Evidence: Wanted, alive or dead[1]

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Abstract: This paper is meant to link the philosophical debate concerning the underdetermination of theories by evidence with a rather significant socio-political issue that has been taking place in Canada over the past few years: the so-called 'death of evidence' controversy. It places this debate within a broader philosophical framework by discussing the connection between evidence and theory; by bringing out the role of epistemic values in the so-called scientific method; and by examining the role of social values in science. While it should be admitted that social values play an important role in science, the key question for anyone who advocates this view is: *what and whose values?* The way it is answered makes an important *epistemic* difference to how the relation between evidence and theory is appraised. I first review various arguments for the claim that evidence underdetermines theory and shows their presuppositions and limitations, using conceptual analysis and historical examples. After broaching the relation between evidence and method in science by highlighting the need to incorporate epistemic values into the scientific method, my discussion focuses on recent arguments for the role of social values in science. Finally, I address the implications of the approach outlined for the current 'death of evidence' debate in Canada.

Keywords: underdetermination, evidence, epistemic values, social values, standpoint epistemologies

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[1] This paper begun its life as a philosophical intervention in the 'death of evidence' controversy in Canada; hence, its seemingly strange title. My talk took place at the University of Toronto in April 2014, as part of the Lives of Evidence National Lectures Series, under the auspices of the Situating Science research project and with the support of the Institute for the History and Philosophy of Science and Technology, the Jackman Humanities Institute Working Group on Scientific Evidence and the Rotman Canada Research Chair in Philosophy of Science, Western University. I want to thank Gordon McOuat and Brian Baigrie for their kind invitation; the audience of the talk for challenging questions; Martin Vezer and Ingo Brigandt for useful written comments on an earlier draft; and two anonymous reviewers for their penetrating comments. Most of all, I want to thank wholeheartedly Maya Goldenberg and Helena Likwornik for thoughtful, deep and challenging critical comments during the event in Toronto. I dedicate this paper to the memory of Joseph L Rotman, a passionate friend of philosophy.

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1. Introduction

On the 10th of July 2012 about two thousand scientists held a rally on Parliament Hill in Ottawa to protest against the Stephen Harper Administration's sweeping cuts to research. They marched in the streets of the capital of Canada holding a mock funeral to mourn what they thought was 'the death of evidence' and the muzzling of scientists by the government. They protested against closure of the Experimental Lakes Area, the Polar Environment Atmospheric Research Laboratory and the First Nations Statistical Institute. As Katie Gibbs, a PhD student in the biology department at the University of Ottawa, who spoke in the rally, said, the demonstration was "to commemorate the untimely death of evidence in Canada." Slightly more optimistic was Scott Findlay, associate professor and former director of the University of Ottawa's Institute of Environment, who said: "evidence is not quite dead, but it is at the very least at death's door".[3]

In the wake of this event scientists, activists and public opinion-makers in Canada have launched a campaign (which has mobilised scientists in 'Stand Up for Science' rallies in 17 Canadian cities) aiming to protest against the conservative government's 'war on science', to promote the value of evidence and the significance of following an evidence-based policy. A key slogan of this campaign is: "no science, no evidence, no truth, no democracy."

This has been a campaign about the value of science (and in particular of evidence in science). But it is also a campaign which highlights the role of values *in* science. The 'death of evidence' debate is a debate about competing sets of values and the role of them in science.

This controversy, important though it is, has not yet been the subject of a philosophical examination.[4] The aim of this paper is to place 'the death of evidence' controversy within a broader philosophical framework by discussing the connection between evidence and theory; by bringing out the role of epistemic values in the method of science; and by examining the role of social values in science.

I hope that this paper will be useful to both scientists *and* philosophers. I will first challenge the credentials of the argument from underdetermination of theories by evidence and defend the view that values are indispensable in theory-choice. I will then focus my attention on the role of social values in science, and capitalising on the work of standpoint epistemologists, I will argue that the key question about social values in science is: *what and*

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[3] See the Globe and Mail 10/07/2012 http://www.theglobeandmail.com/news/politics/scientists-take-aim-at-harper-cuts-with-death-of-evidence-protest-on-parliament-hill/article4403233/

[4] For a detailed and passionate account of this controversy, and the evidence there is for Harper's administration 'war on science', see Chris Turner (2013). See also Linnit (2013). Heather Douglas has also published a short piece in The Scientist Magazine in April 2, 2013.

whose values? Next, I will claim that the answer to this question turns on the universalisability of otherwise perspectival values. This will ground their objectivity without falling foul of the chimerical value-free ideal of science. Finally, I will apply this idea to the 'death of evidence' debate.

2. Evidence and theory

2.1 Evidence and observational consequences of a theory

The claim that evidence underdetermines theory rests on an empirical fact and a logical fact. The empirical fact is that all interesting scientific theories have excess content over and above the various observations, data and other pieces of evidence that probe them and guide their formation. The logical fact is that deduction being what it is, there cannot be a deductively valid argument whose sole premises are statements expressing available observational evidence and whose conclusion is a theory whose content exceeds whatever it is asserted by the premises. Given these two facts, if the theory is not just a *summary* of the available evidence, the evidence cannot possibly prove the truth of the theory. Differently put, the relation between evidence and theory is ampliative.

These two facts are taken to generate an epistemological question: how can we ever justifiably believe in the truth of a theory whose content exceeds the content of the evidence? Answering this question has been the province of the theory of confirmation. But there seems to be a challenge to the very idea of evidence justifying belief in a theory: given that the evidence does not entail a theory, it is possible that two or more rival theories may entail the same evidence; how then can the evidence support one of the theories more than its rivals? What is normally added to this challenge is that for any finite body of evidence, there *always* will be more than one rival theories which entail that evidence.[5]

There is no general and uncontroversial proof that for *any* theory scientists come up with (and *any* body of evidence) there always will be scientifically interesting (and scientifically plausible) empirically equivalent rivals. André Kukla (2001) has proposed certain *algorithms* for the construction of empirically equivalent rivals to any theory T. Here are his two of them:

Algorithm 1: "For any theory T, construct the theory T1 which asserts that the empirical consequences of T are true, but that none of its theoretical entities exist" (2001, 22-3).

[5] Larry Laudan (1990) has called this view 'Humean Underdetermination'. In my (2006), I have called it 'deductive underdetermination'.

Algorithm 2: "Given theory T, construct T2 which asserts that T holds when somebody is observing something, but that when there's no observation going on, the universe follows the laws of some other theory T'" (2001, 23).

Even though there might be some philosophical motivation for these algorithms (they underpin various sceptical stances), there is no scientific motivation for them. T1 and T2 are not theories, strictly speaking. They are totally parasitic on a proper scientific theory T. Algorithm 1 simply captures denialism about unobservables. T1 has no independent scientific motivation. As for theory T2 (in algorithm 2), it is not even empirically equivalent with a proper theory T since T implies nothing about the existence of observers, while T2 implies that there are observers.

Even if one adopted simple versions of the hypothetico-deductive method of confirmation, there would still be good reason for resisting underdetermination: there is more to empirical evidence than the observational consequences of a theory. Two arguments help us see why this is so.

First, since theories entail observational consequences only with the aid of auxiliary assumptions, and since the available auxiliary assumptions may change over time, the set of observational consequences of a theory is not circumscribed once and for all; it is temporally indexed. Hence, even if for any time t, two (or more) theories may entail the same observational consequences, there may be future auxiliary assumptions such that, when conjoined with one of them, say T, fresh observational consequences follow that can shift the evidential balance in favour of T over its rivals.[6]

Second, theories may get support from pieces of empirical evidence that do not belong to their observational consequences. Hence, the kinds of evidence that can support a theory is broader than the set of the observational consequences of the theory.

I will illustrate each of these two arguments with a historical case. A clear example of the first kind of case concerns the discovery of planet Neptune. Here it is in outline. Planet Uranus was discovered by William Herschel in 1781. The so called 'problem of Uranus' was that the trajectory of this planet had proved to be intractable. Following Laplace's monumental calculations of the mutual perturbations exerted by the planets, Alexis Bouvard tried in 1821 to calculate the tables predicting the positions of the three giant planets: Jupiter, Saturn and Uranus. Uranus' positions were not the ones predicted by the Newton-Laplace theory even after taking into account the perturbations exerted by the other

[6] Sober (1999) has exploited this feature of evidence in his own account of contrastive/comparative testing of theories, according to which one theory is always tested relative to another one.

planets. For our purposes what needs highlighting is that the predicted motions of Uranus—those that were at odds with the actual record of its observed motions—were the consequence of conjoining the Newton-Laplace theory with the auxiliary assumption that the possibly disturbing planets were seven. It was Alexis Bouvard himself who first speculated that a new *planète troublante* could cause the anomalous motion of Uranus. But it was Urbain Le Verrier who in 1846 took on the task to calculate the position and mass of the perturbing planet.

The logical structure, as it were, of the task was the following. Given a new auxiliary assumption (a new perturbing planet) would it still follow that the trajectory of Uranus would be anomalous? That is, would the *new* predicted value be at odds with the observational record? Mathematically, the problem was the inverse of this, viz., to use the perturbations as given and to calculate the position and mass of the planet that would cause them if it were there. This involved some significant simplifications—e.g., that the distance of the planet from the sun is known. But in his presentation to the Academy of Sciences on 1 June 1846, Le Verrier could confidently announce:

> I conclude also that one can effectively model the irregularities of Uranus's movements by the action of a new planet placed at a distance of twice that of Uranus from the Sun; and what is just as important, that one can arrive at the solution in only one way. To say that the problem is susceptible to only one solution, I mean that there are not two regions in the sky in which one can choose to place the planet in a given epoch (such as, for instance, 1 January 1847). Within this unique region, we can limit the object's position within certain bounds.

The uniqueness of the region was a significant result, even though there was still considerable uncertainty about the planet's exact location, since it shows that the interplay between theory and evidence can lead to considerable narrowing down of the theoretical space of possible alternatives. In the night of the 23/24 September 1846, the astronomer Johann Gottfried Galle in the Berlin observatory discovered the perturbing planet. Then a number of astronomers, including Le Verrier, observed the planet. A few days later two science journals announced the discovery. The planet was called Neptune, a name proposed by Le Verrier.[7]

A clear example of the second kind of case concerns the theory that continents 'drifted' to their present position over millions of years—the well-known 'continental drift theory' proposed by Alfred Wegener. According to the theory as it was later developed, when

[7] I have drawn from the excellent book by James Lequeux (2013). The quotation is from p. 28.

tectonic plates move across the surface of the Earth, they carry the continents with them. The proposed theoretical mechanism for this is sea floor spreading**,** which was first proposed by Harry Hess. In broad outline the idea is this. Molten magma from beneath the surface of the earth rises and breaks the Earth's crust in certain weak places. A place that this typically happens is a spreading ridge, i.e., a gap in the sea bed which is widening as the tectonic plates move apart. The magma that fills these gaps cools and hardens, thereby pushing older rock aside as new sea floor is created. If this theory is right, there must be spreading ridges to be found in the oceans. Indeed, the largest of all these ridges is the Mid-Atlantic Ridge, which runs north to south down the length of the Atlantic Ocean.

But what is really noteworthy is that this theory gets unexpected support from some piece of evidence that is not geological; nor is it implied by the theory. This is the so-called magnetic stripping. Minerals that contain iron in the magma align themselves with the magnetic field of the Earth as the magma cools. But the orientation of the Earth's magnetic field has changed polarity many times over history. Actually this is something evidence for which became available fairly recently, viz., in the early 1960s. It would therefore be expected that the rocks that make the sea bed would exhibit a pattern of polarity reversals (from normal polarity to reverse polarity) depending on the polarity they recorded when they cooled. This is exactly what was observed by scientists using magnetometers at spreading ridges.[8]

Hence, empirical evidence can support a theory (and concomitantly, it can support a theory more than its rivals) given that what counts as evidence for a theory changes over time and goes beyond the observational consequences of the theory under test.

2.2 Prior probabilities

Laudan (1990, 271) attributes to Quine (1975) a different kind of underdetermination thesis, viz., every theory is as well supported by the evidence as any of its (empirically equivalent) rivals. It's not clear to me that Quine did entertain this view, though as I will show in the next sub-section, there is a reading of him (associated with the Duhem-Quine thesis) which is amenable to this interpretation. Be that as it may, this kind of view could be associated with Popper's anti-inductivism. For on his account of the relation between evidence and theory no evidence can ever inductively support any theory. But if we look at theories of *confirmation*, then on *any* extant theory, the evidence can render a theory probable or more probable than its rivals. So the claim that evidence underdetermines theory in the sense that

[8] This point has been made by Laudan (1990).

it can *never* render a theory probable (or more probable than its rivals) must rest on some arguments that question *the very idea* that evidence can play a confirmatory role vis-à-vis the theory. I will examine one such type of argument.

It is well-known that no evidence can affect the probability of the theory unless the theory is assigned some non-zero initial probability. In fact, given that two or more rival theories are assigned different prior probabilities, the evidence can confirm one more than the others. So, it is enough for differential confirmation by the evidence that the rival theories have been assigned different prior probabilities (cf. Earman 1992, 150). The challenge, then, is: where do these prior probabilities come from? In particular: how can prior probabilities have any *epistemic* force?[9]

Subjective Bayesians appeal to subjective prior probabilities (degrees of belief) and rely on convergence-of-opinion theorems to argue that in the long run, the prior probabilities wash out: even widely different prior probabilities will converge, in the limit, to the same posterior probability, if agents conditionalise on the same evidence. But, though true, this move offers little consolation in the present context because, apart from the fact that in the long-run we are all dead, the convergence-of-opinion theorem holds only under limited and very well-defined circumstances that can hardly be met in ordinary scientific cases (cf. Earman 1992, 149ff).

Is there an alternative way to fix prior probabilities? There have been great strides towards developing objective Bayesianism and various ways to use statistical methods to fix prior probabilities (see Williamson 2010, especially pp. 165ff). But I want to make a more general point, viz., that prior probabilities can have epistemic force because they can be based on plausibility or explanatory judgements. And these may be taken to express rational degrees of belief. Now, to start taking seriously this point requires that a broader conception of rational belief is adopted and in particular one that does not rely on a topic-neutral logic of induction, which is supposedly based on a priori principles of rationality.[10] Such principles are hard to find and even harder to justify. Still, there are rational grounds for assigning initial degrees of plausibility to competing theories; relying, for instance, on theoretical virtues such as simplicity, explanatory power, coherence with other theories, and fecundity. These kinds of virtues are typically of the sort that makes scientists take a theory seriously as subject to further exploration and test. These theoretical virtues are compatible with a

[9] For a discussion of this issue see Douven (2008). Likwornik (2015) discusses how prior probabilities can be influenced by epistemic and social values.
[10] Recent attempts to deny the alleged topic-neutrality of induction include Norton (2003) and Brigandt (2010).

broadly Bayesian probabilistic account of confirmation. But they also play a key role in probabilistic but non-Bayesian accounts of confirmation, such as Peter Achinstein's (2001) theory of evidence. Prior probabilities can certainly be whimsical, but they need not be. They can be based on judgements of plausibility, on explanatory considerations prior to the collection of fresh evidence and other such factors, which—though not algorithmic—are quite objective in that their employment.[11]

2.3 Empirical equivalence and the Duhem-Quine thesis

Can there be *totally* empirically equivalent theories, i.e., theories that entail exactly the same observational consequences under *any* circumstances? The so-called Duhem-Quine thesis has been suggested as an algorithm for generating empirically equivalent theories. Briefly put, this thesis starts with the undeniable premise that all theories entail observational consequences only with the help of auxiliary assumptions and concludes that it is always possible that a theory together with suitable auxiliaries can accommodate *any* recalcitrant evidence. A corollary, then, is that for *any* evidence and *any* two rival theories T and T', there are suitable auxiliaries A such that T' & A will be empirically equivalent to T (together with its own auxiliaries). Hence, it is argued, no evidence can tell two theories apart.

It is doubtful that the Duhem-Quine thesis is true.[12] There is no proof that non-trivial auxiliary assumptions can always be found. But let us assume, for the sake of the argument, that it is true. What does it show? Not much really. From the alleged fact that any theory can be suitably adjusted so that it resists refutation, it does not follow that all theories are equally well-confirmed by the evidence. The empirical evidence does not necessarily lend equal inductive support to two empirically congruent theories since it is not necessarily the case that the auxiliary assumptions that are needed to save a theory from refutation will themselves be well supported by the evidence. Some auxiliary assumptions, for instance, might be totally ad hoc, without any independent plausibility or testability; or even plain wrong.

To illustrate this point lest us look at the case of Mercury. It was well known around 1850 that the orbit of planet Mercury was anomalous. The predicted ellipse was not quite what was observed. Actually, if attention is focused on the perihelion of Mercury (the point

[11] For a specific case of how these explanatory consideration work in practice, see my 2011.

[12] There is a great deal of literature on this thesis. See my 1999, chapter 7 and the references therein. For Quine's views see (1975). Duhem's case is more complicated and it is questionable that his position is similar to Quine's. For Duhem's views see my 1999, chapter 3. See also Ariew (1984).

closest to the Sun), then it was observed that this perihelion advances regularly with an angular velocity usually expressed in seconds of arc per century. Here is a case similar to the case of Neptune above. The Newton-Laplace theory predicts, together with various auxiliaries, an elliptical orbit for Mercury; but this is not quite observed. Even with modified auxiliaries, by taking account of the perturbation by the other planets, most significantly by Venus, the anomalous perihelion was not accounted for. One interesting modification of auxiliary assumptions concerned the mass of Venus. If the mass of Venus was larger by 10% than what it was taken to be, this very fact would explain Mercury's anomaly. But this new auxiliary assumption could be independently tested. If the mass of Venus were larger, the perturbations caused by Venus in the orbit of earth would be inadmissibly large. So Le Verrier came up with a different hypothesis:

> A planet, or if one prefers a group of smaller planets circling in the vicinity of Mercury's orbit, would be capable of producing the anomalous perturbation felt by the latter planet…. According to this hypothesis, the mass sought should exist inside the orbit of Mercury.

A new planet was therefore posited, which if it *were* present between Mercury and the Sun, and if it had the right mass, it would perturb Mercury's motion enough to account for the anomalous perihelion. Though Le Verrier had doubts about the existence of such a planet, there were some reported sightings of it and he came to accept its existence: he called it *Vulcan*. But this new auxiliary hypothesis, which would save Newton's theory from refutation, could be independently tested—and further observations made showed the presence of no such planet. In fact, the solution of the anomalous perihelion of Mercury had to wait the advent of Einstein's general Theory of relativity and the essential revision of Newton's theory of gravity.[13]

Evidence, therefore, can bear on theories in many and variegated ways, turning the balance in favour of a theory over another.

2.4 Evidence and epistemic values

Still, the evidence does not speak with the voice of an angel! Nor does it operate in a theoretical and axiological vacuum. Perhaps the most important lesson that can be drawn from the discussion of the Duhem-Quine problem is that the thought that there is an

[13] Of the total 574 arc-seconds per century precession, 531 arc-seconds were accounted for by Newtonian perturbation theory. 43 arc-seconds anomaly remained unaccounted for, and a new theory was required for its explanation. For the details of this case see James Lequeux (2013). The quotation is from p. 166.

algorithmic relation between theory and evidence is bankrupt. In support of this claim, let us take a leaf from Duhem's (1906) masterpiece *Aim and Structure of Physical Theory*. Perhaps more than anyone else, Duhem felt the fundamental tension between the strict conception of scientific method that he himself had advocated in his attack on role of explanation in science and the need for a broader conception of rational judgment in science. He forcefully argued that there is space for rational judgements in science which is *not* captured by the slogan: scientific method=evidence + logic. What's important here is that evidence-plus-logic are not enough even to decide when a theory should be abandoned (or modified).

The cases of Uranus and Mercury are instructive. They concern the same theory—Newton's theory of gravity—and they have roughly the same conceptual structure: they are about bringing Newton's theory in line with anomalous trajectories of planets. And yet, in the Uranus case, we have a triumph of Newton's theory, whereas in the Mercury case we have a failure of the theory. In the Uranus case, the blame is put on auxiliary assumptions and the theory is saved from refutation; in the Mercury case, the blame is put on Newton's theory itself and the theory is abandoned. No algorithmic account of the relation between evidence and theory can present both moves as rational.

But they both are! And to see why, let us pursue Duhem's line of thought. Duhem made famous what Poincaré had already noted by saying that though evidence does not, strictly speaking, contradict a theory, it can *condemn* it. He is well-known for his view that crucial experiments are "impossible in physics" (1906, 188). A crucial experiment is meant to be an experiment that would *prove* one theory wrong—one that would strictly *contradict* the theory. If a situation such as this is not possible, how do theories get abandoned? Any answer would have to go beyond the strict limits of evidence and logic. And Duhem's own answer does. He employed other criteria of assessment. Here are some that he suggests: the scope of the theory, the number of hypotheses, the nature of hypotheses, novel predictions (1906, 28, 195), compatibility with other theories (1906, 221, 255), unification into a single system of hypotheses (1906, 293).

These are, of course, the usual suspects. They are *values* or *virtues* of a theory that transcend logic (or, at least, they defy a rigorous logical formulation). What Duhem saw clearly was that the employment of such criteria is a) indispensable, and b) not algorithmic. Their employment requires the exercise of *judgement*. The lesson we should draw from Duhem is that judgement is part of the so-called scientific method. An extreme positivistic understanding of scientific method, encapsulated in the fiction of Carnap's robot, as a fully-determined-by-exact-rules algorithmic process which delivers 'yes-no' answers (or exact degrees of confirmation) for each hypothesis formulated in a formal language, is not just a chimera. It is, in addition, a model that does not bear any resemblance to whatever happens in science.

If we are to stay in contact with the way science is practised, we should take it to heart that scientific method is not algorithmic. It requires, and relies on, the exercise of judgement. This judgement is constrained by evidence as well as by several virtues that theories should possess. It can be rational even if it is not *dictated* by evidence plus logic. Its rationality depends, ultimately, on taking account of the *reasons* that favour a certain option and condemn another.

This need for an account of rational judgement which goes beyond experience-plus-logic has been articulated by Ernan McMullin. As he aptly noted: "Values do not function in assessment as rules do" (1996, 19). It's not just that different scientists may weigh different values (or virtues) differently. This, as Kuhn has already noted, is true enough. But it is also true that even if they are weighed similarly, they may be in conflict with each other (say, simplicity vs informativeness). Hence, judgement is required in balancing them out. No recipe is there for choosing among competing theories. It would be too quick, however, to conclude from this that these values have no rational force. This would amount to intellectual paralysis. Take the prime empiricist virtue (and don't forget that it is a *virtue* too): empirical fit. Of course, theories should be consistent with the evidence (or entail it). But judging empirical fit is no (much) less value-laden than judging, say, explanatory fit. It's not just that many competing theories can be consistent with the same observations. It's also that the very empirical fit of a theory to facts requires judgement: Which are the relevant data? Which measurements are reliable? What error-margins are allowed? etc.

3 Social values in science

3.1 Epistemic and social values

Thanks mostly to the work of feminist philosophers of science, a great deal of attention has been given to the role of social values in science in the last few decades. The distinction between epistemic values and social values is not sharp;[14] but there are paradigmatic cases of epistemic values (that is, values which, at least under favourable circumstances, would be related to the probability of a theory's being true) and social values (that is, values whose

[14] The distinction is non-existent for social epistemologies, one anonymous reader remarked. But, to the best of my knowledge, the feminist philosophers of science I know of do draw a distinction between what is normally called 'constitutive values' and 'social values'. The distinction need not be sharp to exist. For an in depth discussion, see Longino (1996).

raison d'etre are social or ideological or moral or political considerations).[15] So simplicity, coherence, explanatory power, novel predictive success are epistemic values; promoting the welfare of humanity; creating equal opportunities; respecting the moral rights of individuals are social values. Note that the last examples are examples of *good* social values. But, not all social values are equally good; nor is it the case that the goodness or badness of a social value is always written on its forehead, as it were. Nor, worse, is it the case that by calling a social value 'value', it makes it inherently valuable.

In the case of epistemic values, there is at least a prima facie argument why they are important (actually, indispensable) in scientific inquiry. Not only do they constitute part and parcel of ordinary scientific judgement and are involved in theory appraisal; but by being at least in principle truth-conducive, or by being truth-conducive under certain circumstances, they affect the probability of a theory's being true. But what exactly is or should be the role of *social* values in science? After all, many philosophers and scientists are still taken by the value-free ideal.[16]

An entry point for social values in science relates to the problem of underdetermination we have discussed in the previous section.[17] Matthew Brown (2013) has codified two relevant arguments. The first is "the gap argument". Evidence underdetermines theory. Yet, theory-choice is not and should not be paralysed in the face of underdetermination. Hence, social values 'fill the gap' between evidence and theory and determine (or partially determine) theory-choice.

There is a variant of this argument, due to Justin Biddle (2013), which is meant to block an immediate response to the 'gap argument'. This response, explored already in section 1, is that even if we were to grant that evidence underdetermines theory, epistemic values can be appealed to in order to break observational ties. Hence, it is epistemic values that can and should determine theory-choice. Biddle's argument against this response is that an appeal to epistemic values is not enough as a tie-breaker since one set of epistemic values

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[15] The same attentive reader noted that there are values "that may be placed on either side or in between". Examples offered are: "fecundity, being non-anthropomorphic, reductionist, materialist". Though I agree that some values might be either hard to classify or Janus-faced (e.g., offering *mechanistic* explanations), I would like to distinguish values (such as respecting human life) from philosophical desiderata (like materialism), which however might themselves be subject to empirical or theoretical investigation. See also Steel (2015, 160ff).

[16] For a critique of the value-free ideal see Douglas (2009, 60-65).

[17] The *locus classicus* of this view is Lynn Hankinson Nelson (1996) and her naturalised Feminist Account of Evidence (FAE). She has taken it to be the case that the evidence that is brought to bear on theories includes observations "and other theories that together constitute a current theory of nature, inclusive of those informed by social beliefs and values" (1996, 100). For a 'state of the field' account of the current debate see Biddle (2013) and the references therein. See also Steel (2010), Carrier (2011) and Steele (2012).

(or one assignment of weights to epistemic values) might favour one theory and another set of epistemic values (or simply another assignment of weights to the same set of values) might favour a rival theory. The conclusion Biddle draws is the social values might well (actually, they *should*) be appealed to in order to break ties between competing sets of (or competing weight assignments to) epistemic values.

The second argument for social values is "the error argument". In broad outline, it is this. Science can never yield certainty; hence, scientific theories can be erroneous despite the evidential support they might enjoy. Yet, theories are nonetheless accepted or rejected and judgements of acceptance or rejection are dependent on decisions about how serious an error it is to accept a theory if it is false or to reject it if it true. These latter judgements are amenable to ethical and social considerations. Hence, theory acceptance in science is subject to social values.[18]

Both arguments share an assumption that was challenged in section 2, viz., that underdetermination is rampant and that evidential considerations are not enough to break occasional observational ties. If cases of underdetermination are not so pervasive, or if standard appeals to evidence break observational ties, then the appeal to social values to address this problem is not so urgent. Yet, even if the role of social values in solving observational ties is not as prominent as it has been supposed by the foregoing arguments, it is important not to lose sight of the fact that social values do play a significant role in science. To put it bluntly, social values play an important role in science because a) scientists are socially situated beings; b) scientific research has important social implications (and sometimes, presuppositions) which are potentially and actually exploitable by social groups. So the point is not to deny the role of social values, but to examine how they function and why. This is the lasting lesson of feminist epistemology of science.

One way to defend the ineliminability of social values has been to re-locate their function from the level of theory-choice to the adjacent stage of decision making and policy-making. The claim is that though social values do not offer evidential reasons to believe a theory, they do (and should) guide decisions about how to handle the uncertainty associated with a theory and how to employ the theory. The rationale for this conception of the role of values is that if social values are taken to play a role in theory acceptance itself (and hence in the reasons to believe a theory), the objectivity of science might be threatened. Heather Douglas, who has done some pioneering work in this area, stresses that social values (should) play an indirect role in theory-choice by acting as reasons to accept a certain level

[18] Steel (2015, 146-149) has a thorough discussion of this argument. Brigandt (2015) offers a critical assessment of the inductive risk approach.

of uncertainty. As she (2009, 87) characteristically puts it: "Values are not evidence; wishing does not make it so. There must be some important limits to the roles values play in science".[19]

3.2 Objectivity vs neutrality

I would be the last to deny that objectivity is important and that it makes science distinctive as a cognitive enterprise. Objectivity is hard to define precisely. I take it to stand for whatever is independent of particular points of view, perspectives, subjective states and preferences. It then follows that there are two distinct senses of objectivity, depending on how exactly we understand the demand of independence. The *first* sense is inter-subjectivity, understood as the 'common factor' point of view: the point of view common to all subjects. Thus understood, objectivity amounts to inter-subjective agreement. The *second* sense is radical objectivity: objective is whatever is totally subject-independent; what belongs to the world and not the knowing subject.[20] Inter-subjectivity can be profitably understood as being connected with *invariance*: objective is whatever remains invariant under transformations, or under change of perspective or point of view. Radical objectivity might be profitably understood as commitment to view that there is a worldly fact of the matter as to whether a theory or a belief is true or false and that this is independent of our knowledge of it. The quest for objectivity is a quest for grounding our beliefs about the world to the world. As Sandra Harding (1993, 92) has nicely put it: "The notion of objectivity is useful in providing a way to think about the gap that should exist about how any individual or group wants the world to be and how in fact it is".

One important lesson that standpoint epistemologies have taught us is that the demand for objectivity should be separated from the demand for neutrality (or disinterestedness) and that situated knowledge (and in particular knowledge that starts from the lives and needs of marginalized subjects) can be objective (see Goldenberg 2015). Objectivity does not imply neutrality or value-freedom. Our previous discussion of the problem of

[19] Douglas (2009, 96) has distinguished between two roles values can play in science. They play a direct role when they "act as reasons in themselves to accept a claim, providing a direct motivation for the adoption of a theory". But values play an indirect role when they are used "to weigh the importance of uncertainty about the claim, helping to decide what should count as sufficient evidence for the claim" (ibid.). Her key claim is that though the indirect use of values is fine, values should be used in a direct way only when it comes to influencing the choice of scientific projects. More specifically, direct appeal to values should be disallowed when it comes to rejecting or accepting hypotheses, or to assessing the evidence, or to the designing of experiments and the like. For a criticism of the Douglas' distinction between the two roles see Brigandt (2015).

[20] When, for instance, it is said that certain entities have objective existence, it is meant that they exist independently of being perceived, or known etc.

underdetermination has shown that objectivity does not require an impossible algorithmic account of how evidence bears on theory. In fact, values influence the evidential judgements of scientists and play a role in filling the gap between evidence and theory. Conversely, evidence influences the value judgements of scientists and plays a role in adjusting and refining values. So evidence and values are in reflective equilibrium and mutual adjustment. Values and evidence get into the scientific inquiry at the same time, and they presuppose each other. This interplay is constitutive of scientific enquiry. Here is a case which illustrates this point.

Isaac Newton's methodological rules ("rules of reasoning in philosophy") are rules of how evidence should be used and assessed. At the same time, they embody *values*. Take the famous rule IV:

> In experimental philosophy we are to look upon propositions inferred by general induction from phenomena as accurately or very nearly true, notwithstanding any contrary hypotheses that may be imagined, till such time as other phenomena occur, by which they may either be made more accurate, or liable to exceptions (2004, 89).

In this very rule, values play a prominent role. Newton makes it clear that a proposition which has been inductively established has to be adhered to *disregarding* alternative hypotheses—but this last requirement is a *value*; not statement of fact. Accuracy is a value too. By disregarding alternatives until more accuracy is needed or recalcitrant evidence is found, the accepted proposition does not, obviously, become wishful thinking. After all, it is the product on induction and hence it is supported by various natural phenomena. But, in Newton's case, these are *epistemic* values.

Can we run a similar argument for social values? I think we can provided we exercise some caution. The caution is needed because there is some prima facie plausible suspicion about the role of social values in science. Elizabeth Potter (2006, 76) sums up (without endorsing) the suspicion as follows: "Scientists use *either* facts *or* values to guide research; but not both. At best, contextual values (moral, social, or political values and interests) displace attention to evidence and valid reasoning; at worst, they lead scientists to bias, wishful thinking, dogmatism, dishonesty, and totalitarianism". The image of value-neutrality of science had gained plausibility by being contrasted to an image of social and political interest-driven science which generates bias, dogmatism, dishonesty etc. But feminist critiques of science have made a case for the claim that science is not value-neutral and, more importantly, that value-neutrality is the wrong image of science. The real issue, as I think Elizabeth Anderson has stressed, is not value-neutrality, but impartiality, which is achieved by "a commitment to pass judgment in relation to a set of evaluative standards that transcends the competing interests of those who advocate rival answers to a question." Evaluative standards are not value-free (they would not be evaluative if they were) but they require fairness, that is "attention to all the facts and arguments that support or undermine each side's value judgments" (Anderson 1995, 42).

The caution when it comes to social values is needed not because social values jeopardise the made-up image of value-free science but because social values are, ultimately, *socially determined values*, typically motivated by political, ideological and class (and not obviously epistemic) interests. But then the question arises: *what and whose social values?* This question has been raised by various radical feminist and Marxist philosophers of science and it is precisely this issue that needs appreciation. Anderson put it in terms of values that are epistemically fruitful; that is social values that guide research "toward discovering a wider range of evidence that could potentially support any (or more) sides of a controversy." (quoted by Potter 2006, 91).

I think the critics of the view of the social value-ladenness of scientific judgement are right in stressing that social values *might* jeopardise rather than promote the objectivity of science. But they are right in this suspicion only to the extent that they do not take into account the issue of what kind of social values they are. In other words, the key issue is not whether scientific judgement is value-laden but rather what kind of values it is laden with: what kind of social values are the *right kind of values*. But who is going to decide what are the right kind of social values and what not? Here again we can learn a lot from feminist epistemology.[21]

Before I attempt to address this key issue, let me examine briefly a case in which social values are part and parcel of a methodological principle of conduct of scientific inquiry. This

[21] The attentive reviewer noted that this point has been raised by Louise Antony in her (2003). The importance, I think, of Antony's approach lies in her attempt to show that feminist epistemology must face the *normative* issue of what makes some processes of belief-formation better than others. Antony rightly argued that feminist epistemology faces a "bias paradox": "Either endorse pure impartiality or give up criticizing bias" (2003, 102). Her way out was to distinguish between good bias and bad bias and to argue that ordinary naturalised epistemology is good at pointing out that all cognitive inquiries have presuppositions; hence they are biased in various ways. The issue, then, is not (the impossible task) to eliminate bias altogether but rather to "*treat the goodness or badness of particular biases as an empirical question*" (2003, 137). In her account "One important strategy for telling the difference between good and bad biases is thus to evaluate the overall theories in which the biases figure" (2003, 137). In a Quinean framework, this strategy is possible because in it values and facts are part and parcel of our theories of the world. I am sympathetic to Antony's challenge to feminist epistemology, though I endorse the perspective of standpoint epistemologies and I will try to address the issue of normativity in a different way.

is the so-called Precautionary Principle (PP).[22] PP is supposed to kick in when, even though there is scientific evidence for harm to health and/or the environment, the evidence is not yet conclusive. Here is how PP is typically stated (working definition)[23]:

> When human activities may lead to morally unacceptable harm that is scientifically plausible but uncertain, actions shall be taken to avoid or diminish that harm.
> Morally unacceptable harm refers to harm to humans or the environment that is
>
> - threatening to human life or health, or
> - serious and effectively irreversible, or
> - inequitable to present or future generations, or
> - imposed without adequate consideration of the human rights of those affected.
>
> The judgement of plausibility should be grounded in scientific analysis. Analysis should be ongoing so that chosen actions are subject to review. Uncertainty may apply to, but need not be limited to, causality or the bounds of the possible harm. Actions are interventions that are undertaken before harm occurs that seek to avoid or diminish the harm. Actions should be chosen that are proportional to the seriousness of the potential harm, with consideration of their positive and negative consequences, and with an assessment of the moral implications of both action and inaction. The choice of action should be the result of a participatory process.

There is considerable debate about this principle, which suggests a strategy to cope with possible risks where scientific evidence is strong but not yet conclusive.[24] Here, I want to focus on just on one aspect of this principle, viz., that it *embodies* social values. The very idea of 'morally unacceptable harm' to humans and to the environment captures a set of social values, the key element of which is that human life and environmental health (so to speak) are intrinsically valuable and should take precedent over other possible social values. What is important about PP is that it can be justified as a principle *only if* the very social values that are embodied in it take precedent over other social values (e.g. economic interests, profit etc). Pretty much as Newton's fourth rule above can be justified as a rule only if the epistemic values that are embodied in it take precedent oven other epistemic values.

There is an interesting case in which we can think of the possible application of PP—the case of mesothelioma, a fatal disease with a very long incubation time, which once it is

[22] Steel's (2015) is an impressive philosophical discussion of PP.
[23] This is taken from *The Precautionary Principle, World Commission on the Ethics of Scientific Knowledge and Technology*, UNESCO, 2005, p.14.
[24] For an overview, see Peter Saunders (2010).

manifested, it is normally fatal within one year.[25] It is now widely acknowledged by scientists that asbestos is the main cause of this disease. It is reported by health experts that some 250,000 – 400,000 deaths from mesothelioma, lung cancer, and asbestosis will occur over the next few decades in the EU countries only, as a consequence of exposure to asbestos in the past. The story is that though there was strong evidence which linked asbestos to lung cancer and other harmful effects, the fact that this evidence was not compelling "contributed to the long delay before action was taken and risk reduction regulation was put in place". The evidence of harmful effects of asbestos was there in the middle sixties but it was only in the late 1990s that EU banned all forms of asbestos. As is stated in the report on PP by the World Commission on the Ethics of Scientific Knowledge and Technology (p.11),

> A Dutch study has estimated that a ban in 1965, when the mesothelioma hypothesis was plausible but unproven, instead of in 1993 when the hazard of asbestos was widely acknowledged, would have saved the country some 34,000 victims and Euro 19 billion in building costs (clean up) and compensation costs.

This suggests to me that there can be evidence for a principle such as PP, that is evidence that speaks in favour of making it a generally accepted principle, even if social values are involved in it. So the choice of principles such as PP can be based on evidence. But I doubt that there can be (direct) evidence for the social values themselves (see also Goldenberg 2015). Their choice is not a matter of evidence; let alone of an instrumental justification. Their choice or adoption has to do with the way we conceive ourselves as human beings and the moral and social implications of our conceptions. Resistance to PP, I claim, is based, at least to a large extent, on a different set of social values, where, for instance, possible harm to the environment is traded off to economic growth and profit.

3.3 Standpoints and values

Let me finally address the key question I raised above: who is going to decide what are the right kind of social values and what not? Raising this kind of question implies that the required account of objectivity should be social in the way Longino (2002) has described it so that the social and moral values that are implicated in science can be made explicit and subjected to criticism. This is required for the process of "social value management" to be in principle possible. But though this is a necessary condition for creating a framework within

[25] I have based this on the facts presented in The Precautionary Principle, World Commission on the Ethics of Scientific Knowledge and Technology, UNESCO, 2005. The quotation is from p.11.

which the role of social values is raised and discussed, it might not be normative enough to allow judgements about the kind of values that ought to be implicated in, or excluded from, scientific research. What is required is what Janet Kourany (2010) has aptly called "socially responsible science" which encourages inclusion of social values that are conducive to human flourishing, promote equality and social justice and, generally, contribute to the making of a good society. Kourany is fully aware that this issue is deeply political. As she (2010, 106) puts it:

> According to the political approach (...) sound social values as well as sound epistemic values must control every aspect of the scientific research process, from the choice of research questions to the communication and application of results, this to be enforced by such political means as funding requirements on research.

This move towards politics highlights that the question of the right kinds of values is not, and cannot be, neutral. Social values depend on ideological, political and moral stances (explicitly or implicitly) and these stances are typically determined by social, political and class interests. The right kind of values, those which promote human flourishing, may well be perspectival values, that is values associated most typically with the interests of certain social groups. Still, there must be ways to show how otherwise perspectival social values can, in principle, become universal, that is values that could and should be adopted and guide the action of the society as a whole, or at any rate of social groups whose initial perspective (or interests) might have led them to adopt different values. I take it that this is a point advanced by feminist standpoint epistemologies and also by Marxist theories of social emancipation.

Standpoint epistemologies have aimed to achieve two things. One is to make a strong case for the claim all knowledge is socially situated and that some "objective social locations are better than others as starting points for knowledge projects" (Harding 1993, 56). Starting from these objective social locations (most typically the marginalised social groups and their lives) will generate various critical questions and projects that would lie hidden if we were to start from the perspective of socially dominant groups. The other thing, however, is to avoid relativism and ethnocentrism. That is, to avoid the claim that a certain social location is inherently superior over the others and at the same time not to fall for the claim that all social locations are equally good starting points. Harding's 'strong objectivity' has honoured both of these things by making the very standpoint from which knowledge is gained to be the subject of critical theoretical analysis and study. This is what Harding has called 'strong reflexivity'.

Advocates of standpoint epistemologies (notably Harding 1993) have contrasted their views to universalism. But they have taken universalism to require a value-free "transcendental standard for deciding between competing knowledge claims" (Harding 1993, 61) or to adhere to a view-from-nowhere (1993, 58), or to demand a value-free objectivity (1993, 73). I too think this kind of universalism is absurd. But it is not the only alternative.

To see that it is not, let me note that a key attraction of standpoint epistemologies (of the feminist standpoint in particular) is that the good epistemic practices that are unearthed by examining science from the standpoint of the lives of the marginalised groups are not good epistemic practices for the members of the group only (or from those who occupy the relevant standpoint) but for *everyone*. Harding (1993, 54) says:

> (T)he activities of those at the bottom of such social hierarchies can provide starting points for thought—for *everyone's* research and scholarship—from which humans' relations with each other and the natural world can become visible.

And later on she says that feminist standpoint theorists "want results of research that are not 'loyal to gender'— feminine or masculine (1993, 72). As she explains: "Standpoint approaches want to eliminate dominant group interests and values from the results of research as well as the interests and values of *successfully colonized* minorities—loyalty to femininity as well as masculinity is to be eliminated through feminist research (1993, 74). To eliminate 'loyalty-to-gender' values is not to endorse value-neutrality, as Harding rightly notes. But it is, I claim, to argue that some values are not universalisable; they cannot transcend the perspective from within which they arise. Conversely, the right values are those that can be shared; that they can be adopted (ideally) by everyone (as the first quotation by Harding in this paragraph suggests).

So, universalisability of values is my alternative. And this should not be confused with the kind of universalism that Harding argues against. To explain my point, I want to go back to the origins of the idea of a standpoint epistemology. As is well known, the very idea of a standpoint goes back to Karl Marx and to Georg Lukacs's (1923) appropriation of the Marxian idea of the 'standpoint of the proletariat'. When Marx famously called the proletariat the "universal class" he did not, obviously, mean that everyone is a proletarian. He meant that the interests of the proletariat (ultimately, human emancipation by the abolition of exploitation) were universal interests; that is interests that could become the interests of the society as a whole (and of other social groups and classes in particular). So the interests of a particular class can at the same time be(come) universal interests. As Marx put it in his 1844 *Economic and Philosophical Writings* "the emancipation of the workers contains universal human emancipation" (Marx 1975, 280). In emancipating itself, a universal class emancipates society as a whole. Self-interest becomes then universal interest.[26]

In the Preface to the 1883 German edition of the Communist Manifesto, Engels noted that the basic thought that "belongs solely and exclusively to Marx" was that "the exploited and oppressed class (the proletariat) can no longer emancipate itself from the class which exploits and oppresses it (the bourgeoisie), without at the same time forever freeing the whole of society from exploitation, oppression, class struggles" (Marx and Engels 2002, 197). This is precisely the sense in which the interests of the proletariat are universal interests: their satisfaction requires and entails the satisfaction of the interests of the society as a whole. My point here is not to defend the proletariat as the universal class; nor to prioritise the role of the proletariat vis-à-vis other oppressed groups. My point is *merely* that it is part of the original standpoint theory—the Marxian one—that the standpoint of a particular social group or class can become a universal standpoint, that is a standpoint which can and should be occupied by other classes or groups. The distinctive element of this approach, and the one I would like to stress, is that the standpoint of a class (or a social group) can be detached from the specific class- or group-interests that motivated and justified its occupation and to become the standpoint of universal human interests.[27] The universalisability of social values is, for all practical purposes at least, their objectivity. This is fully consistent with standpoint epistemologies, in the sense that the standpoint (and hence the values) of a certain socially identified group aims to become the universal standpoint from which society and its structure and values are viewed.[28]

4. The evidence debate in Canada

How can the perspective adopted above cast some light on the 'death of evidence' debate in Canada? Here are some facts, as reported in the press and various blogs. Scientists working for the Government are required to obtain permission by high-level civil servants to discuss

[26] In this reading of Marx I have been influenced by Llorrente (2013).
[27] That's an ideal, of course. In practice, it is enough that perspectival values become multi-perspectival, even though there are social groups that resist them.
[28] For some similar thoughts, see Railton (1984).

research findings with the media and the public. This has been described as the "muzzling of scientists". Some important research institutions have been eliminated or scaled down, thereby eliminating sources of data and scientific evidence, especially related to environmental and climate issues. The Omnibus Budget Bill C-38 (in June 2012) cut funding or dismantled a number of environmental bodies or bills.[29]

The evidence that the Harper Administration is at what has been described as 'war with science' is quite significant. It is so significant that the journal *Nature* dedicated two editorials to this topic in the space of four years. The first in 21 February 2008 was titled 'Science in retreat: Canada has been scientifically healthy. Not so its government". The second, in 19 July 2012, was titled 'The death of evidence'. The *New York Times* more recently (in September 21, 2013) had one of their own editorials devoted to this issue. Its title was "Silencing Scientists". More importantly, scientists themselves have taken action against the trend to silence evidence, as noted in the introduction, by rallying at the Parliament Hill in Ottawa in July 10 2012 and by marching in 17 cities around Canada on September 16 2013.[30]

What is at stake here? As the 2012 *Nature* editorial states:

> Instead of issuing a full-throated defence of its policies, and the thinking behind them, the government has resorted to a series of bland statements about its commitment to science and the commercialisation of research. Only occasionally does the mask slip – one moment of seeming frankness came on the floor of the House of Commons in May, when foreign-affairs minister John Baird defended the NRTEE's demise [National Round Table on the Environment an the Economy—an independent source of expert advice to the government on sustainable economic growth] by noting that its members 'have tabled more than ten reports encouraging a carbon tax'.

Indeed, it has been hard to find some kind of public defence of the Canadian government's policy. In a piece published in the March 2014 of *Canadian Government Executive*, Serge Dupont, the deputy minister of Natural Resources Canada, defended the policy that Government scientists are not "authorised to speak to the media or in public venues on any subject at any time" by noting that "The Communications Policy of the Government of Canada is clear that ministers are the principal spokespersons for the government and senior

[29] For a detailed account of the so called 'war on science', see Turner (2013) and Dupuis (2013). A more recent piece is Linnit http://www.huffingtonpost.ca/carol-linnitt/war-on-science-canada_b_5775054.html

[30] See 'The Death of Evidence' in Canada: Scientists' Own Words', TheTyee.Ca 16 July 2012. http://thetyee.ca/Opinion/2012/07/16/Death-of-Evidence/

management in each department is responsible for designating knowledgeable staff to speak in an official capacity on subjects which they have responsibility and expertise" (2014, 8). He added: "It is not the prerogative of public servants, scientists or others, to engage with the media without training and without proper authorisation". But how, one may wonder, is public interest best served? By filtering or massaging the information that scientific findings make available so that it may be tailored to the Government's interests before it is communicated to the public? Or by giving to the public access to these findings by letting scientists themselves disseminate this information and express their considered judgement about the impact of these research findings for issues relevant to the public (e.g., public health etc)? If the former strategy is followed, then it will be very hard to check the credibility of the research findings and the objectivity of the judgements concerning the possible impact of the policies politicians pursue. If the latter strategy is followed, the public (including other scientists, of course) can be in a better position to know the possible impacts and to evaluate and challenge the various policies.

One of the rather rare defences of the Harper Administration policy came by Philip Cross, former chief economic analyst at Statistics Canada, in a piece that appeared in *Financial Post* (October 21 2013). Cross denies that there is a war on science (without of course denying the facts noted above). His arguments, briefly put, are the following. First, all this fuss about the war on science is done by left-wing scientists and activists. Second, science relates to economic growth and the impediments to economic growth (such as "the science underpinning environmental regulation") should be reduced or eliminated. Third, research should be directed to more commercial ends (and the Harper administration wants to do this). Fourth, government scientists are government employees and hence, the government, like any other private business, has "the right to control what is communicated to the media". Corollary to the fourth argument: if government scientists want 'academic freedom' they should apply for jobs in the academia; but most do not have "the credentials to do so". Fifth, as the journal *Economist* (October 19 2013) has recently stressed, there is lots of shoddy research in science, with results that cannot be replicated or are disproved.[31]

Though more could be said in reply to this battery of arguments, the following seems sufficient. The first argument is simply ad hominem. The second is wrong-headed. Science *does* relate to economic growth, but the latter should not be unregulated; nor of course, should science be subject to the market forces. The third argument relies on the principle

[31] The journal *Economist* titled its Leader: *Problems with Scientific Research: How Science goes Wrong*. The verdict, briefly put, is that "modern scientists are doing too much trusting and not enough verifying—to the detriment of the whole of science, and of humanity".

that those who fund the research should decide what the research should be about. Even if this were correct for a private institution (which is not), it is far from correct for a public institution such as the government, where issues such as the public welfare and the public interest should be prominent. The fourth argument is a variation on the third. The reply is simply that governments of democratic societies should not be like executive boards of private firms. Finally, the fifth argument is, at best, exaggerated. Even if the article by *Economist* were onto something, it is clear that scientists themselves have the tools to make research more error-proof and more reliable. The reproducibility of experimental findings is clearly an important desideratum in science. But given the fact that experiments become all the more complex and delicate, reproducibility is not always achievable. What matters most is not the reproducibility itself but the strength by means of which the evidence supports the theory. The CERN experiments in high-energy physics are hardly replicable. Is *this* a reason to distrust them?

What's important for our purposes, I think, is the conception of the value of science tacitly implied by arguments such as the above, viz., that science should be subordinate to various social, political and economic interests, including the government and its economic and political agenda. It's not far from this that when there is a conflict between science and the dominant social values, or those that are taken to be the dominant social values, it should be science that has to yield. This is an ideological conception of science and its value; and it is not new. What seems to be new in Canada is the way this conception of science is effected, viz., by *curbing* the sources of data and evidence on which science thrives.

Note that the argument from underdetermination we have been discussing lends no credence to any kind of policy or value of curbing evidence. We have already stressed that though evidence does not speak with the voice of an angel, it can decisively turn the balance in favour of one theory over its rivals. Evidence is clearly necessary for doing science and doing it right. And even if evidence is not sufficient, even if, that is, scientific judgement, being non-algorithmic, involves more than evidential considerations, various kinds of epistemic values can and do play a decisive role in determining theory-choice. As I noted in section 3.1, the need to appeal to social values in solving problems of evidential ties is not as rampant as it has been supposed. Far from being supported by the argument from underdetermination, the challenges to underdetermination noted in section 2 suggest that curbing sources of evidence is detrimental to theory-appraisal and choice. Precisely because there can be evidential support to a theory from what is not among its observational consequences, and precisely because there can be evidential support to a theory by hitherto unforeseen evidence (made available when the theory is conjoined with future auxiliary assumptions), the cost of curbing or stifling evidence cannot be anticipated because we cannot predict which theories, and to what extent, will be supported by fresh evidence. In fact, curbing evidence amounts to a sure strategy for cutting off the roots of science and theory-appraisal. It is also worth noting that curbing evidence, as followed by the Harper government policies in Canada, inevitably hinders innovation precisely because of this unpredictable aspect of theory-evidence relations.[32]

Curbing the sources of evidence is a social value. Given what I noted above, it is not the right value since it is *not* universalisable. It expresses the interests of only those who may stand to lose from an unfettered scientific inquiry and its finding. But *valuing evidence* is a social value too. What makes it the right social value is that it is conducive to socially responsible science. It is not, of course, just that. Importantly, evidence is conducive to epistemically responsible science. But though this goes without saying, what matters for our present purposes is that evidence can cast light on important social issues by unravelling their causes and by dispelling various ideological assumptions or prejudices. The precautionary principle we discussed above is a case in point. And though valuing evidence might well be a perspectival value, it is a universalisable value. Barring those whose interests are in suppressing sources of evidence, looking for evidence and subjecting beliefs and theories to evidential scrutiny are values that are conducive to human flourishing.

In the current debate about the death of evidence in Canada, we see in action proof of the claim that though science is not free of social values, it matters a lot *what* these values are and *whose* values they are. What ultimately is at stake is the *value of evidence* in science and in public life. Evidence should always be wanted: alive or dead!

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[32] I owe this point to an attentive reader.

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