Straightening the ‘Value-Laden Turn’: Minimising the Influence of Extra-Scientific Values in Science

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6th October 2023

Abstract

Straightening the current ‘value-laden turn’ (VLT) in the philosophical literature on values in science, and reviving the legacy of the value-free ideal of science (VFI), this paper argues that the influence of extra-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 truth of scientific knowledge, respecting the autonomy of science results users, preserving public trust in science) have not been satisfactorily addressed by proponents of the VLT, it proposes four prerequisites which any model for values in the acceptance / rejection phase of scientific inquiry should abide, coming from the fundamental requirement to distinguish between facts and values: 1) the truth of scientific knowledge must be ensured; 2) the uncertainties associated with scientific claims must be stated clearly; 3) claims accepted into the scientific corpus must be distinguished from 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 Betz’s conception (stating uncertainties associated with scientific claims) and Hansson’s corpus model (ensuring the truth of the scientific corpus and distinguishing it from other claims taken as a basis for action) is proposed. Additional prerequisites are finally suggested for future research, stemming from the requirement for philosophy of science to self-reflect on its own values: 5) any model for values in science must be descriptively and normatively relevant; and 6) its consequences must be thoroughly assessed.

1 Introduction

1.1 The ‘value-laden turn’

In the last decades, the philosophy of science has clearly shifted towards ascribing always more influence to extra-scientific values in all phases of scientific inquiry, at the descriptive and/or normative levels. Although these descriptive and normative dimensions are not always clearly distinguished[1], authors contributing to this ‘value-laden turn’ (VLT) generally ascribe, on the descriptive level, a

[1] For example, these two dimensions respectively correspond to what Douglas (2017) calls the ‘internal’ and ‘external’ arguments for values in science, although she does not clearly present these claims as descriptive and normative, respectively.

greater influence to extra-scientific values than what was previously assumed, and/or also recommend, on the normative level, a greater influence of these values[2], in opposition to the value-free ideal (VFI) of science, which normatively excludes such influence (although it may descriptively acknowledge it). In variable ways, authors of the VLT claim that values do (descriptively)[3], can and/or should (normatively)[4] influence the various phases of scientific inquiry (for helpful review and classification, see Elliott (2022) and Holman and Wilholt (2022), respectively)[5]:

1. the 'upstream' phase of
   (a) choosing research avenues (answering the question of *what* to investigate);
   (b) choosing evidence, methods and models (*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).

It is essentially the phases 1.b and 2 which are still controversial: there is now consensus that extra-scientific values do (perhaps inevitably[6]) and should permeate all other phases[7]. Here I will mainly focus on phase 2, which is only concerned with the *truth* of scientific knowledge, not its *objectivity* which also concerns phases 1.a and 1.b. and is a wider concept (requiring, in addition to truth, balancedness and fairness of knowledge, see section 2.2). Phase 2 covers, but also exceeds (since it also deals with 'true positives' and 'true negatives'), what is called the 'inductive risk argument' or 'error argument' in the literature, 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), clearly formulated by Rudner (1953), and especially developed by Douglas (2000; 2009; 2017). Phase 2 is also related to

[2]In the following, 'value(s)' without further specification designates both intra- and extra-scientific value(s), keeping in mind that it is only the latter which is controversial.

[3]Many authors of the VLT additionally ascribe to this claim a *necessary* character, according to which values *inevitably* influence scientific practice (see section 3).

[4]The 'can' formulation is intended to illustrate some modally ambiguous formulations which can be found in the VLT literature, for example the distinction between 'legitimate' and 'illegitimate' value influence (Holman and Wilholt, 2022; Resnik and Elliott, 2023), or the claim that values 'can appropriately' influence science (Elliott, 2022, 49). But I take such formulations to be ultimately *normative*. Claims that such value influence is 'appropriate', 'legitimate' or 'admissible' (and conversely, that such value influence is not) are *evaluative* judgements presupposing *normative* judgements of what science should be (indeed, Elliott explicitly proposes such norms). Conversely, saying that such value influence is not legitimate means that it *should not* take place.

[5]For a more precise description of these phases, see Elliott (2022, 8).

[6]That does not mean, of course, that their influence is harmless. For example, in phase 1.a, extra-scientific values can indeed harm scientific objectivity by artificially focusing the inquiry on certain aspects (Hoyningen-Huene, 2023, 23).

[7]I am not aware of an 'extra-strong' version of the VFI which would exclude extra-scientific value influence from either phase 3 or 4. Even the most stringent advocates of the VFI accept the influence of extra-scientific values (especially ethical ones) in these phases, and limit value-freedom to phases 1 and 2.

the so-called 'gap' argument, according to which inherently value-laden concepts and background knowledge are used by scientists to connect theory and evidence (e.g. Longino, 1990)[8].

Many philosophers of the VLT[9] endorse a pervasive form of value influence, allowing extra-scientific value influence in *all* phases of scientific inquiry, including phase 2 (which I will call the acceptance/rejection (A/R) phase in the following). Inasmuch as this VLT promotes the social responsibility of science, it is of course to be welcomed. However, it can also threaten the objectivity (including the truth) of scientific knowledge, something many of its proponents seem less concerned about. It seems that, in the current philosophical trend[10] 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 VLT, claims that extra-scientific values are inevitable in scientific practice (on the descriptive level), that they should influence all aspects of the scientific enterprise (on the normative level), and does not clearly distinguish between scientifically established facts and scientifically informed claims taken as a basis for policy-making. 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 still resist this trend, such as Betz (2013; 2017) who excludes extra-scientific values altogether; Hansson (2014; 2017a; 2018; 2020b) who accepts extra-scientific values but only if they reinforce the level of evidence required for accepting a claim; or Lacey (2017) who excludes extra-scientific values for claims 'impartially held'.

Against the VLT, 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 (preserving the truth of scientific knowledge, respecting the autonomy of science results users, protecting 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. Like much of the literature on values in science, my proposal is both normative and descriptive: it proposes normative requirements which science

[8]I leave aside the issue of whether the inductive risk argument can be considered a subcategory of the gap argument (ChoGlueck, 2018). I also leave aside in this article other arguments for or against values in science, such as the 'conceptual' and 'aims' arguments (Elliott, 2022, §3.2 to §3.5).

[9]It is difficult to tell to which extent these philosophers represent the majority view of all philosophers interested in values in science (not to speak of scientists). Within the authors regularly writing on 'values in science', they seem to be in the majority (or at least those most published and publicised), but there may be a 'silent majority' of philosophers not regularly participating in this debate and not endorsing the strongest form of value influence. It would be useful to conduct a systematic review on all philosophical articles written on values in science in order to come up with an estimation, but this lies outside the scope of this article.

[10]This philosophical trend appears value-laden in two senses: 1) it defends the *claim* that science is or should be value-laden (in all phases of scientific inquiry); and 2) it is itself *motivated* by certain values (such as the social responsibility of science), which also have an influence in all phases of *philosophical* inquiry (from the choice of research avenues to the gathering of evidence and the establishing of the previous claim). Of course, the latter claim must be properly substantiated (something this paper intends to contribute to). Such a conception of the VLT illustrates a self-reflection on the influence of values within philosophy itself (as advocated in section 3), and should of course also be applied to the conception advocated in the present article.

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, intra-scientific and extra-scientific 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).

should respect, but it also claims that these normative requirements correspond to actual scientific practice (in other words that this practice obeys these norms, even if they are not always respected of course). 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 what I believe to be 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 truth 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 extra-scientific values is indeed minimised. A model combining part of Betz's conception (stating uncertainties associated with scientific claims) with Hansson's corpus model (allowing extra-scientific value influence while ensuring the truth of scientific claims) is proposed, which respects all four prerequisites. This model minimises the influence of values on the A/R phase and allegedly better corresponds to science and policy practice than many VLT proposals. Additional prerequisites are finally suggested for future research, stemming from the requirement for philosophy of science to self-reflect on its own values: 5) any model for values in science must be descriptively and normatively relevant; and 6) its consequences must be thoroughly assessed.

1.2 Preliminary remarks

Before all this, some preliminary remarks are in order.

1.2.1 Types of decisions

Firstly, as the reader has noticed, I prefer to talk of *intra-scientific* values instead of what is usually called 'epistemic' (or sometimes 'constitutive' (Hicks, 2014), or 'cognitive' (Douglas, 2013)) values, in order to designate those values (such as empirical adequacy, internal coherence within a theory or external coherence with adjacent theories, unifying power, scope, simplicity, etc.) which are generally taken to be intrinsically conducive to scientific knowledge[11] – as opposed to 'non-epistemic' (or 'contextual', or 'non-cognitive') values (such as social or political values, for example public health or economic profit) which I call *extra-scientific*[12]. Intra-scientific values have themselves been classified into various subcategories[13].

[11]Or, as Elliott (2022, 4) puts it, 'indicative of truth or knowledge'. Truth and knowledge are, however, two different things, and the former is only one characteristic of the latter.

[12]I prefer to talk of intra- and extra-scientific rather than scientific and non-scientific, because this terminology seems more appropriate when applied to decisions (see hereafter): extra-scientific decisions may nevertheless be based on (although not taken within) science, something the term 'non-scientific' does not capture, and may even seem to negate.

[13]I will not enter into the controversial debate of the various ways to classify (and sub-classify) values (see e.g. Elliott, 2022, 5-6), which would require an article of its own. What is more, these classifications of course depend on the very definition of science which one subscribes to, more precisely which goals one ascribes to science. According to me, the central (although not unique) goal of science is to produce *objective* knowledge, which includes pursuing *truth* (note that talking of true knowledge would be somewhat pleonastic) (see section 2.2). To reach this goal, many intra-scientific values are certainly helpful. However, with respect to truth alone (which is a necessary, but not sufficient condition of objectivity), I believe that within the set of scientific values, there are values which are necessary to (and in fact definitional of) truth (namely, internal consistency and empirical adequacy) whereas others are just (contingently) indicative of, or instrumental for, truth (such as scope, simplicity or external coherence). In

theoretical				practical
epistemic		non-epistemic		
intra-scientific	extra-scientific	intra-scientific	extra-scientific	

Table 1: Types of decisions.

The first reason for this choice is, obviously, a matter of terminological coherence: we are dealing with *(intra)scientific* beliefs within the purview of philosophy of science, not general, indeed *epistemic* beliefs which belong to the province of epistemology. Since science is only a subdivision of theoretical rationality, it is more accurate to talk of intra-scientific values than epistemic values which, if they relate to extra-scientific epistemic decisions (see hereafter), may be quite different from intra-scientific values such as empirical adequacy or consistency with other theories.

The second reason is motivated by the concern to avoid confusion with the corresponding decisions. Indeed, following Stamenkovic (ming), I distinguish between:

1. Theoretical decisions (concerning knowledge), made up of:
   (a) Epistemic decisions, concerning our choices of what to believe (i.e. our choices to accept or reject a claim);
   (b) 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 (not related 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, intra-scientific decisions should be viewed as a subcategory of theoretical decisions, which also include extra-scientific decisions. Intra-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 intra-scientific decisions can be imbued with (intra- or extra-scientific) values. All practical decisions are extra-scientific. table 1 illustrates how these various types of decision relate to each other. In order not to cause confusion with epistemic and non-epistemic intra-scientific decisions, which are both based primarily on intra-scientific values[14], it is less misleading to talk of intra-scientific values rather than epistemic values (which might suggest that only epistemic decisions are concerned).

distinguishing such two subsets within scientific values, I follow various authors such as Ruphy (2006, 212) (who talks of a 'minimal, stabilized set of values whose cognitive virtues are universal'), Steel (2010, 18) (who talks of 'intrinsic epistemic values', as opposed to 'extrinsic' ones) or Hoyningen-Huene (2023, 14) (who talks of 'epistemic scientific values', as opposed to 'instrumental scientific values'). (Note that Steel (2010, 18) talks of 'predictive accuracy' and Hoyningen-Huene (2023, 14) of 'predictive power' and 'accuracy', but I take them to be subcategories of 'empirical adequacy'.)

[14]Intra-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 extra-scientific values which only potentially play a role) is argued for hereafter, and additional research avenues are suggested in the conclusion.

Finally, talking of intra-scientific values also has the advantage of illustrating the conception advocated here, namely, that extra-scientific values *usually* have no place in the A/R phase of science[15].

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; semi-quantitative, 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 intra-scientific values (e.g. consistency with already held claims), as well as, potentially, extra-scientific values (e.g. public health or safety). It illustrates all the intra-scientific (empirical, theoretical and value-laden) and potentially extra-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[16], 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 intra-scientific or extra-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[17]. For example the IARC[18] *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 intra-scientific values (for example 'sufficient evidence' is based on studies 'in which chance, bias, and confounding were ruled out with reasonable confidence[19]', 31).

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

[16] 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 (for more details, see Stamenkovic, ming). But these are minimal presuppositions without which it seems difficult to say anything at all about values in the A/R phase.

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

[18] International Agency for Research on Cancer.

[19] Of course this term is itself value-laden, but again that does not mean that the values in question need be extra-scientific. See section 2.3.4 and section 3.

1.2.3 Relevance of extra-scientific values for a claim

Finally, although in principle the consideration of extra-scientific values is applicable to any claim, in practice it is limited to claims which have clear extra-scientific consequences, in other words for socially relevant disciplines (or parts thereof), such as regulatory toxicology, medical science, pharmacology, etc.[20]. If there are no extra-scientific applications, then extra-scientific values are irrelevant. Although this point is obvious, it is not always clear in the philosophical literature, and should be clarified for each conception (as e.g. Douglas (2000, 577)[21] or Betz (2013, 210-211) do). Indeed, many participants to the debate on values in science 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. What is more, these examples sometimes do not 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 policy-making should naturally be influenced by extra-scientific values, and distinguished from scientific claims proper (part of 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, prominent philosophers now allow 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 pervasive value influence threatens the truth of scientific knowledge, and its proponents do not seem to have fully assessed its intra- and extra-scientific consequences. There are both intra- and extra-scientific reasons[22] for minimising as much as possible this influence. Starting from the fundamental distinction between fact and value, I will argue in the following that ensuring the truth of scientific knowledge 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 *scientific claims* and *claims taken as a basis for action* (in other words *scientifically informed decisions*), because while we want to ensure the truth of scientific knowledge, 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 our factual beliefs (what we believe to be facts) from our other mental attitudes towards the objects of these factual beliefs (i.e. the facts) is a fundamental and necessary ability without which our life both at the individual and collective

[20] And in fact, even for such socially relevant disciplines, there may be many cases where extra-scientific values are irrelevant or do not make any difference (see footnote (86)).

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

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

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 at 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 entails that:

1. the truth of scientific knowledge be ensured (as a repository of factual statements);
2. the uncertainties associated with scientific statements be stated clearly (in order not to wrongly count as factual, statements which are still uncertain);
3. scientific statements be distinguished from claims that are taken as a basis for non-epistemic decision-making (in order not to wrongly count as factual, statements whose LER has been deliberately lowered).
4. additionally, it is desirable that values be managed in a simple and systematic way if the model for handling them is to be applied.

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 addressed by proponents of the VLT. The first reason in favour of the VFI is, obviously, related to the pursuit of truth, which is the primary goal of science. Since extra-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[23]). The attainment and 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) decision-making. 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 VLT, but there is no consensus and the proposals are often complicated. I will briefly come back to this concern in section 2.3.

[23]Much bigger than the one of intra-scientific values, it seems.

The third reason is related to public trust in science: intuitively, a value-laden science seems less trustworthy than a value-free, fact-based 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 generalised value influence for Elliott et al. (2017), neutral for Hicks and Lobato (2022) and even beneficial in case of scientists 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 Ensuring the truth of scientific knowledge

2.2.1 The truth of scientific knowledge

Without engaging into too much definitional or historical work, the present approach requires that I clarify the relationship between truth and science. In the case of empirical science, it is legitimate to endorse a correspondence conception of truth[24]. I take truth to be a necessary though not sufficient condition, and conceptual component, of objectivity, which is a wider concept (which, like truth, is primarily applied to representations, but can also be derivatively applied to other aspects of the scientific endeavour producing such representations such as methods, individuals, institutions, etc. (Hoyningen-Huene, 2023, 5) whereas truth is exclusively applied to representations (Stamenkovic, 2022, 2)). Objectivity requires, in addition to truth, balancedness and fairness of knowledge (Hoyningen-Huene, 2023, 5). Both concepts refer to subject-independent facts (Stamenkovic, 2022, 2), but objectivity requires in addition to truth that no relevant aspect of the object be ignored: a quality which may be called the truthfulness of knowledge (hence objectivity = truth + truthfulness)[25].

I take truth and objectivity[26] to be the most important, *defining* aims of science (which also has other goals such as explanation, pre- or retrodiction, in addition to other extra-scientific goals such as pursuing social welfare): they are *necessary* parts of science's definition, without which there is no science. The fundamental goal of (empirical) science is to give a true and objective account of the facts, to explain, predict (or retrodict) them in the most systematic way (hence prolonging and ameliorating similar activities which we can undertake in everyday life). Therefore, the first, absolutely essential prerequisite for any model for values in science is that *it ensures the truth* (if limited to the 'core' phase 2 of scientific inquiry) and the *objectivity* (if the 'upstream' phase 1 is considered as well) of scientific knowledge. Since the present article is focused on phase 2, let me focus here on truth and leave aside objectivity. By *ensuring* the truth of scientific knowledge I mean *preserving* it (against detrimental influence, which is a negative characterisation), but also *attaining* it (supporting, furthering it, which is a positive characterisation). Indeed, even if we

[24]For sciences which take our own mental constructions as objects (such as mathematics, philosophy, linguistics or literary studies), other conceptions of truth (such as the coherence theory of truth) are relevant (mathematics is notoriously associated with deductive proofs).

[25]Because of this truthfulness feature, objectivity is also capable of gradation, which is not the case of truth (see hereafter).

[26]Strictly speaking, objectivity alone is sufficient, since truth is a component of it.

restrict ourselves to phase 2, preserving the truth of current claims also helps attain the truth of future claims, as we shall see.

Finally, can truth itself be considered a value? According to Hicks (2014, 3272) it can, although he recognises that it is not only that, and that it is (or can be) 'a necessary condition for accepting a theory' (Hicks, 2014, 3273). According to Hicks both conceptions of truth are 'entirely consistent' with each other, although I find his argument unconvincing[27]. First, because truth is a *defining* aim, a *necessary* conceptual component of the definition of science (without which there is no science), it cannot be considered a value, which is a desirable i.e. optional quality. Second, if we focus on phase 2, for a statement or theory to be scientifically established (i.e. accepted into the scientific corpus, see below), it must reach a specific (discipline-dependent) LER. This is a *binary*, yes-or-no event: truth is either possessed by the claim (in which case it is accepted into the corpus) or not (in which case it is rejected). Indeed, truth appears intuitively as a *binary* quality (a claim is either true or not), and it would feel weird to quantify it (as a gradable quality) or compare it (as one claim being 'truer' than another) (Hoyningen-Huene, 2023, 5), whereas a value (whether intra-scientific, like e.g. simplicity; or extra-scientific, like e.g. public health) is typically capable of such gradation or comparison[28]. Of course this binary status does not mean that the accepted claim is 'absolutely' or 'for ever' true[29], since new evidence may lead us to revise the claim. Neither is it incompatible with the claim stating an uncertainty (see section 2.3.2).

2.2.2 Why should it be preserved?

We have just seen that truth is a necessary, definitional component of scientific knowledge: without truth, there is no scientific knowledge. But there are additional reasons for preserving the truth of scientific knowledge, both within and outside science:

1. Intra-scientific reasons:
   (a) Epistemic reasons (regarding the *preservation* of the truth of current research results):
      i. Scientists are famously 'cautious' and 'conservative', reluctant to state claims if they are not very unlikely to be false. In other words they prefer – within science – false negatives to false positives. In terms of the scientific values of error avoidance and unsettledness avoidance (Hansson, 2020b)[30], scientists prefer the former to the

[27]Hicks (2014, 3273) seems to consider truth as a kind of super-value: truth, or internal consistency (a necessary condition for truth), can be considered as necessary conditions for accepting a theory because 'in this case, we are attaching a great deal of value to truth', and this is what he 'ha[s] in mind by the lexical priority of truth' (over other values).

[28]What *is* gradable is the values which contribute to define truth itself (such as empirical adequacy in empirical science, or external coherence). This explains why certain claims or theories in empirical science appear 'more certain' (as inductive inferences) than others (again, this does not apply to formal sciences like mathematics).

[29]Except perhaps in formal sciences such as logic or mathematics.

[30]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.

latter. I take this descriptive-normative[31] claim to be widely shared[32]. Any model for values in science has to accommodate this normative fact.

   ii. In spite of this scientific *ethos*, there are already enough problems in science, regarding current LERs (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 by lowering current LERs.

(b) Non-epistemic reasons (regarding the *attainment* of the truth of future research results):

   i. 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 (Hansson, 2018). Therefore, the *preservation* of the truth of current results *ensures* the *attainment* of the truth of future results. Note that if the corpus did not have high LERs, both the truth and the productivity of science would be threatened, whereas with high LERs only the productivity of science is threatened, not its truth (again, a trade-off between these two goals has to be made, and one cannot increase indefinitely the LER).

   ii. Since what lies in the corpus represents our best available knowledge, it 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 should not be re-examined.

2. Extra-scientific reasons:

   (a) Direct extra-scientific reasons (related to reliability): since the scientific corpus is used as a general, multipurpose repository of knowledge, it must have high LERs, 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 extra-scientific values (such as safety, health, non-maleficence, etc.) directly demand high LERs.

   (b) Indirect extra-scientific reasons (related to what might be called reliable productivity): ensuring that research is based on reliable results (in accordance with reason 1.b.i) 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).

For all these reasons, the corpus must keep high LERs.

[31] It is a descriptive claim about a norm (a scientific *ethos*) being mostly respected (even if there are deviances, but the very fact that the latter are punished means that the norm is enforced).

[32] 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.

2.2.3 How can it be preserved?

How can the prerequisite to preserve the truth of scientific claims be expressed operationally? With the help of the LER concept introduced in section 1.2.2, this simply means that *values should not be allowed to lower the LER* (set by disciplinary standards) to accept a claim. That does not mean that values have to be excluded. As Hoyningen-Huene (2023) rightly remarks with respect to objectivity, value-freedom is an indicator, a means to achieve objectivity, not a conceptual component of it. In other words value-freedom is not necessarily, but only contingently linked to objectivity, and value-ladenness may actually reinforce it, by raising evidential requirements in some cases (or directing research towards neglected but important aspects of the problem). The same can be said of truth, as we shall see hereafter.

The preservation of the truth (and objectivity, if phase 1 is included) of scientific knowledge was the original motivation for the restricted (and strong, including phase 1[33]) version of the VFI. Of course, this preoccupation is not foreign to proponents of the VLT, although often not expressed sufficiently clearly. 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 phase 2, the whole point is then to distinguish between ‘legitimate’ and ‘illegitimate’ value influence – the question then being transferred to what one means exactly by ‘legitimate’[34]. 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 is, quite naturally, to centre the approach on scientific knowledge, rather than on individual scientists and their cognitive attitudes, or scientific communities and their conventions, as is usually done[35]. 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 the total body of scientific knowledge[36] (see e.g. Hansson, 2018, 68-

[33]Excluding extra-scientific values from the choice of research avenues, evidence and methods of investigation, was supposed to preserve the objectivity of scientific inquiry. However, further philosophical work showed that extra-scientific values in phase 1 can actually be beneficial in that respect (if they contribute to take into account new relevant aspects of the object of study) (e.g. Kitcher, 2001; Anderson, 2004), although they can always, of course, be detrimental (if on the contrary they lead to neglect such aspects).

[34]What they dub the ‘new demarcation problem’, in analogy with the old one between science and non-science. For Holman and Wilholt (2022, 214), ‘veracity’, ‘universality’ and ‘authoritativeness’ were three arguments for the VFI which must be shown to be either satisfied by value-laden proposals, or no longer relevant.

[35]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.

[36]Ideally, the scientific corpus should correspond to all published scientific literature (articles and textbooks), although in practice the published literature contains bad articles or books containing false statements, and conversely there may be good unpublished work containing true statements. Therefore the *published* scientific corpus is not actually equivalent to the *ideal* one (which should be the sum total of scientific *knowledge*): there might be false claims part of the published corpus, and true claims not part of it. The first issue is supposed to be continuously solved by the permanent re-evaluation of the published corpus (although in practice few publications are retracted). To my knowledge, there is no institutional process to take care of the second issue, although it is probably extremely marginal (because most researchers look for acknowledgment). In the present idealised discussion, I leave aside the question of the discrepancy between the ideal scientific corpus (which I consider here) and its concrete, published

71). The corpus is interdisciplinary, universal and hence unique; and it is apt to any (intra- or extra-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). The first to mention the concept of scientific corpus seems to be Kaufmann (1941b; 1941a). The idea that the truth of the scientific corpus should be preserved appears (in a way which in principle excludes extra-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[37]. For his part, Hansson (2018) allows value influence on the corpus only if the 'epistemic integrity' of science is preserved, without precisely defining this concept. The concept of 'epistemic integrity' conveys the idea that scientific statements (and scientific activity in general) are protected from detrimental value influence or other types of distorting factors (e.g. unconscious bias), and can be more or less seen as a negative characterisation of truth (when applied to scientific statements). Hence preserving the truth or the 'epistemic integrity' of the scientific corpus seem to be just two different ways of saying the same thing[38]. More precisely, Hansson allows the influence of values on the corpus only if they contribute to *raise* the LER to accept claims within it[39], i.e. only if they contribute to *strengthen* its truth[40].

I believe the descriptive-normative characterisation of the scientific corpus presented here and centred on Hansson's model, corresponds fairly well to good scientific practice, as well as to the uses made of scientific knowledge outside of science. Hansson's conception, which keeps the best of both worlds between the VFI (whose legacy is the preservation of the truth of scientific knowledge) and the VLT (whose take-away message is to allow values in the A/R phase), nicely answers my first prerequisite and will be part of the model I propose.

2.3 Stating uncertainties associated with scientific knowledge

2.3.1 Why state uncertainties?

Ensuring the truth of scientific knowledge requires that uncertainties associated with scientific claims be stated clearly, instead of being bridged or covered up by values – in which case the scientific corpus may well contain erroneous claims, with all the detrimental consequences men-

form (for more details, see Stamenkovic, ming).

[37]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 utilities 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 mentions 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 influence rules of acceptance/rejection into the scientific corpus, then the scientific corpus is value-laden.

[38]Hansson also defines science as the activity which provides us with the 'most reliable' knowledge about its subject matter (2017c). Reliability conveys the idea of truth, but also that of objectivity (including truthfulness i.e. not missing relevant aspects of the issue at hand) as well as applicability (in the sense that the knowledge obtained can be used for all sorts of applications, including extra-scientific ones).

[39]As remarked previously, the LER cannot be raised endlessly, but only within reasonable limits, and asking for always more evidence can actually be a strategy to deny scientific facts. This point will have to be investigated in a separate paper. I thank André Juthe for contributing to make me aware of this point.

[40]Note that this is not exactly the same as my prerequisite, which allows values only if they *do not lower* the LER to accept a claim. One could imagine that values are only allowed if they keep the LER unchanged, but that would be extremely restrictive, perhaps impossible to implement. Hansson's proposal is the only way to go if one does not want to lower the LER and does not want to (almost completely) exclude values neither.

tioned above. Therefore, all the previously mentioned reasons for preserving the truth of scientific knowledge apply. Additional reasons for stating uncertainties include:

- Intra-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 and prevent the attainment of truth. Indeed, since the corpus represents our best available knowledge, what lies in it is taken for granted and does not require further investigation[41]. On the contrary, stating the uncertainties clearly will motivate further investigation, since the matter will be considered unsettled.
  - Accepting an uncertain claim would also contravene the scientific ethos seen previously (based on cautiousness and conservatism), and one may wonder how a scientist would react if she was told to accept a claim which she considers uncertain.
- Extra-scientific reasons:
  - Stating uncertainties is of course especially important for extra-scientific decision-making, where, if the autonomy of the decision-maker(s) is to be respected (as seen above), the distinction between (intra-scientific) judgements of fact (or risk assessment) and (extra-scientific) judgements of values (part of risk management) must be clear. It seems that, to a large extent, this is indeed how scientific expertise works (see the examples of section 2.3.4).
  - 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).
  - Pushing for clear-cut results freed from uncertainties and competing hypotheses may lead to hype in science communication, abusive press releases, advertisement of individual scientists and universities[42], etc. instead of focusing on the real state of knowledge. This contributes to the neoliberal marketisation of research and privatisation of science, and potentially to public misunderstanding or distrust in science if scientific breakthroughs are prematurely announced.

2.3.2 How to state uncertainties?

In empirical science, claims are always subject to uncertainty, since *in principle* no empirical claim can ever be inductively inferred with certainty. But *in practice,* when the LER by disciplinary standards is reached, uncertainty is supposed to be sufficiently low for the claim to be accepted

[41]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 (in particular, 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).

[42]This is what Douglas's conception (see below) may ultimately lead to: a focus on individual scientists and their values, and on clean and catchy storylines, instead of a focus on rigorous science and less 'sexy' uncertainties.

and relied upon as if it were *certain* (unless of course new evidence comes up and makes us revise the claim: this illustrates its fallible nature). This is indeed how scientists and non-scientists alike proceed all the time for all sorts of intra- or extra-scientific decision-making: they take for granted, and rely upon empirical statements belonging to the scientific corpus (such as 'CO2 is a greenhouse gas' or 'there was a Second World War between 1939 and 1945') or not (such as 'my blood type is A+' or 'the surface of my apartment is 65 m2'), although in principle these statements remain fallible because of their empirical nature (and indeed, new evidence may always lead us to revise them).

However, if the LER is not reached[43], uncertainty becomes significant. In this case, it should not be dismissed or bridged on the basis of values (in other words the LER should not be lowered, since as we have seen, this is incompatible with ensuring the truth of scientific knowledge). Instead, the uncertainty associated with the claim should be stated clearly[44], and the claim should not be incorporated into the corpus. Stating the uncertainty associated with the original claim produces a transformed or 'hedged' (Betz, 2013) claim, which can then itself be (and often is[45]) accepted into the corpus, as is typical in many disciplines whose results rely on statistical methods (for example in medical science: 'this substance is likely to have this toxic effect at this dose'[46]). If the hedged claim is not accepted into the corpus, it can still be used for non-epistemic decision-making (see hereafter and section 2.4). In sum, the corpus can contain either claims formulated in a certain way or claims stating uncertainties[47]. It is misleading to call the latter 'uncertain claims' since they are themselves (sufficiently) certain (i.e. they reach the LER)[48]. 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. Note that claims of the scientific corpus stating uncertainties generally concern recently investigated phenomena[49]: in general, the older the phenomenon, the better it is known and the less uncertainty the claims describing it contain[50] (this does not, of course, eliminate previous statements stating uncertainties related to the same phenomenon: they illustrate thus how knowledge about the phenomenon has evolved). Even if all claims in the

[43]More generally, in case of significant uncertainty the LER is either: not reached (in other words empirical evidence is insufficient, but the evidence in disfavour of the claim is not strong enough neither, and the statement is neither rejected nor accepted); or not operative at all (in other words theoretical understanding is insufficient, see hereafter).

[44]Uncertainty can be expressed in various ways, including of course through a probabilistic statement, but this is not the only way, see Betz (2013, 212-216) and especially Betz (2017, 102-105), as well as the following.

[45]Indeed, researchers prefer to publish their findings (even if they are subject to uncertainty) rather than to keep them in the drawers until they reach full certainty. Of course this also depends on the publication requirements of their disciplines and of the relevant academic journals.

[46]Accepting a claim stating an uncertainty into the corpus is of course more informative and useful than not accepting anything and waiting to accept a claim formulated in a fully certain way (e.g. 'this substance is toxic at such dose').

[47]The corpus can also contain negative results, i.e. statements about the absence of an expected phenomenon.

[48]Note that the present approach should not be confused with the Bayesian approach, where *any* statement is assigned a probability between 0 (false) and 1 (true), where 0 and 1 are excluded, i.e. facts are never certain. In the Bayesian approach *all* claims of the corpus, such as those concerning the structure of all molecules, the distances to celestial objects, etc. are probabilistic, and assigned non-unit probabilities. Here only (some of) the phenomena currently being investigated are described with uncertainties (not necessarily expressible probabilistically). Even if claims stating uncertainties are incorporated into the corpus, they will progressively be abandoned for claims formulated in a certain way (see hereafter).

[49]The problem is of course that extra-scientific applications (e.g. in clinical medicine or policy-making) are often concerned with those most recently discovered, badly known phenomena.

[50]For example, a new drug will progressively be deemed effective and safe with increasing certainty after different rounds of tests and trials.

corpus can be considered certain (in the sense that they have been accepted), some can be said to be 'more certain' than others: namely, those which concern phenomena which have been studied and confirmed for a long time, and which serve as a basis for other claims and applications.

Among the few (open) defenders of the VFI, Betz (2013; 2017) has forcefully advocated the need to make uncertainties associated with scientific claims explicit, in the form of what he calls 'hedged' claims. According to Betz, such 'hedged' claims are sufficiently weakened to be certain 'beyond reasonable doubt' (in the same way as are all the empirical statements which we consider certain in decision-making, although they are always revisable in principle). In other words these 'hedged' claims are themselves exempt from uncertainty, and therefore do not require extra-scientific values to manage inductive risk. Betz (2017, 102-105) 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, higher-order probabilities and normative transparency). Betz only uses uncertainty disclosure for (non-epistemic) extra-scientific decision-making, but as we have just seen it can also be used for (epistemic) intra-scientific decision-making (i.e. incorporation into the corpus).

2.3.3 Science charades

In the case of non-epistemic extra-scientific decision-making, failure to state uncertainties clearly may lead to what Wagner (1995) famously dubbed 'science charades'[51], 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 bearing on the claims and focused instead the discussion on the normative choices involved[52]. 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 mentioned above for preserving the truth and stating the uncertainties of scientific knowledge. Wagner 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' (1617)[53]. 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

[51]I thank Sven Ove Hansson for bringing this article to my attention. Unless otherwise indicated, page references in this subsection as to Wagner's article.

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

[53]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 trans-scientific issue is the assessment of the carcinogenicity of a substance to which people are exposed at low doses, whereas 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 *has not yet*, or *cannot* even, eliminated it), this does not affect my argument here.

willing to respect the agency's area of expertise (1661-1667)[54]. 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 where the policy begins (1628, 1686)[55]: this is an illustration of the autonomy argument above;
- 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 intra- and extra-scientific reasons 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.

It is interesting to note that, while Wagner's *descriptive* assessment of the pervasive influence of extra-scientific values may be compared to writings by Douglas, Elliott, Brown or other proponents of the VLT , she advocates an opposite course on the *normative* level, namely to distinguish between values and factual statements instead of incorporating the former into the latter. In particular, it is enlightening 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

[54] 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).

[55] Wagner 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 (1634-35). 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.

just mentioned. For Douglas (2017, 90-91) scientists acting as experts should deliberately use their own values to bridge inferential gaps[56], 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': '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.' (ibid.)

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 the latter 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[57], or make the debate 'clearer'.

2.3.4 Further examples

Uncertainties associated with scientific claims are typically stated in expert reports from regulation agencies or intergovernmental institutions, such as for example IPCC[58] Assessment Reports (for the latest summary for decision-makers, see IPCC, 2023) or IARC Monographs on the Identification of Carcinogenic Hazards to Humans (IARC, 2019). Such examples show that the statement of uncertainties is paramount *even for practical (e.g. policy-making or clinical) purposes* (a typical application for which the influence of extra-scientific values is most advocated), not only for epistemic purposes related to the scientific corpus, and that these institutions do not advocate bridging uncertainties with values as many proponents of the VLT 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 qualitative 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 a quantitative measure of uncertainty expressed probabilistically, distributed in seven probability ranges: '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 works like 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

[56]More precisely, according to Douglas values should be used to set the LER to accept a claim (what she calls their 'indirect' role), and not act as a reasons to accept a claim (their 'direct' role). For a critique of this distinction, see Elliott (2011b).

[57]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.

[58]Intergovernmental Panel on Climate Change.

high (except for D where it can just be 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 identified but its magnitude is poorly known, C. an order of magnitude can be given) only confidence (or summary terms for evidence and agreement) is (are) given, not likelihood. What is more, the guidance note explicitly states that '[s]ound decisionmaking that anticipates, prepares for, and responds to climate change depends on information about the full range of possible consequences and associated probabilities' (2010, 1), and lists techniques for stating uncertainties as objectively as possible and avoiding value-laden judgements both in the production (e.g. for an expert not to be influenced by the group) and interpretation (e.g. for a statement not to be interpreted in a value-laden way) of the report (2010, 2)[59].

Similarly, the IARC (2019, 35-37) 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'. In the same way, the methodological guidelines for endocrine disruptors (ED) of the French Agency for Food, Environmental and Occupational Health & Safety (ANSES[60]) define five categories of uncertainty on the basis of experts' subjective probability[61] assignments: 'known ED' (the median (50 quartile) of the subjective probability of being an ED is above 90%); 'presumed ED' (between 66% and 90%); '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%) (ANSES, 2021).

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 extra-scientific values (as John and Steel argue), and whether it cannot instead be based (primarily, at least) on intra-scientific values[62]. Indeed, the IARC insists that its categories are based on intra-scientific values, such as absence of chance, bias or confounding; quality[63]; consistency; statistical precision (IARC, 2019, 31) – values which all aim at avoiding error (which is itself a more general and fundamental intra-scientific value). Similarly, the ANSES (2021) formalises its assessment process (on the basis of the Sheffield method for sharing information and expert opinions in order

[59]Note that the 'expert judgment' mentioned in the guidance note does *not* rely on values neither, but rather on objective factors such the 'evaluation of the type, amount, quality, and consistency of evidence and the degree of agreement', or the 'standards of evidence applied, approaches to combining or reconciling multiple lines of evidence, conditional assumptions, and explanation of critical factors' (2010, 2). Such factors must be duly traced and may be combined and quantified into formal elicitation methods.

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

[61]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.

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

[63]Note that the assessment of the quality (and informativeness) of studies is itself defined only in terms of intra-scientific values, mainly avoidance of chance and bias (IARC, 2019, 17-20).

to reach a consensus[64]), making it as much as possible rule-governed rather than based on values (7, 11/20), and the only values mentioned are intra-scientific, such as repeatability, empirical support, consistency, specificity, traceability (26, 30/60), absence of bias, transparency, reliability (33/60)[65]. 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 independently of extra-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 extra-scientific values, it would concern expert reports for non-epistemic decision-making, not the scientific corpus (again 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 extra-scientific 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[66].

Another objection[67] to stating uncertainties is based on higher-order probabilities: stating prob-

[64]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).

[65]I found only one illustration of an extra-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 (seesection 2.4).

[66]Elliott (2022, 26-27) seems to believe that, apart from fully disclosing uncertainties, 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.

[67]Anticipated by Rudner (1953) (and revived by Douglas (2009) and Steele (2012)) to refute a position stating

abilities for a claim 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 nth-order probability statements diminishes rapidly with increasing n, so that, for example, a 5th-order probability statement can be considered as virtually certain for all practical purposes' (Betz, 2017, 104). In fact, it seems that we never, or very rarely[68] 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, stating uncertainties associated with scientific claims instead of bridging them with extra-scientific values seems primordial. Betz is the main advocate of this approach, but he does not allow at all extra-scientific values to influence the scientific corpus, and this is problematic for non-epistemic decision-making. 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 were true

2.4.1 The distinction

While the truth of scientific knowledge must be ensured and uncertainties stated clearly, it is also important to be able to take non-epistemic (intra- or extra-scientific) decisions on the basis of values, for example to pursue research on the basis of a yet unproven hypothesis (intra-scientific decision), to ban a substance which is suspected to be toxic although this is not scientifically established (extra-scientific decision), or to use a scientific claim for applications with high safety stakes (extra-scientific decision) (Hansson, 2017a). For such cases, we may want to base 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[69] that a substance is toxic, we may want to ban this substance even if the toxicity is not scientifically established, thereby lowering the LER for our decision). Therefore values should clearly not be excluded from science *applications*, where we *use* scientific knowledge for non-epistemic (intra- or extra-scientific) purposes (see Stamenkovic, ming, §2.1.2). Since, on the other hand, we still want to preserve the truth of scientific knowledge, 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).

uncertainties, first articulated by Jeffrey (1956).

[68] 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').

[69] 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).

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)[70]. Unfortunately, this essential distinction is often not made or unclear, be it by proponents of the VLT (such as Douglas, 2009, who takes examples in regulatory toxicology)[71], 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)[72]. 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[73], 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 scientific claims and claims taken as a basis for action, 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[74], 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).

2.4.2 Hansson's corpus model

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

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

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

[72]Arguments in favour of generalised value influence 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 these two types of literature. Conversely, arguments in favour of the present conception (closer to the VFI), such as the explicit statement of uncertainties, are even strengthened when illustrated by IPCC or other expert reports (where the influence of values is supposed to be the most pregnant).

[73]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).

[74]More accurately, scientifically established facts.

(which roughly corresponds to accepting a claim into the corpus here), which requires to exclude extra-scientific values (Lacey talks of social values); 'adopting' a hypothesis for further research (which roughly corresponds to a non-epistemic intra-scientific decision here); and 'endorsing' a hypothesis for practical action (which corresponds to an extra-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), 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 between the LER for Non-epistemic decision-making (LERN) and the LER for Epistemic acceptance of a claim (LERE), and hence to preserve the truth of scientific knowledge. Indeed, in case LERN > LERE, the LERE is raised accordingly, so that the truth of scientific knowledge is only reinforced, as we have seen in section 2.2. 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[75], as our most reliable knowledge, applicable to any use. Thus, accepting a claim as true implies accepting it as a basis for action[76], but the converse is not true[77].

Hansson's corpus model has many advantages (for a detailed study, see Stamenkovic, ming), including that is respects the reasons given above for ensuring the truth of scientific knowledge. Because it ensures the truth of scientific knowledge, it also ensures the productivity of science, and indirectly ensures its (intra- and extra-scientific) applicability. In addition, the distinction between LERE and LERN promotes further scientific investigation, in the same way the statement of uncertainties does (see above):

- if LERE < LERN, then the LERE is increased to the 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 evidence which is insufficient to justify acceptance into the corpus: but probably some people (scientists and/or decision-makers, e.g clinicians or regulators) will want to check if the claim is in fact scientifically established.

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

[75] 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.

[76] If, of course, such action is appropriate (there may be true claims on the basis of which we do not want to act, for example true claims about effective methods of torture, as one reviewer suggested).

[77] 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).

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 extra-scientific consequences of a claim (Stamenkovic, ming). As noted in footnote (66), 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 second-order 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, 66-86). 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 make 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 accepted) and experts (recommending the ban of a substance suspected of being toxic even if it is not scientifically established).

2.4.3 Examples

For example, this is how the ANSES (2013) recommended to ban Bisphenol A 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)[78]. 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 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

[78] As noted above (footnote (77)), 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 it is used by cars.

> [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. Further information for the PBT/vPvB assessment is needed. (ECHA, 2017, 96)

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. Now the guidance explicitly considers the second, *as if* alternative of the distinction discussed here:

> If the registrant[79] 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 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. (ECHA, 2017, 28)

In other words, in case of uncertainty and insufficient information, the regulation agency requires 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

[79] The company producing the substance.

philosophical discussion by conceptualising a descriptive-normative[80] 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[81], 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[82]) which already hinder scientists from actually performing research and instead force them towards administrative tasks and increased reporting (Mahoney, 1999)(for an overview, 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. In addition, their formulation is often too vague to be helpful, and would require clarification and additional work. 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 in fact unaddressed. Without further precision, these rules or guidelines may well be similar to those advocated here.

3 Concluding remarks and future research

3.1 Summary

This article has shown why minimising as much as possible – not excluding – the influence of extra-scientific values in the A/R phase is a reasonable approach. So far the original arguments for the VFI (ensuring the truth of scientific knowledge, respecting the autonomy of science results users, preserving public trust in science) have not been satisfactorily addressed by proponents of the VLT. Starting from the fundamental requirement to distinguish between facts and values, this article has proposed four prerequisites that any model for values in the A/R phase must abide: 1) it must ensure the truth of scientific knowledge; 2) it must state clearly the uncertainties associated with scientific claims; 3) it must 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

[80]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.

[81]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 (2022, 27-28).

[82]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).

two conceptual resources for implementing these prerequisites: Betz´s conception (for stating uncertainties, but it does not allow extra-scientific values at all) and Hansson's corpus model (for incorporating extra-scientific values while preserving the truth of scientific knowledge and allowing for different LERs according to whether the claim is incorporated into the corpus or used as a basis for action, but it does not consider uncertainties associated with claims). Betz's conception should not be considered as a kind of input, or as an alternative (with a third option of 'suspending judgement' between accepting or rejecting a claim) to Hansson's: rather, both models apply simultaneously. As long as there is uncertainty associated with a claim, it should remain clearly flagged. The statement expressing an uncertainty can be (and often is) accepted into the corpus, and can also be used for intra- or extra-scientific application on the basis of values[83]. Taken together, Betz's and Hansson's conceptions 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 come from the need for a self-reflection on values by philosophy itself. Philosophy cannot forego reflecting on how values do, and should, influence its own practice, regarding the motivations, the relevance and the consequences (especially extra-scientific) of this practice – indeed, such a reflective approach is consistent with, and required by, allowing values to influence science, which includes philosophy (Hansson, 2017c). Here I will not address specifically the motivations the VLT (although the social responsibility of science can broadly be characterised as its main driver), but these motivations probably have an influence on the *relevance* of the philosophical claims for scientific practice, both in research and expertise (see footnote (10))[84]. I will shortly address this point, as well as the intra- and extra-scientific *consequences* of the philosophical debate on values in science. These last two points can in fact be considered as additional prerequisites to the four ones presented so far: 5) a proposal for values in science must be descriptively and normatively relevant; and 6) its consequences must be thoroughly assessed.

3.2 Ensuring the relevance of proposals for values in science

With respect to the first point, it has become a kind of programmatic claim among some VLT 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

[83]Another way to deal with uncertainty is illustrated by Hansson's 'bypass route' (2018), which relies on data instead of the corpus and goes directly from data to policy for taking non-epistemic decisions, when a claim has not reached the LER by science. Here it is supposed that the claim has reached the LER but in its uncertain form. In both cases the original claim cannot be considered certain.

[84]If this is indeed a claimed goal. Of course, philosophising is also valuable for its own sake, but then one should not pretend to be relevant for scientific practice.

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 (neither is, probably, the vast majority of scientific activities[85]), and some value judgements are inevitable. This does not mean, however, these such values are *extra-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 extra-scientific values when making their claims. *Contra* Douglas, I rather think that scientific practice would be practically *impossible* if scientists had to take extra-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[86] – 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 intra-scientific 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 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.

Because I contest the descriptive part of some VLT accounts, I think their normative parts (which are based on these incorrect descriptive accounts) are unsound. As argued throughout this paper, I believe that a normative framework, in order to be relevant, has to be based on a correct descriptive assessment. It seems that general philosophy of science (as opposed to the philosophies of the special sciences) tends to develop on its own, too far from scientific practice, and to grow into endless analysis and refinement. For example, if second-order probability statements do not appear in expert reports, perhaps it is irrelevant for expert practice to devise sophisticated philosophical arguments on their basis. The same holds for infinite regress (see footnote (66) and section 2.4.2). 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 *actual practice*. This is typical of analytic philosophy to always look for more sophistication in argumentation (e.g. in the form of thought experiments or conceptual refinement), but the relevance and usefulness of this sophisticated argumentation 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 Environ-

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

[86] In the same spirit as footnote (9), 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 extra-scientific values are irrelevant; 2) whether even in fields which are *prima facie* relevant for extra-scientific decisions, there are many claims for which extra-scientific values are irrelevant; 3) whether even for claims where extra-scientific values are relevant, the latter do not make any difference with respect to the disciplinary LER for acceptance of claims. But contrary to footnote (9), 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. These avenues of research have been suggested by Sven Ove Hansson, whom I thank warmly.

ment 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[87] at the EPA, or the GOLIATH[88] project which involves several European institutions) for performing systematic reviews and assessing evidence on a particular claim, following a weight-of-evidence approach[89]. 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[90]. 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 is 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 extra-scientific values, that does not mean that they *should* do so, and that does not automatically invalidate normative models advocating value influence in the A/R phase. Nevertheless, this practice is a fact which must be taken into account by such models, in order to question their desirability (why do these agencies adopt such a minimally value-laden approach?), their possibility (is it realistic to advocate value influence in the A/R phase? is it possible to implement such models?) and their consequences (what happens if we allow values in the A/R phase?).

3.3 Assessing their consequences

With respect to the second point, we have seen that the same overarching value of the social responsibility of science invoked in favour of extra-scientific values in the A/R phase can also be used against them. If we want to ensure the progress of scientific knowledge, and use it for all sorts of applications, we should not allow our intra-scientific standards of evidence to decrease for

[87]Integrated Risk Information System.

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

[89]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 also take 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. Taking into account this difference is also essential for future research.

[90]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 extra-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.

extra-scientific reasons. Accepting a claim insufficiently backed by evidence on the basis of values, while being justified in a certain context, may have disastrous consequences in another. Therefore, great care must be taken with respect to the potentially detrimental extra-scientific consequences that the philosophical debate on values may have, for example with respect to scientific dissent in disciplines with social impact (e.g. 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 extra-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 in support of his position (of course the article does not support this position) (Hansson, 2020a, 22). While the philosophical debate on values is of course to be welcomed like any philosophical discussion, it should also include a careful reflection on its potential detrimental consequences and misuses.

For example, in this topical collection Elliott (2023) argues that scientific dissent about the Post-Treatment Lyme Disease Syndrome (PTLDS) can be understood as a dispute about value 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 (as Elliott is well aware of). 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)[91]. But in the middle of the controversy, where several studies show no objectively supported benefits, and sometimes considerable harm, associated with long term antibiotic treatment (e.g. Feder et al., 2007; Stanek et al., 2011; Ali et al., 2014; Melia and Auwaerter, 2016), this is clearly not 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 (Oliveira and Shapiro, 2015; 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.

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[91] Of course one can mention the somewhat converse principle of *beneficence* (of trying to do as much as is reasonably possible to cure a patient), 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.

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