipline in *Discipline and Punish* (1979), gets a passing mention. A decade later, Daston and Galison's *Objectivity*, like Shapin's *The Scientific Life*, came to draw explicitly on Foucault's late writings about ethics, virtue and self-formation – and in the case of the latter, on Rabinow's work.

and while her book has been cast, not without reason, as exemplary of the critical social constructionist genre (Bloor 1999), Fortun & Fortun rightly note that her focus on subject formation, pedagogy and mentoring prefigured some key concerns of the ethical turn.[7] Particularly productive in this regard is Traweek's attention to subtle differences and varieties in the way epistemic goods are conceived.[8] For instance, Traweek explored the ways in which the different architecture of detectors build by particle physicists came to embody what one might term in retrospect different epistemic virtues,

> "LASS is spare and elegant, meant for refining accepted but little understood knowledge. SPEAR is ingenious architecture, meant for reconstruction and deconstruction. The ESA is fat and overbuilt, meant to be reliable." (Traweek 1988: 72)

Traweek also noted the differences in these technically-mediated epistemic orientations introduced by different ways of organising careers and scientific labour in the US and Japan respectively: whereas the American researchers tended to work at a number of short lived detectors during their careers, and build up a competitive community in which experimenting with diverse architectures is part of a shared sense of "how to do good physics", the Japanese researchers had mostly worked at one detector for their entire careers and passed these long-lived detectors on to new generations sharing a commitment to strong stable research groups (ibid 72-73).

There is much to be gained in mining older ethnographies of science with this retrospective attention for the ethical. For instance, Hugh Gusterson's ethnography of US nuclear weapons scientists and anti-nuclear activists doesn't foreground ethics in the Foucauldian sense as a category of analysis - where the term appears, it is used in the ordinary sense in which the subjects of the study themselves might invoke "the ethics" – the rights and wrongs – of nuclear weapons research (Gusterson 1998: 49–59). Yet, like Traweek, Gusterson actually provides a powerful account of processes of self-formation (cf Cook this volume) and pedagogy (cf. Faubion this volume). He describes the ways in which nuclear weapons scientists' training fosters particular practices of detachment and distantiation from the vulnerability and subjectivity of their own bodies and those of others - from jokes and pervasive mechanistic metaphors of the body, through to specific techniques of visualisation and numerical accounting for mass deaths. Gusterson contrasts these techniques to the various ways in which anti-nuclear protesters learn, on the contrary, to make their own bodies and those of others visible and palpable. While there is an explicit element of Geertzian culturalism to Gusterson's approach, and while he himself occasionally casts the processes above in terms of socialisation (Gusterson 1998: 4–5), the book is fundamentally motivated by a question about the pursuit of incommensurable goods (a longstanding concern in the anthropology of ethics. See for instance Laidlaw 1995; Robbins 2013). This research began, Gusterson writes, when, as an antinuclear activist in 1980s San Francisco Bay, he encountered for the first time a nuclear weapons scientist who, he realised "believed passionately that his work, far from being dangerous, was important and honorable"

[7] although somewhat surprisingly given this praise, their own piece gives little of that kind of ethnographic description, remaining rather more focused on broad discursive shifts in understandings of toxicology and its role as a civic science, which recall rather the mode of exposition and arguments of historians such as Shapin.

[8] A subtlety which goes beyond or lies beneath her often rather blanket arguments about the way these scientists project their gendered assumptions onto Nature - the sorts of arguments which led Latour for instance to dismiss her work as mere Durkheimian social constructionism.

(Gusterson 1998: xi). In seeking to understand the various ways in which that orientation makes sense to weapons scientists, and with what caveats and contradictions, the account prefigures some of what anthropologists of ethics would later write about the pursuit of particular forms of virtue in difficult and uncertain circumstances (Mahmood 2005; Marsden 2005; Pandian 2009; see for instance Robbins 2004).

Perhaps the most thoroughgoing ethnographic exploration of the diversity of epistemic virtues in scientific practices, however, comes from a sociologist rather than an anthropologist of science. Karin Knorr-Cetina's comparison of what she terms the "epistemic cultures" of High Energy Physicists and Molecular Biologists (1999) contrasts the practices, languages, material technologies and social organisation of each discipline. The resulting comparison highlights the deep heterogeneity of the types of epistemic virtues associated with each disciplinary setting. High-Energy Physicists encounter natural objects briefly and intermittently, in rare experiments, in which empirical reality is heavily mediated by complex machinery. Most of their time is spent focusing inwards on the design and redesign of experiments, and the checking, cross-checking and cleaning of the resulting data to hunt down interference and error[9]. By contrast, molecular biologists have a seemingly unending supply of empirical materials at their disposal in the mass-produced animals and biological samples they manipulate at the bench every day. Rather than invest their time and effort in the meticulous understanding and documentation of what happened in any particular encounter with biological materials, they multiply and repeat experiments, vary procedures and protocols through a process of blind variation until something works. Where physicists turn inwards towards a 'negative knowledge' about the limits of their knowledge, molecular biologists tinker and accumulate wisdom about pragmatic procedures. Whereas physicists work with signs painstakingly produced by machines and "encircled [these] by more signs, which were used to interpret the former and to specify their range of variation and effects" {101}, molecular biologists privilege "[t]he body as a silent archive of experience, competence, sensory information processing" (ibid. 100).

In sum, despite some isolated calls (Fortun & Fortun 2005; e.g. Rabinow 1996b, 1999), an anthropology of science as anthropology of ethics has not yet got off the ground as a systematic project. But anthropologists of science interested in the ethical, and anthropologists of ethics interested in the subject of science, can look back over the past three decades to a number of elements which together form something like the building blocks of such an approach: explicit theorisations of the importance of the ethical, historical investigations into epistemic virtues, and rich ethnographic descriptions of scientists at work trying to realise and reconcile specific, diverse values. The next section gives an instance of how these different elements might be recombined in one particular case, and draws some conclusions about what the anthropology of science might distinctively bring to the anthropology of ethics, and vice versa.

People of the why: Becoming behavioural biologists

My own ethnography of behavioural biologists, to which I briefly refered above, was concerned from the start with questions of epistemic virtue and ethical self-formation - particularly the complex ways in which researchers balanced engagement with and detachment

[9] an orientation which Knorr-Cetina describes as a 'care of the self', by reference to Foucault - although the 'self' here is the experiment, rather than the self of the individual scientist.

from the animals they studied (Candea 2010). However, this focus was initially approached in a rather piecemeal way, and only slowly cristallised into a more sustained engagement with the literature and problematics of the anthropology of ethics as work progressed (see for instance Candea 2018b, 2018c). In this section, I will draw together some of the key strands of these various arguments into a more systematic outline of the ethical practices of self-formation in this setting.

One core focus of this description is a long-term behavioural ecology research project based at a field station in the South African Kalahari desert, where researchers and volunteers observe groups of wild meerkats which they have habituated to human presence. The description also draws on time spent with the members of the research group run by Tim Clutton-Brock at Cambridge which runs the Kalahari Meerkat Project (KMP), at behavioural ecology conferences, as well as on documentary and historical work on the development of behavioural ecology. Much of the description below, however, focuses on one particular set of actors within this extended setting: the volunteers. The KMP project relied on a constantly renewed team of 20 or so volunteers, typically recent graduates in biology from the UK, who had elected to spend a year at the isolated farmhouse in the Kalahari desert collecting data and tending to the ongoing habituation of the meerkats - each was also encouraged to undertake a small research project based on the project's data, mainly as a training exercise. For many of the volunteers, their time at this prestigious research site represented the first step towards a career in behavioural biology. Others saw this as a path to a career near to but not directly in science - from work in conservation or applied zoology, through to science writing or documentary film-making. For most, however, their time at the KMP was precisely an open-ended experiment - an opportunity to get extended first-hand experience of scientific research, after their undergraduate training in biology, in order to decide whether to take further their commitment to "the life scientific". The volunteers are thus in one sense 'marginal' actors from the perspective of an account of behavioural biology, and yet precisely for that reason, they provide an ideal subject for an understanding of what becoming a scientist entails, and why one might choose to do so - or not.

These subjects' distinctive position in the hierarchy of scientific knowledge production also raises interesting questions about the scale and the nature of the ethical form of life which they were engaged in. Some of their pronouncements about the nature of science and what it requires of a person might sound naive to professional scientists and their sociologists, who might hold, with the Apollo moon scientists interviewed by Mitroff, that

> "the only people who took the idea of the purely objective, emotionally disinterested scientist literally and seriously were the general public or beginning science students. Certainly no working scientist, in the words of the overwhelming majority, "believed in that simple-minded nonsense."" (Mitroff 1974: 588)

As we saw above, the bulk of science studies seems to have sided with this rather dismissive assumption that ethico-epistemic generalities about "science" are just simpleminded nonsense, good for neophytes (or philosophers), and that accounts of epistemic virtues need to be sought in a more fine-grained study of particular practices. But, aside from the usual anthropological commitment to taking one's informants seriously, which should make one pause before dismissing their views, what is of particular interest to me in volunteers' invocations of "science" is precisely the complex way in which generalities about being or becoming "a scientist" were experienced and encountered by these actors through the more specific form of being or

becoming a "behavioural ecologist" – and yet, these generalities were never entirely eclipsed by the particular form, contrary to what some work in science studies might lead one to assume. One question this material therefore poses, as we shall see below, is precisely that of the coherence of science (both internally and externally) as an object for the anthropology of ethics.

As a starting point for this account, I will borrow a device from Rabinow (Rabinow 1996a: 16–25) which is to provide a condensed 'miniature' of an ethical form of life by parsing an otherwise broad and disparate set of observations through Foucault's four-part typology - ethical substance, askesis, mode of subjectivation, and telos. For a sustained account of these four terms as heuristics for an anthropology of ethics, see for instance Faubion (2011), and Heywood (this volume) - I am using it here merely as a convenient jumping-off point to draw out a number of observations, while fully aware of course that this hardly exhausts what can be said about the ethical in any given setting.

1. **Ethical substance**: *"what is the aspect of myself or my behaviour which is concerned with moral conduct" (Foucault 1994; as quoted in Rabinow 1996a: 16)*

The most obvious and immediate ethical substance in this case was, perhaps unsurprisingly, the mind, envisaged as a centre of perception, reasoning and emotion. Some fairly precise meta-cognitive reflection (cf. Mair 2012) went into the identification of particular virtuous mental states. The broadly 'scientific' attitude of skeptic detachment from immediate experience, for instance (Sloterdijk 2012), was specificially instatiated here in a set of proper mental attitudes concerning animals, often indexed by ways of speaking about them and an ability to maintain the propriety of distinct registers. Being able to speak and think 'scientifically' in this context often meant learning to describe behaviour in ways which minimised attributions of intentionality, and to maintain a vigilance about one's own 'anthropomorphic' assumptions (Candea 2013b; cf. Daston & Mitman 2005). The emotional make-up of the good scientist, here as elsewhere, was depicted in the light of contrasting yet complementary virtues of detachment and distance on the one hand, enthusiasm and passion on the other.

In practice however, and contrary to some banalities about the mind-body dualism of Euro-American persons, the mind here was fundamentally and pervasively an embodied mind. For instance, learning to become a scientist as a behavioural ecologist was crucially about learning to *see* – from the fundamental shift in optic of learning to see action as behaviour (to which I return below), to the mundane skills of being able to discern individual animals (Candea 2018b). Injunctions about maintaining a proper balance of engagement and detachment in relation to one's research subjects was as much about practical control of one's bodily movements around meerkats as they were about emotional mastery; to be "a scientist", also entailed a particular embodied relation to one's emotions, for instance the un-squeamishness of eating lunch while watching an animal being dissected (Candea 2018b).

This particular example points also to a striking and persistent aspect of this setting, to which I return below under the heading of modes of subjectivation: being 'a scientist' or being 'scientific' was frequently described as one part of what a (good) whole person is. Thus two volunteers, talking about the dissection they had just witnessed, commented using what was a broadly understood local vernacular about parts and wholes of persons:

> Ally: "I guess at the end of the day we all are scientists, and that's probably what drew people here, I guess, maybe? And that science part of [you] is like 'ooh, dissection!'" Sue: "... but that's just part of you..."Ally: "and then the part of you that's more personal and

emotional or whatever might be like, 'oh, I knew that meerkat'." (Candea 2018b)

It is worth pausing briefly on the emotional complexity of this example: the contrast is not straightforwardly or only between a detached, cold scientist who can tolerate the death of an animal and the emotional layperson who cannot (cf. White 2005), although volunteers themselves sometimes cast things in this way when they denigrate outsiders to their scientific community such as the viewers of the documentary Meerkat Manor (Candea 2010). Rather the contrast evokes emotions, passions and enthusiasms on both sides, but these are differently oriented: if the 'emotional' part mourns the passing of a known non-human person, the 'scientific' part is manifested precisely by a contrasting set of *emotions*: excitement and passionate curiosity about the discovery of a previously opaque natural process (ooh dissection!). One could read the point backwards: the 'personal, emotional' part of the self is distanced and detached from the fascinating workings of meerkat biology, by their emotional attachment to living meerkat individuals. The broader point here, however, is that the ethical substance upon which scientific virtues were understood to operate, was not the whole self, or the whole embodied mind but rather an aspect of it. I return below to what this might tell us about behavioural ecology, 'Science' writ large, and the question of partible personhood.

Another distinctive aspect of this setting was the role of non-human entities. Building on the strong tradition in the anthropology of science of paying attention to the way knowledge-making is distributed across human persons and their technical apparatus, this case suggests that one might imagine the ethical substance stretched out beyond individual persons and their conduct to take in various material elements of the research environment. Thus we would miss something essential about the substance of some locally key epistemic virtues such as reliability, thoroughness and systematicity in data collection, if one sought to attribute them entirely to the embodied minds of individual researchers. Just as certain forms of objectivity are only understandable as the effect of a relation between scientists and photographic equipment (Daston & Galison 2007), these key virtues emerged and were instantiated in the interaction between careful researchers and well-maintained machines: from the carefully programmed hand-held devices that guided observers as they 'collected' units of behaviour in the field (Candea 2013c), to the painstakingly curated project database files, millions of lines of data, each painstakingly entered by hand on the project computers and cross-checked by a data manager. What Fortun and Fortun (2005) have described as "care for the data" was very much in evidence here, and it required also, a care for the equipment, which in turn scaffolded individual observers' ability to be reliable.

To say this is not to render insignificant questions of human freedom and responsibility, as in Laidlaw's slightly severe reading of Actor-Network Theory (Laidlaw 2014: 183–188). It is only to suggest that in this case, as in other ethnographic accounts of scientific practice, the feedback loops between persons' careful maintenance of technical apparatuses and the reliability of these apparatuses which in turn scaffolds and upholds that of the persons who maintain them, might usefully be treated as a complex assemblage of ethical substance. The pervasiveness of these kinds of human-nonhuman assemblages in scientific practices might make science particularly good to think with for anthropologists of ethics in this respect.[10]

[10] Of course one key particularity of this case resides in the fact that some of these nonhumans were live, interactive, and indeed social animals. This raises the possibility that the meerkats in this context might be seen as something more than what the account has suggested so far, namely as actual ethical actors in their own right. In particular, Donna Haraway's discussions of 'response-ability' (Haraway 2008) has sought to bridge the human

2. **Askesis**: *"The work that one performs on oneself, not only in order to bring one's conduct into compliance with a given rule, but to attempt to transform oneself in to the ethical subject of one's behaviour". (Foucault 1994; as quoted in Rabinow 1996a: 17)*

Aside from the actual practice of volunteers and other researchers, to which I turn in a moment, there is a particular body of literature surrounding behavioural biology that yields valuable insights about *askesis*: the methodology manuals aimed at introducing students to field methods and the measurement of behaviour (Candea 2018b; Dawkins 2007; Lehner 1996; Martin & Bateson 2007). These works outline, and in their form dramatise, the kinds of work on the self that is required to progress as what I have called a "nascent-observer". As I argued at greater length elsewhere, these manuals don't simply give readers the pragmatic tools to become meticulous, careful and imaginative researchers – although they certainly aim to do that too. But more profoundly, they invite their readers to learn to see *action* differently - both that of animals but also their own. This can take the form of an awakening from the idle reverie of life to notice that an animal's activity is in fact mysterious (Dawkins 2007), a careful examination of the grounds of one's unconscious intuitions about an animal's motivations (Tinbergen 1960) or a direct challenge to these intuitive assumptions (Martin & Bateson 2007). In all of these cases, the 'nascent-observer' is simultaneously being asked to observe herself; "to be conscious" as Ruth Benedict once put it "of the eyes through which one looks." (2005: 22). Congruently with what I argued above, these manuals often invite nascent-observers not only to see otherwise, but also to 'see double': to see animals as *both* intentional actors *and* mechanistic behavioural engines; to see themselves as both active, learning scientists, and as "instruments for measuring behaviour in much the same way that say, a thermometer is used to measure temperature." (Martin & Bateson 2007: 74). I argue in that piece that the achievement of such double visions relies on a pedagogy of 'double binds' (Bateson 1972). In characterising and re-charaterising animal action in various ways, these manuals repeat the action of showing something to be true on one level yet also not true (a mere metaphor or heuristic) on another. It thus becomes ultimately unclear which, of seeing an animal (or a person) as an intentional actor or as a machine-like device, is a literal description and which a merely convenient thought experiment. Here we find again the imbrication of the general and the particular. The effect of these perspectival games is, in one sense, very specific to the discipline of behavioural ecology, - it could be read as a contingent historical accretion of very different ways of seeing derived from naturalism, behaviourism, and sociobiology (Crist 1999). Yet on another scale, the effect of these manuals is also to instill epistemic virtues often associated with "Science", writ large: a sceptical attitude of de-familiarisation towards immediate experience (Candea 2013b), and an inquisitive interest in what lies beneath or beyond appearances.

Moving on to the ethnographic observation of what volunteers actually did in practice at the Kalahari Meerkat Project field station raises a particularly interesting analytical challenge surrounding the notion of askesis and self-cultivation: how can one distinguish the regular performance of a task or conduct which exhibits or requires certain virtuous dispositions, through which performance a subject progressively gets better at this task or conduct, from

exceptionalism of discussions of ethics and a kind of vitalist eco-etho-ethical perspective in which ethical action is an emergent property of interspecies assemblages. This raises complex questions which don't pertain directly to the topic of this chapter, which is scientific practice - I refer readers to Rosie Jones-McVey's chapter in this volume.

genuine self-cultivation in the sense of a purposeful "attempt to transform oneself in to the ethical subject of one's behaviour"? Thus, in light of the points made above, it is tempting to describe a number of the daily activities of volunteers as forms of askesis: the daily discipline of waking up before dawn to collect one's equipment, partake of a swift breakfast and ship out to various parts of the reserve, the painstaking, silent trudge behind one meerkat group for hour after hour under the increasingly hot sun, carefully keeping up one's concentration in order to be able to tell apart one dusty brown body from another, the occasional scramble to enter lines of behavioural code into one's hand-held device as a group of meerkats suddenly flew into a flurry of activity, the dedicated, patient input of lines of data after one's field session, and so forth. Volunteers themselves frequently described their time at the project in terms of progress in these and other abilities: they were getting better at seeing behaviour, at recognising individuals swiftly and accurately, at detecting and correcting anomalies in data, at managing their interactions with meerkats; as they undertook their own individual research projects, at understanding what counts as an interesting research question, a solid piece of evidence, a good hypothesis.

Yet there is an element of sleight of hand in describing *all* of these practices as instances of self-cultivation, which comes clearly into view once one remembers that only some of these volunteers were actively and single-mindedly in the process of 'becoming scientists'. For absolute beginners, for many of whom the field station was an experimental clearing house of future plans, and even more so for experienced volunteers who, at some point during their stay had decided that the life scientific was not for them, these practices would surely be better described simply as 'conduct in compliance with a rule'. The peculiarity of this situation shouldn't hide the broader epistemic difficulty: if the characterisation of a practice as askesis (by contrast to mere compliance) relies on the practising subject's commitment to the particular transformative goal of that practice, there will be many cases in which the difference is ethnographically unknowable, slight or fundamentally fluctuating. In this setting as in others, decisions about one's path in life are often taken slowly, hesitantly and rethought, reconsidered and reversed. It is reasonable to suppose that the same volunteers at various points in their stay, maybe in various moods, perhaps at various times of day, may have switched between an engagement with these practices as valuable and purposive self-transformation, and experiencing them as tedious rule-following. This opens up on to much broader questions concerning the internalism or externalism of anthropological accounts of ethical self-cultivation. Given the common, albeit distant, roots of anthropologists and behavioural scientists's respective epistemic traditions, it is perhaps not entirely surprising that this descriptive problem sounds a little bit like the descriptive oscillations I have described above in behavioural ecology manuals. This rejoins the suggestion, made by some anthropologists of ethics, that we migght wish to cultivate the ability to see our informants action double - as both ethical self-cultivation and rule-following (Clarke 2015; Cook 2010; see also Mahmood 2005).

3. **Modes of subjectivation**: "The way in which the individual established his relation to the rule and acknowledges oneself to be a member of the group that accepts it, declares adherence to it out loud, and silently preserves it as custom." (Foucault 1994)

This is a particularly thorny question, which takes us back to the difficulties surrounding the notion of 'Science' as a unitary ethical form of life. In describing the complex of actions and virtues I have outlined above, volunteers themselves frequently invoked 'science' as the practice to which these belonged, and refered to themselves as 'scientists' insofar as they pursued these

forms of conduct. Yet the previous section highlights the extent to which their practice was a historically and materially specific one - not simply 'science' but 'field behavioural biology'. Field behavioural biologists are scientists who work with mostly whole, live animals. Volunteers were learning, not only to be 'scientists', but more specifically to be the particular kind of scientists who must cultivate precise, contextual knowledge of a group of individually named wild animals – a knowledge necessarily scaffolded by affective engagement with these animals – while also learning to disagregate their behaviour into elements of a data stream for statistical analysis, and to explain their actions in terms of genetic and environmental forces that have nothing to do with the intentions and purposes which they might read off the individual animals themselves. In other words, field behavioural ecology is, like so many contemporary scientific disciplines, itself a composite: it recombines practices and epistemic virtues drawn from fields as diverse as ethology, primatology, behavioural psychology or population ecology. Yet it provides a distinctive mode of subjectivation in relation to widely shared epistemic virtues.

In Paolo Heywood's helpfully clear formulation, given a broadly shared code, "a mode of subjectivation is the way in which an individual subscribes to such a code […] the manner in which they are 'invited or incited to recognise' (Foucault, 2000: 264) that moral obligation" (Heywood 2015: 211). Take for example "detachment" which might be invoked as a "rule" of sorts for many if not all scientists. To be even more precise, let us consider three types of biological scientists who work with animals, and subscribe, as a moral obligation to a kind of detachment from them. Disciplinary and sub-disciplinary differences affect the way in which one might establish a relation to such a rule. For molecular biologists, for instance, detachment might involve learning to remain unaffected while sumarily killing laboratory mice, because this is a pragmatic necessity in order to access important physiological data. A number of students of animal cognition I spoke to, by contrast, were unambiguous about being personally opposed to – and feeling that they would be unable to countenance undertaking – invasive work on the animals they studied, and did not feel that this impugned in any way their credentials as scientists. For them, detachment still featured as rule, but it emerged in the context of learning to distance oneself from one's immediate assumptions about what an animal might be thinking or intending, in order not to contaminate one's reading of the results of experiments in which the animal's cognitive abilities were being tested (Candea 2013b). In that respect, they were just as emphatic as any 'wet biologist' might be, in contrasting their own superior capacities for detachment to those of their lay friends or family members. For the bulk of the work undertaken at the KMP, by contrast again, questions of the intentional state of meerkats were fairly marginal, and meerkats provided data whilst alive rather than dead. There, the main way in which detachment – still an important rule – featured scientifically, was in relation to non-interference into the natural lifecycle of the animals. Habituation, for instance, was a procedure calibrated to enable closeness to the animals while – ideally at least – having little or no effect on how much the meerkats ate, for instance, or how likely they were to be predated by other animals (Candea 2013a). Detachment was mainly a rule ensuring that the animals remained what they crucially needed to be for the project to have value – *wild*.

These three cases show the diversity of modes of subjectivation that might attach to the superficially similar code or rule, here detachment from animals. Yet we also saw above, that all three of these modes of subjectivation were in fact in evidence amongst volunteers at the KMP. They occasionally expressed concern at removing attributions of intentionality from their language, in ways akin to those of cognitive scientists; they occasionally took pride in their lack of squeamishness, as a laboratory biologist might. But the maintenance of properly distanced

interspecies relations in the habituation-wildness sense described above was a much more sustained, fundamental and all-encompassing aspect of their practice. This speaks to the fact that these volunteers were not just on the margins of the professional scientific community - those of them who aimed to pursue a career in science, were in the process of turning from 'natural scientists', or perhaps 'biologists' in an undergraduate sense, into 'field behavioural biologists'. In the process, their generalities about the epistemic virtues of science were in the process of being whittled down and sharpened into more precise sense of the epistemic virtues of field behavioural biology. Their mode of subjectivation was in the process of shifting from a broader concern with relating to 'detachment' as a scientist, to the more specific recognition of a particular kind of detachment which is epistemically virtuous for a field behavioural ecologist. To see this process in action opens up a comparative question, again, for anthropological accounts of ethical 'traditions' in other settings. While much has been written about internal diversity and (in)coherence (Pandian 2008) and about historical changes (Asad 2003; Marsden 2005; Robbins 2004; Yan 2009) in ethical traditions, this case suggests that it might be worth asking also about the ways in which particular persons' trajectory through an ethical tradition might become specialised (and not simply, say 'intensified' or progressively more skilled), or conversely, how what is on one scale a tradition might simultaneously contain other traditions within itself.

Just as thought-provoking is the fact that, if behavioural biology is only a part of science, science itself was only a part of what these volunteers saw themselves as cultivating. I noted above that volunteers occasionally referred to distinct scientific and non-scientific 'parts' of themselves. This was a vernacular I encountered with various modifications, amongst other professional animal behaviour scientists. I have suggested above how this sense of 'double vision' might be rooted in the methodological and conceptual particularities of field behavioural biology. This particular, precise disciplinary dualism gives form and substance to the broader and perhaps 'naive-sounding' dualisms the volunteers invoked between being a scientist and being emotional. Yet one ought not, I think, rush to dismiss those broader formulations. For, on the other hand, the figure of the double person, who is *both* a scientist *and* an 'emotional person' (another informant – a post-doc, rather than a volunteer – described the latter as 'the animal-hugging part of me') has a broader significance beyond the specific disciplinary context it is rooted in. It suggests an internalisation of the widespread discourses about the anethical nature of science described in the first section of this chapter. The volunteers were drawing on broad stereotypical depictions of science as cold, detached, unemotional and amoral even in its enthusiasms, in order to make sense of their experiences as budding field behavioural ecologists. In so doing, they find that while these depictions capture something, they can hardly describe the totality of their person, which therefore emerges as divided or split between different contrasting parts: science on one side, emotions on the other. To put it otherwise, since being 'a scientist' is popularly cast as something slightly less than being a total person, actual scientists come to understand 'being a scientist', recursively, as being part scientist, part (lay)person.

This opens up interesting comparative questions for anthropological accounts of self-formation more broadly. Mark Mosko, in a provocative article suggested that Christian individuality might be re-analysed as a species of partible personhood such that

> the seeming "individuality" of Christian persons consists merely in singular moments of overarching processes of elicitive detachment, gift-transfer, incorporation, and reciprocation whereby the constituent parts of total or overall dividual persons are transacted (Mosko 2015)

Might one make a similar claim about scientific individuality in a world in which 'being a scientist' is conceived of as something that persons are while only ever being a part of who they are? If so, this case stands as evidence that partible personhood and ethical self-formation are in no way mutually exclusive (cf. Humphrey 2008; Laidlaw 2017). One might of course counter that less theoretically contrived accounts of such cases as simply instances of 'value pluralism' might fit the bill just as well. There is certainly much to be said about the pursuit of different values and conflicting virtues in this setting, as in many other contemporary scientific settings (Shapin 2008). But asking, additionally, about the subdivision of the self into partial ethical substances, each with a different mode of subjectivation, does open up some distinctive questions which resonate in this case and might provide food for thought to anthropological accounts of ethics in other settings: how are the different parts of the person worked on in different ways, put in tension, reconciled or, as Mosko puts it, 'transacted' both within oneself and with others? In sum, what does self-formation look like in settings in which the self in question is understood as partible?

4. **Telos** "that activity in which one finds the self" (Foucault 1994; as quoted in Rabinow 1996a: 22)

Understanding the telos of the practices undertaken at the Kalahari Meerkat project is in a sense the capstone of this description. This requires a detour through the history of biological theory. This slightly arid discussion repays the effort, however, as it leads up to an intriguing observation: the key telos of this scientific form of life lies precisely in discovering the telos of animal life. Behavioural ecologists are the particular kinds of scientists who find meaning and purpose in finding meaning and purpose in the actions of animals, in a very specific way.

Niko Tinbergen, one of the founding fathers of the biology of behaviour, famously wrote – loosely and implicitly echoing Aristotle's thoughts on the nature of explanation and causality (Tinbergen 2005) – that four main questions can be asked of any animal behaviour: one can ask about the physiological mechanisms which underpin it ("causation"), about the evolutionary history which led to the development of this behaviour ("phylogeny"), about the way it develops during the life course of the animal ("ontogeny"), and finally about the role it plays in the animal's adaptation to its environment and the survival of the species ("function"). The rise of behavioural ecology profoundly transformed the biology of behaviour by foregrounding Timbergen's final question – that of function – almost to the exclusion of the other four (Barrett et al. 2013). While there are many interesting things to be said about animal behaviour in terms of mechanism, phylogeny and ontogeny, behavioural ecologists have tended to hold that to *explain* animal behaviour is primarily to answer the question of 'function': a behaviour is explained when one has shown in what sense this behaviour is *adaptive*, which is to say, how it contributes to the individual animal's "reproductive success" - its ability not simply to survive, but to maximally spread its genes into the next generation. When behavioural ecologists ask "why" an animal performs a particular behaviour, this is the question they are asking.

This theoretical outlook gave its initial impetus and purpose to field sites such as the Kalahari Meerkat Project. The KMP is one of a number of long-term field-sites in which the behaviour of a large number of known individual animals is observed over generations. This particular set-up is intended to allow for the framing and ideally quantitative testing of the sorts of "functional" questions described above: tracing how inter-individual differences in behaviour

impact upon animals' differential ability to reproduce successfully. Or, in other words, asking "why" they do it.

Much has been written in biology, philosophy and even anthropology about this theoretical shift, which is often criticised as a form of reductionism - anthropologists will be most familiar with this approach as an explanation of specifically *social* behaviours, under the name of 'sociobiology' (Hrdy et al. 1996; Wilson 1975), even though contemporary biological anthropologists who take this theoretical line tend to call themselves 'human behavioural ecologists'. This was also the core focus of the KMP, which was set up to ask sociobiological questions about "the evolution of cooperation" (Clutton-Brock et al. 2002).

My ethnographic interest, however, is in the way this 'functional' approach produces a particular kind of telos for the study of animal behaviour which relies on identifying the telos of that behaviour itself. The following extended quote dramatizes this relationship. It comes from my interview with a behavioural ecologist who, as a junior researcher working with Tim Clutton-Brock in the 1990s, was instrumental in setting up some of the research protocols and practical arrangements of the KMP. Here he is describing both the rise of behavioural ecology as a discipline after the 70s, and the way he himself discovered it, as a young man passionate about the observation of nature

> "Before that you had... everyone knew what ecology was, it was kind of 'go out and describe plants and animals you see and stuff'. Animal behaviour was like 'put a dog in a lab and do some experiments on it. […]Animal behaviour in the field was […] the naturalist watching his birds, kind of thing. And behavioural ecology was really about... I guess to my mind it was two things. One was the theoretical underpinning to allow you to ask interesting questions in terms of the evolutionary implications of this behaviour. So that was really key. And obviously the quantification. […] I'm not just merely going to watch this group of [for instance] sand pipers and note that they're seen to be pecking around and they fly off when a bird comes over, I'm going to, you know, systematically record how many of them are scanning, how many of them have got their beaks in the ground, and the group size, so that I can test theories about whether individual sand pipers are at a greater risk of predation in ...bigger groups. So it's that kind of making it more rigorous that turned it into a field as far as I can see. And that's certainly what I encountered that really excited me because you know *it took the stuff I loved, you know, and made it something that was worth doing.*"

The final sentence echoes a sentiment broadly shared amongst behavioural ecologists who reflected upon their entry into the discipline, or what they enjoyed about it. A number of those I spoke to described themselves as young people who already loved watching animals, and found in behavioural ecology a way to understand *why* they behaved in this way. Behavioural ecology, one might say, injects purpose into a contemplative encounter with animals, by identifying a particular kind of (evolutionary) purposefulness as the hidden mystery behind animal behaviour (Dawkins 2007: 1–4).

Once again we find the way in which generalities are refracted through disciplinary particularities. The vision of a discipline that 'takes the stuff you love and makes it worth doing' adds another layer to the pervasive dualisms I have been describing between the 'emotional

person' and 'the scientist'. These broad contrasts provide a kind of loose frame for talking about and recognising the more precise awakening that might occur as some of the volunteers discover that they are espousing behavioural ecology, and become people enthused by a particular teleological "why"? In that process, what were just forms of rule-following or even drudgery become asketics, the ethical substance and the mode of subjectivation to which it corresponds become more precise and specific, and something like an epistemic-ethical form of life is espoused.

Conclusion

Historians, and to a lesser extent anthropologists of science, have for a number of decades explored the ways in which scientific practices rely upon and entail certain kinds of ethical self-formation and virtuous conduct. Some aspects of these explorations predate the emergence of a self-conscious "anthropology of ethics". However, the clarity, depth and range of discussions of the ethical generated within this new field (and reflected in the other chapters of the present book, for instance) have yet to be reflected systematically in the study of science. Anthropologists of science have much to gain from engaging with the anthropology of ethics in a sustained way, as I have tried to suggest through the example above. Conversely, this extended example was also intended to suggest some of the ways in which in doing so, they might produce some useful comparative questions and perturbations for the anthropology of ethics to reflect upon.

One particularly productive conversation between these two anthropologies might concern the question of units (cf. Candea 2019). Debates within the anthropology of ethics about the nature and coherence of ethical "traditions" and debates within science studies about the unity and diversity of "science" would both gain from being put into more sustained conversation. I have tried to show in one case how seemingly naive generalities about the normative orientations of "Science" writ large on the one hand and on the other the precise epistemic virtues, askesis and telos associated with particular disciplines might actually be mutually sustaining, particularly from the perspective of those in the process of becoming scientists, which is to say, *necessarily but not merely,* scientists of a particular kind.

For anthropologists of science, this might help foreground a difficult problem: at what point do virtues cease to be distinctively epistemic? Thus, Shapin's magisterial account of contemporary scientists as, in part, virtuous managers or fearless and passionate entrepreneurs occasionally begs the question of what if anything these virtues (widely shared beyond scientific settings) still have to say to the specific question of knowledge production or truth-speaking? Meerkat project volunteers, and more advanced field biologists also cultivate an outdoorsy resilience and resourcefulness, a particular kind of easy-going attitude that enables them to avoid conflict while cooped up with a small group of people in a research station, and other virtuous dispositions which I have not mentioned above. Are these also epistemic virtues? Ethnographically speaking, these are not things that volunteers would often identify as part of being 'scientists' even though they or more advanced researchers might identify them as marking off say 'field scientists' from others such as 'statisticians' or 'theorists'.

Earlier Daston had written that

> Although moral economies in science draw routinely and liberally upon the values and

> affects of ambient culture, the reworking that results usually becomes the peculiar property of scientists. Traces of the original cultural models - for example, the simplicity, dedication, and humility of Christian saints or the unworldly innocence of the pastoral idyll - lie ready to hand, and can be evoked by the spokesmen of science to win public approval and support. But the ultimate forms that moral economies assume within science, and the functions that they serve, are science's own. (Daston 1995: 7)

Describing the contours of that process of appropriation that makes virtues into *epistemic* virtues requires a particularly fine brush - but it also requires an at least heuristic demarcationism about what the epistemic is to begin with. This in turn requires some sense of how the specific forms of science seek to speak to a broader general form – the Mertonian question which Rabinow rightly noted scientists still care about even when their sociologists and historians no longer do.

For anthropologists of ethics, this example might prompt comparative questions about the ways in which other ethical traditions are sub-divided and branch internally. It might also raise the converse question of the one above: might there be anything distinctive about those virtues and forms of self-cultivation which aim at the epistemic, beyond the usual ethnographic stomping-grounds of Euro-American technoscience – such as, for instance when Inuit parents seek to foster in their children an "experimental way of living" (Briggs 1991)?

In this and in other respects – the place of non-human actants in the distribution of ethical conduct, the partibility of ethical persons, the role of claims about the anethical within ethical forms of life, the epistemic difficulties surrounding the identification of askesis – the study of science as ethical practice(s) still has much to offer to anthropology's understanding of both science and ethics.

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