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The Undecidable Grounds of Scientific Expertise: Science Education and the Limits of Intellectual Independence

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Motivated by the work of Hardwig (1985, 1991) on epistemic dependence and trust in expertise, we enquire into the nature and extent of the critical assessment that non-scientists can make—and that they should be taught to make—with regard to science. Our thesis is that critical assessment of science is possible for non-experts because at the basis of science is a set of norms, beliefs and values that are contestable by non-scientists. These norms, beliefs and values are of critical importance to science education and valuable to explore from a pedagogical perspective.

What is the nature and extent of the critical assessment that non-scientists can make—and that they should, as a science education goal, be taught to make—with regard to science? The answer to this question has broad significance for the relationship between non-experts and experts. Given the status of science as a body of expert knowledge, and given the limited grasp of scientific knowledge held by most non-scientists (National Science Board, 1998, pp. 7–16; Select Committee on Science and Technology, 2000, p. 54), science provides a context for making more perspicuously than in other contexts many points about the non-expert's capacity for critical assessment of expert bodies of knowledge. The access by non-scientists to the critical assessment of science provides a rough estimate of the minimum access non-experts can gain to the critical assessment of other areas of expertise.

Our thesis is that critical assessment of science is possible for non-experts because the intellectual independence that is attributed to scientific experts is an ideal in which one necessarily trusts, but not an actual feature of scientific expertise. Scientific expertise is, rather, a communal product. Moreover, this communalism is not simply a contingent feature of science that might be remedied by experts' increased efforts to achieve greater rationality in their scientific practice. On the contrary, it is a necessary requirement for scientific expertise to exist. For this reason, normative assumptions are an inextricable feature

of scientific claims, and these assumptions are vulnerable to critique by experts and non-experts alike.

The argument proceeds in three steps. In the first, we argue that, in order for scientific knowledge to qualify as properly rational, an ideal of intellectual independence must be posited by the scientific experts who possess that knowledge. On the basis of this posit, scientific expertise is taken to be a generally trustworthy source of rationally justified belief. In the second step, we show that a logical feature of this generalised trustworthiness is that it is impossible to substantiate. Insofar as the trustworthiness of expertise is presupposed by scientific practice, and insofar as trustworthiness in general is unjustifiable, scientists' dependence on one another's expertise requires the importation of a further set of norms, beliefs and values that are derived from the epistemic community at large, and that do not fall exclusively within the purview of science. In the third step, we show that it is these norms, beliefs and values that are contestable and open to critique by non-scientists, and thus are of critical importance to science education and valuable to explore from a pedagogical perspective.

In the first section, we extend the analysis developed by Hardwig (1985, 1991) of epistemic dependence of non-scientists upon scientists, and of epistemic communalism as a structural feature of the production of scientific knowledge itself. On our view, the 'Hardwig limit' of intellectual independence, the point beyond which we must trust, signals an epistemological instability that is inherent in scientific knowledge itself. This limit is the undecidable ground that provides the non-scientist scope for critique. The second section shows specifically how this instability can be mobilised critically, even by those who lack specialised scientific expertise, through a focus on the cultural, moral, social, political and prudential normative assumptions that are necessary to underwrite science. The conclusion of our analysis is that a theoretical account of the undecidable grounds of scientific expertise lays the foundation for a theory of science education that explains how science can be accessible to critique by non-scientists, and shows how science can be taught better.

THE HARDWIG LIMIT: A DESCRIPTION OF HARDWIG'S POSITION

The issue Hardwig (1985) sets out to explore is the rational status of scientific beliefs when these beliefs cannot be appraised independently on the evidence:

> The list of things I believe, though I have no evidence for the truth of them, is, if not infinite, virtually endless. And I am finite. Though I can readily imagine what I would have to do to obtain the evidence that would support any one of my beliefs, I cannot imagine being able to do this for *all* of my beliefs. I believe too much; there is too much relevant evidence (much of it available only after extensive, specialized training); intellect is too small and life too short. (1985, p. 335)

Based on this reflection, Hardwig urges that '*rationality* sometimes consists in deferring to epistemic authority' (1985, p. 343). He offers a number of arguments in support of this thesis. First, not only does the pursuit of various kinds of knowledge require an aptitude or talent that not everyone possesses, but also specialised disciplines such as physics, mathematics or social scientific research demand extensive training, a high level of competence and in some cases expensive research facilities. Given the epistemic demands produced by a complex culture, the independent justification of most of our beliefs is simply not tenable. Thus, unless we are willing to conclude that, 'the more that is known in a culture, the less rational the beliefs of individuals in that culture' (1985, p. 339), Hardwig concludes that we must agree that belief based on the authority of someone who does have the relevant expertise, competence and specialisation in question is justified rationally.

Second, Hardwig suggests that the model of the rational person as one who always thinks for him or herself independently of others is an unrealistic, romantic ideal. This model does not represent adequately the reality of our epistemological situation, which is that dependence on others for evidential support of our beliefs is unavoidable. In the first place, 'the fact that I believe more than I can become fully informed about' means that, 'I can never avoid some epistemic dependence on experts' (1985, p. 340). Indeed, 'if I were to pursue epistemic autonomy across the board, I would succeed only in holding relatively uniformed, unreliable, crude, untested, and therefore *irrational* beliefs' (1985, p. 340). In the second place, any independence in judgements the layperson can have regarding the expert or the evidence in question must be drawn from, and ultimately deferred to, expert appraisal. For example, I may happen to know that an expert in physics is biased, insufficiently informed of a recent development in the field or potentially incompetent in comparison with another expert (e.g. Adler, 1994; Siegel, 1988). I even may be able to raise a devastating objection to a theory about quantum mechanics. However, I must still admit that my judgement is inferior to that of a physicist, and must therefore rely on yet other physicists to judge whether my judgements about a given physicist or my objections to a claim about quantum mechanics are indeed relevant or persuasive (1985, pp. 341–343). Once again, epistemic dependence is unavoidable if one is behaving rationally, and it would be *irrational* and epistemologically unrealistic to insist on making up one's own mind in many situations.

Third, Hardwig argues that there can be no such thing as an intellectually independent knower. He asserts that this is so because, 'the expert-layman relationship is essential to the scientific and scholarly pursuit of knowledge' (1985, p. 336), because no matter what level of expertise a person holds, no person is able to ground all of his or her beliefs in independently known evidence. In a fashion parallel to the reliance of non-experts upon those more knowledgeable than themselves, even experts must rely upon one another if they are to produce any knowledge at all. Neither knowledge nor rationality can be said to

stem in the *first* instance from intellectual independence, and there is no ultimate expert to whom anyone can appeal.

The fact of epistemic dependence raises a number of important challenges to the concept of rationality. In response, Hardwig considers three alternatives. The first response is to adhere to a characterisation of rationality that demands independent judgement. The second response is to abandon the ideal of intellectual independence, and to agree that knowledge is something that can be had only vicariously—'i.e., without possessing the evidence for the truth of what [one] knows, perhaps without even fully understanding what [one] knows' (1985, p. 348). The third response is to deny that knowledge can be had vicariously, and to conclude from the reality of epistemic *inter*dependence that knowledge is the property of communities rather than of individuals (1985, p. 349).

Each of these responses comes at a cost. First, to insist in the face of the intrinsically co-operative nature of scientific and humanistic research that intellectual independence must be realised if knowledge is to qualify as rationally grounded, is to insist that things we think we know rationally—for example, that the moon orbits the earth, that there is a vaccine for polio, that water freezes at zero degrees Celsius and that the nature of light can be described both as a particle and as a wave—are not things we actually know, properly speaking, but merely things that we accept. In short, we would commit ourselves to the position that knowledge as we understand it simply cannot be produced through scientific inquiry. Second, if the view of communalism in science is elaborated and extended such that it becomes impossible to determine anything independently of the beliefs, arguments and experimental research of others, then dramatic changes would be required to our view of knowledge. Sacrificed would be the idea that '*knowing* a proposition requires understanding the proposition and possessing the relevant evidence for its truth' (1985, p. 349). We would continue to see ourselves as knowing creatures, but our view of knowledge would be altered radically. If this conclusion seems unacceptable, the third option carries a sacrifice that may be unpalatable as well. In particular, to give up the ideal of intellectual independence as intrinsic to rationality, because knowledge is necessarily produced and possessed communally, means that no person knows anything rationally, because rationality and knowledge are by definition not properties of individuals but rather properties of communities.

ANALYSIS AND CRITIQUE OF HARDWIG'S POSITION

Hardwig's analysis suggests that, due to the limitations inherent in being a single, finite individual, and due to the level of specialised training required to appraise evidence (particularly of the technical and scientific sort), so-called rational beliefs are often second-hand constructs, based on someone else's expertise. This situation presents a philosophical problem, because one of the classical features of the concept of rationality is the demand to think for oneself, a view that translates into

intellectual independence being a widely valued educational aim among both philosophers and practitioners of science education (Kitchener, 1992; Munby, 1980). According to the classical view, second-hand beliefs are not strictly rational, because they are not gained independently of the beliefs or knowledge of others, and thus cannot be justified absolutely—that is to say, justified with reference only to one's own sphere of knowledge, expertise and judgement. The implication of Hardwig's argument that intellectual independence obtains only rarely, therefore, would seem to be either that the majority of our beliefs must be deemed irrational or nonrational, or that there are other good reasons for believing propositions—'reasons which do not reduce to having evidence for the truth of those propositions' (1985, p. 335)—that might qualify as rational nonetheless. In particular, Hardwig wonders, 'can knowledge, as well as rational belief, be based on an appeal to epistemic authority?' (1985, p. 344). If so, what is to be made of the venerable educational goal of intellectual independence?

These concerns and conclusions from Hardwig's analysis are sound to a point—generally speaking, it is not rational to base a belief on lesser knowledge or comprehension when a more competent appraisal is available. However, this claim is particularly interesting in the context of Hardwig's thesis as a whole, because the claim presupposes the very distinction between expertise and non-expertise that Hardwig undercuts in other parts of his analysis. The argument that scientific expertise must be borrowed second-hand, because it is beyond the purview of most laypersons in any complex culture and because it is ultimately up to the experts to judge the relevance of appraisals made by non-experts, presupposes that the rationality of epistemic dependence hinges on there being someone who *does* have the relevant expertise and who *is* epistemically independent in the relevant way. According to this part of Hardwig's argument, beliefs are said to be rational if they are based on the say-so of someone *else*, who has engaged in an independent appraisal of the evidence. This understanding of rationality—as that which is at one or more steps removed from intellectual independence but not logically divorced from it—is the very basis of Hardwig's claim that epistemic dependence is rationally justified. Hardwig's defence of the validity of trusting the knowledge of others is precisely that it is irrational not to defer to those who are expert—that a good reason for a wide variety of claims is the say-so of those individuals who do have knowledge, and who do so in the specific sense of individually possessing and understanding the evidence supporting it. Furthermore, Siegel rebutted Hardwig's claim that 'it is sometimes *irrational* to think for oneself—that *rationality* sometimes consists in deferring to epistemic authority and, consequently, in passively and uncritically accepting what we are given to believe' (Hardwig, 1985, p. 343)—by arguing as follows: '... recognizing my epistemic inferiority requires my intellectual autonomy and epistemic individualism. Without these, deference to expert opinion cannot be *rational* deference' (1988, p. 4). As Siegel insightfully shows, the very claim that epistemic dependency is the result of a

rational choice presupposes, rather than undermines, epistemic independence.

Significantly, therefore, it does not matter how one comes down on Hardwig's thesis that epistemic dependence gives rise to rational beliefs. Intellectual independence still figures centrally within it: the layperson's trust in the expert can be determined either as rational or as irrational only if the (ideal) possibility of independence is allowed **either** for the expert **or** for the layperson respectively. In other parts of his analysis, however, Hardwig shows that experts must depend on yet other experts in order to obtain knowledge in the first place, and in order to continue the advancement of knowledge: 'the expert is an expert partially because he so often takes the role of the layman *within his own field*' (1985, p. 346). Thus, notwithstanding the logical necessity that intellectual independence is implicated *in principle* in the question of the rationality of epistemic dependence, *in fact* there is according to Hardwig no final court of appeal, no ultimately independent epistemological agent, to which to submit for appraisal the trustworthiness of expertise underlying knowledge. Hardwig's exposition of communalism in science thus presupposes an ideal of epistemic independence at the very base of scientific knowledge. It does so because it must, despite the fact that there is no way to substantiate scientifically the truth of this presupposition. In other words, to accept with Hardwig the reality of communalism in science is to acknowledge that a trust in the validity of expertise is a condition for the constitution of expert knowledge, rather than the layperson's alternative to intellectual independence. The implication of his argument is that an ultimately unjustifiable ideal is assumed in the service of the knowledge that scientists possess. On this view, trust is an *a priori* condition *for* scientific knowledge rather than its replacement.

Hardwig himself does not develop this last claim. However, we suggest that it can be elaborated as follows. First, the rationality of any particular scientific belief hinges on an appeal to epistemological independence that must be made in advance by scientists. For what qualifies scientific knowledge as rationally grounded is just the supposition that the evidence adduced in support of it can be appraised independently. To say that we have knowledge of a scientific sort is to say that scientific beliefs are grounded on the competence, training, specialisation and facilities that enable judgements of evidence to be made by scientists independently of others. Undergirding scientific knowledge is an ineluctable supposition that, at least ideally and in general, there is something that qualifies as intellectual independence, something like a capacity for an independent appraisal of evidence, something like the trustworthiness of expertise.

In all of this lies a paradox. On the one hand, the necessity of referring to others' expertise for the production of new knowledge and, by extension, the necessity of assuming that scientific expertise in general is trustworthy, is precisely what the communalism of scientific research entails. We gain what knowledge we have, Hardwig showed, by

beginning with the knowledge of others. It follows that a trust in general in the possibility of independence is necessarily prior to every body of knowledge, and so is prior to anyone's expertise in particular. The trustworthiness of expertise in general is the opening to science, in the sense that anyone who desires scientific expertise must at the outset accept the assumption that epistemic independence obtains in general. If the ideal of independence *were* reducible to the independent knowledge of any one expert, new and unanticipated knowledge could not be recognised as such, because, being new, it would transcend the sphere of knowledgability of any given expert. For this reason, that expertise in general is trustworthy is the logical condition of possibility, not just for what knowledge there is, but also for what knowledge is still to come.

On the other hand, the trustworthiness of scientific expertise *in general* must exceed infinitely the scope of any one scientist's particular finite sphere of knowledge. If it did not, and the trustworthiness of expertise in general were rationally justifiable in reality, then it would have to be referable to the finite body of knowledge held by a given expert. However, no expert's knowledge, no matter how broad, can be adequate to this task. The ideal of the trustworthiness of expertise in general must precede and exceed any existing body of knowledge, since the ideal must have been taken for granted to produce the expertise, and because the ideal trustworthiness of expertise in general must be assumed to produce scientific knowledge in the future. Thus, the condition of the possibility for expertise in general is just the impossibility of its independent appraisal.

If the above suggests that intellectual independence is impossible, however, it is not *simply* impossible. It must be remembered that the assumption that an independent appraisal is possible ideally is precisely what *must* be maintained in the name of science. Communalism in science means that science begins with, and continues from, the supposition that the knowledge, arguments, and experimental research of others is the legitimate starting point for research in a field. In order that a body of knowledge may become recognisable as expert, independence must have been postulated as a justifiable ideal. The condition of possibility for expertise in general is just the assumption that reasons for belief *can* be independently appraised and so rationally justified by experts, namely, those with the competence, training, time and facilities to do so by themselves. If we conclude that this is not possible in general, we are effectively supposing that there can be no such thing as expert knowledge at all. To just this extent, the independent appraisal of scientific evidence in general is, and must be considered, possible.

The upshot is that the rational status *in general* of scientific knowledge possessed by experts is strictly undecidable. Significantly, this does not mean that the judge of such expertise is merely likely to be uncertain. The term 'undecidable' should be taken here in its technical sense—the rational status of expertise in general is itself neither formally provable nor formally disprovable or, alternatively, its status is both provable and

disprovable (Nagel and Newman 1959, p. 86). In other words, in order to qualify as the substance of legitimate expertise, the evidence put forward by experts would have to be appraisable by someone with the requisite knowledge to do so independently. The moment expert belief in general is referred to the finite body of knowledge possessed by somebody in particular, however, it loses its infinite scope. On the other hand, if one accepts that expertise in general is such that it cannot be rationally grounded on the capacity for independent appraisal of any one expert, then one will have undermined the possibility of establishing expertise in the particular sense of possessing a finite body of rationally justified belief. Reasons for beliefs are said to be rational and thus to comprise genuine knowledge, precisely to the extent that they *can* be appraised independently. Paradoxically, therefore, scientific knowledge is premised on both the possibility and the impossibility (or, on neither the possibility nor the impossibility) that expertise obtains. Or, to phrase this differently, the paradox of the trustworthiness of scientific expertise in general is that *its condition of possibility is its own impossibility*. The trustworthiness of scientific expertise in general exceeds the choice between a) grounded on the independent appraisal of someone with the relevant expertise (rationally justified); and b) not grounded on the independent appraisal of someone with the relevant expertise (not rationally justified).

IMPLICATIONS FOR NON-EXPERT APPRAISAL

In the previous section we showed that to ask whether there can be reasons that qualify as rationally justified for scientists' dependence upon each other's expertise, is already to assume that individuals *can* have knowledge in the specific sense of individually possessing and understanding the evidence supporting it. It is already to mobilise the idea of independence, *as though* this criterion of expertise is a finite feature of one particular scientist's knowledgeability. By virtue of the presence of this *a priori* assumption, new inductively grounded scientific research can be produced. Thus, the presence in science of this idealising assumption concerning the trustworthiness of expertise in general is what first opens the field of scientific determination.

However, this assumption cannot be definitively established, scientifically or otherwise. For, any attempt to justify the condition of possibility for scientific validation—i.e. the ideal that expertise is trustworthy in general—reveals an infinite chain of epistemic authority to which scientists must refer. Our hypothetical submission of the trust in intellectual independence in general itself to independent scientific appraisal exposed a logical impossibility at the ground of scientific proof. It thereby is clear that what first allows for scientific proof is an unprovable, idealising assumption that evidence supporting beliefs can be evaluated independently—that absolute independence, ultimately, can be obtained.

Once the trustworthiness of experts' knowledge of science is seen to derive from an undecidable ideal of intellectual independence, it is possible to show that scientific claims are susceptible to a particular kind of critique that can be undertaken by non-scientists. Specifically, the implication that this ideal is the undecidable opening of scientific knowledge *in general* is that substantive scientific claims will bear an internal trace of science's infinite structural opening in the form of *particular* undecidable characteristics that the non-scientist (as well as the scientist) can interrogate. The epistemic purchase of the concept of undecidability with regard to science, in short, is that it allows us to see that scientific claims are established objectively as true or false only by virtue of a constitutively impossible ideal of intellectual independence—an ideal that itself ensures that scientists must depend upon one another's expertise and that they will do so with reference to various social and cultural norms. An awareness of the structural opening at the foundation of science, in short, leads to the recognition that various assumptions will inhere in any given scientific claim.

With a view to highlighting the social and political dimensions of science, for example, Webb (1993) has argued that Hardwig's (1985, 1991) insights should radicalised. According to Webb, trust in testimony is not only necessary to gain knowledge in fields in which we are not expert. Much more fundamentally, trust in testimony must be given in advance as a self-evident but unprovable principle that is 'necessary for reasoning and the conduct of life' (1993, p. 268). Webb sets against the position proffered by Siegel (1988) and Adler (1994) that one can gauge independently the reliability of testimonial evidence, and can retain one's autonomy accordingly, the argument that we often do not reason at all about testimony-based beliefs and that, if we did, our inductions would be based on evidence that is insufficient to warrant them. As Webb says, the demand that testimony be scrutinised to the bottom of its reliability 'places too great a burden on the believing subject, since it requires of him all kinds of knowledge about people, their areas of expertise, and their psychological propensities, which knowledge most subjects simply lack' (1993, p. 263), and, we would add, cannot acquire. Webb therefore maintains that it must be accepted as a self-evident first principle that there is a certain deference to testimony regarding matters of fact: 'We have this natural propensity to believe others, and this propensity makes it possible for us to acquire all kinds of knowledge that would otherwise be unavailable to us. That this principle is a first principle is sufficient to justify many particular cases of belief based on testimony' (1993, p. 268). On Webb's view, moreover, there is no ground to reject this principle that is not also a ground to reject the principles that inform deductive reasoning, because all deductive reasoning begins from accepted first principles. Webb's argument does not speak to the rationality of believing on the basis of testimony; it speaks to the claim that no such argument is necessary (pp. 268–269).

The thrust of Webb's argument is that the reliability of testimony on matters of fact is neither provable nor disprovable; it is simply what

must be accepted as among the 'grounds of reasoning' for the daily conduct of life (p. 268), and it pertains to scientists and to non-scientists alike. For just this reason, Webb concludes, science is understood rightly as an essentially social project. Once it is agreed that science is inherently social, in the sense that scientists often depend on one another's testimony without undertaking to justify it scientifically, it must be acknowledged that 'certain kinds of nonepistemic forces (like power relations and differences in social status) [are] able to play a role in theory formation. This in turn makes political critiques of some scientific projects pertinent' (1993, p. 270), critiques that expose, for example, the presence of sexism and racism in scientific research.

Code (1987) makes a similar point, although her target is specifically the moral, rather than the socio-political, dimensions of epistemic claims. Like Hardwig, Code believes that 'knowledge in any significant sense of the word, together with the lies and deceptions parasitic upon it, *presupposes* community,' (p. 178) because, '[h]uman beings are cognitively interdependent in a fundamental sense, and knowledge is, essentially, a *commonable* commodity. (p. 167). As she explains:

> scientists themselves must rely heavily for their facts upon the authority they acknowledge in their fellow scientists. They use the results of sciences other than their own and of other scientists in different areas of their own field, results they may feel neither called upon *nor* competent to test for themselves. Already the picture of a complex network of interdependence is becoming clearer. For this interdependence to be workable, there must be a tacit basis of trust and trustworthiness similar to what I have described as a sort of epistemic contract. (p. 230)

In particular, she says that knowledge cannot be produced outside of an epistemic community that is quasi-contractually bound by the moral value of epistemic responsibility. According to Code, the essence of this idea is captured in a statement by C. I. Lewis, and cited by Code: 'Concern for consistency in supposition and belief, for validity in inference, and for cogent determination of beliefs according to the weight of the evidence is not avoidable for the animal that thinks deliberately' (Code, 1987, p. 94). Similarly, Code writes, 'to strive for insight into the extent of one's own cognitive capacities, to distance oneself as much as possible so one can be critical of one's own knowing, is a crucially important aspect of epistemic competence' (p. 176). On Code's view, in short, a specific set of extra-scientific values underwrites morally responsible epistemic conduct, and these values are essential to the flourishing of scientific practice (pp. 230–231). Code shares with Webb the view that certain epistemic values are integral to the communal form of life through which humans are able to continue to exist *as* human beings (pp. 178–180). Code thus concurs with Webb that certain moral and epistemic givens are necessary for the conduct of life.

Significantly, however, Code points out that the very communalism underlying science—characterised as it is by tacitly shared understandings

of morally responsible epistemic behaviour—means that scientific practice may well be unduly constrained from within. Science may be dogmatically, rather than legitimately, restricted. The implications of this gatekeeping are profound. It means that those who set scientific standards do so not only for its acceptability, but also for its *plausibility*, 'indeed, one might well argue, for *truth*' (p. 232). Experts must therefore be trustworthy not only with regard to their competence, but also with regard to their ability and willingness to fulfil a specific *moral* obligation, namely, that of maintaining a 'policy of circumspection' (Code, 1987, p. 175) or 'reflective scepticism' (McPeck, 1981, p. 8), such that they are on guard against imprudent dismissal of research that is merely unfamiliar in its substance or its method. If experts are the ultimate arbiters of scientific truth, and if the legitimacy of their decisions is supported in part with reference to a set of moral values and beliefs that are neither fully explicit, consistently achieved, nor universally shared (values which include, for example, consistency in suppositions, open-mindedness, rigor in the appraisal of evidence and a capacity for self-reflexivity and critique), then the trustworthiness of scientific expertise is sustained with reference not only to the social and political assumptions that Webb identifies, but also with reference to distinctly moral beliefs as well. These moral beliefs are subject to interrogation and critique by non-scientists.

From another perspective, Driver (1989) has drawn attention to the fact that what counts as scientifically true depends to some extent on the knowledge scheme through which the claim is viewed, and such frames are social conventions. As she writes:

> [The] social dimension to the construction of scientific knowledge has resulted in the scientific community sharing a view of the world involving concepts, models, conventions and procedures. This world is inhabited by entities such as atoms, electrons, ions, fields and fluxes, genes and chromosomes; it is helpfully organized by ideas such as evolution and procedures of measurement and experimentation. These ideas, which are constructed and transmitted through the culture and social institutions of science, will not be discovered by individuals through their own empirical enquiry; learning science involves being initiated into the culture of science'. (1989, p. 85)

According to Driver, there is thus something that must be learned *first*—namely, the cultural givens and disciplinary norms of scientific fields—before principled grounds for the appraisal of science can be taught.

In specifying further just the sorts of socially constituted givens to which scientists refer, Norris (1992) argues that scientific research is characterised by a series of decisions based on communal values. For example, scientists engage in practical reasoning whenever they make an initial determination as to which conditions will qualify as standard and which will qualify as anomalous in experiments, as well as when they are confronted with uncertainty in the process of testing their theories

against data. In the face of uncertainty, scientists ultimately judge which theoretical hypothesis is more compelling, based not only upon relevant data but also upon other reasons, such as communally given criteria of beauty and simplicity (p. 206). Scientists also engage in practical reasoning each time they simplify their initial assumptions for the sake of 'a neat, but approximate, solution to an otherwise intractable problem' such as determining a solution to the equation that represents the motion of a pendulum (p. 208), and whenever they make prudential judgements about which observations can be inferred on the basis of pre-existing beliefs, and which need to be justified by additional theoretical hypotheses (pp. 209–212). 'So the testing of theory against observation is mediated by scientists' practical reasoning judgements of what is to be taken for granted in the context, and what ideas are most valuable to preserve' (Norris, 1992, p. 213).

These analyses specify the nature of the communally given norms that, taken together, first open the possibility of scientific knowledge and support the general legitimacy of a meta-scientific assumption about what constitutes expert scientific knowledge in its actual sense. Webb, Code, Driver and Norris thus give content to the way in which the unrealisable ideal of intellectual independence exceeds the finite sphere of knowledge, and the finite capacity for independent appraisal, of any one expert. In particular, these analyses show that scientists regularly make normative judgements based on practical reason—judgements that are deemed to be communally acceptable rather than scientifically justified—and that they regularly mobilise communal agreements about what constitutes a scientific observation. Furthermore, if testimony on matters of fact provides an *a priori* ground of reasoning that is necessary for the daily conduct of life, there arises the inherent possibility that certain kinds of non-epistemic forces (like power relations and differences in social status) are able to play a role in theory (Webb, 1993). Scientific knowledge is also produced by virtue of scientists' participation in an epistemic community distinguished by a trusting form of life (Code, 1987), by reason of their initiation into scientific culture (Driver, 1989) and through their judicious use of reflective scepticism based on their learned understanding of discipline-specific concepts and norms (McPeck, 1981).

Together, such communally given background assumptions produce a normative sense of what science actually is, and what scientific expertise actually entails. This sense forms the horizon towards which scientific efforts are aimed, and it provides the basis on which scientific practice potentially can take place. The postulation of this socially, politically and morally imbued ideal is the undecidable basis on which a finite body of evidence comes into view. The implication is that cultural, moral, social, political and prudential questions should not simply be added to epistemological questions as supplementary criteria of appraisal *after* a scientific belief has been established and communicated. On the contrary, the significance of the facts that an idealised conception of intellectual independence is present *a priori*, is internal to science and is also undecidable, is that something unjustifiable is integral to scientific

proof as such. The substance of scientific claims therefore bears close scrutiny, on cultural, moral, social, political and prudential grounds. Notwithstanding the unavoidability of assuming in principle that the ideal of intellectual independence in science can obtain, we have seen that, in fact, the trustworthiness of evidence can never be deemed fully rational in the sense that the criterion of the 'preservation of and adherence to one's own independent judgment' has been met (Hardwig, 1985, p. 340). Indeed, insofar as scientists always already have deferred to the research, knowledge and ideal capacity for independent critique of others—insofar as they have mobilised the infinite ideal of expertise in general—scientific knowledge is communally rather than independently produced. Moreover, the structurally open ideal of expertise that scientists trust is composed of a variety of tacit cultural, moral, social and political norms that are communally shared. For just these reasons, the undecidable ideal of expertise is, paradoxically, what can *undo* the rationally justified belief in specific scientific claims. It is to this potential for critique that we now turn.

Once an infinite set of cultural, moral, social, political and prudential determinants are revealed at the ground of science, it becomes clear that the debates which ensued in response to Hardwig's original article—debates about whether epistemic trust is rational or irrational in nature—beg the most interesting question that Hardwig's demonstration of communalism in science provokes: What is the status of the ideal of expertise *in general*, if trust in a particular expert is to be a rational determination? To this, our response can only be that 'such a question cannot expect a determined response'. It only indicates the beginnings of an infinite chain of deferral (Derrida, 1989, p. 56). The most significant corollary of the argument that science is a communal practice is that it allows us to see that what unifies the field of science, and what gives rise to the finite body of rationally grounded scientific knowledge that science educators aim to instil, is an *undecidable* assumption concerning the ideal trustworthiness of expertise.

This means that the trust the layperson places in experts is no different in kind from the trust experts themselves place in expertise. In claiming that a belief is rationally grounded, both non-scientists and scientists mobilise the ideal assumption that the chain of epistemological authority has an ultimate end that is scientifically justified. Both groups appeal, though at varying levels of knowledgeability, to a chain of authority that is in principle endless, and that embodies a host of meta-scientific assumptions that are vulnerable to critique.

Of course, Hardwig's analysis does not blur the distinction between experts and non-experts. There will always be those who understand the content of scientific knowledge better than others. What it does show is that scientific knowledge is *essentially* incomplete, and that scientific claims therefore embody an epistemological lever that can be prised by those who do not have a firm grasp of scientific content. Although only an expert can appraise the methodological validity of evidential claims, the non-expert can, and indeed should, always ask such questions as:

Does this scientific belief embody or support any particular social hierarchies such as those based on race, on gender, or on class? If so, what normative assumptions are being made? Have these norms been thematised and justified scientifically, or are they simply assumed? Have alternate accounts of the same phenomenon been developed? By whom? What were the grounds for choosing one account over another? Are these grounds themselves free of normative assumptions; are they as certain as they appear? Who decided?

The benefit of asking such questions about the status of knowledge received from experts, questions concerning whom to trust and why, is twofold. First, such questions can bring to light assumptions concerning the nature and relevance of differences in social status that are merely implicit in scientific claims, and that may provide an illegitimate support for the research. They are thus essential questions if one is to engage critically with science, and they do not require any particular *scientific* competence, specialisation or expertise. Second, the process of interrogating science in this way also provides a fuller understanding of how the evidence was actually adduced for the claim in question, and what this evidence substantively entails. These questions are thus of a distinct pedagogical value for students in the science education classroom. Although it is true that the non-scientist is rarely in a position to appraise the substantive content of science, he or she can always ask what sacrifices are being made in order for values such as consistency, internal coherence, and completeness to be sustained. This issue is pertinent to any scientific finding, since the fact that our communally given epistemic values are idealised means that the messy contradictions, incoherencies and inconclusiveness that scientists actually encounter have to be finessed in practice. To this end, researchers select their data carefully with a view to consistency, they frame their experiments judiciously with a view to coherence and they draw their conclusions accordingly. Truths, facts and observable realities in nature do in fact ensue from just this kind of careful, methodological rigour. But the possibility always persists that other truths, facts and observable realities may be simultaneously foreclosed.

By this token, students of science could gain a richer understanding of substantive science if they were taught to ask such questions as the following: Was any part of the data left out of the analysis as a result of its anomalous status? Which part or parts were omitted for the purposes of the experiment, and why? Would the inclusion of this data alter the findings in any significant way? Which questions were deemed unanswerable and thus inappropriate as frames for the study? Which contradictions, uncertainties or perplexities were actually resolved, and which ones were instead simplified for scientific purposes? Learning to take this kind of critical distance from science, by learning about and interrogating the extra-scientific norms, values and beliefs that inform the production of scientific knowledge, is not only possible for the non-scientist, it is in fact integral to learning about the content of science as it actually is. One can ask, for instance, whether a claim that is reasonably certain now was

always taken to be so. If not, what was believed in its place? What brought about a change of view in this particular case, and how recently? Under what conditions would this claim no longer be thought to be plausible? What other beliefs would have to go with it?

The answers to such questions will not determine definitively the trustworthiness of any particular claim made by an expert. By the same token, even an expert's own appraisal of evidence will not determine anything absolutely. What such content-transcendent modes of inquiry can do, however, is expose some of the cultural, moral, social, political and prudential judgements that underlie expert knowledge, and that can *potentially* destabilise it from within. Such norms constitute expertise in its infinite, undecidable sense. In the case of science, therefore, they thus provide a fruitful path for pedagogy in the science education classroom, and a basis for the critical appraisal of science by scientists and non-scientists alike. For these reasons, an emphasis on the undecidable grounds of scientific expertise should be featured in the science education curriculum.

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REFERENCES

Adler, J. (1994) Testimony, trust, knowing, *The Journal of Philosophy*, 91.5, pp. 264–275.

Code, L. (1987) *Epistemic Responsibility* (Hanover, MA, University Press of New England).

Derrida, J. (1989) *Edmund Husserl's 'Origin of Geometry': An Introduction* (trans. John P. Leavey, Jr.) (Lincoln, NB and London, University of Nebraska Press).

Driver, R. (1989) The construction of scientific knowledge in school classrooms, in: Robin Millar (ed.) *Doing Science: Images of Science in Science Education* (London, New York and Philadelphia, Falmer Press).

Hardwig, J. (1985) Epistemic dependence, *The Journal of Philosophy*, 82.7, pp. 335–349.

Hardwig, J. (1991) The Role of Trust in Knowledge, *The Journal of Philosophy*, 88.12, pp. 693–708.

Kitchener, R. (1992) Piaget's genetic epistemology: epistemological implications for science education, in: Richard Duschl and Richard Hamilton (eds) *Philosophy of Science, Cognitive Psychology, and Educational Theory and Practice* (Albany, NY, State of New York Press).

McPeck, J. (1981) *Critical Thinking and Education* (Oxford, Martin Robertson & Company).

Munby, Hugh (1980) Analysing teaching for intellectual independence, in: Hugh Munby, Graham Orpwood and Thomas Russell (eds) *Seeing Curriculum in a New Light: Essays from Science Education* (Toronto. Ontario Institute for Studies in Education Press).

Nagel, E. and Newman, J. (1959) *Gödel's Proof* (New York, Routledge & Kegan Paul).

National Science Board (1998) *Science and Engineering Indicators—1998* (NSB 98-1) (Arlington, VA, National Science Foundation).

Norris, S. (1992) Practical reasoning in the production of scientific knowledge, in: Richard Duschl and Richard Hamilton (eds) *Philosophy of Science, Cognitive Psychology, and Educational Theory and Practice* (Albany, NY, State of New York Press).

Select Committee on Science and Technology, House of Lords (2000) *Science and society* (Session 1999–2000, 3rd Report) (London, The Stationery Office).

Siegel, H. (1988) Rationality and epistemic dependence, *Educational Philosophy and Theory*, 20.1, pp. 1–6.

Webb, M. (1993) Why I know about as much as you: a Reply to Hardwig, *The Journal of Philosophy*, 90.5, pp. 260–270.