Chapter 14
Private Epistemic Virtue, Public Vices: Moral Responsibility in the Policy Sciences

Merel Lefevere and Eric Schliesser

Abstract In this chapter we address what we call "The-Everybody-Did-It" (TEDI) Syndrome, a symptom for collective negligence. Our main thesis is that the character of scientific communities can be evaluated morally and be found wanting in terms of moral responsibility. Even an epistemically successful scientific community can be morally responsible for consequences that were unforeseen by it and its members and that follow from policy advice given by its individual members. We motivate our account by a critical discussion of a recent proposal by Heather Douglas. We offer three, related criticisms of Douglas's account. First, she assumes that scientific fields are communicative communities. Second, in a system where the scientific community autonomously sets standards, there is a danger of self-affirming reasoning. Third, she ignores that the character of a scientific community is subject to moral evaluation. We argue that these omissions in Douglas's theory leave it with no adequate response to TEDI Syndrome. Moreover, we deny that science ought to be characterized by unanimity of belief among its competent practitioners, this leads easily to the vices of close-mindedness and expert-overconfidence. If a scientific community wishes to avoid these vices it should create conditions for an active pluralism when it and its members aspire to the position of rational policy decision-making.

14.1 Introduction

In this chapter we provide a new approach to analyze the moral responsibility and duty of scientific communities and individual scientists in these, especially those engaged in policy science. We motivate our account by a critical discussion of a recent proposal by Heather Douglas. In particular, our approach addresses what we call "The-Everybody-Did-It" (TEDI) Syndrome. Our main thesis is that the character of scientific communities can be evaluated morally and be found wanting in

M. Lefevere (✉) • E. Schliesser
Department of Philosophy and Moral Science, Ghent University, Gent, Belgium
e-mail: merel.lefevere@ugent.be

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terms of moral responsibility. In particular we argue that even an epistemically successful scientific community can be morally responsible for consequences that were unforeseen by it and its members and that follow from policy advice given by its individual members. We sketch what we call an active pluralism in order to give content to the duties that follow from the character failure of scientific communities.

In Sect. 14.2 we summarize Heather Douglas's proposal and elucidate its character. In Sect. 14.3.1 we offer three, related criticisms of Douglas' account. First, she assumes that scientific fields are communicative communities. Second, in a system where the scientific community autonomously sets standards, there is a danger of self-affirming reasoning. Third, she ignores that the character of a scientific community is subject to moral evaluation. We argue that these omissions in Douglas's theory leave it with no adequate response to TEDI Syndrome. In a fourth section we sketch an argument for the claim that if a scientific community wishes to avoid the vices of close-mindedness and overconfidence it should create conditions for an active pluralism when it and its members aspire to the position of rational policy decision-making.

Before we turn to our argument, we offer one methodological comment and a real-life (albeit stylized) example. We are primarily interested in recommendations from policy scientists to policy makers and the public. Most of our examples focus on economics, but our claims do not turn on these. However, the focus on economics is not only due to our scholarly interests; we argue that Douglas's conception of a scientific community has non-trivial similarities with an efficient market hypothesis in economics.

So, what do we have in mind when we talk about TEDI syndrome? Our example is grounded in a remarkable self-study of the Dutch Central Planning agency about the gross failures in considering the possibility of the macro-economic consequences (liquidity-trap, collapse in world-trade, etc.) of a Lehman style collapse in 2008. The self-study repeatedly points to this failure in "all other forecast agencies" (de Jong et al. 2010, p. 7, p. 27, p. 40, cf. p. 63). As the authors of the study admit, at the time, their whole modeling approach is unable to think systematically about such events.[1] We claim that the presence of TEDI syndrome is evidence that one may be dealing with an instance of *collective negligence*.

14.2 Science: Responsible Scientists

In her increasingly influential book (2009) *Science, Policy and the Value-Free Ideal*, Heather Douglas proposes that the longstanding idea of science as a value-free ideal is not only mistaken but also undesirable. Here we focus only on the

[1] They treat the fall of Lehman as a discretionary policy choice (61) that cannot be modeled. For more discussion of this case see Schliesser (2011).

fourth chapter of her book, where Douglas offers an account of the moral responsibilities of science. She argues that scientists need to consider the consequences of error in their work, without expecting them to be fortune-tellers. However, we do expect reasonable foresight and careful deliberation from scientists. This means that scientists are accountable for expected results and for certain side-effects of their actions. To elaborate on unforeseen consequences, Douglas returns to Joel Feinberg's well-known distinction between negligence and recklessness: "When one knowingly creates an unreasonable risk to self or others, one is reckless; when one unknowingly but faultily creates such a risk, one is negligent" (Feinberg 1970, p. 193). Thus, according to Douglas, a reckless scientist is fully aware of unjustified risks his choices entail, a negligent scientist is unaware of such risks, but he or she should have been (Douglas 2009, p. 71).

But how does one determine a scientist's negligence or recklessness? Difficulties arise in determining how much foresight and deliberation we ought to expect from scientists, without blaming them for every trivial use or misuse of their work. According to Douglas scientific communities provide the benchmarks of responsibility.[2] Reasonable foresight is to be evaluated in light of the judgments of the scientific community. While this still leaves a lot of details unanswered, Douglas offers a quite plausible suggestion; non-scientists generally lack the technical expertise to evaluate what would be foreseeable consequences of following policy advice. Her position can be strengthened intuitively: for a clear way to operationalize the very notion of expert-understanding of a scientific theory is to know how to reliably derive consequences from it.

Before we turn to our criticism of Douglas's approach, we provide some context for it. In particular, we argue that Douglas leans towards a juridical interpretation of scientific responsibility in terms of "what would a reasonable person do"?

14.2.1 The Reasonable Person

But how reasonable is that "reasonable person"? In current tort law and liability cases a person is considered responsible for his actions, or his negligence or imprudence. But the discussion about what is negligence and imprudence is still going on. Before we apply this to a more serious example, let's toy with this situation:

It's Monday morning, and as usual before work you pull over at your favorite magazine shop. You hop out of your car, leave the engine running, and quickly pop into the shop to pick up your journal. In the meantime a sinister figure approaches your car, sees that it is unlocked, jumps in and drives of. A few hundred meters further, the thief runs over a pedestrian.

[2] Douglas (2009), p. 83ff. Ian Hacking (1992) has developed a sophisticated treatment of the self-vindicating norms of various scientific communities.

Who's to blame for the death of the pedestrian? The thief you say? Not so fast. Are you not at least negligent or even reckless by not locking your car? Should you not have foreseen the possibility that an unlocked car attracts thieves? Sure, it's the thief that pushed the gas pedal, but if you had locked your car, he would not have stolen it and would not have hit the pedestrian. According to juridical standards, you may be co-responsible for the accident (especially if your insurance company has deep pockets), if a reasonable person would have acted in such a way that the accident would not have happened. So if a reasonable person would have locked his or her car, you may be, in part, to blame.

But who is that reasonable man (or woman) everyone should act like? The reasonable person is a legal fiction; it represents an objective standard to measure the actions of a real person against. While its philosophical roots may be traced back to impartial spectator theories of Hume and Adam Smith, it was first introduced into a legal context in 1837, in the case Vaughan versus Menlove. The defendant built a haystack on his land, close to the border of the plaintiff's land. The haystack had a "chimney" to prevent the hay from spontaneous inflammation. Unfortunately, the haystack caught fire anyway. However, the defendant had been warned several times in 5 weeks' time that the haystack was wrongly built, and thus dangerous, but he would not change it. The hay caught fire, spread to the plaintiff's land, and two of his cottages were destroyed in the fire. The jury was asked to judge whether the defendant's showed such reasonable caution as a prudent man would have acted with. This "reasonable person" test became a standard in the English and U.S. courts.

Steven P. Scalet argues that this "reasonable person" functions like an empty vessel, "allowing courts to use various norms and moral judgments to determine what seems reasonable in the circumstances" (Scalet 2003, p. 75). The standard of reasonableness is not very informative; it is the jury or the judges who interpret "reasonable" along the lines of community norms, legal principles, precedents, or moral judgments. This creates a serious tension. At one end, laws should be formulated in general terms, so that they can be applied to a variety of particular cases. At the other end, laws should provide the necessary information for citizens in order to guide their behavior. Reasonableness can indeed be applied to particular cases, but is no stable guide of conduct (ibid., p. 77). Scalet thinks of reasonable standards as "binoculars that focus our attention on some actual practices that have won our approval as an appropriate standard to guide our conduct" (ibid., p. 78). This changes the direction of interpretation: it is no longer the fiction of the reasonable man that instructs what is reasonable, but the behavior of actual people with certain relevant traits. The question remains which traits are relevant and which are not. There is a tendency in criminal law to individuate the characteristics of the reasonable man, and have a certain tolerance for traits such as hot-headedness or carelessness (ibid., p. 85). Others such as Honoré (1988), Holmes (1881) and Greenawalt (1992) argue for standards based on moral beliefs or other principles. Once the court has set the relevant traits, the conduct of the defendant has to be compared to the virtual or counterfactual conduct of the reasonable person. The case-law reflects these divergent tendencies (Scalet 2003, p. 85 etseq). In American jurisprudence guidelines are being formulated by the American Law Institute to give judges and lawyers information about general principles. For tort law there is the Restatement of Torts.[3] However in the legal literature those general principles have come under critical scrutiny. For example, Bernstein (2001) claims that the General Principles look at tort law from a gendered, mostly male, perspective. Hetchers argues that in the Third Restatement of tort law the interpretation of reasonableness is based on a normative characterization that transforms the reasonable person standard into a tool for promoting social welfare, and thereby ignores the moral heterogeneity of the community (Hetcher 2001).

For our purposes we need not take a stance on the debates over these general principles. All we claim is that without a lot of further contextual detail mere appeal to the legal framework of a "reasonable man" may not settle any interesting cases of scientific responsibility.[4] We now turn to a more critical engagement with Douglas's framework.

14.2.2 The Reasonable Scientist

In this subsection we show that Douglas uses something like a "reasonable person" framework to analyze responsibility in science. To argue for the responsibility of scientists, Douglas critically analyzes Bridgman's claim that "scientific freedom is essential and that the artificial limitations of tools or subject matter are unthinkable" (Bridgman 1947, p. 153). This means that scientists should have full autonomy and should not be bothered with social or moral responsibilities beyond ordinary responsibilities.

However, Douglas presents evidence that scientists do weigh epistemic goals against other non-epistemic considerations: the use of human subjects in research, animal rights, determining or cutting budgets for certain projects such as the supercollider in the 1990s, etc.[5] Douglas focuses on two particular cases in order to show that scientists even act against the weak interpretation of Bridgman; scientists frequently consider potential unintended outcomes of their research. For example, she provides historical evidence that before testing the explosive chain reaction of an atomic bomb, physicists worried that the energy that would come free with the explosion of such a bomb may generate an unwanted chain reaction in the earth's atmosphere itself. Similarly, before pursuing recombinant DNA techniques,

[3] Restatement Third of Torts: Liability for Physical and Emotional Harm (2010), Apportionment of Liability (2000), and Products Liability (1998).

[4] Neil Levy reminded us that in the United States courts use the so-called Daubert and Frye tests in evaluating admissibility of scientific facts and theories – both tests crucially make reference to widespread acceptance within a scientific community. From our vantage point this makes the question we are pursuing in the chapter only more urgent.

[5] Douglas 2009, p. 76. Of course, the requirements and incentives to take non-epistemic (e.g., legal, financial, ethical, etc.) factors into consideration may themselves be extra-scientific.

scientists discussed the possible risks for public health and changed lab practices in light of these. From these examples, Douglas infers that scientists themselves do not consider themselves free of social or moral responsibilities.[6] Of course, there are wide disciplinary divergences in such matters. For example, not all scientific professional societies have ethical codes or codes of conduct that govern their members. Economics does not.[7]

In Douglas's view *scientific* responsibility boils down to the duty to be neither reckless nor negligent as a scientist. (Of course, she allows that scientists may have all kinds of non-scientific responsibilities.) Even if we assume that scientists have good intentions, there can be (i) unintended foreseeable consequences and (ii) unintended unforeseeable consequences that may raise concern. But more central for Douglas are (iii) "the potential unintended consequences of making inaccurate or unreliable empirical claims" (Douglas 2009, p. 72). Externalizing the responsibility for situations like (iii) beyond the scientific community, is an unlikely option because "presumably only the scientist can fully appreciate the potential implications of the work" (ibid., p. 73). The only qualified people who can consider the potential errors and their consequences are often the scientists themselves.

Of course, we cannot expect scientists to be fortune-tellers. The responsibility they bear should be limited by the standards of reasonable foresight. While she discusses other cases, too, Douglas focuses on policy scientists (as will we). When giving advice, scientists should consider the consequences of error and avoid negligence or recklessness. "This means that when a scientist makes an empirical claim in the process of advising, they should consider the potential consequences if that claim is incorrect" (ibid., p. 81). A scientist should acknowledge uncertainties in empirical evidence, but the weighing and listing of potential consequences is at play in choosing to emphasize or minimize the importance of uncertainties.

As an aside: a note on our terminology: we deploy the old distinction between measurable risk (with, say, a probability distribution attached to it) and un-measurable uncertainty (Knight 1921; Keynes 1921). In practice, an event that is not possible within a model-world – like the fall of Lehman for the Dutch CPB in 2008 – is uncertain. Both (ii) and (iii) may be uncertain.

But which consequences should a scientist be able to foresee? Here the standard of reasonableness comes in. This standard should be provided by the scientific communities: "because scientists work in such communities, in near constant communication and competition with other scientists, what is foreseeable and what is not can be readily determined" (Douglas 2009, p. 83). Douglas claims that because a scientific community is essentially a communicative community,

[6] We leave aside to what degree this practice is itself a historic relic from a different scientific culture or organization.

[7] In private correspondence Heather Douglas pointed out that on her view "if there are scientific groups without ethical codes or indeed any sense of responsibility for the consequences of error . . . they are in moral error."

ideas of potential errors and their consequences will spread quickly and be discussed.

Douglas illustrates this by an example from nuclear physics. The discovery of the neutron by James Chadwick gave a boost to nuclear physics, but the discoveries that followed the neutron did not have important implication outside of the discipline. This changed when fission was discovered in 1938. In early 1939 scientists saw the possibilities of fission in bomb making or energy production, which started a political debate. But can we blame or praise Chadwick for the invention of the atomic bomb or nuclear power? According to Douglas the scientific community in early 1939 saw the potentially disturbing consequences of fission, and thus it is that community that provides the benchmark of what should have been foreseen. In Douglas's words, "what is reasonable is to expect scientists to meet basic standards of consideration and foresight that any person would share, with the reasonable expectations of foresight judged against the scientist's peer in the scientific community. [. . .] They are held to only what can be foreseen, and thus discussed and considered." (ibid., p. 84). Douglas's case nicely shows that once a view about a consequence reaches some threshold of wide currency within a scientific community in a field, it becomes reasonable to presuppose it in one's consideration of reasonable foreseeable consequence, and – if needed – change one's practices in light of them.[8] Of course, one might think that there is some further responsibility to conduct inquiry into whether there are relevant consequences. Below we argue that there is indeed such a duty for a class of scientific agents.

Before we turn to our three, interconnected criticisms of Douglas, we should note a qualification about our approach. According to adherents of the so-called "doctrine of double-effect," unintended consequences are never morally blameworthy. This doctrine states that "it is permissible to bring about as a merely foreseen side effect a harmful event that it would be impermissible to bring about intentionally."[9] The doctrine concerns circumscribing permissibility, but our views concern what, among those things that are impermissible, is blameworthy; we simply set it aside here.[10]

[8] This is a stricter standard than an appeal to what is to be found in textbooks, which are often trailing scientific findings at the so-called "research frontier." On the latter concept, see de Solla Price (1965).

[9] McIntyre (2011). Of course, as she remarks, "traditional formulations of double effect require that the value of promoting the good end outweigh the disvalue of the harmful side effect;" so it is not a blanket principle.

[10] We thank Neil Levy for this formulation.

14.3 Collective Negligence and the "Everybody Did It"-Syndrome

14.3.1 Three Criticisms of Douglas

In this section, we offer three, interconnected criticisms of Douglas: first, she assumes that scientific fields are communicative communities in a way that begs the question. Second, in a system where the scientific community autonomously sets standards, there is a danger of self-affirming reasoning. Third, she ignores that the character of a scientific community is subject to moral evaluation. We argue that these omissions in Douglas's theory leave it with no adequate response to TEDI Syndrome. Along the way, we introduce motivations to take our alternative approach, which we dub "active pluralism," seriously.

First, in practice it is not so obvious that scientific fields are always communicative communities of the sort required by Douglas. Recall that Douglas offers two characteristics of scientists who make up these: they are "[a] in near constant communication and [b] competition with other scientists." Douglas presupposes something like an efficient market in scientific ideas.[11] Even if there are no barriers to communication at all, any given scientist is exposed to a flood of information. So, the mere fact that an issue is discussed openly in a scientific community is not enough to ensure that any given scientist is aware of the discussion, let alone all the relevant details of it.[12]

Moreover, policy-sciences do not always instantiate constant communication and competition; there is plenty of classified (e.g., defense-related) or so-called sponsored research that often is bound by non-disclosure requirements. This is not an idle thought: financial trading houses try to keep their trading strategies and the consequences of their proprietary financial products a secret for competitive advantage – often these presuppose non-trivial technical and technological improvements that will not be available and, thus, not well understood by the larger community, including regulators and assessors of systemic risk. This issue generalizes more widely; in medical sciences and engineering it is quite common to keep new techniques secret by patenting first before publishing results. Some important results never get published when the financial stakes are high.

Further, policy scientists, in particular, are not always transparent about the explicit or subtler tacit financial incentives of their consulting work.[13] Also, fields

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[11] For an influential statement of this idea within economics, see Stigler (1969). For recent critical engagement see Schliesser (2011), and Boettke et al. (2010).

[12] We thank Neil Levy for pressing this point.

[13] The locus classicus is Gordon Tullock: "Not all of the advocates of tariffs, of course, are hired by 'the interests.' But the existence of people whose living does depend on finding arguments for tariffs and the further existence of another group who think that maybe, sometime in the future, they might need the assistance of either someone who believes in tariffs or an economist who is in this racket makes it possible for them to continue to publish, even in quite respectable journals.

have very diverging practices when it comes to replicating results or sharing data (Feigenbaum and Levy 1993). There are well-known incentives and barriers against publishing replications or dis-conformations. So even if scientific fields are essentially communicative communities it is by no means obvious that scientific communities communicate the right contents. It is unreasonable to expect Douglas's approach to apply without some finessing.

Our two other criticisms also focus on Douglas's commitment to what can be reasonably foreseen is linked to what is discussed and considered within the scientific community. In both cases we highlight different problematic features of her implicit commitment to efficiency in ideas within scientific communities. So, our second criticism is that community standards are often the product of ongoing scientific practices. In general, these practices are tuned to facilitate *epistemic* practices not potentially *moral* implications of these practices or even the unintended *social* impacts of these practices. To rephrase this point slightly in economic terms: the incentives that govern the evolution of reasonably successful epistemic norms need not have taken into account possible social and moral externalities. For example, competent geneticists need not be well placed to foresee or calculate the potential social costs of their mistakes or successes.[14]

This is not to deny that various policy sciences can have evolved in certain directions in order to be attractive to policy-makers. For example, the mathematical econometric techniques and tools – and more generally inferential technologies that produce univocal and stable figures in calculating the implications of policy alternatives – were promoted since the 1940s within economics, in part, because they would make economists attractive as policy advisers (as opposed to say, sociologists, lawyers, anthropologists, and historians).[15] But it is not *prima facie* obvious that attractiveness to policy makers automatically translates into being socially responsible.

Be that as it may, in Douglas's system it is the scientific community itself that sets the benchmark for reasonable foreseeability. New findings are communicated through conferences, journals, books, and so forth to peer-scientists. During this contact (and the way it disseminates through graduate training and textbooks) a community develops something of a benchmark for what a reasonable scientist

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Thus a dispute which intellectually was settled over a century ago still continues." The point generalizes. Tullock ([1966] 2005): Chapter VII: The Backwardness of the Social Sciences.

[14] In standard applied welfare economics distribution effects are ignored in calculating so-called "consumer surplus," but this means that some of the most controversial social consequences of policy-advice is systematically neglected. See Harberger (1971), for an important defense, and Khan (1992a) for criticism.

[15] This was also contested. See, for example the Koopmans-Vining debate; the papers are nicely available here: http://cowles.econ.yale.edu/P/cp/p00a/p0029.pdf, accessed on May 16, 2011. We thank Roger Backhouse for calling our attention to it. See also Harberger (1971). See also Düppe and Weintraub 2013.

should foresee and how he or she should deliberate. If enough scientists adopt this benchmark, it becomes the benchmark of the scientific community.[16]

The problem with this architecture is that there is a danger of self-affirming reasoning. It is the individual scientist, as a member of the scientific community, who determines the standard he or she will be judged by. Warnings of possible consequences that do not make it into journals or conference presentations, or that are not taken seriously by peer scientists, do not help shape the benchmark and are, even if they turn out to be prescient, after all, therefore not taken into consideration when blameworthiness (or praiseworthiness) of the scientific community is evaluated. There is no need for conspiracies or malicious intent here. After all, time is one of the scarcest commodities for active researchers; it is often not worth their effort to actively seek out all consequences of a theory. Such an individual cost-benefit analysis may well be replicated through a whole field. Moreover, if journal articles in a field do not reward publications of, say, lack of replication then there may well be incentives that prevent possible consequences from ever being noticed.[17] A socially significant question may never be asked in the pursuit of interesting science. Again, to put this in economic terminology: competition for scarce resources does not by itself guarantee that externalities are properly incentivized.[18]

In practice, self-affirming benchmarks may well be woven into a field's standard practice. It is well known that alternative models and even long-standing objections can get suppressed from a discipline's collective tool-kit and memory. In the philosophy of science literature, the suppression of long-standing objections or even reliable alternative approaches is known as a "Kuhn-loss."[19] In particular, insights of discarded theories that cannot be articulated or recognized by the new theory are instances of Kuhn-losses.[20] Here we use "suppression" in non-moral sense; we have in mind epistemically important practices that set aside, say, questions, anomalies, or results in pursuit of more epistemically promising alternatives. Kuhn is significant here for a related point. He helped popularize a view of paradigms that allowed social-scientific practitioners to claim that they need not

[16] This need not be explicit; criticisms of a new benchmark my come to an end. See Pickering (1992). In private correspondence Douglas insisted that on her view "it is not the case that what is foreseeable is only what is discussed and considered." The following goes beyond this point.

[17] Not all sciences have what we may label a *Popperian ethos* in which concepts, models, and theories are deliberately constantly stress-tested. Plenty of sciences have what we may call a *confirming ethos*; that is they seek to provide evidence for theories. For the sake of argument, we stipulate that such a confirming ethos may be the most efficient epistemic practice.

[18] See Mäki (2011), who points out that the attainable truths may not necessarily track the truths worth having. See also Schliesser (2005).

[19] According to I. Votsis (2011) the term "Kuhn-loss" seems to be coined by Heinz Post (1971).

[20] Hasok Chang (2004) offers ingenious arguments for the significance of Kuhn-losses, and he uses these to motivate the pursuit of non-standard science. For extensions of the argument, see Schliesser 2008, 2009.

answer all objections; a welcome result in some consensus-aiming policy sciences.[21]

So, the community standards themselves can be flawed for some (say) non-epistemic purposes, but still be accepted by the community. That is to say, the members of the community as well as their funders, grant agencies, and "consumers" (i.e., politicians and the public) may well unintentionally create the conditions by which a community instantiates the vice of close-mindedness and its related vice of overconfidence. In such cases there is *collective negligence*. Often the presence of TEDI syndrome is evidence that one is dealing with an instance of *collective negligence*.[22]

We propose that if the benchmark is created by the scientific community itself, there should at least be more to reasonableness than merely accepting the (vast) majority's opinion. In particular, if there is intolerance of alternative approaches and suppression of historical knowledge of the discipline's past (or the routine propagation of mythic history in textbooks) these may well be enabling conditions for collective negligence. Again, to put this in terms of Douglas's implied efficiency claim; there may well be institutional barriers to entry that prevent the kind of intellectual competition worth having within a scientific community from society's point of view.

Collective negligence is not the only flaw in Douglas's approach. Our third criticism is this: it is perfectly possible that every individual scientist has acted according to community standards, and therefore should be free of any moral responsibility, but that there is a problem with those community standards. One of those problems is that scientific communities seem to have difficulties to think outside their paradigm and have limited tolerance of heterodoxy. Moreover, expert over-confidence is a now well-established empirical fact (Angner 2006). To put this in modal terms: There is a tendency for experts to treat their own model as necessary.[23] This tendency can be reduced if we treat the model as just one *possible* world (or a member of a portfolio of theories).

The benchmark of foreseeability for any claim is often made within a "paradigm" itself. Sometimes this means that events that are or were foreseeable within an incommensurable paradigm become impossible to state within the ruling paradigm. For example, in the tool-kit of recent mainstream economics it became very difficult to talk about or even discern bubbles; the efficient market hypothesis (understood in terms of random walks and arbitrage-free environments) makes no conceptual space for it. When asked recently, "*Many people would argue that, in this case, the inefficiency was primarily in the credit markets, not the stock market –*

[21] Stigler 1975, pp. 3–4. We thank David Levy for calling our attention to it. Stigler was also an active promoter of Kuhnian views about science within economics. For the larger story, see Schliesser 2012.

[22] The desire to produce consensus may, in fact, sometimes be the distant cause of the negligence; in such cases philosophies of science that promote an image of science as a consensus activity may be thought complicit in the negligence.

[23] We thank David M. Levy for pressing this point.

that there was a credit bubble that inflated and ultimately burst," the economist that actively promoted so-called efficient market theory, Eugene Fama, replied: "I don't even know what that means. People who get credit have to get it from somewhere. Does a credit bubble mean that people save too much during that period? I don't know what a credit bubble means. *I don't even know what a bubble means*. These words have become popular. I don't think they have any meaning"[24] (emphasis added).

Fama's words here exhibit very nicely what we have in mind. His world-view is so caught up with his evidentially well-supported, particular paradigm that he finds concepts that do not fit it utterly unintelligible. To speak metaphorically: he sees and understands economic phenomena through his model. Anybody that adopted his toolkit – as was widely done among economists – could claim all scientists within the community are free from any blame, because "everybody did it". But, of course, if he had wanted to, Fama could have learned about serious, empirical studies of bubbles at the margins of the economics profession that seem to have no problem operationalizing the term successfully.[25]

This concludes our critical engagement with Douglas. We now turn to sketch a bit more fully our alternative approach that can promote a morally more sound character to scientific communities and the duties of individuals within them.

14.3.2 The Duty of Epistemic Pluralism

In this section we explore the duties and obligations that prevent collective negligence. The moral upshot of our analysis in the previous section can be articulated in Douglas's terms: the autonomy of a field with policy implications comes with increased responsibility if not an outright duty to be open-minded, that is, to be actively striving for a variety of pluralisms. For present purposes we adopt De Langhe's definition of pluralism, which is "an epistemic position which acknowledges the validity of different possible perspectives on reality in an active way, which means that they are not only tolerated but also taken into account when goals of knowledge (prediction, problem-solving, truth, curiosity, policy advice, funding decision, . . .) are to be achieved" (De Langhe 2009, p. 87).

We now turn to exploring briefly what this entails. In order to obtain scientific pluralism it is not necessary that every individual scientist in a community is a pluralist, but the scientific community as a whole should be. We cannot stress this enough; we are tackling collective negligence at the level of the composition of the

[24] http://www.newyorker.com/online/blogs/johncassidy/2010/01/interview-with-eugene-fama.html#ixzz1fQoeflSE. See, for example, the canonical paper by Fama (1970), which has over 8,000 citations.

[25] Including work done by those awarded the Nobel prize in economics. See Smith et al. (1988).

community.[26] From individual scientists we expect no more than ordinary, scientific open-mindedness, this is why we focus on the *character* of the community. We also focus on the responsibilities and duties of a sub-set of policy scientists those involved in aggregating scientific knowledge.

At the community level we advocate the ongoing cultivation of competing, potentially incommensurable paradigms. So, we reject the once-widespread idea that a field's unanimity is trumping evidence for it to be considered scientific or a sign that a field is "mature" in Kuhn's sense.[27] We have in mind, of course, the reality that in the wake of Kuhn's *Structure* some fields of inquiry pursued near-unanimity *in order* to be considered scientific. For example, the Nobel-laureate, George Stigler defended this as a "fundamental tenet" of "those who believe in free discussion that matters of fact and logic can (eventually) be agreed upon by competent men of good will, that matters of taste cannot be."[28]

In fact, there may well be many reasons that a science naturally becomes pluralistic, if permitted.[29] Diversity in science, while no magic cure, can help ensure that a variety of research questions are asked and a corresponding variety of possible solutions, problems and applications are discussed. This is a long-standing concern of so-called standpoint theory that is very popular among feminists philosopher of science and critical race theorists.[30] To forestall misunderstanding: we are not arguing that some standpoints are *a priori* better because they are minority standpoints.

As an aside, while here we promote a pluralist approach at the level of the composition of approaches within a scientific community, it is worth noting that it is not the only possible way to avoid collective negligence. Given that even foreseeable consequences may also have unintended side-effects, creating (incentives for) a willingness to assertively articulate known or knowable uncertainty over possible consequences of policy may be a viable alternative approach.[31] Even if policy-

[26] Audiences to earlier drafts of this chapter worried that we demand a change of behavior in the epistemic practices of individual scientists.

[27] Some readers might wish to claim that our position has been decisively disproved in a famous article by Robert J. Aumann 1976, "Agreeing to Disagree,". But even if we grant the appropriateness of his Bayesian conceptual apparatus, Aumann does not provide an institutional framework that ensures that equilibrium in the information exchange will be reached such that rational disagreement becomes impossible. Our approach offers reasons for thinking that the preconditions that would make his proof actual for real scientific communities sometimes (often?) do not exist. We thank M. Ali Khan for urging us to consider Aumann.

[28] Stigler 1975, pp. 15–16. See Levy and Peart (2008). Stigler was an early, enthusiastic reader of Kuhn; within economics it is common to encounter Kuhnian concepts (see Schliesser 2011 for details).

[29] De Langhe 2009, p. 88. See also Kitcher 2001, pp. 55–62. Here we ignore the question of what causes a lack of pluralism. When we presented this material to an audience of economists at NYU, these proposed that government funding practices may be the source of monopoly power within many sciences.

[30] For an excellent introduction, see section "2. Feminist Standpoint Theory" in Anderson 2011.

[31] This is, in fact, the approach favored by one of the co-authors.

makers may not wish to hear about uncertainty this does not exculpate the policy-scientists who provide their "clients" with what they want to hear. Even independent of our approach, scientists have professional duties that regardless of their policy-makers' wishes may demand fuller disclosure or remaining silent.[32] This aside suggest that a community can be morally responsible for the unintended and unforeseen consequences of its research, namely if the community is not critical enough such that it does not pay enough attention to the potential consequences of the research. This means pluralism is not a necessary consequence of our criticism of Douglas.

We claim that in order to avoid collective negligence at the policy level, having different possible answers can help to make more responsible decisions. In the active pluralism view promoted here, what is reasonable for a scientist is not what is foreseeable according to the benchmark the majority has set, but it should be evaluated in light of all the different (empirically well supported) perspectives that are available in that discipline.[33]

Such evaluation is a requirement at the level where science and policy intersect. In what follows we call that level the "aggregate level;" policy scientists that work at an aggregate level do have special duties to seek out and be familiar with scientific approaches other than their own and, perhaps, different from the ruling paradigm(s). These duties follow from, in the first instance, from their ability to influence policy. In practice such policy scientists working at the aggregate level also gain special benefits from their status (e.g., recognition, access to lucrative consulting gigs, etc.). This means that if a policy scientist chooses to ignore more marginal voices within his or her discipline, this does not free him or her from responsibility to take potential warnings from that group seriously to weigh the consequences of possible error. There are known cases where, for example, in development economics economists with "local" backgrounds pointed out the biased assumptions of leading economists and were ignored.[34]

14.3.3 Some Distinctions[35]

Our chapter challenges a widely held truism: (I) one can never be blamed for things that were not foreseen (by you and your community). By contrast, our position is

[32] Such professional duties have long been recognized by economists, including Alfred Marshall and A.C. Harberger (1971).

[33] Boundary policing of a discipline makes it a bit tricky to say when such perspectives are still available. Moreover, different theories may, of course, be differently empirically supported. But even theories that are empirically less supported along many dimensions may do better in a sub-set of problems.

[34] See the criticism of Lawrence Summers by Khan 1992b, 1993 and the subsequent discussion by Ron Jones in the same issue, pp. 580–582.

[35] This section is greatly indebted to [names omitted].

that (II) a scientific community can be morally responsible/blameworthy for the consequences of its research even if they are unintended and unforeseen in the community. In particular, (III) a scientific community can be morally responsible or blameworthy for the consequences of its research even if these are unintended and unforeseen (by the community), namely if the broader community is not pluralistic (such that it does not pay enough attention to the potential consequences of research).

Of course, there is a closely related alternative to (I): (IV) One can never be blamed for things that were not foreseeable. This is more appealing than (I); if consequences of research are unforeseeable then one can never take them into account, no matter how much attention one pays to them. Nevertheless, (IV) is ambiguous between: (IV*) one is never to be blamed for things that were not foreseeable by a community at the time; (IV**) one is never to be blamed for things that were and are not foreseeable in principle, that is, genuine uncertainty. Let's grant (IV**) for the sake of argument (even if one can imagine cases where not preparing for, say, any unexpected emergencies may be inexcusable).

Now, let's operationalize the foreseeable in terms of the portfolio of models (and paradigm-preserving extensions of these) within the paradigmatic science. In particular, the foreseeable is the possible in the paradigmatic models (plus bridge principles, know-how of the expert, etc.). This suggests that (IV*) is too weak because due to Kuhn-loss phenomena, paradigmatic models never incorporate all the models available in discarded or non-paradigmatic models. So, a community may in some circumstances not be able to foresee consequences that would have been available if some of the non-paradigmatic models would be in use. So (V) a community can be blamable for things that were not foreseeable (in the sense of IV*) by a community at the time (because of overreliance on a ruling paradigm). In fact, (V) is our argument for pluralism.

14.4 Active Pluralism as a Condition for Rationality/Reasonableness

Before we offer some modest, preliminary suggestions on how to meet such duties, we briefly characterize pluralism in epistemology in order to offer an account of benchmarking that can avoid the vices of collective close-mindedness and collective negligence.

14.4.1 Pluralism in Epistemology

Pluralism in science is part of an ongoing debate, especially in epistemology. There are philosophers who advocate pluralism in one specific issue of epistemology, such as explanation. Several *philosophers*[36] advocate an explanatory pluralism, by acknowledging that one can have different interests or types of questions, and thus requiring a different type of explanation. But there are also philosophers who have more expansive frameworks for articulating pluralism. For example, Mitchell and Dietrich show how "integrative pluralism" succeeds in biology[37]; Van Bouwel offers considerable motivation for pluralism in the social sciences and economics.[38] In both cases, pluralism seems to be positively received, but "if it is to be more than a liberal platitude, we need to delimitate more clearly"[39] what pluralism entails, where we want it and how we can achieve it. In the remainder of this subsection we'll dig deeper into the concept of pluralism.

Batens, Nickles, Nersessian and Schliesser[40] defend pluralism as a key concept in context-based epistemology.[41] On this view, a belief or decision is only justified if it is actively compared with available alternatives. This happens in a local process, a context, since not all of our knowledge is used, doubted or accepted at the same time. Accept this for the sake of argument. There are at least two important features of this context-based proposal that raise questions. The first question is what is considered an available alternative? This will be considered in Sect. 14.4.2. The second worry is whether we can demand from an individual, policy scientist to switch between views or hypotheses as if it were a change of clothes.

De Langhe (2009) addresses the second by making a useful distinction between pluralism at the level of the individual scientist and at the level of the scientific community. He argues that there are plenty of reasons to expect lack of consensus in science: its presence can be the consequence of the problem of underdetermination of theory by evidence (or data); the world's complexity; the limits to our cognition; the contingency thesis; experiential diversity and path dependence. If an individual scientist accepts all alternatives simultaneously, his decisions (such as research questions or methods) are empty. It could just as well have been another decision. If he considers the multiple alternatives as a reason not to make a decision at all, he would in a certain sense stop being a scientist.[42] How does one make choices as a pluralist, and how can those choices be warranted if there are multiple

[36] To ensure an anonymous referee process this sentence has been adapted. The original sentence can be found on the title page.

[37] Mitchell and Dietrich (2006). It turns on recognizing different levels that need not require general unification.

[38] Van Bouwel (in print) and Van Bouwel (2004, 2005).

[39] Keating and Della Porta 2010, p. S112.

[40] Batens (1974, 2004). There are other philosophers who advocate similar views, such as: Thomas Nickles (1980) Nancy Nersessian (2008) and Eric Schliesser (2005).

[41] For an interesting discussion about this form of contextualism, we refer to Demey (forthcoming).

[42] This is what happened to the Chicago economist Frank Knight, who indirectly created the foundations for an understanding of economics as an applied policy science as made famous by "Chicago-economics" (e.g., Milton Friedman, George Stigler, A.C. Harberger, and Gary Becker), but who himself was a deep pluralist about the way social science could influence policy and who

views rationally justifiable? De Langhe points out that this is a false dilemma, since it is not the individual scientist that needs to be a pluralist, it is the scientific community. "Warranted choice can go hand in hand with pluralism on the condition that pluralism is confined to the aggregate level. In other words, the cost of warranted choice is individual level pluralism" (De Langhe 2009, p. 92). The individual scientist qua scientist can continue doing research starting from his situation, his epistemic interest, his experience, and so forth. This makes his or her choices warranted at the individual level.

De Langhe infers that "advocates of pluralism should not bother trying to convince individual scientists of adopting pluralism in their own research nor blame them for not doing so" (p. 94). He advocates that it is far more important to concentrate efforts to structuring scientific community in such a way that it reflects the diversity at the community level. In particular, when scientific knowledge gets aggregated for policy-makers (and regulators) this diversity should be available. We have offered moral arguments for the same conclusion. We now return to discuss the problem of benchmarking in order to assess the moral responsibility of a scientific community and the duties of policy scientists within it.

14.4.2 Benchmarking in a Pluralist Epistemology

If a scientific community constitutes the benchmark by which the consequences of its policy recommendations are to be judged then we argue that this community should be pluralistic in a way that we characterize more exactly in this section. In doing so we build on Batens's proposal that reasonable decisions can only be made after comparing a certain proposed action (claim or hypotheses, etc.) with available alternatives (Batens 1974, 2004); we also agree with De Langhe's proposal that pluralism is only required at the aggregate level. By this aggregate level we mean not just the composition and methods of the scientific community as a whole, but also the manner in which these are deployed in policy advice. Here we focus on some suggestions that can assure a supply of alternatives for the aggregate level. We propose that an active pluralism can contribute to ensuring a morally responsible scientific community.

Douglas refuses to separate the roles of a scientist-as-researcher from a science-advisor (Douglas 2009, p. 82), but perhaps she would be inclined to accept De Langhe's distinction between the individual roles of a scientist (such as doing research, writing articles, giving lectures), and the aggregate roles such as editing a journal, organizing conferences, teaching, refereeing, policy-advisor, regulator, media spokesperson, etc.[43] On that aggregate or composite level, De Langhe writes,

embraced (epistemic) uncertainty as a fact of life in most policy decisions. For a very good discussion of Knight see Ross Emmett (2009), chapter 12.

[43] This distinction is in many respects a manner of degree, of course. A grant-making, lab-director straddles our distinction for example.

the effort should be focused on structuring scientific community in such a way that it reflects the diversity at the individual level. Unfortunately he stops, where the real problem begins.

We propose that scientists who work at the intersection with policy (understood in its widest sense, including consulting, editorializing, regulating, etc.) take that aggregate-responsibility seriously. Pluralism at the level of scientific content should then be a value of paramount importance.[44] Scientists who enter this level should be prepared to question the current structure of the scientific community. In particular, they have a *duty* to seek out alternative orientations within the scientific community to their own approach. To forestall misunderstanding: even within this pluralist framework we allow that many aggregating mechanisms (conferences, journals, etc.) will not be and need not be in themselves pluralist. All we claim is that when these mechanisms intersect directly or indirectly with policy that then policy scientists and those that fund and listen to them have a duty to consider more than one scientific perspective. Sometimes this can be as simple as letting, say, one group of social scientists (say, sociologists) evaluate the policy advice of, say, another group of social scientists (economists), or – recalling an earlier point – geneticists. We agree with Douglas that it should still be scientists who judge scientific claims.[45]

14.5 Conclusion

In the final paragraph of the section, our discussion has slid into practical suggestions. In our conclusion we will offer a few more in order to stimulate further reflection. For example, in grant-making or regulatory agencies one can try to create distinct panels that ensure aggregate diversity and pluralism. This may appear to make, say, the grant-process, or policy-advice generation less efficient and messier. But we assume, by contrast, that scientific monocultures, which may benefit from all kinds of economies of scale and internal efficiencies, can cause far worse kinds of social externalities.[46]

In practice this means that grants should be awarded to economists, physicists or psychologists of different positions, backgrounds, and so forth. We believe grant agencies and ought to incentivize the presence of such alternatives. For example,

[44] There are other potential benefits to our proposal: if one paradigm bluntly fails, there are alternatives available that can provide decent answers. Not to mention that scientific monocultures may be vulnerable to extinction. So our proposal may increase the robustness of science. We thank Dunja Seselja for pointing out this benefit.

[45] To forestall misunderstanding: an argument that all people affected by a policy decision should be included in policy discussion falls beyond the scope of our more limited concern here.

[46] This is not just arm-chair philosophizing. Consider the massive damage done to environments and indigenous people by large multinational lending institutions in the grip of one-sided economics paradigms.

10–25 % of a government research budget could be devoted to foundational and methodological criticisms of dominant research programs; to ensure proper replication of fundamental results; to promote transparency of data; to do effectiveness studies; to explore social consequences of policy; and to fund empirically or conceptually promising alternatives to the main approaches. None of these suggestions are radical and some would build on existing initiatives.[47] Of course, there would be plenty of resistance to such a proposal, too.

In public governance, and even in private companies, ombudspersons are appointed to mediate between an organization and the interested stakeholders of that organization. Plenty of institutions are capable of pursuing more than one (perhaps hierarchically organized) goal at once. Something similar could be set up within the policy sciences. Such an institution allows an individual scientist qua scientist to focus on his or her research, while improving the moral character of the scientific community. Moreover, such an institution can help the policy scientist who is active on the aggregate level to cope with responsibilities that come with his or her function.

Finally, our chapter challenges two deep-seated commitments in our thinking about science. First, we deny that science ought to be characterized by unanimity of belief among its competent practitioners. We have argued that this leads easily to the vices of close-mindedness and expert-overconfidence. Second, we deny that the current way in which research is organized is optimal if one wishes to prevent social externalities; rather it seems especially prone to what we call TEDI Syndrome. We advocate a reform of aggregate scientific institutions that promote active pluralism. We also believe that given the great privileges and powers accorded to policy scientists, it is their duty to seek this out.

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[47] Some grant agencies already do this on a modest scale: The NSF's "STS considers proposals for scientific research into the interface between science (including engineering) or technology, and society. STS researchers use diverse methods including social science, historical, and philosophical methods. Successful proposals will be transferrable (i.e., generate results that provide insights for other scientific contexts that are suitably similar). They will produce outcomes that address pertinent problems and issues at the interface of science, technology and society, such as those having to do with practices and assumptions, ethics, values, governance, and policy." http://www.nsf.gov/funding/pgm_summ.jsp?pims_id=5324

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