Research Article

Straightening the 'Value-Laden Turn': Minimising the Influence of Values in Science

Philippe Stamenkovic[1]

1. Uppsala University, Sweden

Straightening the current 'value-laden turn' in the philosophical literature on values in science, and reviving the legacy of the value-free ideal of science, this paper argues that the influence of non-scientific values should be minimised – not excluded – in the core phase of scientific inquiry where claims are accepted or rejected. Noting that the original arguments for the VFI (ensuring the epistemic integrity of science, respecting the autonomy of science results users, preserving public trust in science) have not been satisfactorily addressed by proponents of the value-laden ideal, it proposes four prerequisites for any model for values in the acceptance / rejection phase of scientific inquiry, coming from the fundamental requirement to distinguish between facts and values: 1) to ensure the epistemic integrity of scientific knowledge; 2) to state clearly the uncertainties associated with scientific claims; 3) to distinguish between claims accepted into the scientific corpus and claims taken as a basis for action. An additional prerequisite of 4) simplicity and systematicity is desirable, if the model is to be applicable. Methodological documents from international institutions and regulation agencies are used to illustrate the prerequisites. A model combining Hansson's corpus model (preserving the epistemic integrity of the scientific corpus and distinguishing it from other claims taken as a basis for action) and Betz's conception (stating uncertainties associated with scientific claims) is proposed. Future directions for research regarding the relevance and the consequences of the philosophical debate on values in science are finally suggested.

 doi.org/10.32388/Y10XW3

1. Introduction

1.1. The ‘value-laden turn’

In the last decades, the philosophy of science has clearly shifted towards allowing always more influence for non-epistemic values in all phases of scientific inquiry. Although this normative[1] stance covers a host of different positions (for helpful review and classification, see Elliott (2022) and Holman and Wilholt (2022), respectively), it might in general be called the ‘value-laden ideal’ (VLI), as opposed to the value-free ideal (VFI) of science which normatively excludes non-epistemic values. Both ideals can be applied to all phases of scientific inquiry, in their more or less strong versions:

1. the ‘upstream’ phase of
    1. defining research avenues (answering the question of *what* to investigate);
    2. choosing evidence and methods (*how* to investigate it);
2. the ‘core’ justification phase of accepting or rejecting claims (*what to conclude* from the investigation);
3. the ‘downstream’ phase of communicating and using results;
4. the ‘parallel’ phase of organising research (including with respect to research participants)[2].

It is essentially the core phase 2 (and to a lesser extent the upstream phase 1.b) which is still controversial: there is now consensus that non-epistemic values should permeate all other phases. I will therefore focus on this phase 2 here, leaving aside other arguments for or against values in science (within the so-called ‘gap’, conceptual and aims arguments (Elliott, 2022, §3.2 to §3.5))[3]. This phase covers, but also exceeds (since it also deals with ‘true positives’ and ‘true negatives’), what is sometimes called the ‘inductive risk argument’ or ‘error argument’, according to which a scientist has to consider the risk of being in error in accepting or rejecting a hypothesis, by either wrongly accepting an actually false hypothesis (‘false positive’) or wrongly not accepting an actually true hypothesis (‘false negative’) – an argument originally appearing in Churchman (1948) and clearly formulated by Rudner (1953), and especially developed by Douglas (2000; 2009; 2017).

A majority of philosophers[4] now seems to endorse the strongest form of the VLI, which allows non-epistemic value influence on all phases of scientific inquiry, including the acceptance/rejection (A/R) phase[5]. Inasmuch as this ‘value-laden turn’[6] promotes the social responsibility of science, it is of course to be welcomed. However, it can also threaten the epistemic integrity of science, something many authors seem less concerned about. It seems that, in the current philosophical trend to advocate for always more value influence in science, the very goal of (empirical) science, which is to provide statements of *facts* – as opposed, precisely, to *values* – about the world in the most reliable way, has been somewhat lost of sight. For example, Douglas (2017), one of the major proponents of the VLI, claims, on both the descriptive and normative levels, that non-epistemic values not only should influence all aspects of the scientific enterprise, but that they are necessary, inevitable for its practice, in other words that it is impossible to do science without recourse to non-epistemic values. Brown (2013; 2017) has even disputed the 'lexical priority of evidence' over values, and argued that evidence may be supplanted by values in some cases. Only a few authors (such as Betz (2013; 2017) who excludes non-epistemic values altogether; Hansson (2014; 2017a; 2018; 2020b) who accepts non-epistemic values but only if they respect the integrity of science; or Lacey (2017) who excludes non-epistemic values for claims 'impartially held') still resist this trend.

Against this trend, and reviving the legacy of the VFI, this article argues for the need to *minimise* as much as possible (although not exclude) the influence of values in the A/R phase. Noting that the original arguments for the VFI (ensuring the epistemic integrity of science, respecting the autonomy of science results users, preserving public trust in science) have not been satisfactorily addressed by proponents of the value-laden ideal, it proposes four prerequisites by which any model for values in the A/R phase should abide. The first three prerequisites are not new, but they are further developed here, linked to the literature and defended against objections, illustrated by several brief examples, and assembled to constitute a good basis for incorporating values in science. A first, fundamental principle is to distinguish between facts and values. Thereof, three prerequisites follow: 1) to ensure the epistemic integrity of scientific knowledge; 2) to state clearly the uncertainties associated with scientific knowledge; 3) to distinguish between scientific knowledge and claims taken as a basis for action. An additional prerequisite of 4) simplicity and systematicity is desirable, if the model is to be applicable. Some reports from regulation and intergovernmental agencies are used to illustrate the applicability of this approach, where the influence of non-epistemic values is indeed minimised. A model combining Hansson's corpus model (incorporating values in the scientific corpus while preserving its epistemic integrity) and Betz's conception (stating uncertainties associated with scientific claims while excluding values) is proposed, trying to keep 'the best of both worlds', so to speak, i.e. to allow values in the A/R phase (the 'take-away message' of the VLI trend) but only if the epistemic integrity of science is preserved (the legacy of the VFI). This model allegedly better corresponds to scientific and policy practice than many VLI proposals. Future directions for research are finally suggested, stemming from the need for philosophy of science to self-reflect on its own values, concerning the relevance and the consequences of the philosophical debate on values.

1.2. Preliminary remarks

Before all this, a few preliminary remarks are necessary.

1.2.1. Types of decision

Firstly, a terminological remark is in order. In the following, *scientific* (*non-scientific*, respectively) values will designate what is usually called 'epistemic values' ('non-epistemic values', respectively), in order not to cause confusion with the corresponding decisions[7]. Indeed, following Stamenkovic (ming), I distinguish between:

1. Theoretical decisions (concerning knowledge), made up of:
    1. Epistemic decisions, concerning our choices of what to believe (i.e. our choices to acceptor reject a claim);
    2. Non-epistemic decisions, concerning our choices of what to do in order to achieve theoretical aims, related to the pursuance of knowledge (in other words, our choices of theoretical action);
2. Practical decisions, concern our choices of what to do in order to achieve practical aims (notrelated to knowledge), in other words our choices of practical action. Practical decisions are all non-epistemic.

Since science is just one way (although the most reliable and sophisticated one) to gain knowledge, scientific decisions should be viewed as a subcategory of theoretical decisions, which also include non-scientific decisions. Scientific decisions can be either epistemic (choice to accept or reject a claim) or non-epistemic (during all our scientific endeavours, for example when we choose research avenues, and in general when we decide to perform actions in order to gain further information). Both types of scientific decisions can be imbued with (either scientific or non-scientific) values. All practical decisions are non-scientific.

In order not to cause confusion with epistemic and non-epistemic scientific decisions, which are both based primarily on scientific values[8], it is less misleading to talk of scientific values rather than epistemic values[9] (which might suggest that only epistemic decisions are concerned). In addition, since science is a subdivision of theoretical rationality, it is also more accurate to talk of scientific values than epistemic values (which, if they relate to non-scientific epistemic decisions, may be quite different from scientific values such as empirical adequacy or consistency with other theories). This terminology also has the advantage of illustrating the conception advocated here, namely, that non-scientific values usually have no place in the A/R phase of science[10].

1.2.2. Level of evidence required

Secondly, it is helpful to think in terms of the level of evidence required (LER) to accept a claim. This simple, general characterisation varies of course according to the disciplinary field: it can be quantitative, such as the level of statistical significance or just an instrument reading; semiquantitative, such as the size and colour intensity of a protein band on a Western blot membrane; or qualitative, such as answers to interviews or surveys. It is influenced by scientific values (e.g. consistency with already held claims), as well as, potentially, non-scientific values (e.g. public health or safety). It illustrates all the scientific (empirical, theoretical and value-laden) and potentially non-scientific (e.g. regarding the practical applications of the claim) considerations related to the acceptance of a claim. Admittedly, talking of the LER in general is a simplifying idealisation[11], but so are many concepts in philosophy of science, and it is very helpful inasmuch as it accurately captures the fundamental idea and requirement for accepting a claim (namely, that there is a certain requirement related to the evidence we have, which can be more or less precisely expressed) and for balancing false negatives vs false positives (which is the chief concern in the argument about inductive risk). The LER can be stated both at the level of individual scientific publications, and at the meta-level of meta-analyses and systematic reviews which assess and synthesise individual scientific publications bearing on the same claim, for scientific or non-scientific (e.g. regulatory or clinical) purposes. It also corresponds to the general 'weight of evidence' approach adopted by many agencies or institutions providing scientific expertise, which basically consists in trying to measure as objectively, exhaustively and relevantly as possible the evidence supporting or undermining a hypothesis[12]. For example the IARC[13] Monographs on the Identification of Carcinogenic Hazards to Humans identify carcinogenic substances and exposures on the basis of qualitative assessment of human, animal and mechanistic evidence. Regarding for example carcinogenicity in humans, it classifies the evidence from studies in humans into four categories: 'Sufficient evidence of carcinogenicity', 'Limited evidence of carcinogenicity', 'Inadequate evidence regarding carcinogenicity' and 'Evidence suggesting lack of carcinogenicity' (IARC, 2019, 31-32). Note that although the definition of such categories is of course arbitrary hence value-laden to some extent (there might have been for example more categories), nevertheless the categories are based on scientific values (for example 'sufficient evidence' is based on studies 'in which chance, bias, and confounding were ruled out with reasonable confidence[14]', 31).

1.2.3. Relevance of non-scientific values for a claim

Thirdly, there are three remarks to be made regarding the very relevance of non-scientific values for the A/R phase. First, this issue only arises in cases of *uncertainty* associated with the claim in question, i.e. when neither empirical evidence nor theoretical understanding enable to accept or reject it 'beyond reasonable doubt' (Betz, 2013). In this case, since the LER to accept the claim is neither empirically nor theoretically determined, values are required. In all other cases[15]where there is (practically) no uncertainty, values are irrelevant. Note that although in principle *no* empirical claim can ever be inductively inferred with certainty (hence in principle any empirical science would be susceptible to values), *in practice* there are many scientific claims which can be made beyond reasonable doubt (see again Betz) – indeed, this is how scientists and non-scientists alike proceed all the time.

Second, and again although in principle the issue of non-scientific values is applicable to any claim, in practice it is limited to claims which have *clear* non-scientific consequences, in other words for socially relevant disciplines (or parts thereof), such as regulatory toxicology, medical science, pharmacology, etc.[16] If there are no non-scientific applications, then non-scientific values are irrelevant. This point is usually far from clear in the philosophical literature, and should be clarified for each conception (as e.g. Douglas (2000, 577)[17] or Betz (2013, 210-211) do). Indeed, many participants to the debate on values often give the impression that their conception applies to science in general, whereas their examples or case studies are taken from policy-relevant disciplines such as toxicology, climate science, medical science, etc.

In addition – and this is the third remark – these examples sometimes do not even come from the scientific literature, but from reports for regulation or policy purposes written by various governmental agencies or institutions. That such science-informed claims for action-taking should naturally be influenced by values, and distinguished from the scientific corpus, will be argued for in section 2.4.

2. Prerequisites for a model for values in science

As said in the introduction, the majority of the philosophical literature now allows value influence in the A/R phase (in particular following the so-called inductive risk argument), including when this means decreasing the LER to accept a claim. Such a strong version of the VLI threatens the epistemic integrity[18] of science, and its proponents do not seem to have fully assessed its scientific and non-scientific consequences. There are both scientific and non-scientific reasons[19] for minimising as much as possible the role of non-scientific values in the A/R phase. Starting from the fundamental distinction between fact and value, I will argue in the following that respecting the epistemic integrity of science – which was the major motivation behind the VFI, and represents its legacy – is a *conditio sine qua non* for any model of values in science, otherwise insurmountable problems both within science and outside are to be expected. Another prerequisite is that the model does not cover up scientific uncertainties with values, for similar reasons as well as reasons specifically related to policy-making. Finally, we should distinguish between the scientific corpus and claims that we take as a basis for non-epistemic decision-making, because while we want to preserve the epistemic integrity of science, we also want to be able to choose other LERs (in particular lower ones) for non-epistemic decision making (e.g. to avoid a potential danger).

2.1. The distinction between facts and values

I take the distinction between facts and values for granted here and refer to Hansson (2017a; 2018) and Stamenkovic (2022)). In a nutshell, separating facts (more precisely, factual beliefs) from our mental attitudes towards them is a fundamental and necessary ability without which our life both at the individual and collective levels would be impossible. Identifying facts is in particular what we (try to) do in science, which provides us with 'a common repository of reliable factual beliefs' (the scientific corpus, see below) (Hansson, 2018, 66, my translation), in contradistinction to values which vary with the individual or the community. A science based on facts (further generalised in the form of laws and principles) represents the ideal of scientific inquiry. This is indeed how most people (scientists, policy-makers, lay persons) view science: as an enterprise aiming for truth and stating facts. Distinguishing between facts and values is thus a fundamental requirement, which, even if not always fulfilled, represents an ideal towards which we must strive – and which we reach in fact very often in a satisfactory way, both in science and outside (including, most prominently, in everyday life). This fundamental requirement in turn requires that:

doi.org/10.32388/Y10XW3

1. we ensure the epistemic integrity of scientific knowledge (as a repository of factual statements);
2. we state the uncertainties associated with scientific statements clearly (in order not towrongly count as factual, statements which are uncertain);
3. we distinguish between scientific statements and claims that we take as a basis for nonepistemic decision-making (in order not to wrongly count as factual, statements whose LER has been deliberately lowered).
4. An additional prerequisite is that values be managed in a simple and systematic way (seesection 2.5).

The first three prerequisites support the traditional arguments in favour of the VFI (in addition to providing new ones, see below), as summarised by Elliott (2022, §3.1), and whose enduring relevance has not been satisfactorily answered by proponents of the VLI.

The first reason in favour of the VFI is, obviously, related to the pursuit of truth, which is the primary goal of science. Since non-scientific values do not as such contribute to the attainment of truth, there is no reason to expect they will help the scientific enterprise which is precisely to produce true statements (McMullin, 1982), but rather detract from it (all the more so because of their endless variability[20]). The preservation of the truth of scientific statements is not sufficiently taken into account in much of the literature on values in science. The following will mainly deal with this issue.

The second reason is related to the moral autonomy of both individual and collective users of science results (Betz, 2017, 99). Allowing decision-makers to make their own choices on the basis on their own values (instead of those of scientists', or any other persons) respects the moral autonomy of individual decision-makers and/or the democratic character of collective (political) decisionmaking. Traditionally, democratic decision-making is based on a division of labour between political decision-makers who are responsible for the normative part of policy justification (setting the goals of policies and their relative weights) whereas scientists (when acting as experts) are responsible for the descriptive part of policy justification (explaining the ways to reach those goals) (Weber, 1949). Again, this argument presupposes of course that, besides their own, separate values, decision-makers have information about (scientifically established) facts at their disposal, on which to base their choices. The concern about the autonomy of decision-makers has been variously addressed by proponents of the VLI, but there is no consensus and the proposals are often complicated. I will briefly come back to this concern in section 2.3.

doi.org/10.32388/Y10XW3

The third reason is related to public trust in science: intuitively, a value-laden science seems less trustworthy than an impartial, value-free science (and indeed, famous examples include the so-called 'climate-gate' which, although unfounded, led to a decrease of public trust in climate science in the US (Lewandowsky et al., 2015)). This point has recently begun to be empirically investigated on the basis of on-line experiments (Elliott et al., 2017; Hicks and Lobato, 2022), but more studies are needed to assess this phenomenon, with other methodological approaches and especially for other countries (where political cultures may be very different). The results are not clear-cut (rather unfavourable to the VLI for Elliott et al. (2017), neutral for Hicks and Lobato (2022) and even beneficial in case of a scientist acknowledging the value of public health) and they add again complexity to the management of values. The question of the representativity of such online-experiments is crucial. I will return very briefly to the issue of public trust in section 2.3.

2.2. *Preserving the epistemic integrity of scientific knowledge*

The first, absolutely essential prerequisite for a model for values in the A/R phase is that it respect the epistemic integrity of science. By epistemic integrity (a term adapted from Hansson (2018)) I mean that the truth of scientific statements is protected from illegitimate influence of (conscious) values, (unconscious) bias and other distorting factors. It is a concept more specific than truth, which insists on the absence of undue influence in knowledge claims (if applied to the (main) product of scientific activity, i.e. the scientific corpus) or knowledge production (if applied to science considered as an activity: in this case it means that epistemic decisions are taken properly)[21]. In the following I will only talk of the epistemic integrity of science for short, assuming both meanings are ultimately equivalent. Ensuring the epistemic integrity of science thus means ensuring that the truth of scientific statements is preserved and not undermined by value influence (which does not necessarily mean that the latter is excluded).

Of course epistemic integrity is not foreign to proponents of the VLI. As Holman and Wilholt (2022, 211) put it, 'that some values must, at times, play some role, does not entail that anything goes', and if one accepts that values should play a role in the A/R phase, the whole point is to distinguish between legitimate and illegitimate value influence (what they dub 'the new demarcation problem', in analogy with the old one between science and non-science). One can also find this concern articulated in Douglas (2009, 148), who wants to 'illuminate the sound science-junk science debate, with junk science clearly delineated as science that fails to meet the minimum standards for integrity', or Resnik and Elliott (2023) who equate this 'new demarcation problem' with the distinction between good and bad science. But in contradistinction to these authors, I believe that the best way to approach this problem of epistemic integrity is, quite naturally, to centre the approach on scientific knowledge, rather than on individual scientists and their decisions or cognitive attitudes, or scientific communities and their conventions, as is usually done[22]. For this I rely heavily on Hansson (2007; 2010; 2014; 2017a; 2018; 2020b) (for a summary, see Stamenkovic, ming).

Scientific knowledge is represented by scientific statements, gathered in the scientific corpus. The scientific corpus is the 'common repository of factual statements' provided by science and mentioned above. It is basically the total body of scientific knowledge, in the form of published scientific literature (articles and textbooks) (see e.g. Hansson, 2018, 68-71). The corpus is interdisciplinary, universal and hence unique; and it is apt to any (scientific or non-scientific) application since it represents our best available, most reliable (although always revisable) knowledge (e.g. Hansson, 2007)(see again Stamenkovic, ming, for a detailed summary). To my knowledge the first to mention the concept of scientific corpus is Hempel (1960; 1981), Hansson being the one who most developed it. The idea that the epistemic integrity of the scientific corpus should be preserved appears (in a way which in principle excludes non-scientific values) in Hempel (1965, 91-92), where he claims that science as a system of knowledge should not presuppose values, although he acknowledges that values influence the methodological aspect of accepting or rejecting claims, which of course has a direct impact on the content of the system of knowledge itself[23].

I believe this descriptive-normative characterisation of the scientific corpus corresponds fairly well to good scientific practice, as well as to the uses made of scientific knowledge outside of science. If one accepts this conception of scientific knowledge, there are various reasons for preserving its epistemic integrity, in other words to ask for high LERs to accept a claim into the corpus:

1. Scientific reasons:
    1. Epistemic reasons (regarding the truth of current research results): scientists are famously 'cautious' and 'conservative', reluctant to state claims if they are not very unlikely to be false. In other words they prefer – inside science – false negatives to false positives (I take this descriptive claim to be widely shared[24]). In terms of the scientific values of error avoidance and unsettledness avoidance (Hansson, 2020b)[25], they prefer the former to the latter. What is more, and in spite of this (partly descriptive and partly normative) scientific

doi.org/10.32388/Y10XW3

ethos, there are already enough problems with the epistemic integrity of science, regarding current levels of evidence (see the so-called 'reproducibility crisis' in practically all the empirical sciences (Baker, 2016)) and detrimental value influence (e.g. the 'publish or perish' culture, research misconduct, etc. (Begley and Ioannidis, 2015)), not to add new ones.

2. Non-epistemic reasons (regarding the truth of future research results):
    1. Relatedly, and somewhat between epistemic and non-epistemic reasons, since what lies in the corpus represents our best available knowledge, it does (should) not require further investigation (the burden of proof falls upon those who want to modify it), so that resources are liberated for other research. Therefore we want to make sure that what is incorporated in the corpus is correct, since it will (should) not be re-examined.
    2. Future research is based on current research, hence the progress and productivity of science require solid knowledge to build on, on pain of leading research into dead-ends. Note that if the corpus did not have high entry requirements, both the integrity and the productivity of science would be threatened, whereas with high entry requirements only the productivity of science is threatened, not its integrity (obviously a trade-off between these two goals has to be made, and one cannot increase indefinitely the LER).

2. Non-scientific reasons:
    1. Direct non-scientific reasons (related to reliability): since the scientific corpus is used asa general, multipurpose repository of knowledge, it must have high entry requirements, in order to be applicable to any use (e.g. in applications of science such as engineering for building bridges or aircrafts, or clinical medicine for treating patients, or policy-making for deciding to authorise or ban a pesticide, etc.). Obviously, some non-scientific values (such as safety, health, non-maleficence, etc.) directly demand high entry requirements.
    2. Indirect non-scientific reasons (related to what might be called reliable productivity): ensuring that research is based on reliable results (in accordance with reason 1.b) also paves the ground for further socially beneficial research. Inversely, accepting false hypotheses into the corpus (e.g. in toxicology) would be detrimental to its usefulness (for example it would hinder our understanding, detection and prevention of adverse effects of toxic substances).

Because of all these reasons, the corpus must keep high entry requirements. For a given claim, whatever non-scientific values are considered, they must not be allowed to decrease the LER to accept it into the corpus.

2.3. Stating uncertainties

Respecting the epistemic integrity of science implies that uncertainties associated with scientific claims be stated clearly, instead of being bridged or covered up with values (in which case the scientific corpus may well contain erroneous claims, with all the detrimental consequences mentioned above). Additional reasons for stating uncertainties include:

- Scientific reasons:
    - If the uncertainties associated with a claim are hidden or discarded, and if instead the claim is accepted into the scientific corpus (on the basis of values), it will probably discourage further investigation of the claim, by definition of the scientific corpus. Since the corpus represents our best available knowledge, what lies in it is taken for granted and does not require further investigation (the burden of proof falls upon those who want to modify it)[26]. On the contrary, stating the uncertainties clearly will motivate further investigation, since the matter will be considered unsettled.
    - On a more concrete level, on may wonder how a scientist would react if she was told to accept a claim which she considers uncertain. Such an attitude would contravene the ethos of science, a fact which has to be taken into account by any model for values in science.
- Non-scientific reason: stating uncertainties is of course especially important for non-scientific decision-making, where, if the autonomy of the decision-maker(s) is to be respected (as seen above), the distinction between (scientific) judgements of fact (or risk assessment) and (nonscientific) judgements of values (risk management) must be clear. It seems that, to a large extent, this is indeed how scientific expertise works (see the examples below).

Conversely, failure to do so may create what Wagner (1995) famously dubbed ‘science charades’, where scientists or decision-makers, by covering up uncertainties with values instead of acknowledging them, disguise normative choices as facts. By doing so, they take sides in, and feed, intractable controversies, which could be solved if they agreed on the uncertainties involved and focused instead the discussion on the normative choices involved[27]. This example is analysed in more detail in the next section.

 doi.org/10.32388/Y10XW3

2.3.1. Science charades

Although Wagner also mentions scientists (apparently acting as researchers) covering up uncertainties with values (1628), her long, and extremely well documented essay is focused on environmental regulation agencies and scientists acting as experts, from the perspective of legal science. It is a nice illustration of many of the reasons invoked above. Wagner (1995, 1617) defines ‘science charades’ as situations ‘where agencies exaggerate the contributions made by science in setting toxic standards in order to avoid accountability for the underlying policy decisions’[28]. The main motivation for regulation agencies to engage in science charades is to protect their rulings against judicial reversal (which they experience on a regular basis): cast as decisions purely based on science, the agency rulings are less likely to be reversed by reviewing courts, who will be more willing to respect the agency’s area of expertise (1661-1667)[29]. But the detrimental consequences of science charades are numerous, among others:

- policy judgments disguised as scientific facts make public scrutiny of policy (by scientists, policy-makers or the lay public) impossible, since one does not know where the science ends and policy begins (1628, 1686)[30] (this is an illustration of the autonomy argument);
- inconsistencies in regulation (between different agencies or even departments of the same agency) can happen if scientists impose their own value judgements (1639);
- science charades self-perpetuate themselves, since different interest groups (representing industrial, environmental, consumer or other interests) also tend to disguise their preferences as science issues, opposing (allegedly) counter-scientific claims instead of addressing the underlying policy choices where they have less chances to win their case (1657-58);
- science charades also discourage further research to elucidate scientific uncertainties (since the latter are not acknowledged), and consequently may lead to detrimental extra-scientific consequences (1687) (an illustration of the argument above);
- science charades make science appear adversarial rather than truth-seeking (1688), hence undermining public trust in science (an illustration of the public trust argument above).

In the face of these, and many other, detrimental consequences, Wagner recommends that agencies clearly distinguish between policy considerations and the science behind their decisions, and that they state clearly the uncertainties concerning the science (1706-1709). Wagner’s article has been criticised for its characterisation of trans-scientific issues, allegedly understating the role science can play in some of them, thereby falling prey to the opposite, '"reverse science charade"', where 'agencies (or others) exaggerat[e] the *limitations* of science, and risk analysis, in order to justify regulation on the basis of policy choices – choices that are commonly embodied in default assumptions and safety factors' (Conrad Jr, 2003, 10306). But whatever the accuracy of Wagner's description of some trans-scientific issues, the bulk of her normative argument remains – as indeed Conrad Jr (2003, 10306) concedes: the best way to avoid both the science charade and its reverse is to clearly state what falls under values and what falls under science, neither over- nor under-estimating the latter[31].

It is important to note the similarities between science charades and Douglas's (2017) conception. Of course Douglas does not recommend that experts hide their values and disguise them as facts, but rather that they publicly acknowledge them. Nevertheless this position results in a situation partly similar to science charades, and can bring about many of the detrimental consequences just mentioned. For Douglas (2017, 90-91) scientists acting as experts should deliberately use their own values to bridge inferential gaps (by setting the LER to accept a claim, and not acting as a reason to accept this claim[32]), and publicly acknowledge these values. Then, 'with values that help assess evidential sufficiency made apparent, the public can decide which experts match their own values most closely, and choose to rely upon those experts whose assessments of evidential sufficiency would most match their own' (2017, 91). According to Douglas, this would help 'resolving a disagreement among experts' (ibid.): 'making the values apparent also allows for informed debate on what the right values are in a particular case. Rather than undermining democratic accountability, rejecting the value-free ideal and making the values apparent can bolster it. What to ask of experts and where to focus debate is made clearer once we relinquish the value-free ideal.'

But on the contrary, one does not see how the public may hope to get out of the controversy, if the involved experts present conflicting facts on the basis of conflicting values – even if they are openly acknowledged. One seems just condemned, as Douglas puts it, to choose the expert closest to one's values, without any hope to distinguish what is factual from what is value-laden (how could a non-scientist, policy-maker or lay person, be able to separate herself what falls within facts from what falls within values?), hence making the discussion about values themselves impossible (or at least uselessly difficult) and relinquishing any hope to reach an agreement. Indeed, it seems much easier and efficient to separate values from facts, and focus the discussion on the former while agreeing on the latter. Thus, one does not see how a proposal such as Douglas's could 'bolster' democratic accountability[33], or make the debate 'clearer'.

2.3.2. *Other arguments in favour of stating uncertainties*

Among the few defenders of the VFI, Betz (2013; 2017) is known for having forcefully advocated the need to make uncertainties associated with scientific claims explicit. Such 'hedged' claims are sufficiently weakened to be certain 'beyond reasonable doubt' (in the same way as are, in general, many scientific statements which we consider certain in decision-making[34], although they are always revisable in principle). In other words these 'hedged' claims (stating uncertainty) are themselves exempt from uncertainty, therefore they do not require non-scientific values to manage inductive risk. Betz (2017, 101-102) mentions four types of uncertainties potentially bearing on scientific results (observational, model, theoretic and methodological uncertainty), and four methods for full uncertainty disclosure (comprehensive sensitivity analysis, non-probabilistic frameworks, higherorder probabilities and normative transparency). Note that like the LER, uncertainties can be stated at the level of either individual scientific publications or at the meta-level of meta-analyses and systematic reviews.

Uncertainties associated with scientific claims are typically stated in expert reports from regulation agencies or intergovernmental institutions, such as for example IPCC[35] Assessment Reports (for the latest summary for decision-makers, see IPCC, 2023) or IARC Monographs on the Identification of Carcinogenic Hazards to Humans (see the preamble IARC, 2019). Such examples show that the statement of uncertainties is paramount *even for non-epistemic (e.g. policy-making or clinical) purposes*, not only for the epistemic purposes related to the scientific corpus, and that these institutions do not advocate bridging uncertainties with values as many proponents of the VLI do. For example, the IPCC guidance note (Mastrandrea et al., 2010) defines two different and complementary measures of uncertainty, 'confidence' and 'likelihood'. Confidence is a two-dimensional measure of uncertainty based on the levels of evidence and degrees of agreement (positively correlated with both), expressed as five qualifiers: 'very low', 'low', 'medium', 'high' or 'very high' (2-3). Likelihood is basically the assignment of a probability range, distributed in seven categories: 'exceptionally unlikely' (0-1% probability); 'very unlikely' (0-10%); 'unlikely' (0-33%); 'about as likely as not' (33-66%); 'likely' (66-199%); 'very likely' (90-100%); 'virtually certain' (99-100%). Confidence is a precondition of likelihood: in order for likelihood to be expressible (at least D. a range can be given for a variable, or E. a likelihood or probability, or F. a probability distribution or set of distributions), confidence must be high or very high (except for D where it can be only stated if the likelihood or probability cannot be stated). Otherwise (in cases where A. a variable is ambiguous or not measurable, B. its sign can be

doi.org/10.32388/Y10XW3

identified but its magnitude is poorly known, C. an order of magnitude can be given) only confidence is given, not likelihood. Similarly, the IARC defines four categories of carcinogenicity to humans, on the basis of various levels of human, animal and mechanistic evidence: an agent can be either 'carcinogenic to humans', 'probably carcinogenic to humans', 'possibly carcinogenic to humans' or 'not classifiable as to its carcinogenicity to humans' (IARC, 2019, 35-37). In the same way, the methodological guidelines for endocrine disruptors (ED) of the French Agency for Food, Environmental and Occupational Health & Safety (ANSES[36]) define five categories of uncertainty on the basis of experts' subjective probability[37] assignments: 'known ED' (the median (50 quartile) of the subjective probability of being an ED is above 90%); 'presumed ED' (between 66% and)=%); 'suspected' (between 5% and 66%); 'non categorised' (the subjective probability of being an EDC, taking into account 95% (Q95>=5) of uncertainty is above 5% but the 5 percentile is below 5%); 'non ED' (the subjective probability of being an EDC, taking into account 95% (Q95<5) of uncertainty is below 5%).

Of course, these uncertainty categories, which are needed for communication purposes, are arbitrary to some extent, hence value-laden (like those of the LERs of the IARC). Steele (2012, 899) is probably right to argue that scientists must simplify their nuanced beliefs when communicating them to decision-makers. Therefore uncertainties probably cannot, and should not, be *fully* stated in a value-neutral way to decision-makers, and some translation into a standardised language (with uncertainty categories) is necessary Steele (2012), in particular for communication purposes (John, 2015a, 4). However, it is debatable whether this categorisation really has to be based on non-scientific values (as John and Steel argue), and whether it cannot instead be based (primarily, at least) on purely scientific values[38]. Indeed, the IARC insists that its categories are based on scientific values, such as absence of chance, bias, or confounding, quality, consistency, statistical precision (IARC, 2019, 31). Similarly, the ANSES (2021) formalises its assessment process (on the basis of the Sheffield method for sharing information and expert opinions in order to reach a consensus[39]), making it as much as possible rule-governed rather than based on values (7, 11/20), and the only values invoked are scientific, such as repeatability, empirical support, consistency, specificity, traceability (26, 30/60), absence of bias, transparency, reliability (33/60)[40]. The ANSES also recommends to 'state the level of uncertainty *without reference to any specific regulation context*', and 'insists on the necessity that the evaluation of a substance with respect to the endocrine disruption danger be made, in view of its categorisation, *in a unique way, independently of any regulation context*' (ANSES, 2021, 10, 13, italics added), in other words

doi.org/10.32388/Y10XW3

independently of non-scientific values linked to these contexts. These elements, very much in conformity with Hansson's corpus model (seesection 2.4), illustrate the separation between factually evaluating what is known (risk assessment), and deciding on this basis (risk management). Note that even if such categorisation necessitated non-scientific values, it would concern expert reports for nonepistemic decision-making, not the scientific corpus (in conformity with section 2.4).

These reports also show that, contrary to what Elliott (2022, 27) claims, scientists hedging their claims *à la* Betz do not necessarily end up making 'extremely vague claims about a host of potential threats and opportunities', thereby being 'much less helpful' for decision-makers. For example, in the summary for policy-makers of the IPCC (2023) sixth assessment report, one can read statements such as: 'Historical cumulative net CO2 emissions from 1850 to 2019 were 2400 ± 240 GtCO2 of which more than half (58%) occurred between 1850 and 1989, and about 42% occurred between 1990 and 2019 (*high confidence*).' (4); 'In the near term, global warming is *more likely than not* to reach 1.5°C even under the very low GHG emission scenario (SSP1-1.9) and *likely* or *very likely* to exceed 1.5°C under higher emissions scenarios.' (12); or 'Over the next 2000 years, global mean sea level will rise by about 2–3 m if warming is limited to 1.5°C and 2–6 m if limited to 2°C (*low confidence*).' (18). Such claims are certainly quite precise and helpful for policy-making (for climate change mitigation and adaptation), including the last one made with low confidence. Conversely, the best or only way for scientists to be heard is not necessarily, as Elliott (2022, 27) claims, to avoid communicating uncertainties (see also Cranor (1990, 139)) and instead communicate plain results with the help of non-epistemic values (see also Douglas (2009, 135) and John (2015b, 82)). As Betz (2017, 107) remarks, this is indeed 'a very ambitious social prediction' which must be empirically assessed. In addition, within research, pushing for clear cut results can promote publication bias while reporting confidence intervals and probabilities can reduce it. For example, Cumming (2012) has shown that estimation of size and confidence interval decreases publication bias, whereas the dichotomous nature of null hypothesis significance testing, based on an acceptance / rejection threshold, facilitates it (Meehl, 1967)(quoted in Fidler and Wilcox, 2021).

Another objection[41] to stating uncertainties is based on higher-order probabilities: stating probabilities for a claim (i.e. stating the uncertainty) would itself require second-order probabilities bearing on the first statement (for example, it is highly likely that it is high unlikely that it will rain tomorrow). But according to Schurz (2013), 'the practical relevance of *n*th-order probability statements diminishes rapidly with increasing *n*, so that, for example, a 5th-order probability

 doi.org/10.32388/Y10XW3

statement can be considered as virtually certain for all practical purposes' (Betz, 2017, 104). In fact, it seems that we never, or very rarely[42] assign second order probabilities. For example in the IPCC summary mentioned above, there are no second-order probabilities (note that 'confidence' should not be interpreted as such, as explained above). Neither are they mentioned in ANSES methodological guidelines for endocrine disruptors.

To conclude this section, Betz's conception seems a very good candidate for stating uncertainties as neutrally as possible (given the remarks mentioned above about communication), and minimising non-scientific values as much as possible. However, it does not allow values at all[43], and this is problematic for taking non-epistemic decisions. But there is a very convincing model for doing so, namely Hansson's corpus model, to which I now turn.

2.4. Distinguishing between accepting a claim as true and acting as if it is true

Even if the epistemic integrity of science must be preserved and uncertainties stated clearly, it is also important to be able to take value-laden, non-epistemic (scientific or non-scientific) decisions on the basis of a given claim, for example to pursue research on the basis of a yet unproven hypothesis (scientific decision), to ban a substance which is suspected to be toxic although this is not scientifically established (non-scientific decision), or to use a scientific claim for applications with high safety stakes (non-scientific decision) (Hansson, 2017a). For such cases we may want to make our decision on lower (first two cases) or higher (last case) LERs than those for acceptance into the scientific corpus, and which are influenced by values (for example, if we have a suspicion[44] that a substance is toxic, we may want to ban this substance even if the toxicity is not scientifically established, thereby lowering the LER). Therefore values clearly should not be excluded from science *applications*, where we *use* scientific knowledge for non-epistemic (scientific or nonscientific) purposes (see Stamenkovic, ming, §2.1.2). Since, on the other hand, we still want to preserve the epistemic integrity of science, we have to introduce *separate* LERs for non-epistemic decision-making, i.e. we have to distinguish between:

- accepting a claim as true (epistemic decision to accept the claim into the scientific corpus); and
- doing *as if* the claim were true (non-epistemic decision to act on the basis of the claim).

Historically, Jeffrey (1956) is among the first to distinguish between accepting a hypothesis as true and accepting it as a basis for action (without committing oneself to its truth), in other words doing ‘as if’ it were true (Levi, 1960). Some recent authors have revived (Giere, 2003; Mitchell, 2004) or refined (Lacey, 2017) this distinction, developed especially clearly by Hansson (2014)[45]. Unfortunately, this essential distinction is often not made or unclear, be it by proponents of the VLI (such as Douglas, 2009, who takes examples in regulatory toxicology)[46], of the VFI (such as Betz, 2013, who takes the example of the IPCC), or of some middle-ground position (John, 2015a, who also takes the example of the IPCC)[47]. Recently, a discussion on the ‘cognitive attitudes’ of scientists has progressively developed (Elliott and Willmes, 2013; McKaughan and Elliott, 2015), which shows both the theoretical and the practical relevance of this distinction[48], and its potential for resolving problems related to values in science. This discussion, which provides very detailed and insightful analyses, has much in common with the present approach, and illustrates Elliott’s (2022, 36-37) remark that proponents and critics of the VFI may have closer positions than they initially appear. Nevertheless, proponents of the ‘cognitive attitudes’ approach do not build on this distinction to distinguish between the scientific *corpus* and other types of *claims*, and do not make clear that the scientific corpus should remain unaffected by these various cognitive attitudes. Rather, they focus on scientists’ mental attitudes related to this distinction, whereas I believe one should focus on the status of the claims themselves, which, once accepted into the corpus, become independent from the scientists who produced them (they become, as it were, scientific facts[49], in conformity with the fact / value distinction), and can be used for all sorts of purposes. More precisely, I agree that:1) the cognitive attitude of ‘believing’ a claim should correspond to the claim being accepted into the corpus; 2) that of ‘accepting’ a claim to the claim serving as a basis for action. In this latter sense, talking of the cognitive attitude of those acting on the basis of this claim seems indeed relevant (various people, including scientists, act as if the claim were true, i.e. entertain a certain cognitive attitude towards the claim, which varies according to the application). But in the former case the claim becomes independent from its potential applications, and becomes a fact, which imposes itself onto us, so to speak (Stamenkovic, 2022). This claim-based distinction also somewhat reflects the cognitive attitude-based distinction between the passivity involved in ‘believing’ in a claim (or in being confronted to a fact), and the deliberate will of ‘accepting’ a claim (acting as if it were true), underlined by McKaughan and Elliott (2015).

Apart from Hansson (whose model I directly borrow), the author closest to the conception advocated here is probably Lacey (2017), who distinguishes between: ‘impartially holding’ a hypothesis (which roughly corresponds to accepting a claim into the corpus here), which requires to exclude non-

 doi.org/10.32388/Y10XW3

scientific values (Lacey talks of social values); ‘adopting’ a hypothesis for further research (which roughly corresponds to a scientific non-epistemic decision here); and ‘endorsing’ a hypothesis for practical action (which corresponds to a non-scientific decision here). But it is Hansson who has developed the most complete and systematic claim-based model, in the course of several publications (2007; 2010; 2014; 2017a; 2018; 2020b), and which can be designated as the ‘corpus model’ (Stamenkovic, ming). Strangely enough, Hansson’s corpus model has been consistently ignored by the philosophical literature on values in science. I will not go into the details of this model here, and refer to Stamenkovic (ming) for a critical summary. The corpus model enables to distinguish the LER for Non-epistemic decision-making (LERN) from the LER for Epistemic acceptance of a claim (LERE), and hence to respect the epistemic integrity of science. Indeed, in case LERN > LERE, the LERE is raised accordingly, so that the epistemic integrity of science is only reinforced (see Stamenkovic, ming). Following Hansson, I think we may: not accept a claim as true, but act on its basis as if it were true; but not the converse (which is nevertheless envisaged by Elliott (2011b)), namely accept a claim as true but not act on its basis. Indeed, that would contradict the concept of scientific knowledge[50], as our most reliable knowledge, applicable to any use. Thus, accepting a claim as true implies accepting it as a basis for action, but the converse is not true51.

Hansson’s corpus model has many advantages (for a detailed study, see Stamenkovic, ming). Because it preserves the epistemic integrity of science, it also preserves its productivity, and indirectly preserves its (intra- and extra-scientific) applicability. In addition, the distinction between LERE and LERN promotes further scientific investigation:

- if LERE < LERN, then LERE is increased to LERN (following the corpus model), which necessarily requires further scientific work in order to reach this higher level;
- if LERN < LERE, the non-epistemic decision is taken on the basis of data which is insufficient to justify acceptance into the corpus: but probably some people (scientists and/or decisionmakers, e.g clinicians or regulators) will want to check if the claim is in fact scientifically established (this effect is similar to the statement of uncertainties mentioned above).

Conversely, the (not often discussed) disadvantages of not making the distinction are: that it damages the epistemic integrity of science, its productivity and its intra- and extra-scientific applicability; and that it may also discourage further scientific investigation and indirectly further reduce its applicability.

doi.org/10.32388/Y10XW3

Finally, Hansson's corpus model also has the advantage of synthesising different approaches to values in science, what Elliott (2011b) calls the 'logical distinction' (between values and scientific knowledge and method), the 'distinction based on consequences' (of accepting or rejecting a claim) and 'the distinction based on epistemic attitudes' (of believing in a claim or accepting it as a basis for action). Here, all three are dealt with: 1) the corpus model distinguishes between values and the scientific corpus, and how values can influence scientific methodology (i.e. acceptance or rejection of claims); 2) it considers the consequences associated with accepting a claim into the corpus or as a basis for action; 3) it relies on the distinction between the epistemic attitude of believing a claim and accepting it as a basis for action (although it is centred on the claims).

Among the potential objections to the corpus model, one can mention the objection (made to any model for dealing with inductive risk) that it is difficult to predict the non-scientific consequences of a claim (Stamenkovic, ming). As noted previously (footnote (43)), Betz (2017, 105) also remarks that there is an infinite regress in trying to predict the social consequences of a scientific statement: since these consequences are themselves uncertain, they require a moral management of their inductive risk, which in turn involves social predictions, and so forth. However, I tend to think that this sophisticated counter-argument can be neglected in the same way secondorder probability statements can (see above). On this aspect I side with Douglas who simply requires that all reasonably foreseeable applications of a claim be identified (Douglas, 2009, 6686). Admittedly, this can be difficult in itself (Stamenkovic, ming, §3.1), but not because of infinite regress, it seems. Finally, contrary to Elliott (2011b, 314, 319) who argues that scientists may not be able to do the distinction between belief and action in their daily practice, one can observe that it is already part of their daily practice both as researchers (exploring for example the consequences of a hypothesis or performing experiments on its basis, even it is not proven) and experts (recommending the ban of a substance suspected of being toxic even if it is not scientifically established). For example, this is how Bisphenol A was banned by ANSES (2013) for all articles in contact with food (on precautionary grounds), in spite of scientific uncertainty regarding the toxicity of the substance. Other European or national agencies have also adopted similar precautionary measures (Hansson, 2017b, 259). In general, the distinction between belief and action in case of negative effects of a claim seems widely accepted among experts and policy-makers, following the precautionary principle (Wiener and Rogers, 2002)[52]. In clinical practice, it is common to distinguish between high (low, respectively) requirements for establishing the absence (presence, respectively) of side effects (Hansson, 2018, 78). Rather than being a

doi.org/10.32388/Y10XW3

distinction about ‘psychological states’ as Elliott (2011b, 314) writes, it can be seen, very concretely, as a distinction between publishing something in the corpus (with all the rigorous associated process), and pretty much any other action performed in the scope of scientific activity (whether research or expertise) or its applications (e.g. in policy-making). Another illustration of this latter case is given by the Guidance on Information Requirements and Chemical Safety Assessment in its Chapter R.11. about the assessment of persistent, bioaccumulative and toxic (PBT) and very persistent and very bioaccumulative (vPvB) substances, written by the European Chemicals Agency (ECHA, 2017), which manages the technical and administrative aspects of the implementation of the European Union regulation REACH (Registration, Evaluation, Authorisation and Restriction of Chemicals). The guidance states that, following the assessment of the substance, only (96)

- [t] hree conclusions for the comparison of the relevant available information on the PBT properties with the criteria listed in REACH Annex XIII Section 1 are possible.

  i. **The substance does not fulfil the PBT and vPvB criteria**. The available information show that the properties of the substance do not meet the specific criteria provided in REACH Annex XIII Section 1, or if the information does not allow a direct comparison with all the criteria there is no indication of P or B properties based on screening information or other information.
  ii. **The substance fulfils the PBT or vPvB criteria**. The available information show that the properties of the substance meet the specific criteria detailed in REACH Annex XIII Section 1 based on a Weight-of-Evidence determination using expert judgement comparing all relevant and available information listed in Section 3.2 of Annex XIII to REACH with the criteria.
  iii. **The available data information does not allow to conclude** (i) or (ii). The substance may have PBT or vPvB properties. <u>Further information for the PBT/vPvB assessment is needed</u>.

Note that, contrary to what Biddle (2013) claims, this example shows that scientists acting as experts are not required to ‘bridge the gap’ of ‘transient underdetermination’ with values, and that they can simply state that the available data does not allow to draw a conclusion[53]. Now the guidance explicitly considers the second, *as if* alternative of the distinction discussed here (28):

> If the registrant[54] arrives at the conclusion (iii): **The available information does not allow to conclude (i) or (ii)**, he can also decide - based on REACH Annex XIII, Section 2.1 - not to generate further information, if he fulfils the conditions of exposure based adaptation of Annex XI, Section 3.2(b) and (c). Uniquely to the PBT assessment, the

 doi.org/10.32388/Y10XW3

registrant must additionally consider the substance "**as if it is a PBT or vPvB**", i.e. state that he wishes to regard the substance as a PBT/vPvB without having all necessary information for finalising the PBT/vPvB assessment. This option has exactly the same consequences for the registrant and his supply chain, as if the substance had been identified as PBT or vPvB based on a completed PBT/vPvB assessment.

In other words, in case of uncertainty and insufficient information, the regulation agency require the registrant to consider the substance as if it were PBT or vPvB, thereby lowering the LERN (this decision leaving of course open the issue as to whether the substance actually is a PBT or vPvB, since the LERE has not been reached).

2.5. Simplicity and systematicity

Finally, in addition to the previous prerequisites, it is desirable that a model for values in science be as *simple* and *systematic* (i.e. addressing all possible cases) as possible (Stamenkovic, ming). Scientists – and even more so decision-makers – who generally (and regrettably) do not have much time to indulge in philosophising about their practice, need a few, simple principles to follow, if the model is to be applied. The goal of the present article was to provide a few prerequisites for such a model. The model would be most useful if it could contribute to the following goals: 1) the philosophical discussion by conceptualising a descriptive-normative[55] ideal for values in science; 2) the formulation of professional guidelines for scientists acting as researchers (e.g. publishing academic papers or making presentations in academic settings); 3) the formulation of mandates for scientists acting as experts (e.g. providing advice or publishing reports for policy-making). It should not only be conceived abstractly, but really as a decision tool. Elliott (2022) underlines the need to formulate professional guidelines, and also criticises excessive complexity[56], but his own 'norm-based approach' nevertheless contains at least 9 different norms for good science, and at least 11 'rules, guidelines, policies and procedures for implementing' these norms (2022, 49-52), whose application and prioritisation must be made on a case-by-case basis and is left for further clarification. Such profusion of norms and guidelines, if used for policing scientific research (and not only for feeding the philosophical discussion), may also worsen over-regulation and bureaucratisation of research (including with respect to compliance requirements such as conflicts of interest or responsible conduct of research[57]) which already hinder scientists from actually performing research and instead force them towards administrative tasks and increased reporting (Mahoney, 1999)(for an overview,

doi.org/10.32388/Y10XW3

see Bienenstock et al., 2014, Introduction). Admittedly, many of these norms (e.g. transparency) or rules (e.g. policies that define and prohibit research misconduct, such as fabrication or falsification of data or plagiarism) are already (or should be!) implicitly endorsed by scientists. But listing them and expecting scientists to go through them exhaustively seems overly complex and unrealistic[58]. In addition, their formulation (e.g. 'rules or guidelines concerning standards of evidence for accepting or rejecting hypotheses') is often too vague to be helpful, and would require clarification and additional work.

3. Concluding remarks and future research

This article has shown why minimising as much as possible – not excluding – the influence of nonscientific values in the A/R phase is a reasonable approach. So far the original arguments for the VFI (ensuring the epistemic integrity of science, respecting the autonomy of science results users, preserving public trust in science) have not been satisfactorily addressed by proponents of the VLI. Starting from the fundamental requirement to distinguish between facts and values, this article has proposed four prerequisites for any model for values in the A/R phase: 1) to ensure the epistemic integrity of scientific knowledge; 2) to state clearly the uncertainties associated with scientific claims; 3) to distinguish between scientific knowledge and claims taken as a basis for action. An additional prerequisite of 4) simplicity and systematicity has been proposed, if the model is to be applicable. Some examples have shown that these prerequisites are actually implemented by international institutions and regulation agencies. There are notably two conceptual resources for implementing these prerequisites: Betz´s conception (for stating uncertainties, but it does not allow non-scientific values at all) and Hansson's corpus model (for incorporating non-scientific values while respecting the integrity of science and allowing for different LERs according to whether the claim is incorporated into the corpus or used as a basis for action). Taken together, they enable to respect the four prerequisites. Of course, I do not claim that this combination represents a final, unsurpassable model for values in science, but it constitutes at least a good basis to elaborate further, and answers major concerns expressed in the existing literature.

Beyond the conception advocated here, I would like to propose two avenues for further research suggested by the work in this paper. They both illustrate the need for a reflection on its values by philosophy itself (which, indeed, is also a science in the large sense (Hansson, 2017c)). Philosophy cannot forego reflecting on how values do, and should, influence its own practice, regarding the motivations, the relevance and the consequences (especially non-scientific) of this practice – indeed, such a reflective approach is consistent with, and required by, allowing values to influence science, which includes philosophy. This requirement demands in particular that we evaluate:

1. the enduring *relevance* of the philosophical debate on values for scientific practice (if that is indeed a claimed goal[59]), both for research and expertise;
2. the intra- and extra-scientific *consequences* of this philosophical debate.

With respect to the first point, it has become a kind of programmatic claim among some VLI proponents that values are inevitable in scientific practice. For example, Douglas (2017, 83-84) claims that 'none of these jobs [performed by epistemic values] can tell you whether the evidence you have is *strong enough* to make a claim at a particular point in time. [...] the "internal" or "epistemic" virtues of science are not designed to assist with the judgment of whether the evidence is sufficient. They can assist with assessments of whether the theory or claim at issue is minimally adequate, with how strong the evidential support is, and with whether further research is likely to be productive. The question of how strong the evidence needs to be remains unanswered by such considerations.' Brown (2013; 2017) has disputed the 'lexical priority of evidence over values', advocating 'an account [which] would allow that evidence may be rejected because of lack of fit with a favored hypothesis and compelling value judgements, but only so long as one is still able to effectively solve the problem of inquiry' (2013, 838). One thing seems clear: accepting a claim is not fully, algorithmically rule-governed (as is, probably, the vast majority of scientific activities[60]), and some value judgements are inevitable. This does not mean, however, these such values are *non-scientific*. It seems doubtful that not only a mathematician checking his proof, or a particle physicist setting his statistical significance level, but also a molecular biologist exploring the structure of an enzyme, a palaeontologist studying a fossil or even a toxicologist studying a structure-activity relationship of a molecule, have recourse to non-scientific values when making their claims. *Contra* Douglas, I rather think that scientific practice would be practically *impossible* if scientists had to take non-scientific values into account each time they make a claim – and not that they make such claims possible in the first place, as Douglas seems to think. It seems more plausible that in many (and probably most) cases, especially – but not only – for disciplines which don't have social implications, scientists follow their own, intra-scientific and intra-disciplinary standards of evidence (much in the spirit of Levi's (1960) 'canons of inference'), governed by intrascientific values, the first of which is probably, and simply, error avoidance. Brown's position seems even more extreme, and one wonders what the reaction of a scientist would be if she

 doi.org/10.32388/Y10XW3

was told to disregard evidence in favour of values. Such claims, which are apparently aimed at all scientific fields, do not seem to correspond to actual scientific practice and in any case must be *empirically* assessed[61].

It seems that general philosophy of science (as opposed to philosophy of the special sciences) tends to develop on its own, too far from scientific practice, and grow into endless analysis and refinement. For example, if second-order probability statements do not appear in expert reports, perhaps it is irrelevant for scientific and expert practice to devise sophisticated philosophical arguments on their basis. The same holds for infinite regress (see footnote (43) and p. 18). In the same way Betz uses the common scientific and decision-making practice of holding many scientific statements for virtually certain as a benchmark, and in the same way Hansson (2018) recommends that we should not build a model for values in science assuming we can behave like ideal Bayesian agent juggling with probability statements, I think it is important to create philosophical models for science which are realistic and take into account scientific and expert *practice*. This is typical of analytic philosophy to always look for more conceptual refinement and sophistication in argumentation, but the relevance and usefulness of these refinement and sophistication should not be lost sight of.

In this respect, I believe much is to be gained from the philosophical study of methodological documents from regulation and intergovernmental agencies or institutions such as the US Environment Protection Agency (EPA), the EFSA, the IARC, the Organisation for Economic Co-operation and Development (OECD, which is authoritative for setting standards of evidence in regulatory toxicological tests), the ECHA, or the ANSES. All these organisations develop methods and tools (such as the IRIS[62] at the EPA, or the GOLIATH[63] project which involves several European institutions) for performing systematic reviews and assessing evidence on a particular claim, following a weight-of-evidence approach[64]. The few examples briefly mentioned in this article suggest a minimisation of the influence of values and a maximisation of the role of evidence, an explicit statement of uncertainties, and go against the current value-laden trend in the philosophy of science, making the latter look unrealistic and far from scientific practice[65]. Of course, the process of evaluating evidence cannot be fully value-free, in the sense that the assessment is not governed by algorithmic rules (for example regarding the definition of uncertainty categories). Nevertheless, the methodological documents mentioned in this article all seem to *minimise* as much as possible this leeway and strive to provide an assessment as value-free as possible (again, this claim has been only briefly illustrated here and is left for further research). If such institutions minimise the influence of values in their reports, which are intended for specific (policy-making) applications, it seems to be an additional reason for doing so for the multi-purpose scientific corpus. Any conception in philosophy of science, even if normative, must take into account actual scientific practice, if it wants to be realistic, relevant and applicable. A normative conception impossible to apply (too unrealistic, too demanding or just too complicated) is useless. Of course if expert agencies indeed minimise the influence of non-scientific values, that does not mean that they *should* do so, and that does not automatically invalidate normative models following strong versions of the VLI. Nevertheless, this practice is a fact which must be taken into account by such models, to question their desirability (why do these agencies adopt such a minimally value-laden approach? what are their reasons?) as well as their possibility (is it realistic to advocate a strong VLI approach? is it possible to implement such models?).

With respect to the second point, the same overarching value of social responsibility invoked in favour of non-scientific values in the A/R phase can also be used against them, as we have seen: namely, if we want to have reliable scientific knowledge applicable for all sorts of purposes, and if we want to ensure the progress of this knowledge, we should not allow our standards of evidence to decrease for non-scientific reasons (value judgements). Accepting a claim on the basis of insufficient evidence and a value judgement, while being justified in a certain context, may have disastrous consequences in another. Hence, distinguishing between the scientific corpus and other types of claims, between accepting a claim as true and acting as if the claim were true, is absolutely essential.

In particular, great care must be taken with respect to the potentially detrimental extrascientific consequences that the philosophical debate on values may have, for example with respect to scientific dissent in disciplines with social impact (typically in medicine or toxicology). This holds not only with respect to ‘science charades’ or public trust in science, but, more critically, with respect to consumer and patient safety. Patients may for example require medical treatments insufficiently backed by evidence and motivated by non-scientific values, and use philosophical literature to support their case, in the same way an HIV/AIDS denialist has used an article by de Melo-Martin and Intemann (2014) on scientific dissent (which of course does not support this position) in support of his position (Hansson, 2020a, 22). Therefore, while the philosophical debate on values is of course to be welcomed like any philosophical discussion, it should also include a careful ethical reflection on its potential detrimental consequences and misuses. For example Elliott (2023) argues that scientific dissent about the Post-Treatment Lyme Disease Syndrome (PTLDS) can be understood as a dispute about value

doi.org/10.32388/Y10XW3

judgements (involved in assessing evidence for and against long term antibiotic treatments), and should be analysed using the philosophical literature on values in science. Although Elliott is careful to present the controversy as divided between a majority view (endorsed by medical authorities) advising against the use of long term antibiotic treatments given the associated risks and doubtful benefits, and a minority dissenting view advocating their use, he ultimately characterises the controversy 'as a dispute about value judgments' (13) rather than evidence. Hence 'patients suffering from severe long-term symptoms that could not be alleviated by other means' could choose long-term antibiotics treatments on the basis of value judgements (14). However, long-term antibiotics treatments can have serious detrimental effects, and there should be serious evidence suggesting their effectiveness to propose them to patients, if the ethical principle of *primum non nocere* is to be respected. According to this principle 'there must be a *large preponderance* of benefits over detriments in order for the treatment to be justified' (Hansson, 2020b, 386)[66]. But in the middle of the controversy, this clearly does not seem to be the case. While the question as to what causes PTLDS remains open, it is known that some patients experiencing the syndrome do not have laboratory signs of previous *Borrelia* bacteria infection, and it does not seem to be a plausible hypothesis that the syndrome is uniquely connected with Lyme disease (Nilsson et al., 2021). Again, this issue must be left for further research, but for now one can only recommend that great care be taken by philosophers on values in science when performing case studies on controversies still open, and even in general conceptual arguments which can have a social impact.

Footnotes

[1] There are also descriptive claims, as we shall see.

[2] For a more precise description of these phases, see Elliott (2022, 8). I am not aware of an 'extra-strong' version of the VFI which would exclude non-epistemic value influence from either the 'downstream' or the 'parallel' phase. Even the most stringent advocates of the VFI accept the influence of non-epistemic values (especially ethical ones) in these phases.

[3] I leave aside the issue as to whether the inductive risk argument can be considered a subcategory of the gap argument (ChoGlueck, 2018).

[4] It would be useful to conduct a systematic review about this proportion, but this lies outside the scope of this article.

[5] In the following, the VLI includes the A/R phase, and the VFI is limited to the A/R phase.

[6] In both senses of a 'value-laden' philosophical trend: advocating values in science (as its claim); and being itself value-laden (as its motivation), i.e. promoting the social responsibility of science, at the potential expense of its epistemic integrity. This trend can also be qualified as relativistic, in the sense that scientific facts are established relatively to the context (and hence values) of interest. Although this kind of philosophical relativism is different from, much more rigorous and less extreme than the one advocated by some authors in science studies (such as Latour and Woolgar (1986); Latour (1984) (for a critique, see Stamenkovic, 2020)), nevertheless it shares (to a lesser extent) the same approach to put into question conceptual distinctions such as that between facts and values, epistemic and non-epistemic values (e.g. Longino, 1996; Rooney, 2017), or science (descriptively establishing the facts) and politics (normatively deciding what to do with these facts) (Douglas, 2009; Kourany, 2010).

[7] Another equivalent wording could be that of *intra-scientific* and *extra-scientific* values.

[8] Scientific values play a role both for accepting or rejecting a claim (epistemic decisions), and more generally for choosing a research avenue, an investigation method, gathering evidence, etc. (non-epistemic decisions). The claim that these values play a primary role (as opposed to non-scientific values who only potentially play a role) is argued for hereafter, and additional research avenues are suggested in the conclusion.

[9] One might then say that the only epistemic value is truth.

[10] Of course, this terminology seems to completely exclude non-scientific values (such as ethical values) from science, which is not the view advocated here. But so does the terminology of epistemic / non-epistemic values.

[11] It also presupposes that a specific LER can be assigned to a claim in the first place, and that different LERs for various decisions can at least be sorted, as we will see below. But these are minimal presuppositions without which it seems difficult to say anything at all about values in the A/R phase.

[12] This extremely coarse characterisation is of course unsatisfactory but the study of weight-of-evidence approaches lies outside the scope of this paper.

[13] International Agency for Research on Cancer.

[14] Of course this term is itself value-laden, but again that does not mean that the values in question need be non-scientific. See section 2.3.2 and section 3.

doi.org/10.32388/Y10XW3

[15] Which I take to be the vast majority of all scientific statements, since those which are uncertain are those currently or recently investigated (the older, and the more deeply nested into the scientific corpus a statement is, the more certain it is). But of course non-scientific applications (especially policy-making) are often concerned with those most recent, uncertain statements.

[16] And in fact, even for such socially relevant disciplines, there may be many cases where scientists do not, and/or cannot, consider non-scientific values (see footnote (61)).

[17] Douglas seems to have later (2009; 2017) radicalised her conception of the VLI, apparently applying it to all of science, not just policy-relevant science.

[18] More on this concept in section 2.2.

[19] Although analysing the concept of reason falls outside the scope of this article, it seems it can be linked to a (normative) value judgement (a valuation). For example, a scientific reason for entertaining a hypothesis may be that we value the possibility to perform new experiments, and a non-scientific reason that we value public health.

[20] Much more than scientific values, it seems.

[21] Note that 'scientific integrity' is a wider concept (apparently first mentioned in OECD, 2007) aimed at delineating 'best practices' for fighting 'misconduct' in research. It applies to all phases of research (including upstream, downstream and parallel) and includes non-epistemic considerations (e.g. plagiarism, data fabrication or laboratory animal abuse).

[22] Although these conceptions are not excluded from the present approach, they are not central. The present approach is focused on the (main) product of science (which, as a human enterprise, can be characterised in many ways), i.e. the scientific corpus (see hereafter). In other words the approach is centred on (scientifically established) facts, which are represented by (empirical) scientific knowledge.

[23] This seems contradictory, since the hypothesis we accept (on the basis of values) *becomes* a scientific statement, part of the system of knowledge (supposedly without relation to values). Hempel remarks that: 1) values (the utility assigned to outcomes) are inevitable in decision-making such as hypothesis acceptance/rejection; 2) these values can perfectly be epistemic ones (as Hempel seems to have in mind, although he evokes other types of values, but then according to him this does not correspond to our usual conception of science). Still, his position seems contradictory: if values

doi.org/10.32388/Y10XW3

influence rules of acceptance/rejection into the scientific corpus, then the scientific corpus is value-laden.

[24] Even if, like any descriptive claim, it should be backed by empirical evidence from scientific practice, but this (enormous) task clearly falls outside the scope of this paper, and I take it for granted as many philosophers of science do (e.g. John, 2015b). There are at least two examples of (both intra-scientific and extra-scientific) detrimental consequences of this systematic preference (Stamenkovic, ming, §3.3), but they are not fully convincing nor sufficient to put it into question.

[25] Error avoidance (avoiding making false statements, i.e. avoiding false positives) means believing in as few erroneous statements as possible. Unsettledness avoidance (avoiding keeping issues open, i.e. avoiding false negatives) means believing in as many true statements as possible. Obviously, these two values conflict: prioritisation of error avoidance leads to increase the LER at the expense of unsettledness avoidance and may lead to false negatives, whereas prioritisation of unsettledness avoidance leads to decrease the LER at the expense of error avoidance and may lead to false positives. Equally obvious is the fact that the LER cannot be increased or decreased indefinitely: there is a trade-off to be made between error avoidance and unsettledness avoidance.

[26] Of course, things are usually not so clear-cut, and often several concordant studies will be needed before a phenomenon is considered known (this varies according to the disciplinary field). Nevertheless, each study is an element of this consensus (particularly powerful studies such as randomised clinical trials (RCT) in medicine) and often a few such studies are sufficient to close a matter (typically after a few concordant RCTs, all the more so because resources are limited).

[27] This example also illustrates the confusion between science and its applications, to which I shall return in section 2.4.

[28] Note that Wagner is concerned about what she calls, following physicist Alvin Weinberg, 'trans-scientific' issues: 'In contrast to the uncertainty that is characteristic of all of science, in which "the answer" is accompanied by some level of unpreventable statistical noise or uncertainty, trans-scientific questions are uncertain because scientists cannot even perform the experiments to test the hypotheses. This can be due to a variety of technological, informational, and ethical constraints on experimentation. [...] To reach a final quantitative standard, policy considerations must fill in the gaps that science cannot inform.' (1619-1620) A typical example of such a transscientific issue is the assessment of the carcinogenicity of a substance to which people are exposed at low doses, whereas

doi.org/10.32388/Y10XW3

the only ethical and practical way to settle the issue is to expose a small number of laboratory animals to high doses. Extrapolation from the latter to the former requires policy assumptions. Whatever the type of uncertainty considered (whether science *cannot* even eliminate it, or *has not yet* eliminated it), this does not affect my argument here.

[29] Wagner also documents other incentives which both scientists and policy-makers have in covering up policy judgements with science (1670, 1700). In particular, scientists enjoy greater prestige and get more funding (1673).

[30] Wagner (1995, pp. 1634-35) also mentions the interesting case of scientists who, instead of imposing their own values, look for always more scientific evidence, in the hope of settling the science-policy issue purely scientifically. By doing so, they only perpetuate the science charade and halt the regulation process. In either case, the only way out is to accurately distinguish values from facts.

[31] It is interesting to note that, while Wagner may be compared, on the descriptive level, to Douglas, Elliott, Brown or other proponents of the VLI who underline the importance of non-scientific values in scientific issues, she advocates an opposite course on the normative level (i.e. to distinguish between values and factual statements instead of incorporating the former into the latter).

[32] What Douglas respectively calls the 'direct' and 'indirect' roles for values. For a critique of this distinction, see Elliott (2011b).

[33] Another problem is that even if scientists declare their values as Douglas recommends, it is still doubtful that they will, or even can, be held accountable for those value choices, since they are not accountable as elected or appointed governmental officials are, as Wagner (1995, 1673) remarks.

[34] Such as 'the Earth has a moon' or 'Dinosaurs got extinct 65 million years ago'.

[35] Intergovernmental Panel on Climate Change.

[36] *Agence nationale de sécurité sanitaire de l'alimentation, de l'environnement et du travail.*

[37] Representing the measure of the expert's degree of belief in the plausibility that the substance studied has the potential to cause an adverse effect through an endocrine mode of action.

[38] In the same way Ruphy (2006) recommends evaluating Longino's (1990) value-laden background assumptions on the empirical basis of scientific values.

[39] The Sheffield method itself promotes the transparency, the reliability and the reproducibility of the elicitation. See for example O'Hagan et al. (2006); EFSA (2014).

[40] I found only one illustration of a non-scientific value (precaution) on p. 22: 'Dealing with human health, studies performed in environmental organism (ex. fish) can be considered only if they reinforce the level of evidence on the adverse effect.'; 'The knowledge of other members of the structural analogy substance could be used if these data can reinforce the level of evidence of an adverse effect.' Since this is an expertise document (not part of the corpus), such influence is not problematic, and indeed illustrates Hansson's model (see section 2.4).

[41] Anticipated by Rudner (1953) (and revived by Douglas (2009) and Steele (2012)) to refute a position stating uncertainties, first articulated by Jeffrey (1956).

[42] Note that weather forecasts (taken as an example by Betz) typically do *not* assign second-order probabilities. Rather, they either make a first-order probabilistic statement (usually for precipitation: 'it will rain tomorrow with 70% probability') or a deterministic statement (for non-precipitation weather: 'tomorrow it will be mostly sunny with some clouds').

[43] Elliott (2022, 26-27) seems to believe that Betz (2017) advocates another hedging strategy, that of explicitly stating all the values associated with claims, thereby also reaching virtually certain statements (conditional statements of the type "Given these non-epistemic value judgements (which we have used to fill the inferential gaps we faced because of substantial uncertainties) we arrive at the following findings:..." (Betz, 2017, 104)). In this way Betz's second strategy would come surprisingly close to the one of Douglas (2017) mentioned above. Elliott (2022, 27-28) then criticises the fact that it seems unrealistic for scientists to keep track of all their value judgements (see Havstad and Brown, 2017), and that even if they could, this would confuse decision-makers (following Elliott, 2011a). But clearly this is not Betz's position (2017, 105), who criticises this method of 'normative transparency' as 'not viable', because of the infinite regress associated with the argument of inductive risk, which relies on the prediction of the societal consequences of different types of errors in accepting / rejecting a claim. According to Betz, these predictions are highly uncertain and require a management of their inductive risk too, which requires further social predictions, etc.

[44] Note that this suspicion must itself be scientifically motivated, i.e. based on the same *type* of evidence and with the same *evaluation* of this evidence, as those of scientific claims which are accepted into the corpus. Only the *level* of evidence can be different (here, lower). See Hansson (2018).

doi.org/10.32388/Y10XW3

[45] For a rich list of references (excluding however Hansson) on this distinction, see McKaughan and Elliott (2015).

[46] For a critique aimed at clarifying Douglas's conception on this issue (and others), see Elliott (2011b).

[47] Arguments in favour the VLI based on the IPCC reports (such as John's (2015a) claims that they contain value-laden uncertainty categories, or that IPCC experts have to choose in a value-laden way what evidence to incorporate into the report) are thus irrelevant in the present conception, since the IPCC produces expert reports (explicitly dedicated to policy-making) and not literature to be incorporated into the corpus. Therefore such reports do not invalidate models (such as the corpus model) distinguishing the two. Conversely, arguments in favour of the present (closer to the VFI) conception, such as the explicit statement of uncertainties, are even strengthened when illustrated by IPCC or other expert reports.

[48] Note that participants to this debate often call 'believe' (a claim) what I call 'accept' (a claim into the corpus), and 'accept' (a claim) what I call 'act on the basis of' (a claim).

[49] More accurately, scientifically established facts.

[50] And of knowledge in general, since, as Hansson (2018) remarks, it makes little sense to claim that one knows something but then admit that one is not sure after all.

[51] This asymmetric conception does in fact justice to the American pragmatic conception according to which holding a belief implies being ready to act upon it (see Elliott, 2011b, 313). However, the equivalence is, again, in one sense only, and being ready to act on the basis of some claim does not necessarily imply that one believes it: I will take this unknown berry out of the hand of my little daughter even if I do not know that it is poisonous (I do not *believe* that it is poisonous, but I *act as if* it were).

[52] As noted above (footnote (51)), this is also a distinction we make all the time in our everyday life, especially for precautionary reasons (I forbid my little daughter to play on the road even if I don't know that cars are coming).

[53] Biddle (2013) addresses the underdetermination, or 'gap' argument (between theory and evidence), but his argument can be applied to the inductive risk argument as well.

[54] The company producing the substance.

[55] It bridges these two dimensions in the sense that it hopes to be a description of how science works at its best. The question of the realistic character of a model for values is addressed in the conclusion.

[56] He laments the fact that explicitly stating the values involved in reaching scientific claims is too demanding for scientists and too confusing for policy-makers (27-28).

[57] I am of course not saying that these aspects should not be regulated, and I am sure Elliott does not advocate bureaucratisation of research! Nevertheless, I do think there is a danger that such philosophical approaches may promote procedural aspects of research (at the expense of research itself), which are already burdensome and only increasing according to many scientists (Schneider, 2020).

[58] Most of these norms relate to phases outside the A/R phase, whereas for the latter Elliott mentions 'rules or guidelines concerning standards of evidence for accepting or rejecting hypotheses', leaving this essential issue (the only one controversial) in fact unaddressed. Without further precision, these rules or guidelines may well be similar to those advocated here.

[59] Of course, philosophising is also valuable for its own sake, but then one should be clear that one does not have any *scientific* pretence.

[60] One can perhaps think of the calibration of instruments, or performing standardised experimental tests, as counter-examples.

[61] In the same spirit as footnote (??), but this time with a look at the non-philosophical scientific literature, it would be interesting to try to assess empirically, and as systematically as possible: 1) the scientific fields where nonscientific values are irrelevant; 2) whether even in fields which are *prima facie* relevant for non-scientific decisions, there are many claims for which non-scientific values are irrelevant; 3) whether even for claims where non-scientific values are relevant, the latter do not make any difference with respect to the acceptance of claims. But contrary to footnote (??), it would probably be impossible to perform a truly systematic review of these issues (which would require to screen the entire scientific corpus), and one should be content with representative examples.

[62] Integrated Risk Information System.

[63] Generation Of Novel, Integrated and Internationally Harmonised Approaches for Testing Metabolism Disrupting Chemicals.

[64] There are of course many other tools and collaborations implementing weight-of-evidence approaches, for example the Cochrane or the GRADE (Grading of Recommendations Assessment, Development and Evaluation) collaborations in health care. An important difference between these institutions is that between regulation agencies which rely primarily on standardised (following OECD guidelines), often confidential data provided by manufacturers (although they take also into account scientific literature, but often to a lesser extent because it does not meet their standard requirements), and intergovernmental institutions such as the IARC or the IPCC which rely on scientific literature. Taken into account this difference is also essential for future research.

[65] The author is currently performing interviews of regulatory toxicologists to gather their own normative views on their practice, in order to bring empirical material to, and shed light on the philosophical debate. The ten interviews so far performed illustrate the importance for the interviewees to strictly separate risk assessment from risk management, and to exclude as far as possible non-scientific values from the assessment of evidence and the acceptance of a claim, even in such a socially impactful field as regulatory toxicology. This work will give rise to a subsequent publication.

[66] Of course one can mention the somewhat converse principle of *beneficence*, but for patients who are not terminally ill and 'have something to lose' such as those affected by PTLDS, the principle of *primum non nocere* can reasonably be considered to have precedence.

References

- ANSES (2013). Évaluation des risques du bisphénol a (bpa) pour la santé humaine.
- ANSES (2021). Avis de l'anses relatif à l'élaboration d'une méthodologie d'évaluation du caractère pe des substances chimiques en vue d'un classement en catégories «avérées, présumées, suspectées». Technical report, Agence nationale de sécurtié sanitaire, de l'alimentation, de l'environnement et du travail.
- Baker, M. (2016). 1,500 scientists lift the lid on reproducibility. *Nature 533*(7604), 452–454.
- Begley, C. G. and J. P. Ioannidis (2015). Reproducibility in science. *Circulation Research 116*(1), 116–126.
- Betz, G. (2013). In defence of the value free ideal. *European Journal for Philosophy of Science 3*(2), 207–220.

doi.org/10.32388/Y10XW3

- Betz, G. (2017). Why the argument from inductive risk doesn't justify incorporating non-epistemic values in scientific reasoning. See Elliott and Steel (2017), pp. 94–110.
- Biddle, J. (2013). State of the field: Transient underdetermination and values in science. *Studies in History and Philosophy of Science Part A 44*(1), 124–133.
- Bienenstock, A., B. L. Bassler, K. K. Droegemeir, A. I. Leshner, W. C. Lineberger, D. L. Souvaine, J. Leffler, L. M. Nichols, and J. J. Veysey (2014). Reducing investigators' administrative workload for federally funded research. Technical report, National Science Board.
- Brown, M. J. (2013). Values in science beyond underdetermination and inductive risk. *Philosophy of Science 80*(5), 829–839.
- Brown, M. J. (2017). Values in science: Against epistemic priority. See Elliott and Steel (2017), pp. 64–78.
- ChoGlueck, C. (2018). The error is in the gap: Synthesizing accounts for societal values in science. *Philosophy of Science 85*(4), 704–725.
- Churchman, C. W. (1948). Statistics, pragmatics, induction. *Philosophy of Science 15*(3), 249–268.
- Conrad Jr, J. W. (2003). The reverse science charade. *Envtl. L. Rep. News & Analysis 33*, 10306. Cranor, C. F. (1990). Some moral issues in risk assessment. *Ethics 101*(1), 123–143.
- Cumming, G. (2012). *Understanding the new statistics: Effect sizes, confidence intervals, and meta-analysis*. Routledge.
- de Melo-Martin, I. and K. Intemann (2014). Who's afraid of dissent? addressing concerns about undermining scientific consensus in public policy developments. *Perspectives on Science 22*(4), 593–615.
- Douglas, H. (2000). Inductive risk and values in science. *Philosophy of Science 67*(4), 559–579.
- Douglas, H. (2009). *Science, policy, and the value-free ideal*. University of Pittsburgh Press.
- Douglas, H. (2017). Why inductive risk requires values in science. See Elliott and Steel (2017), pp. 81–93.
- ECHA (2017). *Guidance on information requirements and chemical safety assessment: Chapter R.11: PBT and vPvB assessment*. European Chemicals Agency.
- EFSA (2014). Guidance on expert knowledge elicitation in food and feed safety risk assessment. *EFSA Journal 12*(6), 3734.
- Elliott, K. (2011a). *Is a little pollution good for you?: Incorporating societal values in environmental research*. OUP USA.
- Elliott, K. and D. Steel (2017). *Current controversies in values and science*. Routledge.

- Elliott, K. C. (2011b). Direct and indirect roles for values in science. *Philosophy of Science 78*(2), 303–324.
- Elliott, K. C. (2022). *Values in Science*. Elements in the Philosophy of Science. Cambridge University Press.
- Elliott, K. C. (2023). Navigating dissent by managing value judgments: The case of lyme disease. *manuscript*.
- Elliott, K. C., A. M. McCright, S. Allen, and T. Dietz (2017). Values in environmental research: Citizens' views of scientists who acknowledge values. *PloS one 12*(10), e0186049.
- Elliott, K. C. and D. Willmes (2013). Cognitive attitudes and values in science. *Philosophy of Science 80*(5), 807–817.
- Fidler, F. and J. Wilcox (2021). Reproducibility of Scientific Results. In E. N. Zalta (Ed.), *The Stanford Encyclopedia of Philosophy* (Summer 2021 ed.). Metaphysics Research Lab, Stanford University.
- Giere, R. N. (2003). A new program for philosophy of science? *Philosophy of Science 70*(1), 15–21.
- Hansson, S. O. (2007). Values in pure and applied science. *Foundations of Science 12*(3), 257–268.
- Hansson, S. O. (2010). Changing the scientific corpus. In O. Erik and E. Sebastian (Eds.), *Belief revision meets philosophy of science*, pp. 43–58. Springer.
- Hansson, S. O. (2014). How context dependent is scientific knowledge? In *Epistemology, Context, and Formalism*, pp. 127–140. Springer.
- Hansson, S. O. (2017a). How values can influence science without threatening its integrity. In *Logic, methodology and philosophy of science–Proceedings of the 15th International Congress*, pp. 207–221.
- Hansson, S. O. (2017b). The precautionary principle. *Handbook of safety principles*, 258–283.
- Hansson, S. O. (2017c). Science and pseudo-science. In E. N. Zalta (Ed.), *The Stanford Encyclopedia of Philosophy* (Summer 2017 ed.). Metaphysics Research Lab, Stanford University.
- Hansson, S. O. (2018). Politique du risque et intégrité de la science. In A. Guay (Ed.), *Risque et expertise*, pp. 57–86. Presses universitaires de Franche-Comté.
- Hansson, S. O. (2020a). Social constructionism and climate science denial. *European Journal for Philosophy of Science 10*(3), 1–27.
- Hansson, S. O. (2020b). Values in pharmacology. In A. LaCaze and B. Osimani (Eds.), *Uncertainty in Pharmacology*, pp. 375–396. Springer.
- Havstad, J. C. and M. J. Brown (2017). Inductive risk, deferred decisions, and climate science advising. *Exploring inductive risk: Case studies of values in science*, 101–123.
- Hempel, C. G. (1960). Inductive inconsistencies. *Synthese 12*, 439–469.

- Hempel, C. G. (1965). *Aspects of scientific explanation; and other essays in the philosophy of science*. New York: The Free Press.
- Hempel, C. G. (1981). Turns in the evolution of the problem of induction. *Synthese 46*, 389–404.
- Hicks, D. J. and E. J. C. Lobato (2022). Values disclosures and trust in science: A replication study. *Frontiers in Communication* 7.
- Holman, B. and T. Wilholt (2022). The new demarcation problem. *Studies in history and philosophy of science 91*, 211–220.
- IARC (2019). Preamble to the iarc monographs. Technical report, International Agency for Research on Cancer.
- IPCC (2023). *CLIMATE CHANGE 2023 Synthesis Report Summary for Policymakers*. Intergovernmental Panel on Climate Change.
- Jeffrey, R. C. (1956). Valuation and acceptance of scientific hypotheses. *Philosophy of Science 23*(3), 237–246.
- John, S. (2015a). The example of the ipcc does not vindicate the value free ideal: a reply to gregor betz. *European Journal for Philosophy of Science 5*(1), 1–13.
- John, S. (2015b). Inductive risk and the contexts of communication. *Synthese 192*(1), 79–96.
- Kourany, J. A. (2010). *Philosophy of Science after Feminism*. Oxford University Press.
- Lacey, H. (2017). Distinguishing between cognitive and social values. See Elliott and Steel (2017), pp. 15–30.
- Latour, B. (1984). *Les Microbes. Guerre et Paix.* Paris: Métailié.
- Latour, B. and J. Woolgar (1979/1986). *Laboratory Life. The social construction of scientific facts*. Princeton University Press.
- Levi, I. (1960). Must the scientist make value judgments? *The Journal of Philosophy 57*(11), 345–357.
- Lewandowsky, S., N. Oreskes, J. S. Risbey, B. R. Newell, and M. Smithson (2015). Seepage: Climate change denial and its effect on the scientific community. *Global Environmental Change 33*, 1–13.
- Longino, H. (1990). *Science as Social Knowledge. Values and Objectivity in Scientific Inquiry*. Princeton: Princeton University Press.
- Longino, H. E. (1996). Cognitive and non-cognitive values in science: Rethinking the dichotomy. In L. H. Nelson and J. Nelson (Eds.), *Feminism, science, and the philosophy of science*, pp. 39–58. Dordrecht: Springer.
- Mahoney, J. (1999). Nih initiative to reduce regulatory burden. Technical report, National Institues of Health.

- Mastrandrea, M. D., C. B. Field, T. F. Stocker, O. Edenhofer, K. L. Ebi, D. J. Frame, H. Held, E. Kriegler, K. J. Mach, P. R. Matschoss, et al. (2010). Guidance note for lead authors of the ipcc fifth assessment report on consistent treatment of uncertainties.
- McKaughan, D. J. and K. C. Elliott (2015). Introduction: Cognitive attitudes and values in science. *Studies in History and Philosophy of Science Part A 53*, 57–61. Special Section: Formal Epistemology and the Legacy of Logical Empiricism Special Section: Cognitive Attitudes and Values in Science.
- McMullin, E. (1982). Values in science. In *PSA: Proceedings of the biennial meeting of the philosophy of science association*, Volume 1982, pp. 3–28. Philosophy of Science Association.
- Meehl, P. E. (1967). Theory-testing in psychology and physics: A methodological paradox. *Philosophy of science 34*(2), 103–115.
- Mitchell, S. (2004). The prescribed and proscribed values in science policy. *Science, values, and objectivity*, 245–55.
- Nilsson, K., E. Skoog, V. Jones, L. Labbé Sandelin, C. Björling, E. Fridenström, M. Edvinsson, A. Mårtensson, and B. Olsen (2021, 03). A comprehensive clinical and laboratory evaluation of 224 patients with persistent symptoms attributed to presumed tick-bite exposure. *PLOS ONE 16*(3), 1–17.
- OECD, G. (2007). Best practices for ensuring scientific integrity and preventing misconduct.
- O'Hagan, A., C. E. Buck, A. Daneshkhah, J. R. Eiser, P. H. Garthwaite, D. J. Jenkinson, J. E. Oakley, and T. Rakow (2006). Uncertain judgements: eliciting experts' probabilities.
- Resnik, D. B. and K. C. Elliott (2023). Science, values, and the new demarcation problem. *Journal for General Philosophy of Science*.
- Rooney, P. (2017). The borderlands between epistemic and non-epistemic values. See Elliott and Steel (2017), pp. 31–45.
- Rudner, R. (1953). The scientist qua scientist makes value judgments. *Philosophy of science 20*(1), 1–6.
- Ruphy, S. (2006). "empiricism all the way down": a defense of the value-neutrality of science in response to helen longino's contextual empiricism. *Perspectives on Science 14*(2), 189–214.
- Schneider, S. L. (2020). 2018 faculty workload survey. research report: Primary findings. Technical report, FEDERAL DEMONSTRATION PARTNERSHIP.
- Schurz, G. (2013). Wertneutralität und hypothetische werturteile in den wissenschaften. *Werte in den Wissenschaften. Neue Ansätze zum Werturteilsstreit*, 305–334.

- Stamenkovic, P. (2020). The contradictions and dangers of Bruno Latour’s conception of climate science. *Disputatio. Philosophical Research Bulletin* 9(3), 00–00.
- Stamenkovic, P. (2022). Facts and objectivity in science. *Interdisciplinary Science Reviews* 0(0), 1–22.
- Stamenkovic, P. (forthcoming). Remarks on hansson’s model of value-dependent scientific corpus. *Lato Sensu*.
- Steele, K. (2012). The scientist qua policy advisor makes value judgments. *Philosophy of Science* 79(5), 893–904.
- Wagner, W. E. (1995). Science charade in toxic risk regulation, the. *Colum. L. Rev. 95*, 1613.
- Weber, M. (1917/1949). The meaning of "ethical neutrality" in sociology and economics. In E. A. Shils and H. A. Finch (Eds.), *The Methodology of the Social Sciences*, pp. 1–47. Glencoe: Free Press.
- Wiener, J. B. and M. D. Rogers (2002). Comparing precaution in the united states and europe. *Journal of risk research* 5(4), 317–349.

Declarations

Funding: This work was supported by the MTA Lendület Values and Science Research Group, and the European Union’s Horizon Europe Marie Skłodowska-Curie Actions (grant agreement #101066502, project SYSVAL).

Potential competing interests: No potential competing interests to declare.