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TABLE OF CONTENTS

INTRODUCTION

Elisabetta Lalumera, Giovanni Tuzet
Experts and Expertise. Interdisciplinary Issues III

PAPERS

Carlo Martini
The Paradox Of Proof And Scientific Expertise 1

Christian Dahlman, Lena Wahlberg, Farhan Sarwar
Robust Trust in Expert Testimony 17

Susan Haack
The Expert Witness: Lessons from the U.S. Experience 39

Ronald J. Allen
A Note To My Philosophical Friends About Expertise And Legal Systems 71

Elisabetta Lalumera
Overcoming Expert Disagreement In A Delphi Process. An Exercise In Reverse Epistemology 87

Matt Stichter
Philosophical and Psychological Accounts of Expertise and Experts 105

Ben Trubody
A Paradigm for Your Thoughts: A Kuhnian Analysis of Expertise 129

Gloria Origgi
What Is An Expert That A Person May Trust Her? Towards A Political Epistemology Of Expertise 159

Suzanne Uniacke
Responsibility, Expertise and Trust: Institutional Ethics Committees and Science 169

Itay Snir
Experts Of Common Sense: Philosophers, Laypeople And Democratic Politics 187

Jean-Marie Chevalier
The Day After The Day Of The Experts. Lessons From J.M. Cattell, B.I. Gilman And C.S. Peirce 211

Francesca Ervas
(Becoming) Experts In Meaning Ambiguities 225

BOOK REVIEWS

Federico José Arena
Democracy, expertise, and academic freedom: a First Amendment jurisprudence for the modern state
by Robert C. Post 245

Fabrizio Esposito
Nudge and the Law
by Alberto Alemanno and Anne-Lise Sibony (Eds.) 255

Introduction
Experts and Expertise. Interdisciplinary Issues

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Today the role of experts is pervasive in the everyday life of both individuals and communities. At the collective level, governments and groups routinely delegate scientific, economic and technological decisions to experts; expert witnesses play a key role in legal contexts, and the evaluation of academic and scientific institutions is demanded to expert peers. At the individual level, each of us defers to experts for the correct understanding of problems, issues, concepts and word meanings in some domains, and trusts experts blindly at least in some cases. Finally, both communities and individuals face the problem of what to do when experts disagree.

The study of experts and expertise lies at the intersection of cognitive and social psychology, epistemology, economics, philosophy of law, and philosophy of language, but the various perspectives seldom meet together. For these reasons, we think it is timely to pose fundamental questions on the notions of expert and expertise in an interdisciplinary manner, so that issues raised within a specific debate may find solutions and integrations from other debates. The aim of this issue of the present journal is to collect a variety of points of view on the topics of experts and expertise, with a special focus on the following issues:

- what experts are, and what the criteria are for individuating them;
- how expert cognition differs from layperson cognition in specific domains;

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- whether and to what extent it is rational to trust experts, provided that we cannot assess their competence;
- how it is correct to characterize experts' disagreement, and what we are to do when experts disagree.

Thus the essays collected here range from theoretical questions about expertise to practical ones related to expert knowledge and advice. Among the former, some of the contributions discuss the features of expertise not only to better grasp it but also to distinguish true experts from fake ones, or at least reliable from less reliable expert opinions. Among the latter, several contributions touch upon the issue of democracy in the age of expertise, starting from the assumption that expertise applied to political, legal and economic decisions is in tension – to say the least – with the idea that citizens should be on the same footing when public decisions are made. It is not surprising that it is so. On the one hand, it seems perfectly reasonable to defer to expert cognition and have a world characterized by some division of the cognitive labor. On the other hand, it seems equally reasonable to let people decide what they want from life and institutions. For instance, should expert medical advice constrain our habits and lifestyle in general? Should expert economic advice constrain governments? Should expert forensic opinions constrain judges and juries? If not, why ask them to give their advice? If yes, why not let them decide in the first place what is good for us as individuals and communities?

There is an easy way-out, in theory. It is the salutary Humean division of questions of fact and questions of value. Emphatically put, it is the fact/value dichotomy that saves us from all sorts of expert confusion. If someone gives us expert cognition, they give us a piece of knowledge that we non-experts could not get (or could get at a significantly greater cost). But as a piece of knowledge it simply relates some fact. And according to Hume we cannot infer values from facts. Or, to put it differently, we cannot derive an ought from an is. So, expertise correctly understood does not constrain practical decision in any strong sense. It simply provides knowledge for a better-informed decision-making. In a liberal society, it is good to have some medical advice, but it's bad to have the physician decide in our place what we should do, eat and drink. Similarly, it is good to have the government supported by economic advice, but it's bad to have economists decide in place of elected bodies. And it's good to have judges and juries informed by experts, but it's bad to let experts decide. It

is conceptually and logically bad, first and foremost, because knowledge by itself doesn't tell us what to do, if Hume was right.

However, it's true that in the real world important issues are terribly complex, and it's often hard to discriminate fact from value, let alone true from fake experts and what to do when experts disagree (which happens almost always). On a philosophical tone, we need also consider the (slippery?) distinction of "knowledge that" and "knowledge how"; the first is theoretical, so to say, and the second practical. Does this blur the Humean division? Is it possible to have an expert practical advice if it expresses a form of "knowledge how"? In addition, what is the relevant notion of "experience" at play here? And what is the appropriate propositional attitude towards expertise? Belief or acceptance?

In sum, there are many questions and issues raised by the pervasive role of experts in our world. Of course the present collection doesn't provide any definitive answer to them, but we think it provides at least some good insight and food for thought.

The Paradox Of Proof And Scientific Expertise

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ABSTRACT

In this paper I criticize the current standards for the acceptability of expert testimony in current US legislation. The standards have been the subject of much academic literature after the *Frye* and *Daubert* cases. I expose what I call the Paradox of Proof, and argue that the historical and current standards have sidestepped the problem of determining who is an expert and who is not in a court of law. I then investigate the problem of recognizing expertise from the layperson's standpoint, and suggest what courses of action the future research ought to take on the problem of identifying expertise.

Keywords: expertise, experts, laypeople, proxies, legal proof, demarcation, Harry Collins, Robert Evans.

Introduction

Living in societies, we rely constantly on the work of others for our needs: we rely on the baker to provide us bread in exchange for money, or on the construction worker to build our house. But reliance on others is not only material, it is also epistemic. Contemporary philosophers and social epistemologists, inspired by what Adam Smith called "division of labor" – he probably had in mind mostly material labor (see Smith, 1976) – have investigated the division of "epistemic labor" among the members of our epistemic communities. Kitcher (1990), focusing on scientific communities, has called it "division of cognitive labor"; it takes place, for instance, when modelers rely on experimenters to parameterize their models. But division of cognitive or epistemic labor can be found among the members of all epistemic communities, not only among scientists: We rely on doctors to know what our

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symptoms indicate, and we rely on bankers, sometimes incorrectly, to know what retirement or mortgage plan suits our personal and professional needs. One can safely assume that the division of epistemic labor is what allows us to get around our daily needs with the minimum amount of knowledge, or epistemic effort, required to survive and perform our daily tasks. We don't need to know what kinds of food are poisonous, or how to build a safe water system or a shelter, because others have learned how to do all those things for us.

The focus of this paper is the legal sector, where epistemic reliance on others is ubiquitous. In particular, I will be mostly concerned with how judges rely on experts to disentangle technical and scientific issues in cases where common knowledge of facts is not sufficient for a verdict. Such cases are more and more common in liability litigations (e.g., pharmaceutical or medical cases) and criminal cases where scientific techniques are employed (e.g., DNA testing, psychological evaluation). A much-studied 1993 U.S. Supreme Court case, "Daubert v. Merrell Dow Pharmaceuticals", determined that in order to admit an expert's testimony to a trial the court must "look not to an expert's conclusions, but to his "methodology", to determine whether proffered evidence is really "scientific ... knowledge" and hence reliable." (Haack 2005, S66). In practice, the ruling asked the court to determine whether the expert's testimony could be admitted on grounds that it was "scientific", that is, that it derived from the correct application of the scientific method. Decisions on whether to admit an expert's testimony or throw it out are calls that courts and judges must often make, even when they do not have sufficient knowledge to judge on the complicated issues for which reliance on experts was required. Courts and judges are, in this sense, laypeople, in relation to the experts whose testimonies they have to evaluate.

The problem just illustrated is the problem of how a layperson – that is, someone who has no expertise in a certain field – can adjudicate who is and who is not an expert in that field. In many instances in which we rely on others we are in an easier position to assess whether the people, or groups, we rely on have the necessary expertise for our needs. For example, as long as our standards of consumption are similar, we might be able to recognize a good carpenter by the quality of their crafts, or a good baker by the tastiness of their bread (see Collins and Evans 2007, 57-60). But there are harder cases, like legal litigations, which bring to the fore an apparent Paradox of Proof: How can someone who doesn't know the subject matter know who is an expert on that subject matter? As a simple example we could ask "how can someone who does

not have specific knowledge of mathematics know which mathematician has the proof to a certain theorem?" The Paradox of Proof exists in the mathematical case because there do not seem to be external standards, by which we can evaluate the goodness of a proof, that are independent of the mathematical standards that make a proof a good one. There are many other cases like this one in science, and they have consequences for legal adjudication.

In the legal scenario, for instance, how can a judge, who does not have domain knowledge of the complexities of DNA testing, know which experts' opinions are more qualified to weigh on the verdict? The problem is a complicated one from the point of view of legal theory (see Haack 2002), but for the purposes of this paper I will focus on the narrower philosophical issue of recognizing experts while standing in the shoes of the layperson. As it should become clear in the following sections, judges are equivalent to laypeople when it comes to evaluating expertise.

In the next section I will look more in detail into two landmark answers that the U.S. legal system has given to the problem of adjudicating expertise. I will argue that those answers are sidestepping the very problem. Next, I will introduce and explore one prominent stance on how to recognize expertise, suggested by Collins and Evans (2007). I will highlight some of the limitations in Collins and Evans's proposal. I will then consider a different proposal: Shanteau's contribution (1992) to the problem of expertise. I will argue that recognizing expertise should be done through *proxies* and *indicators*, which I define in the last section; but the task of finding such proxies and indicators is not a simple one, or one that can be done with theory alone. I will suggest a number of sub-problems that research on expertise will have to undertake to try to solve both the paradox of proof and the current lack of criteria for evaluating expertise in legal cases.

1. *Frye* and *Daubert* on expertise

Why is it important to find criteria of expertise, even if all we can formulate is only a tentative and probably imperfect list of them? The two major decisions on what counts as expertise in court litigations should be enough proof that criteria for identifying expertise are badly needed: these landmark court decisions about the involvement of experts testimony as evidence in court were the Frye test (see Frye v. United States 1923) and the ruling over Daubert vs.

Merrell Dow Pharmaceuticals, Inc. (Daubert v. Merrell Dow Pharmaceuticals 1993). In this section I will present the two cases, and argue that in both of them the decisions on the admissibility of expert evidence were an attempt to evade the question "who is an expert?"

In Frye v. Unites States (1923) the court decided to reject "the results of a then-new blood-pressure deception test on grounds that novel scientific testimony "crosses the line between the experimental and the demonstrable," and so is admissible, only if it is "sufficiently established to have gained general acceptance in the particular field to which it belongs."" (Haack 2005, S66). The Frye test established that the criterion for the admissibility of expert testimony is the "general acceptance" of the testimony – i.e., the science invoked by the testimony – in the relevant scientific community. At the time, since the science of lie-detector tests had not reached a consensus, a court could not accept the test.

The standard of general acceptance, applied by the court in Frye v. Unites States, seems to be founded on the idea that good science tends to generate consensus around established facts and method; so the consensus criterion is, prima facie, a reasonable standard for the admissibility of scientific evidence. Much of the epistemic work in science is done by consensual processes: scientists formulate hypotheses, they gather evidence, they present their evidence to their peers through conferences and journals, their peers evaluate the evidence, respond, criticize, reject what is inadmissible, and, slowly, a consensus may form as to what can be accepted on a more or less definitive basis, or at least until new evidence is brought to the fore. This is no doubt a rather idealized characterization of how science works; in practice, the process of accepting scientific facts and theories is much less linear. But we can still claim that under normal and slightly idealized circumstances science progresses by consensus (see also Kuhn, 1970, on the role that consensus has in the achievement of "normality" in science).

The problem with the use of consensus criteria in law is that too often consensus is only a byproduct, not a cause of good, and therefore, court-admissible, science. Scientific consensus forms because the science in question is grounded on good evidence, but sometimes it forms around bogus science as well, possibly caused by extra-scientific reasons like biases and political and economic interests. Therefore, we can accept the thesis that consensus is necessary for science without being committed to the thesis that it is also sufficient, and if that is the case a court should not accept scientific

claims based on the presence of consensus (i.e., the general acceptance criterion). Even more importantly though, there are additional reasons for rejecting the general acceptance criterion established in *Frye v. United States*, and they rest on the fact that consensus, as a byproduct of good science, typically forms too slowly for the need of courts to ascertain the truth – or, at least, the "provable" – in legal trials.

The Frye test and the general acceptance criterion remained the standard for expert testimony in United States courts for several decades until *Daubert v. Merrell Dow Pharmaceuticals*, in 1993, set a new standard for the admissibility of expert testimony. What happened in the meantime was that, in 1975, Congress had adopted the Federal Rules of Evidence, on the basis of which the consensus standard could no longer be upheld as the only standard for the admissibility of expert testimony. In light of *Daubert*, the Federal Rules of Evidence were further revised, and rule 702 now states:

> If scientific, technical, or other specialized knowledge will assist the trier of fact to understand the evidence or to determine a fact in issue, a witness qualified as an expert by knowledge, skill, experience, training, or education, may testify thereto in the form of an opinion or otherwise, if (1) the testimony is based upon sufficient facts or data, (2) the testimony is the product of reliable principles and methods, and (3) the witness has applied the principles and methods reliably to the facts of the case. (Saks and Faigan 2005, 109)

Clauses 2 and 3 of the rule are important here, as they call upon scientific methodology. Judges are called to judge on the reliability of principles and method of the testimony given, and on the correct application of principles and methods to the facts under investigation. The concept of reliability in science is a technical one, and refers to a method's ability to give consistent results (see Buekens and Truyen, 2014); it does not apply to the ability of a method to give true results. The ability of a method to give true results is called "accuracy" and it is a harder requirement to meet for a scientific method. Reliability, however, provides enough evidence that the method is at least not random – i.e., that it provides results based on underlying facts – and this is an important requirement of any objective method, and of scientific methods in particular. The applicability of a given method to a particular case is also important in science, since a method demonstrated to be reliable on a certain domain may not be reliable under different circumstances (e.g., a method for DNA testing applied on samples that have not been properly handled or collected). In standard procedures like DNA testing, the rules of applicability may be

relatively straightforward, but, in general, in science such rules are not easy to formulate, especially in novel science. Yet despite the difficulties of establishing reliability and applicability, rule 702, above, requires the triers of fact to do just that.

Both the Frye test and the new rule 702 in the Federal Rules of Evidence are in fact ways to avoid the question "who is a legitimate expert in a court of law?" The Frye test and rule 702 do not try to establish who has legitimate expertise, and can thus provide reliable testimonial evidence of technical or scientific facts that neither the court nor the general public could assess. Instead, they ask the trier to evaluate the evidence that is brought forth by the experts. That does not seem to make much sense though, since the need to call upon experts to provide their judgments is exactly the inability of courts and judges to evaluate and weigh complex technical and scientific issues that bear on the matter under trial. Both the Frye test and rule 702, then, shift the problem from evaluating the validity of expertise to evaluating the validity of the evidence presented. Of course, in principle, this more is sensible: We would rather accept valid arguments and good evidence than just trust that our experts are giving us valid arguments and good evidence. But the move misses the point: It is because we are not in a position to assess the evidence directly that we resort to rely on expert testimony.

We may imagine a counterfactual scenario in which we were asked to judge whom we trust the most to be able to heal us from an ailment, and whose opinion we would rather not listen to. Among Western-educated people, it may be safe to assume that we trust medical doctors, rather than karma healers; but, if pressed, would we claim that we trust a doctor's judgment because it is agreed upon by most of the medical community? Against this, one must note that many of the cures that doctors provide are far from being accepted as the medical consensus. More importantly, whether there is or is not such consensus is hard to adjudicate from the standpoint of the layperson. Would we claim, instead, that we trust a doctor's judgment because we have assessed their method as reliable, as well as the applicability of their method to the specific case at hand? This answer is not a sensible answer either, because people untrained in science and its method cannot easily make judgments about reliability and applicability.

One could still claim that judges stand to experts in a different relation than patients stand to doctors. Perhaps judges are more capable of recognizing the trappings of expertise by looking at the science itself. This hypothesis is also

untenable. It is hard to see how judges could have the kind of training required to assess scientific evidence and method, especially since even scientists themselves often do not have the capacity to assess evidence and method outside of their field of specialization, and judges are often required to assess the acceptability of expertise in many different fields, each with different domain-knowledge and methods. If that was not enough prior evidence, Gatowski et al. ran an empirical survey to ascertain whether the judges could be relied upon to have enough scientific expertise to apply rule 702 of the Federal Rules of Evidence, and concluded that that was not the case: "The survey findings strongly suggest that judges have difficulty operationalizing the *Daubert* criteria and applying them, especially with respect to falsifiability and error rate." (2001, 452)

Good alternatives to either the *Frye* criterion or the current rule 702 of the Federal Rules of Evidence are needed, if we trust the findings from Gatowski et al. (2001) that "judges overwhelmingly support the "gatekeeping" role as defined by *Daubert*, irrespective of the admissibility standard followed in their state. However, many of the judges surveyed lacked the scientific literacy seemingly necessitated by Daubert." (2001, 433). An alternative, however, should deal directly with the question of who is a legitimate expert in a court of law on a given subject matter. Any attempt to ask the judge or jurors to evaluate the evidence the experts bring forth, their methods, and similar aspects of an expert's testimony will fall into the trap of requiring the kind of knowledge that jurors and judges do not possess, which was the reason why experts were consulted in the first place. Of course, a judge or a court might still check for consistency of an expert's method; whether the expert is giving contradictory statements, and other basic checks that a layperson would be able to perform on an expert's testimony. But much more than logical consistency and similar requirements is needed.

The next sections will focus on how we can define expertise, and what alternative criteria for the admissibility of an expert's testimony we can hope to develop. Any such criterion will have to make it possible for a layperson to recognize an expert, keeping in mind that the paradox of proof casts doubt over the entire enterprise: "How can someone, ignorant in a certain domain, know who is an expert in that domain?"

2. Collins and Evans on Expertise and Experience

Collins and Evans (2007) have written extensively on the problem of "ways to separate those who fall into the envelope of potential judges in respect of various expertises from those who fall outside that envelope" (2007, 67) They recognize that in most cases where we lack domain-specific knowledge needed to assess expertise – that is, whenever we are not experts ourselves, trying to identify other experts – we rely on "externally measurable criteria" (2007, 67). I call these criteria "proxies" of expertise: i.e., factors that indicate the presence of substantial expertise, when such presence cannot be detected directly.

Collins and Evans review a number of proxies of expertise, the first one being credentials and the second one being track-record. According to the former criterion, we can allegedly identify expertise by means of "certificates attesting to past achievements of proficiency", while according to the latter we would identify expertise by looking at one's past success in solving problems related to the relevant field of expertise. They dismiss both criteria on the grounds that in both cases there can be significant expertise even in the absence of credentials or a track-record. On the one hand, accreditation is a social practice, but expertise is substantial, and there can then be expertise that goes unaccredited. On the other hand, a track-record is not always available, if not in principle, at least in practice because, like accreditation, it is a social practice, whereas expertise is substantial and personal.

After correctly discarding credentials and track-record, Collins and Evans turn to experience – i.e., experience within the relevant domain – as the preferred criterion for expertise: "We know from the outset that without experience within a technical domain, or experience at judging the products of a technical domain, there is no specialist expertise. Without experience of doing science, talking to scientists, playing or listening to violin-playing, or looking at and discussing bathroom tiling, the minimal standards for making judgments in these areas have not been met." (2007, 68) The concept of experience is helpful for understanding expertise, but it is not fully analyzed in Collins and Evans's book. To be fair, they do talk extensively about experience throughout the book; for example, they describe experience as embeddedness in the relevant epistemic community. But the discourse always falls short of a detailed analysis of the concept.

Perhaps the assumption is that we have a very good common-sense understanding of experience, and that it seems also obvious to think that

experience must be a hallmark of expertise. Given this common-sense understanding of experience, it may then seem unnecessary and pedantic to provide a philosophical analysis of the concept of experience before we dare use it. However, in the following I hope to show that unpacking the concept of experience will reveal problems that ought not be left unanswered and that relate to expertise and a layperson's ability to identify experts.

I will argue we cannot use experience as a proxy for expertise – that is, as a trait we look for, under the assumption that it is correlated with expertise. Instead, I will argue that experience is part of the substance of expertise. This will open two problems: 1) To identify experience one needs to be able to tell apart relevant from irrelevant experience and, in turn, this implies that only experts, as possessors of experience, can identify other experts; 2) Experience maybe a necessary but not sufficient condition for expertise and, in turn, even if the former problem could be resolved, we would still have no clear indication on how to detect expertise.

The first thesis in this section is that experience seems to be a substantial trait of expertise, not a proxy. To illustrate, let us imagine a scientist, working for several decades in a narrow and highly specialized field, and accomplishing great and substantial success in that field. With that in mind, we would certainly be confident in the fact that the scientist is a true expert in her field. That is because the experience of the scientist is relevant to its genuine expertise. But we cannot know, a priori, that the expertise is genuine, we infer it from the fact that we observe the imagined scientist's experience. But a scientist, through accomplishments achieved in her own field, may try to act as an expert in a much broader field, and on topics that are outside her own narrow field of specialization.

When that happens – when scientists speak outside their own field of genuine expertise – laypeople do not have a way to recognize which experience warrants which expertise; or, in other words, when a scientists speaks as an expert on a given matter, the public does not typically have the means to recognize, on the basis of an observation of that scientist's experience, whether the matter the scientist talks about is within her domain of genuine expertise, or whether she has overstepped the boundaries of that domain. This is to say that we typically do not have a direct way to assess the relevance or irrelevance of experience to one's putative expertise.

The scientist that was described in the preceding paragraphs is not only imaginary: Kitcher recounts the story of what Oreskes (2010) calls "merchants

of doubt"; namely, scientists who, while gaining the status of expertise in their own specialized field, where they had considerable experience, spoke publicly about climate change issues at large, despite their experience not being relevant to qualify them as experts on climate science.

> [...] a few scientists, with strong ties to particular industries and with conservative political connections, have played a disproportionate role in debates about controversial questions, influencing policy-makers and the general public alike. *Typically, these scientists have obtained their stature in fields other than those most pertinent to the debated question.* Yet they have been able to cast enough doubt on the consensus views arrived at by scientists within the relevant disciplines to delay, often for a substantial period, widespread public acceptance of consequential hypotheses.(Kitcher 2010, 3, my italics).

It seems evident that in order to be able to tell relevant from irrelevant expertise one needs to be at least an "interactional expert" on the field in question. Interactional expertise, in the terminology used by Collins and Evans, is "expertise in the language of a specialism in the absence of expertise in its practice." (2007, 28). Being able to discriminate experts from non-experts via the criterion of experience requires interactional expertise because one needs to be able to separate relevant from irrelevant experience. There may be cases where experience can easily be ruled out as irrelevant: For example, most laypeople would be able to say that experience in a bakery will never make one an expert in piloting airplanes. In general, however, laypeople would not be able to tell relevant from irrelevant experience in specialized sectors because of their lack of domain knowledge.

However, for the sake of the argument, let us imagine for a moment that we were able to bypass the problems just mentioned. We could imagine a nearly perfect organization of science (and its technological applications) where it is clear which kinds of experience are relevant for which domains of expertise. The problem that remains is whether experience is correlated with expertise; we may safely assume that it is at least a necessary condition, but is it also sufficient?

This is not just an appeal to the abstract philosophical requirement of providing "necessary and sufficient conditions for the occurrence of X". If Collins and Evans's concept of experience is indeed meant to stand as a proxy of expertise, then their account seems to overlook the fact that one may have considerable experience without having, in relative terms at least, much expertise. To be fair, the authors do acknowledge that experience may not be

sufficient for expertise, when they note that Harry Collins never acquired interactional expertise in the field of amorphous semiconductors, in spite of the several interviews with scientists on the physics of amorphous semiconductors (p. 33). It is possible then that Collins and Evans never intended experience to stand as a proxy for expertise, despite the fact that they list it after credentials and track-record. But the thesis defended here is simply that there can be experience without there being expertise, and because that does not hinge on the interpretation of Collins and Evans (2007) – who indeed make the same point – it should not affect the following considerations.

Shanteau et al. (2002) report on the relation between experience and expertise: If by experience we mean something that can be measurable (for example number of years on the field), then one can provide evidence of the fact that "there are many examples of professionals with considerable experience who never become experts. Such individuals may even work with top experts, but they seldom rise to the performance levels required for true expertise." (2002, 254) Shanteau and his coauthors conclude that while we should expect instances where more experience correlates positively with expertise, we cannot generalize for all instances.

To conclude, in this section I have argued that experience is not a proxy of expertise, regardless whether that may or may not have been the suggestion in Collins and Evans (2007). Collins and Evans have provided much valuable work on the concepts of expertise and experience. But for the purposes of this paper, the problem of identifying experts still seems elusive: We would like to be able to tell experts from non-experts, and we would like to be able to do so, even at the cost of some imprecision, while standing in the shoes of a layperson. For that, the criteria we have seen so far (credentials, track-record, and experience) do not seem to work.

The trappings of expertise

There are at least two substantial components of expertise: experience and competence. They are the past and future components of the concept of expertise; or equivalently, the backward- and forward-looking components. Experience is the amount of practice experts have put into solving problems relevant the their field. Experience must be relevant; that is, it must be focused on a specific domain of knowledge or practice. That experience be relevant is a requirement because there

can be putative experts with considerable experience who are not truly competent experts, though it seems unlikely that an expert with considerable relevant experience would lack the competence necessary to solve future problems. Competence, here, means the ability to solve problems in the relevant field of expertise. It seems obvious to think that a person's ability to solve problems depends, albeit not exclusively, on the amount of past attempts at, and successes in, solving problems. Solving problems can of course be real or virtual; much textbook training into a profession involves "solving" virtual problems, or at the very least the provision of tools for solving problems.

To repeat, while experience is the backward-looking component of expertise, competence is the forward-looking one. Both, however, are substantial traits of expertise, and it was shown in the previous section that we cannot take layman-perceived experience directly as a proxy for relevant experience and, thus, for expertise. Moreover, the two components are related: In human experts, it is unlikely to have the former without the latter. The fact that experience and competence are substantial traits of expertise is both accurately descriptive – we certainly observe considerable relevant experience and competence in experts – and normatively compelling, because we want experts to be able to solve problems in their field of expertise, and that is more likely to happen if said experts have solved problems in their field of expertise in the past.

Experience[1] and competence, however, as substantial traits, cannot be detected directly; the former for reasons explained in the previous section (a track-record, for instance, is a good proxy, but it can fail to detect experience if the field of experience differs from the field the track-record is taken from), and the latter for the obvious reason that competence is a forward-looking property: We cannot detect competence until it has been applied, namely, until it has become a past event. Of course there are proxies for competence, as well as for experience, but the question is how to identify, measure, and weigh the contributions of these proxies to true expertise.

The psychological literature has analyzed proxies for expertise at great length, both theoretically and experimentally. Shanteau lists a number of those proxies and evaluates them normatively: (1) experience; (2) certification; (3) social acclamation; (4) consistency within reliability; (5) consensus; (6) discrimination ability; (7) behavioral characteristics; (8) knowledge tests (see Shanteau et al. 2002). It is relevant to note what the behavioral and psychological characteristics

[1] In the following, I will use 'experience' to mean relevant experience, for brevity, except where otherwise stated.

that can be attributed to experts are: (a) possession of content knowledge; (b) perceptual/attention abilities; (c) ability to discriminate relevant from irrelevant information; (d) ability to simplify complex problems; (e) ability to communicate their expertise; (f) ability to handle adversity and difficult situations; (g) ability to follow established practices and make exceptions when appropriate; (h) self-confidence; (i) ability to adapt; (j) sense of responsibility (see Shanteau 1992). In the end, Shanteau et al. (2002) propose a ratio between discrimination and inconsistency and test it against available data. They conclude that the ratio (named *CWS*), if successful, "may provide an answer to the long-standing question of how to identify expertise in the absence of external criteria." The difference between a proxy and the solution Shanteau et al. propose is that the CWS ratio is a function of several proxies – that is, a derived measure – whereas each proxy is a direct measure: i.e., it could be measured directly with empirical data, for example, experience can be measured in years, and specific abilities can be tested in experimental conditions. This is an important distinction: On the one hand, direct measures are proxies, in the sense that they are candidates for something that we can detect directly instead of expertise, which cannot be detected directly. On the other hand, indicators are mere numerical values; i.e., combinations of proxies, which, ideally, point us in the direction of expertise. The better the indicator is, the more likely we will find genuine expertise there.

The approach by Shanteau and his collaborators is reductionist – that is, it reduces expertise to identifiable traits that we take to be proxies for expertise – and it seems to be a valuable step in the correct way to pose the problem of expertise. In other words, while the Federal Rules of Evidence mentioned in the previous sections seemed to do away with the problem by asking the judges not to evaluate the experts, but the content and form of what they say (i.e., method, relevance, etc.), what one should do, instead, is to find ways to evaluate the experts' expertise itself by means of proxies and indicators.

But is the ratio suggested in Shanteau et al. (2002) the ultimate word on what proxies we can use to identify expertise? Probably not. More empirical research is needed, as well as more development of theory, for better understanding expertise along the lines opened by Collins and Evans (2002, 2007) and Collins (2013). In concluding this section I make a number of observations on the issues that future research on expertise will have to tackle in order to make progress on the problem of expertise; that is, the problem of identifying experts while standing in the shoes of laypeople, both in the legal sector and in other sectors where expertise is needed.

The first observation concerns the analogy between proxies/indicators of expertise and evidence in science. There can clearly be many proxies of expertise, and even more indicators, given our definition of indicators as functions that combine more than one proxy. When evaluating which combination of proxies will likely give a good indicator, we should take into account the literature on the method of combining (i.e., amalgamating) scientific evidence. In other words, considering different proxies as bundled indicators of expertise is equivalent to combining evidence in science. If we are allowed to combine different sources of evidence in support of a thesis, there are methodological rules that we should follow: For instance, two sources of evidence, *a* and *b*, may not support a thesis more than another source, *c*, if there is a correlation between *a* and *b*. More concretely, if expert *X* has, say, both *accreditation* and *acclamation*, that may not give us evidence that *X* is more expert than *Y*, where *Y* only has *accreditation*, if *acclamation* and *accreditation* are correlated so that *acclamation* implies *accreditation*. Possible dependencies between proxies should be taken into account when developing indicators.

The vast literature on amalgamating evidence, then, ought to be considered when we look for evidence of expertise: Stegenga (2013) and Lehtinen (2013) present different results on the possibility (or impossibility) of amalgamating evidence coming from diverse sources. Bovens and Hartmann (2002) and Claveau (2013) also offer different conclusions in discussing the "variety of evidence thesis": i.e., the thesis according to which the warrant given to a hypothesis increases, ceteris paribus, when the body of evidence is more varied. Both the problem of amalgamating evidence and the problem of the variety of evidence need to be considered in the study and development of indicators of expertise. The theoretical results by Stegenga, Lehtinen, and the other authors mentioned in this section, could inform the empirical application of indicators to real data, which Shanteau et al. (2002) conduct in their paper.

A further issue with the study of expertise is that the definition of expertise is likely to be a moving target: "being an expert on X might change with the X". That is because there are likely very different types of skills and abilities (competences) involved with different domains of expertise, and these variations are likely to be reflected in the development and study of a certain indicator of expertise. Notwithstanding the problem of the moving target, some categorization might help here: are there kinds of expertise that may involve different sets of skills and demands? Collins and Evans (2007) have provided very useful categorizations for

expertise. Martini (forthcoming) also offers some suggestions on how to categorize some types of expertise, but more work on that needs to be done.

In the end, it is likely that a good understanding of expertise will come from the sociological and philosophical literature, but only in combination with the empirical and psychological literature on recognizing experts by means of proxies and indicators (see Shanteau 1992; Shanteau et al. 2002). Such collaboration is still in its infancy, but it might help in at least two ways: On the one hand, to move past the current standards for the acceptance of expert testimony in court, by giving criteria for separating true experts from just putative experts; instead of focusing on issues of method and relevance that laypeople, courts, or judges, are unlikely to be able to evaluate. On the other hand, it might help to sidestep what I called, at the beginning of this paper, the "paradox of proof": how can a layperson recognize expertise. A layperson can recognize expertise by looking at proxies and indicators of expertise, all of which need to be carefully developed and tested against evidence: This is both an empirical and theoretical task.

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Robust Trust in Expert Testimony

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ABSTRACT

The standard of proof in criminal trials should require that the evidence presented by the prosecution is robust. This requirement of robustness says that it must be unlikely that additional information would change the probability that the defendant is guilty. Robustness is difficult for a judge to estimate, as it requires the judge to assess the possible effect of information that the he or she does not have. This article is concerned with expert witnesses and proposes a method for reviewing the robustness of expert testimony. According to the proposed method, the robustness of expert testimony is estimated with regard to competence, motivation, external strength, internal strength and relevance. The danger of trusting non-robust expert testimony is illustrated with an analysis of the Thomas Quick Case, a Swedish legal scandal where a patient at a mental institution was wrongfully convicted for eight murders.

Keywords: expert testimony, robustness, Thomas Quick case.

1. Robust Evidence

It is obvious that some evidence is better than other evidence, but it is not obvious to everyone that 'better evidence' can mean two different things. A&B can be better evidence than A for a certain hypothesis H, in the sense that the

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probability that H is true is increased more by the conjunction of A and B than by A on itself, $P(H|A,B) > P(H|A)$, but A&B can also be better evidence than A for H in the sense that it takes more relevant information into account. These qualities must be distinguished from each other. A&B is always better evidence than A in the latter sense, but it is by no means necessary that A&B is better evidence than A in the former sense. It could just as well be the case that taking account of B decreases the probability of H, $P(H|A,B) < P(H|A)$. The distinction was first observed by Charles Sanders Peirce.

> [...] to express the proper state of belief, not one number but two are requisite, the first depending on the inferred probability, the second on the amount of knowledge on which that probability is based (Peirce 1932, 421).

The notion that evidence is better if it takes more information into account has been explored in different ways, with different terminology. John Maynard Keynes says that new information can improve the 'weight' of the evidence (Keynes 1921, 71), while Peter Gärdenfors and Nils-Eric Sahlin explain the improvement in terms of 'epistemic reliability' (Gärdenfors & Sahlin 1982, 362). Neil Cohen says that more knowledge increases the ´confidence´ in the evidence (Cohen 1985, 405), and Alex Stein says that it makes the evidence more ´resilient' (Stein 2005, 48). We will use the term 'robustness'. In the tradition of Scandinavian legal theory, we will say that the evidence for a hypothesis is more *robust* if it takes more relevant information into account (Ekelöf 1992, 128-129; Strandberg 2012, 515-607).

The robustness of evidence is a question of sensitivity to new information. Robust evidence is not sensitive to new information. It is not likely that additional information will alter the assessment. When the evidence is not robust, we have a situation where the assessment is sensitive to new information. The probability that more information will change the probability of the hypothesis is relatively high.

In criminal trials, the standard of proof requires that it has been proved beyond reasonable doubt that the defendant is guilty. This requirement should entail two conditions. Firstly, the probability of the hypothesis that the defendant is guilty (H_g), given the evidence that has been introduced to the court (E_i), must reach a certain standard (p^*), e.g. 99%.

$$P(H_g|E_i) \geq p^*$$

Secondly, this assessment must be robust. It must be unlikely that the assessment that the probability of the hypothesis reaches the p*-standard would change, in the hypothetical case that further evidence is introduced to the point where all attainable evidence (E_{max}) is given to the court. This condition introduces a second order probability over the first order probability measured by the p*-standard, and sets a standard of proof (p**) for the second order probability.

$$P(P(H_g \mid E_{max}) \geq p^*) \geq p^{**}$$

An increase in the second order probability means that it is more likely that the assessment of the first order probability will hold in the light of additional information. It should be noted that the p*-standard can differ from the p**-standard. It could, for example, be the case that p* = 0.99 and p** = 0.90. The p**-standard should be high enough to prevent conviction in cases where the evidence introduced to the court is too scarce. At the same time, it is important not to set the p**-standard too high. If the p**-standard is extremely high, no defendant can ever be convicted. There is always some risk that further information could change the assessment.

The setting of the p**-standard must also consider the cost of additional evidence. Gathering evidence comes with a cost, and it would be very costly to require that all attainable evidence is given to the court. The cost of additional evidence must, therefore, be balanced against the social cost of wrongful convictions (Keynes 1921, 77). In cases that deal with major offenses, like murder or rape, the social cost of a wrongful conviction is very high. The p**-standard should, therefore, be high in these cases. In cases that concern minor offenses, e.g. traffic violations, the social cost of a wrongful conviction is considerably lower. This is a reason for setting the p**-standard lower in such cases.

A problem with the p**-standard is that it requires us to assess what would happen if we had information that we do not have. This seems almost paradoxical. How can we assess the effect of evidence that we do not have? How can we know something about what we do not know? At a closer look, this is not as strange as it seems. Imagine, for example, that we know that Mrs. Brown was at the scene of the crime when it took place, but we do not know what Mrs. Brown knows about the incident. We have not heard testimony from Mrs. Brown, since she has not been called as a witness. In this situation, it is

possible, that our assessment of the probability that the defendant is guilty, $P(H_g \mid E_i)$, would change dramatically if we get to hear what Mrs. Brown has to tell. Maybe she made some very important observations that exonerate the defendant. It is, of course, also possible that Mrs. Brown's testimony would make no difference at all. Her observations could turn out to be completely irrelevant. We do not know how Mrs. Brown's testimony will affect our assessment of the hypothesis that the defendant is guilty, but our knowledge that she was at the scene of the crime makes it more probable that a testimony from her will change that assessment than a testimony from a randomly picked person in the general population. As a general rule, the probability that a person has some important information to offer is greater if we know that the person was present at the scene of the crime when the crime took place. It is generalizations like these that make it possible to assess the second order probability against the p**-standard. We know from experience that some inquiries are more likely to produce information that will change the first order probability than other inquiries. The probability that further investigations will change the assessment of the first order probability is higher, if there is a possible line of inquiry that has not been explored, but which, according to general experience, has the ability to change the assessment. Hearing the testimony of a person who was at the scene of the crime when it was committed is an example of such an inquiry. Another example of a kind of inquiry that typically has the ability to change the assessment of the first order probability is DNA profiling. In cases where DNA profiling is possible but has not been conducted, the probability that the defendant is guilty is sensitive to the additional information that a DNA investigation would produce.

The assessment of the second order probability can be summarized as follows. If a certain piece of evidence has not been introduced to the court, in spite of the fact that this would have been possible and the evidence is of a kind that, generally, is likely to change the probability that the defendant is guilty, the evidence assessment based on the existing evidence is not robust. If, on the other hand, there is no potential piece of evidence that is likely to make such a difference, the existing evidence is robust. The dimension of robustness applies to all types of evidence. In the remainder of this article, we will discuss the usefulness that this notion can have to court assessments of expert testimony.

2. Robustness and Expert Testimony

Expert testimony plays a crucial role in many legal cases, but is by definition difficult for non-experts to assess. It goes without saying that judges and juries to a large extent must trust the opinions of an expert. At the same time, it would be naïve to think that an expert's opinion is always correct - there are both empirical and theoretical reasons to take seriously the risk that an expert witness goes wrong (see e.g. Huber 1993, Angell 1997, Meester et al. 2006, Dwyer 2008, Wahlberg 2010 a and b, and Råstam 2012). Just like non-experts, some alleged experts are dishonest or incompetent, and even an honest and competent expert can commit a reasoning error, disregard relevant studies or misunderstand the factual question raised by a legal norm. If experts were trusted indiscriminately, many verdicts would hence rest on inadequate facts.

Ideally, therefore, the trust that judges and juries put in experts should be critical, not blind. Yet, the idea that experts should be trusted "critically" has a paradoxical flavour. Judge Learned Hand made this paradox explicit in an article published in *Harvard Law Review* in 1901. According to Hand, the jury is placed in an impossible position when the prosecution and the defence call expert witnesses that make contradictory statements and the jury has to assess which expert to trust.

> [...] how can the jury judge between two statements each founded upon an experience confessedly foreign in kind to their own? It is just because they are incompetent for such a task that the expert is necessary at all. [...] Knowledge of such general laws can be acquired only from a specialized experience such as the ordinary man does not possess [...] The jury by hypothesis have no such experience directly, it being of a kind not possessed by ordinary men [...] Therefore, when any conflict between really contradictory propositions arises, or any reconciliation between seemingly contradictory propositions is necessary, the jury is not a competent tribunal. [...] [the jury] will do no better with the so-called testimony of experts than without, except where it is unanimous. (Hand 1901, 54-56)

As a response to the paradox, Hand proposed that juries should be composed of experts: In a case of poisoning, the jury would be composed of people with expert knowledge in toxicology, in a case of murder by arson the jury would be composed of people with special knowledge on fires, and so on. In such a system, expert witnesses would no longer be necessary. Hand's proposition was never adopted by the American legal system. On the contrary, the use of

expert witnesses in court has increased tremendously (Graham 1977, 35), and experts' testimony is still assessed by juries and courts and not by peers. Fortunately, the paradox that Hand puts forward can be solved - at least in part. Jurists and philosophers who have engaged with the problem of when to trust an expert witness have put forward a number of tools that can be used by non-experts to assess the reliability of putative experts. Below, we will present and systematize these tools. More importantly, we will show that the notion of robustness makes a valuable contribution to this toolbox. On our definition, the robustness of scientific evidence *qua* evidence for a certain hypothesis is a measure of how likely it is that additional attainable evidence would alter the probability of the hypothesis. Hence, and as explained in more detail below, the robustness of scientific evidence is in part a measure of the extent to which the available tools for evaluating expert testimony have been put into use. In other words, the notion of robustness allows courts to assess scientific evidence *without* actually putting the suggested tools into use. This is important, considering that applicable procedural rules often constrain courts' mandate to initiate further investigations.

The crucial question of when to trust an expert has engaged both jurists and philosophers. Within common law, judges and legislators have developed criteria for the admissibility of expert testimony. A well-known example is the so-called *general acceptance test* which was first laid down in *Frye v. United States 293 F. 1013, D.C. Circ., 1923*. The Court in *Frye* held that in order to be admissible, expert testimony must be based on scientific principles and discoveries that are "sufficiently established to have gained general acceptance in the particular field" (at 1024). Another, more recent example is *Daubert v. Merrell Dow Pharmaceuticals 509 U.S. 579 (1993)*, where the court referred to the works of Karl Popper and Carl G Hempel and identified testability, peer review, error rate and general acceptance as criteria for determining the reliability of expert testimony. In *General Electric Co. v. Joiner, 522 U.S. 136 (1997)*, the Supreme Court later stated that nothing in the Daubert guidelines requires a court "to admit opinion evidence which is connected to existing data only by the *ipse dixit* of the expert" (at 137), and thereby implicitly encouraged courts to scrutinize the inferences underlying expert testimony. In the subsequent *Kumho Tire Co. v. Carmichael, 526 U.S. 137 (1999)*, the Court explained that the Daubert criteria might apply to non-scientific expert testimony too, depending on "the particular circumstances of the particular case at issue" (at 150). (Similarly, the discussion in this article focuses on

robustness assessment of scientific expert testimony, but is applicable to relevantly similar non-scientific evidence too.)

In philosophy, Douglas Walton, Alvin Goldman and others have contributed to the development of criteria by which non-experts can evaluate an expert's statement. Walton has devised a list of critical questions that non-experts can use to challenge an argument from expert opinion. The list includes questions regarding the alleged expert's education, experience and personal reliability (Walton 1997, 223):

> *Expertise* question: How credible is E as an expert source?
> *Field* question: Is E an expert in the field that A is in?
> *Opinion* question: What did E assert that implies A?
> *Trustworthiness* question: Is E personally reliable as a source?
> *Consistency* question: Is A consistent with what other experts assert?
> *Backup evidence* question: Is A's assertion based on evidence?

Similarly, Goldman has identified and discussed five sources of evidence that a non-expert can use in determining the reliability of expert testimony: "arguments presented by contending experts", "agreement from additional putative experts", "appraisal by 'meta-experts' of the expert's expertise", "evidence of the expert's interests and biases" and "past track records" (Goldman 2001, 93).

The referred discussions identify measures that a non-expert can take to review an expert's testimony. Roughly put, the notion of robustness adds to this picture that it estimates the relevance of the inquiries made, as compared to those omitted. Hence, a robustness estimate requires consideration not only of the degree to which possible inquiries have been performed, but also of the omitted inquiries' capacity to alter the probability of the hypothesis.

The various inquiries so far discussed are to a large extent over-lapping. In this section, we will propose a tentative taxonomy (summarized in Figure 1 below) arranged according to the different aspects of expert testimony that these inquiries address. As will be elaborated below, this taxonomy can be used as a basis for courts' robustness assessments. First, we note that some of the measures for reviewing expert testimony that have been put forward in the literature relate to the reliability of the expert's *person* whereas others relate to the reliability of the expert's *reasoning*. These two different objects provide the first partition in our taxonomy. We will refer to reviews of the expert's person

as *ad hominem reviews* and to reviews of the expert's reasoning as *de re reviews*.

Ad hominem reviews challenge the reliability of an expert's opinion by drawing attention to attributes of the expert's person that put her reliability into question. This kind of review is likely to be the most obvious way for a non-expert to confront an expert's opinion. An argument that attacks an arguer's person rather than her reasoning is often treated as a fallacy (Copi and Cohen 2002, 143). However, in contexts like the present, where the arguer's reliability as a source is a relevant factor for trusting her conclusion in the first place, drawing attention to personal attributes that affect her reliability is both relevant and warranted (Walton 1997, Hahn et al. 2009, Dahlman et al. 2011, Dahlman and Wahlberg 2015). Roughly, attributes of relevance to *ad hominem* reviews can be divided into two categories: those that relate to competence, and those that relate to motivation (Dahlman and Wahlberg 2015). The expert's *competence* is of obvious relevance to her reliability and moreover a factor that is relatively easy to assess. Not surprisingly, then, this is a factor that is frequently highlighted in discussions on the reliability of expert testimony. Thus, we have seen that Douglas Walton points out that we can ask critical questions about the expert's experience, education, and field of expertise (Walton 1997), and that Alvin Goldman advises us to make use of meta-experts and past track records to assess the expert's reliability in this respect (Goldman 2001). Similarly, professional organisations have carved out standards that their members must meet when testifying as expert witnesses. For example, the American Psychological Association's (2013) and the British Psychological Associations' (2010) demand that expert witnesses possess the psychological and legal knowledge, experience, training, and required skills to perform the requested expert role. The competence should be established either by professional certification or by providing proof of active practice and up-to-date knowledge in the area in which the expertise are requested. An expert's *motivation* is perhaps more difficult to control, but likewise a factor that is regularly stressed as relevant. Walton (1997) and Goldman (2001) both point at the importance of taking into account evidence of the expert's interests and biases, and many legal rules and policies go as far as to treat secondary interests as reasons for disqualification.

In contrast, *de re* reviews address not the expert's person, but her reasoning. By definition, an expert's reasoning is in part a result of knowledge and skills that the non-expert lacks. *Prima facie*, it is more difficult for a non-

expert to call in question an expert's reasoning than to call in question the expert's competence or motivation. At closer look, however, several ways in which non-experts can contest an expert's reasoning can be discerned. To begin with, *de re* reviews can address the *external strength* of the expert's assumptions and conclusions and examine how her opinion relates to external factors, such as available evidence and the views of other experts. Thus, Walton (1997) suggests assessors to ask questions about the evidence that backs up the expert's assertion as well as about how well the assertion accords with the views of other experts. Goldman (2001) recommends consideration of arguments presented by contending experts and the level of agreement from additional putative experts. Another example of the relevance of external strength is provided by the so called *general acceptance test* formulated in *Frye v. United States 293 F. 1013, D.C. Circ., 1923*, in which the Court ruled that to be admissible, expert testimony must be based on scientific principles and discoveries that are "sufficiently established to have gained general acceptance in the particular field". In the superseding case *Daubert v. Merrell Dow Pharmaceuticals 509 U.S. 579 (1993)*, the Court mentioned peer review and general acceptance, which both relate to external strength, as criteria relevant for determining the reliability of expert testimony.

A *de re* review can also address the *internal strength* of an expert's reasoning. For example, the review can assess the consistency of the expert's own reasoning and examine to what extent the expert's conclusion follows from her premises. It should be noted that this kind of assessment addresses formal properties of the expert's reasoning and therefore does not necessarily require additional expert knowledge. In this vein, the Court in *General Electric Co. v. Joiner*, 522 U.S. 136 (1997) held that "a court may conclude that there is simply too great an analytical gap between the data and the opinion proffered" (at 146). As a parallel, many professional guidelines, such as the British Psychological Society's guidelines for psychologists as expert witnesses (2010) require experts to provide the court with criteria that allow the court to evaluate the basis of the expert's opinion (Standard 1.5). Internal strength can potentially also be assessed by generic quality criteria for scientific evidence. For example, in the spirit of Hempel and Popper, the *Daubert* court mentioned *testability* as a relevant criterion for assessing the reliability of scientific evidence. The idea seemed to be that testability is an intrinsic quality, which can be assessed *a priori*, without considering further evidence.

Finally, *de re* reviews can address the *relevance* of the expert's reasoning by assessing its relation to the questions at stake in the particular case (Walton 1997). An assessment of relevance requires that the expert's statement is sufficiently transparent to allow for inter-disciplinary comparisons. Many expert statements, such as "the accident didn't cause A's disability" may *appear* transparent but in fact contain implicit assumptions and values. There is hence a risk that these statements conceal significant epistemological differences between legal and scientific standards of proof, as well as ontological differences between legal and scientific notions (such as *cause* and *disability*), which can hinder effective cross-discsiplinary communication. (See for example Cranor 1993, and Shrader-Frechette and McCoy 1993. See also Wahlberg 2010 a and b for a comprehensive discussion on epistemological and ontological differences between law and science).

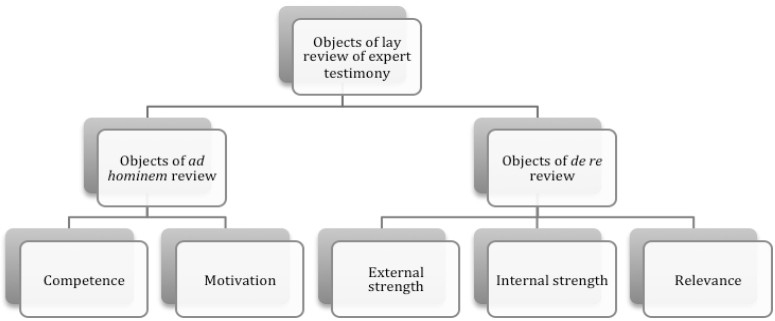

Figure 1. Lay review of expert testimony

Figure 1 above summarizes the objects of the above-discussed inquiries for reviewing expert testimony. A taxonomy of this sort can certainly be of help for those who might want to make further inquiries into the reliability of the expert's opinion. However, this taxonomy is also a potentially useful tool for those assessing the robustness of inquiries already performed. A robustness assessment is an estimate of how sensitive the current probability of the hypothesis is to additional inquiries, and the now proposed taxonomy can hence serve as a check-list for considering what inquiries a non-expert *could*

make. The assessor should then ask herself 1) to what extent such inquiries *have* been performed and 2) how likely it is that they (given her knowledge of their typical relevance) *would* alter the current probability of the hypothesis, if performed. For example, an assessor should ask to what extent measures such as further control of the expert's secondary interests (*ad hominem* review pertaining to motivation), and consultation of additional experts on the same topic (*de re* review pertaining to external strength) are likely to alter the probability of the statement to which the expert testifies. A typical case of low robustness with respect to external strength is at hand when the expert has stated her opinion but not disclosed the assumptions and premises that the opinion is based on, or explained what degree of support these premises and assumptions have in the scientific community. Insufficient robustness means that the evidence should be deemed not to meet the standard of proof required. If, on the other hand, it is likely that the current probability of the hypothesis will sustain in the light of additional evidence, the present evidence is robust. In the remainder of this article, we will show how a robustness evaluation along these lines could have been put into use by the courts in the infamous Swedish Thomas Quick cases, by many considered as the biggest scandal in Swedish legal history.

3. The Thomas Quick Case

The Säter Clinic is a psychiatric care facility in Mid Sweden, located in the Dalarna forest 200 km north west of Stockholm. It is a high security facility that treats convicted criminals who have been sentenced to forensic psychiatric care. In 1992 one of the patients at the Säter Clinic was a 42-year-old man called Thomas Quick. The name on his birth certificate was Sture Bergwall, but he had legally changed his name to Thomas Quick to disassociate himself from his father. Quick had been convicted for armed robbery, assault with a deadly weapon, and several sexual offences against young boys. He had been diagnosed with personality disorder and *pedofilia cum sadismus*. In the spring of 1992 Thomas Quick read a newspaper article about an unsolved police case from 1980, the disappearance of an eleven-year-old boy, Johan Asplund, in Sundsvall. The police suspected that Johan Asplund had been abducted and possibly killed, but in spite of extensive investigations his body had not been found. Thomas Quick told his therapist at Säter that the newspaper article

about Johan Asplund's disappearance gave him very uneasy feelings. He was not sure, but he had a feeling that he was responsible for what had happened to Asplund. Over the course of the following months Quick reached a point in his therapy sessions where he confessed that he had killed Johan Asplund, chopped up the body, and buried the pieces (Råstam 2012, 118-122). Quick said to his therapist that he wanted to take responsibility for his actions, and wished to contact the police.

The first police interview was conducted at Säter in March 1993. The police were impressed by Quick's vivid story of the killing, and he was escorted to Sundsvall to show the police to the location where he had buried the remains of Asplund. The police made several excavations on locations indicated by Quick, but no body parts or other evidence was found. After several months of interviews with Quick the police were stuck with a confession that was not backed up by any forensic evidence. In the meantime, Quick had confessed to several other killings. One of them was the murder of Charles Zelmanovits, a fifteen-year-old boy from Piteå who had disappeared in 1976. Some parts of Zelmanovits body, his skull and some bones dressed in decomposed clothes, had recently been found in the woods north of Piteå, and in September 1993 several Swedish newspapers had published articles about the unsolved case and the findings in the woods. The police interviewed Quick about Zelmanovits, and Quick explained that he had killed Zelmanovits and buried parts of the body in different places. A problem with Quick's confession was that he was not able to remember any details that would confirm that his confession was genuine. He was asked to describe the clothes that Zelmanovits was wearing but was not able to recall them correctly. In April 1994 the police called in Sven Å Christianson to help out with the investigation (van der Kwast 2015, 41-42). Christianson was a professor in psychology at Stockholm University, and an expert in issues related to memory. He suggested to the police that they should use a method known as the 'cognitive interview' (Fisher & Geiselman 1992) to help Quick recall details that he had difficulties to remember. In a cognitive interview the interviewer uses various techniques to have the subject mentally recreate and reenact an event. Sven Å Christianson describes in one of his scientific publications how the cognitive interviews with Quick were conducted.

> [...] memories of smells, body positions, various sounds and emotions were triggered. After the reinstatement of his internal context, he [Quick] showed strong emotions and could describe vivid memories of the killings. He was

> able to give specific details, which he had not had access to in previous interrogations (Christianson & Engelberg 1997, 241).

In November 1994 Thomas Quick stood trial for the murder of Charles Zelmanovits. The case presented by the prosecutor, Christer van der Kwast, consisted of Quick's confession, testimony from Detective Sergeant Seppo Penttinen, and an expert testimony from Professor Christianson. There was no forensic evidence. Penttinen testified about the interviews that he had conducted with Quick, and said that Quick had described several details about the vegetation on the location in the forest where the skull and bones had been found, and some details on how the remains of Charles Zelmanovits had been buried. Penttinen testified that Quick had been able to describe these details without information or help from him or other police officers. According to the prosecution, this proved that Quick had knowledge about the crime that only the killer could have, and, thereby, proved that Quick must be the killer.

Christianson had written an expert opinion about Thomas Quick that was submitted as evidence by the prosecution. It addressed the issue of false confessions and listed three circumstances that have been established by science to indicate the possibility of a false confession: 1) situations where the confessor is seeking attention, 2) situations where the confessor has something to gain by confessing, and 3) situations where the confessor is unsure about his own memory and is convinced by others that he is guilty.[1] Christianson was called by the prosecution as an expert witness and testified that Quick's confession was genuine (Josefsson 2013, 367). According to Christianson, there were no circumstances in the Quick case that indicated a false confession. Thomas Quick's defense attorney, Claes Borgström, did not question the prosecutor's case and did not bring in any evidence against it. Quick had instructed Borgström that he wanted to be convicted, and Borgström assisted him in accordance with this instruction. On 16 November 1994 Thomas Quick was found guilty of the murder of Charles Zelmanovits, and was sentenced to continued psychiatric care. The court says in its verdict that the testimony by Penttinen strongly supports that the murder was committed by Quick, and the testimony by Christianson strongly supports that Quick's confession was genuine.[2]

[1] Piteå Tingsrätt, B 179/94, Christianson, S.Å., Sakkunnigyttrande angående betingelser för Thomas Quicks (500426-7190) utsaga i psykologiskt avseende, p. 2-3.

[2] Piteå Tingsrätt, B 179/94, Dom 1994-11-16, p. 11-12.

Quick continued his treatment at the Säter Clinic and continued confessing murders in unsolved cases. In late 1994 he confessed to the murder of two Dutch hikers, Marinus Stegehuis and Janni Stegehuis, who had been stabbed to death in a tent at Lake Appojaure in Lapland, in the summer of 1984. Quick stood trial for double murder in January 1996, and was found guilty. Just like the Zelmanovits case, there was no forensic evidence. Quick was convicted on the testimony of Detective Sergeant Penttinen and Professor Christianson. For this trial, Christianson handed in an expert opinion that ended with the following statement.

> In this report I have discussed false confessions of various types ... Each and every one of these types fit badly with the circumstances of Thomas Quick's confession.[3]

Over the following years, Quick was convicted for five more murders that he had confessed. In May 1997, he was convicted for the murder of Yenon Levi, an Israeli tourist found dead in Hedemora, in June 1998 he was convicted for the murder of Therese Johannesen, a nine year old Norwegian girl who had disappeared in Drammen, in June 2000 he was convicted for the murders of Trine Jensen and Gry Storvik, two young Norwegian women who had been found dead on a parking lot in Oslo, and, finally, in June 2001, he was convicted for the murder of Johan Asplund, the very first murder he had confessed. None of the cases relied on forensic evidence. In each case, the court found that Quick's confession and the testimonies of Detective Sergeant Penttinen and Professor Christianson were enough a guilty verdict.

Parallel to his work as a consultant to the police Sven Å Christianson also took interest in Thomas Quick as a scientific research subject. Christianson was interested in the psychology of serial killers, and interviewed Quick in detail about his childhood and his emotions when he killed his victims. The result of this research was published by Christianson in a book (Christianson 2010, 401-421) entitled *Inside the Head of a Serial Killer* (*I huvudet på en seriemördare*).

However, not everyone was convinced that Thomas Quick was guilty. Some sceptics said that serial killers normally follow some sort of pattern, and pointed out that no such pattern could be seen in the killings that Quick had been convicted for. Some victims were men, others were women. Some victims

[3] Gällivare Tingsrätt, B 26/95, Christianson, S.Å., *Sakkunnigyttrande angående betingelser för Thomas Quicks (500426-7190) utsaga i psykologiskt avseende*, p. 9. See, also, Råstam 2012, p. 256.

where children, others were adults. The modus operandi was different for each crime. Some victims were strangled, other victims were stabbed, and some were clubbed to death with a heavy object. The crimes had been committed at various geographical locations, spread all over Sweden and Norway. Another circumstance that raised doubt about Quick's guilt was the sheer number of confessions. By 2001 he had confessed to 39 killings, and not even Sven Å Christianson believed that all of them were genuine (Christianson 2010, 86). In some cases, it was obvious that Quick's confession did not correspond to the truth. For example, he confessed that he had killed two Somali boys that were reported missing in 1996, not knowing that the boys had later been found and were alive and well. So, if some of Quick's confessions were false, could it not be the case that they were all false? In 2008, Hannes Råstam, an investigative reporter working for Swedish Television (SVT), started to take interest in the Quick case. Råstam went to the Säter Clinic to interview Quick, who had now changed his name back to his birth name Sture Bergwall. During the course of these interviews, Quick/Bergwall confided in Råstam that all of his confessions were false. He explained to Råstam that the psychologists at Säter and the police rewarded him for his confessions by giving him their undivided attention, and granting him extra doses of the medicine that he asked for (Råstam 2012, 94; Josefsson 2013, 461). At the time, Quick was heavily addicted to benzodiazepines. They even rewarded him for his confessions by granting him a leave of absence to go to Stockholm for a couple of days (a rather imprudent decision, considering that they were dealing with a confessed serial killer). Råstam digged deeper into the Quick confessions than anyone had done before, and unearthed a number of things that undermined the prosecution's case. Råstam went through transcripts and videos from the interviews that Seppo Penttinen had conducted, and showed that they were full of leading questions that had helped Quick "remember" the right details (Råstam 2012, 198-202, 206, 222-224, 268-269, 283-284, 297-299, 312-316, 322-325, 370-374).

In 2009 Sture Bergwall requested the court of appeal to order a retrial in the case of Yenon Levi. The request had been worked out by defense attorney Thomas Olsson, and was based on the withdrawal of the confession, in combination with the weaknesses in the police investigation that Råstam had exposed. The request for a retrial was granted, and in September 2010 Quick/Bergwall was acquitted for the murder of Yenon Levi. Subsequent requests for retrials were handed in for all of the other convictions. In June

2011 Quick/Bergwall was acquitted for the murder of Therese Johannesen, in August 2012 he was acquitted for the murder of Johan Asplund, in November 2012 he was acquitted for the murders of Trine Jensen and Gry Storvik, in July 2013 he was acquitted for the murders of Marinus and Janni Stegehuis, and in November 2013 he was acquitted for the murder of Charles Zelmanovits.

The Thomas Quick case is by many considered to be the greatest scandal in the history of Swedish criminal law, and the people responsible have been massively criticized. Christer van der Kwast, who prosecuted all of the cases, has been criticized for leading the investigation in a way that was strongly biased towards the theory that Quick was guilty, Seppo Penttinen has been accused of committing perjury, when he testified that he had not asked leading questions in his interviews, Christianson has been blamed for architecting the fatal interview technique practiced by Penttinen, and Borgström has been criticized for his passive performance as Quick's defense attorney. In contrast, the judges who convicted Quick have not been criticized. The general view seems to be that the judges cannot be blamed, since Quick appeared to be guilty, given the evidence that was presented to them in court. The evidence that exonerated Quick surfaced afterwards, and, as it were, you cannot blame the judges for not taking account of information that they did not have at the time. This line of reasoning overlooks that the standard of proof should not only require that the probability that the defendant is guilty, given the evidence that has been presented, meets the p*-standard. The standard of proof should also require that the probability that this assessment would not be changed by additional information meeta the p**-standard. The evidence must be robust. The evidence presented in the trials against Thomas Quick did not meet this requirement. The judges who convicted Quick should not have trusted Penttinen and Christianson blindly. The judges should have reviewed Penttinen's and Christianson's reliability with regard to *competence* and *motivation*, and they should have reviewed the value of their testimonies critically, with regard to *external strength*, *internal strength* and *relevance* (figure 1 above).

Penttinen testified that he had not helped Quick with leading questions, and the judges trusted him. If the judges had reviewed his testimony more critically with regard to *motivation*, they would have realized that Penttinen was in fact evaluating himself when he made this statement, and was therefore motivated to cover up any mistake that he might have made during the interviews. The transcripts and videos from the interviews should have been

studied by an independent expert, to check if Penttinen had helped Quick with leading questions. The absence of such an inquiry made the case weak with regard to robustness, and the judges should have realized this, instead of trusting Penttinen uncritically. If the transcripts and videos from the interviews had been evaluated by an independent interrogation expert, that expert would have found, like Råstam, that the interviews were full of leading questions.

Christianson testified that none of the circumstances that indicate false confessions were present when Thomas Quick made his confessions. As we have seen, Christianson made this assessment on the assumptions that Quick was not seeking attention and had nothing to gain from confessing. The evidence that was presented to the court included no information that supported these assumptions, and no information that supported the opposite assumptions. In fact, the evidence that the verdicts were based on did not include inquiries into these matters. If such evidence had been brought in, the court would have seen evidence to the effect that Quick was seeking attention and evidence that he had plenty to gain from confessing. If the newspaper reporters who covered the Quick case had been called to testify about their experiences when interviewing Quick, they would have testified that he was seeking attention. Quick was very keen to be interviewed and always made very theatrical statements that made good headlines ("I am an Evil Man"[4], "I Must Carry my Guilt"[5] etcetera). Quick even wrote newspaper articles himself about his case.[6] If the psychiatrists that treated Quick at the Säter Clinic had been called to testify, they would have informed the court that Quick was rewarded for his confessions with extra medication. If the medical records had been introduced as evidence, they would have shown that Quick received extra doses of benzodiazepines as a payment for his confessions. There is an entry in Quick's medical records from 1994 that reveals that the prosecutor van der Kwast put pressure on the staff at Säter to give Quick the pills he asked for, with the argument that Quick "must receive something in return" (Råstam 2012, 157). The judges never received the information that such inquiries would have produced, and they should have realized that the evidence that they were given was not robust, since these lines of inquiry had not been pursued. They should have realized that the missing lines of inquiry typically have the

[4] *Expressen*, 2 September 1994 ("Jag är en ond man").

[5] *Dagens Nyheter*, 10 April 1995 ("Jag måste bära min skuld").

[6] E.g. *Dagens Nyheter*, 12 July 1994 ("Jag flydde för att dö") and 1 January 1995 ("Jag kan bli frisk").

ability to change the picture completely. The probability that the case against Thomas Quick would not hold for additional information was so high, that he should not have been convicted on the existing evidence. As we have seen, an expert testimony based on assumptions that are not supported by evidence does not have the *external strength* to pass as robust evidence. It should be noticed that a critical review of the assumptions in Christianson's expert testimony can be conducted without expertise in psychology. As we observed above, it is problematic for judges to review expert testimony critically, as judges lack expert knowledge on the issue of the testimony (Learned Hand's Paradox). With regard to Christianson's assumptions, however, this does not pose a problem. You do not need any special expertise in psychology to see that a confessor who is rewarded with extra doses of a drug that he is addicted to has something to gain by confessing.

Furthermore, if the reliability of Christianson as an objective scientific expert had been critically reviewed with regard to *motivation*, the judges would have learned that Christianson also had another relationship with Quick. He was interviewing Quick for his study on the psychology of serial killers. This clearly put Christianson in a conflict of interests. The study of Quick as a serial killer relied on the assumption that Quick was guilty. If his confessions were false the entire study would be worthless. Christianson therefore had a strong personal interest in Quick being guilty. This information obviously undermines Christianson's reliability as an objective scientific expert. The court should not have trusted Christianson blindly. They should have reviewed his reliability critically, and they should have realized that the lack of inquires with regard to his motivation weakened the robustness of the evidence provided by his testimony.

In conclusion, the case against Thomas Quick was not robust enough for a conviction. It did not meet the p**-standard in any of the murders that he confessed. The judges who found him 'guilty beyond reasonable doubt' did not apply the standard of proof correctly. They should have acquitted him. Someone might say that this harsh criticism is unfair, since it is passed in hindsight, with all the information uncovered by Råstam and others, that the judges who convicted Quick did not have. We disagree with this defense of the incorrect convictions. It is true that the judges who convicted Quick did not know all that we know today, but they knew that they lacked information on many things, where additional evidence typically makes a difference, and they should have realized that this made the case against Quick insufficiently robust.

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Sweden

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The Expert Witness: Lessons from the U.S. Experience*

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ABSTRACT

The first section of this paper explains why assessing the worth of expert testimony poses special epistemological difficulties. The second traces the history of the various rules and procedures by means of which the U.S. legal system has tried to ensure, or at least control, the quality of the expert testimony on which it so often relies–from the *Frye* Rule, the Federal Rules of Evidence, and the *Daubert* trilogy to recent constitutional cases regarding the appearance of forensic witnesses in court and experiments with court-appointed experts and scientific education for judges. The third and final section suggests some lessons to be learned from the limited success of these efforts, and explores what might be better strategies going forward.

Keywords: epistemology; U.S. law on expert witnesses; *Frye* Rule; *Daubert* trilogy; court-appointed experts; *Melendez-Diaz* and its progeny; forensic science.

> In order that we may have the right to accept [another person's] testimony as ground for believing what he says, we must have reasonable grounds for trusting his *veracity*, that he is really trying to speak the truth as he knows it; his *knowledge*, that he has had opportunities of knowing the truth of this matter; and his *judgement*, that he has made proper use of those opportunities in coming to the conclusion which he affirms.
> W. K. Clifford (1877)[1]

> The whole object of the expert is to tell the jury, not facts ..., but general truths derived from his specialized experience. But how can the jury judge between two statements each founded upon an experience confessedly foreign in kind to their own? It is just because they are incompetent for such a task that the expert is necessary at all. [T]his is setting the jury to decide, where doctors disagree
> Judge Learned Hand (1901)[2]

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[1] W. K. Clifford, "The Ethics of Belief" (1877), in *The Ethics of Belief and Other Essays*, eds. Leslie Stephen and Sir Frederick Pollock (London: Watts & Co., 1947), 70-96, p. 79.

[2] Learned Hand, "Historical and Practical Considerations Regarding Expert Testimony," *Harvard Law Review* 15 (1901): 40-58, p. 54. (In Hand's text, the sentence I have put last here occurs *before* the rest.)

If you look in the back of the *ABA Journal*, the official publication of the American Bar Association, you will find advertisements offering expert-witness services—in a recent issue: A & A Legal Nurse ("plaintiff or defense"); Independent Lab Testing; Pediatrics Experts; Domestic Violence Researcher; Surety Expert; Franchise Expert Witness; Attorney-Endorsed Medical Experts; Hospital Medical Director; Jail/Prison Medical Director; Emergency Medicine/Medical Toxicology; Emergency Medicine/Trauma; Neurologist ("on faculty of prestigious university"); Neurosurgeon; Accredited Psychiatry & Medicine ("Harvard alumni & faculty"); Nursing Home Medical Director; Medical Expert Available for Social Security Disability Claims; Forensic Accounting.[3] As this suggests, the expert-witness business is booming.

As it also suggests, medical experts seem to be particularly in demand; but there are many, many other kinds of expertise on which attorneys and, sometimes, judges, call. In fact, the sheer variety of experts who play a role in litigation of one kind and another is overwhelming: experts on asbestosis, accident reconstruction, automobile design, the authenticity of works of art; experts on blood spatter, bite-marks, bullets, behavioral analysis, Bendectin;[4] experts on construction techniques, cancers, causation evidence, criminology; experts on DNA, domestic violence, denture adhesive; experts on engineering, economic losses, epidemiology, evolution, eyewitnesses, the valuation of real estate; experts on fingerprints, footprints, forensic document examination, future dangerousness, Fosamax,[5] the design of folding lawn-chairs; ..., etc., etc.—in fact, experts on just about *everything*; all the way through the alphabet.

Moreover, experts appear in cases of almost every kind. In the criminal justice system we encounter not only DNA analysts, fingerprint examiners, specialists on handwriting and documents, tool-mark experts, etc. but also (among many, many others) psychiatrists testifying about Post-Traumatic Stress Syndrome, Battered Woman Syndrome, Rape Trauma Syndrome, Child Sexual Abuse Accommodation Syndrome, etc., and psychologists testifying about the weaknesses of eyewitness testimony and memory. DNA analysts also turn up in, for example, paternity and immigration cases, and handwriting and document specialists in cases of contested wills. Epidemiologists, toxicologists, experts on occupational safety, representatives of virtually every

[3] *ABA Journal*, November 2014, pp. 68-69.

[4] A drug for the treatment of morning-sickness in pregnancy, alleged to cause limb-reduction birth defects. See p. 50 and note 39 below.

[5] A drug for the treatment of osteoporosis, alleged to cause osteonecrosis of the jaw in some patients.

conceivable medical specialty, even experts on "weight of evidence methodology," show up in toxic-tort and medical-malpractice cases. Historians of art, computer specialists, forensic accountants, and, once again, those forensic document examiners, show up in fraud cases. In *Brown v. Board of Education*, the landmark civil-rights case in which the Supreme Court held that segregated "separate but [supposedly] equal" schools for minority children are unconstitutional, the appellants' brief included an appendix summarizing a fact-finding report at the White House Conference on Children and Youth "bringing together the available social science and psychological studies related to ... how racial and religious prejudices influence the development of a healthy personality."[6] And in other constitutional cases we encounter (among many, many others) professors of religion testifying as to whether being obliged to attend public high schools after the age of fourteen would cause psychological damage to Old Order Amish adolescents,[7] specialists in biology, paleontology, biochemistry, sociology, and philosophy testifying as to whether Intelligent Design Theory is a scientific theory or a theological one, even a professor of theology testifying as to whether it's a good theological theory or a bad one,[8] ..., and so on, again almost without limit.

Heavily as it has come to depend on them, however, the U.S. legal system has always found expert witnesses problematic; and from the beginning there have been complaints about how readily such witnesses conform their opinions to the interests of the party that hires them, and how often, rather than clarifying the factual matters at issue in a case, they confuse or obscure them.

My purpose here is, first—giving the epistemological backdrop—to explain why expert witnesses pose special difficulties (§1); second—sketching the very complex legal history—to describe the various rules and procedures by means of which the U.S. legal system has tried to ensure, or at least control, the quality of the expert testimony on which it so often relies (§2); and finally, to suggest some lessons to be learned from the limited success of these efforts, and to explore what might be better strategies going forward (§3).

[6] Appellants Brief, Brown v. Bd. of Educ. of Topeka, Kan., 347 U.S. 483 (1954), WL 47265 (1952), section II, *8-*13.

[7] Wisconsin v. Yoder, 406 U.S. 205, 210-13 (1972).

[8] Edward Humes, *Monkey Girl: Evolution, Education, Religion, and the Battle for America's Soul* (New York: HarperCollins, 2007), 271-72. Kitzmiller v. Dover Area Sch. Dist., 400 F. Supp. 2d. 707 (M.D. Pa. 2005).

1. The Epistemological Backdrop: Weathering A Perfect Storm

A core epistemological question—I might even say, *the* core epistemological question—is how to assess the worth of evidence.[9] This question encompasses a series of nested sub-questions: among them, how to determine in an effective, reasonable way whether (or to what degree)[10] what another person tells us is credible. This in turn encompasses further sub-questions: among them, how to determine in an effective, reasonable way whether what an expert (or purported expert) tells us on specialized matters of which we have none but the vaguest and most general knowledge ourselves is credible. And this in turn encompasses yet further sub-questions: among them, the question mot relevant here—*how to determine in an effective, reasonable way whether what we are told by an expert witness in a legal context is credible*.

This already suggests a preliminary explanation of why expert witnesses have proven so problematic: assessing the worth of testimonial evidence always involves a kind of indirection; assessing the worth of specialized and technical evidence requires substantive knowledge of relevant facts; assessing the worth of testimony presented in court involves not only taking account of how what's said may be skewed by legal constraints or by the interests of the parties, but also resisting the temptation to allow the horrific nature of a crime or the terrible injury suffered by a plaintiff to skew our appraisal of the evidence that the responsibility falls on this person or that company. And when a juror, or a judge, has to assess the credibility of an expert witness in a legal setting, he encounters *all* of these problems at once—facing, as the sub-title of this section signals, a perfect epistemological storm.

The remarkable paper of W. K. Clifford's from which I took my first opening quotation, though often read simply as an epistemological critique of religious belief,[11] is also an important, though seldom-acknowledged, contribution to what would nowadays be called "social epistemology." And the short passage I quoted identifies the core of what's involved in deciding whether what another person tells you can be trusted: making a sound

[9] See, e.g., Susan Haack, *Evidence and Inquiry* (1993: second, expanded ed., Amherst, NY: Prometheus Books, 2009); "Epistemologia: Chi Ne Ha Bisogno?" *Epistemologia*, XXXIV, 2011: 268-88.

[10] I won't keep repeating this qualification, but it should be understood as implicit in what follows.

[11] In part, no doubt, because this was the paper to which William James was responding in an even more famous paper, "The Will to Believe." William James, "The Will to Believe" (1897), in Frederick Burkhardt and Fredson Bowers, eds., *The Will to Believe and Other Essays in Popular Philosophy* (Cambridge, MA: Harvard University Press, 1979), 13-33.

assessment of (i) his truthfulness (his "veracity," as Clifford says), and (ii) his competence on the matter in question (his "knowledge" and his "judgement").[12] When we ask for directions, for example, we grow skeptical if we notice that our informant hesitates and changes his mind ("go right at the traffic lights—oh, no, sorry, go left—oops, no, silly me, right"), or has a lengthy disagreement with his friends in their own language before he finally tells us to take the no. 7 tram and get off at the fifth stop; and if we're wise, we bear in mind that in certain parts of the world *machismo* may demand that our informant give us directions whether or not he knows the way!

Deciding whether what an expert tells us on some specialized matter can be trusted is significantly *more* difficult. Why so? Our knowledge, and our experience, is limited. That's why we ask the advice of a doctor about the treatment of these symptoms, a plumber about the cause of this leak, a financial advisor about the risks involved in this investment—because we take ourselves to be in need of their expertise. And when we have virtually *no* relevant knowledge or experience of our own, we have to rely almost entirely on experts' knowledge and experience. Of course, if we're wise, we'll take steps to find out whether the plumber telling us we need a whole new drain field makes most of his money replacing such systems; we'll seek a second medical opinion; we'll check out several financial advisors; and so on. If we're lucky, these common-sense precautions will be enough; but, as we all know to our cost, they won't always be.

When an eye-witness describes an accident or a crime, a reasonable juror will try to assess his truthfulness by looking to his demeanor and thinking about his motivation: is he matter-of-fact, or evasive? Is he hesitant, or perhaps too emphatically confident? Does he have reason to lie? And he will try to assess the witness's competence: was he actually present at the scene, and in a position to see? Was the light was adequate? Is he short-sighted, and if so, he was wearing his glasses? Had he seen the defendant elsewhere, or previously identified another person as the perpetrator? But the legal context introduces further complications: e.g., was the witness induced to testify in return for some concession on his own case? Was the police line-up conducted or the photo-array presented in such a way as to encourage him to identify the suspect as the person he saw? And so on.

[12] As I have argued elsewhere, however, Clifford's account of when it's reasonable to believe what a scientist tells us is somewhat naïve. Susan Haack, "Credulity and Circumspection: Epistemological Character and the Ethics of Belief," forthcoming in *Proceedings of the American Catholic Philosophical Association* (2015).

No wonder, then, that deciding whether an expert witness can be trusted is more difficult yet. As Judge Learned Hand said in the celebrated paper from which the second of my opening quotations was taken, in the legal context we will likely be faced by the *opposite* opinions of *competing* expert witnesses—any one of whom is better-equipped and better-qualified to form an opinion on the contested matter than we are. Of course, an expert witness won't just offer a bare opinion, but will normally explain what the evidence is on the basis of which he arrived at that opinion. But that evidence will often be couched in a technical vocabulary that we can, at best, only partially understand; and will, moreover, often rely on background factual assumptions the truth of which we can't judge for ourselves. Even to know what evidence is relevant to a claim, after all, let alone to judge how strong or how weak that evidence is, *requires substantive knowledge of the subject-matter*.[13] Is the fact that a child has this mitochondrial disorder relevant to whether she is especially susceptible to a bad reaction to the MMR (mumps, measles, and rubella) vaccine?[14] Is the fact that the concrete used in building this parking structure has this composition rather than that relevant to why it collapsed? Is the fact that this DNA sample from the crime-scene matches that sample from the defendant at 13 loci sufficient to establish that the chance that the match is random is a one in a million? Etc., etc.

In short:

- Determining whether or to what degree it's reasonable to believe what another person tells us is always in some degree indirect, involving reliance (implicit or explicit) on surrogate indicators of his truthfulness and his competence.
- Sometimes even this poses considerable difficulties; but determining whether or to what degree it's reasonable to believe what an expert tells us tends to be *more* indirect, and so *more* difficult, because it's harder to identify such indicators.
- Determining whether or to what degree it's reasonable to believe what a witness says in court involves *further* complications.
- And determining whether or to what degree to believe an expert witness in a legal context is *even more* difficult, because:

[13] As I argue in, e.g., Susan Haack, "Legal Probabilism: An Epistemological Dissent" (first published, in Spanish, in 2013) in Susan Haack, *Evidence Matters: Science, Proof, and Truth in the Law (*New York: Cambridge University Press, 2014), 47-77, p.61.

[14] See. e.g., Poling *ex rel.* Poling v. Secretary of Health & Human Services (2008), No. 02-1466 V, *1.

(i) There is likely to be competing testimony from an expert witness, or witnesses, on the other side; and a lay juror (or judge) is likely to understand the contested factual issues only, at best, in part.
(ii) Expert witnesses are not likely to volunteer information that might be damaging to the party by which they were hired, even if this information would be helpful to the fact-finder.[15]
(iii) Moreover, legal cases often involve ugly crimes or grave injuries evoking powerful emotions, which can impede jurors' (or judges') assessment of evidence.

It's hardly surprising, then, that modern legal systems struggle to handle expert witnesses effectively; nor that in the recent history of the U.S. legal system there have been many efforts to ensure that, rather than being a hindrance or an impediment, expert testimony helps juries reach factually correct, substantially just verdicts

2. The U.S. Experience: Tracing a Tortuous Path

The conventional contrast between "adversarial" and "inquisitorial" systems is too crude to capture what is really a complex mesh of differences, and commonalities. Still, for readers unfamiliar with common-law procedures, I'll start with a brief description of some key characteristics of the U.S. system, beginning with the stress on precedents, i.e., on decisions in earlier cases, and the division of labor between the judge, charged with determining questions of law, and the "fact-finder," normally a jury, charged with determining questions of fact. Most to the present purpose: witnesses, including expert witnesses, are prepared and presented by the parties, and subject to cross-examination by the other side;[16] and a whole battery of rules makes certain kinds of relevant

[15] As this reveals, we depend (usually implicitly) on informants' good will, specifically, their intent to be informative, as well as on their truthfulness and their competence.

[16] By contrast, I understand, in Italy experts—who may be consulted either on the court's initiative or at the request of a party—must be selected from a register of technical consultants kept at each tribunal, and are considered, not witnesses, but auxiliaries of the court. Mauro Cappelletti and Joseph M. Perillo, *Civil Procedure in Italy* (The Hague, the Netherlands: Martinus Nijhoff, 1965), pp. 230 ff. A more recent source tells me that "the new Italian system .... has retained the system of official experts—whilst giving the parties the right to designate an expert of their own, who can check the work of the official expert, and be heard with him or against him at trial." J. R. Spencer, "Evidence," in Mireille Delmas-Marty and J. R. Spencer, eds., *European Criminal Procedures* (Cambridge: Cambridge University Press, 2002), 641-87, p. 634. The

evidence (for example, illegally-obtained evidence, and any further evidence obtained as a result of such evidence) inadmissible—meaning that the jury should not hear it or, if they hear it anyway, should be instructed by the judge to put it out of their minds. The admissibility or inadmissibility of evidence is a legal question, and hence the province of the judge; the weight of evidence, its sufficiency or insufficiency to meet the standard of proof, is a factual question, and hence the province of the finder of fact. Proffered evidence will be excluded if the opposing party challenges it under the evidentiary rules, and the judge upholds the exclusion.[17]

The world, and the U.S. legal system, have changed significantly since Hand wrote—1901, the year before fingerprint evidence was first used in a criminal case.[18] Now, as then, medical experts of one kind and another turn up all the time; but by now, as we saw, the legal system also calls on a vast range of other kinds of expertise. Moreover, by now relatively few cases go to a jury; indeed, relatively few cases ever go to trial—the great majority of criminal cases are plea-bargained, the great majority of civil cases settled.[19]

Most to the immediate purpose, at the time of Hand's article the distinctive characteristic of an expert witness was that he was exempt from the "opinion rule," under which ordinary, lay witnesses were required to confine themselves to reporting their perceptions, and not permitted to offer their opinions;[20] and all that was necessary for the testimony of an expert to be admissible was that the witness be suitably qualified, and his proffered testimony relevant to some

reference to the "new" Italian system seems to be to the 1988 Code of Criminal Procedure. See Antionette Perrodet, supervised by Mario Chiavario, revised by Elena Ricci, "The Italian System," in the same volume, 248-414, pp. 349 and 379. (There have been several amendments to the code since 1988 but, so far as I know, none is relevant to what is said in this note.)

[17] Moreover, except in cases of egregious legal error, evidentiary determinations cannot be appealed unless they were previously challenged at trial. The U.S. evidentiary regime is, in short, thoroughly adversarial.

[18] Michael Kurland, *Irrefutable Evidence: Adventures in the History of Forensic Science* (Chicago: Ivan Dee, 2009), p. 93.

[19] See, e.g., Marc Galanter, "The Vanishing Trial: An Examination of Trials and Related Matters in Federal and State Courts," *Journal of Empirical Legal Studies* 1, no.3 (November 2004): 459-70; Marc Galanter and Angela M. Frozena, "A Grin without a Cat: The Continuing Decline and Displacement of Trials in American Courts," *Daedalus* 140, no.3 (Summer 2014): 115-28.

[20] This is why—taking the crucial point to be that an expert witness, unlike a lay witness, isn't confined to testifying to his experience but may give his opinion—Hand tells us that the first case he can find of "real expert testimony," i.e., of the conclusions of skilled persons being submitted to the jury, was *Alsop v. Bowtrell* (1620); where physicians testified that a child born to a woman "forty weeks and nine days" after her husband died might well be his child. Hand, "Historical and Practical Considerations" (note 2 above), pp. 46-47.

fact at issue in the case. By now, the opinion rule has been relaxed somewhat;[21] and legally speaking, the distinctive characteristic of an expert witness is simply that he has specialized knowledge, skill, education, or training beyond that of the average juror.[22] Expert witnesses are still given much more latitude in expressing their opinions than lay witnesses are; but, as we'll soon see, the requirements for the admissibility of their testimony are now significantly more complex and demanding than they were a century ago.

(i) *Judicial Screening of Expert Testimony: The* Frye *Rule*

This part of the story begins with *Frye v. United States*,[23] a 1923 murder case in which, for the first time, a court placed restrictions not only on *the qualifications of a proffered expert witness*, but also on *the content of proffered expert-witness testimony*. James Alfonso Frye was accused of murdering a physician. He had confessed; but subsequently withdrew his confession. At trial, his attorney had proffered an expert who would testify that Frye had been subjected to a (then very new) blood-pressure deception test,[24] which allegedly showed that he was telling the truth when he claimed that his confession had been false– he *hadn't* killed Dr. Brown. This proffer was denied; and, in a very short, citation-free ruling, arguing that the technique underlying the proffered testimony was just too new to have established its evidentiary credentials, the D.C. Court of Appeals upheld the trial court's decision. The key passage reads:

> Just when a scientific principle or discovery crosses the line between the experimental and demonstrative stages is difficult to define. Somewhere in this twilight zone the evidential force of the principle must be recognized, and while courts will go a long way in admitting expert testimony deduced from a well-recognized scientific principle or discovery, the thing from which the

[21] Federal Rule of Evidence 701 allows a lay witness to testify to opinions or inferences "rationally based on [his] perception," provided that these inferences are *not* based on scientific, specialized, or other technical knowledge.

[22] How to apply this very pragmatic conception of an expert has, however, sometimes been controversial. In *Downing*, for example, the trial court had denied the defendant's proffered (supposedly) expert testimony about the unreliability of eyewitnesses on the grounds that jurors would already know that eyewitnesses aren't very reliable: a claim that seems, at least in light of what we know now, dubious in the extreme. United States v. Downing, 735 F.2d 1224 (1985). Hal Arkowitz and Scott O. Lillefeld, "Why Science Tells Us Not to Rely on Eyewitnesses," *Scientific American*, 1.8.2009, available at http://www.scientificamerican.com/article/do-the-eyes-have-it/1 (last visited 9.1.15).

[23] Frye v. United States, 293 F. 1013 (D.C. 1923).

[24] The test–much simpler than a modern polygraph–measured changes in the subject's systolic blood-pressure under questioning. *Id.*, 1013.

> deduction is made must be *sufficiently established to have gained general acceptance in the field in which it belongs*.[25]

How is a judge to tell when a scientific principle or discovery has crossed the line between the "experimental" and the "demonstrative" stages? Judge Van Ordsel declines to say.[26] How is a judge to identify the "principle or discovery" on which proffered scientific testimony is based? Again, he doesn't say. And neither does he tell us how large a majority of those in a field must accept an idea if it is to qualify as "generally" accepted, or explain how fields are to be identified or individuated.

It's clear, however, that the "*Frye* Rule" (as it came to be known) is *conservative in intent*, i.e., meant to exclude as-yet untested, highly speculative scientific ideas; and that it does this by *deferring to the judgment of those in the field concerned*. It's also clear that it's very flexible—not to say easily manipulated. In particular, while *Frye* can be quite demanding if "the field to which [novel scientific testimony] belongs" is construed broadly, it can be very easily satisfied if the field is construed narrowly. This flexibility probably partly explains why, in due course, *Frye* proved so attractive.

"In due course" because, for many decades, *Frye* was rarely cited, and when it was, was usually construed as a precedent for excluding lie-detector evidence.[27] But over the years its influence grew. In fact, by 1975, when Congress ratified the Federal Rules of Evidence (FRE) to codify existing evidentiary practice, some version of the *Frye* Rule—usually in the abbreviated form of a requirement that novel scientific testimony be generally accepted it its field—was accepted in many jurisdictions, and construed, not just as excluding polygraph evidence, but as restricting the admissibility of novel scientific evidence of whatever kind.[28]

[25] *Frye* (note 23 above), 1014 (my italics).

[26] *Frye* is usually taken to apply only to "novel" scientific testimony. This can't mean simply that the idea or technique at issue is completely new *to the relevant scientific community*, since by definition such evidence couldn't have gained *any* acceptance in its field; nor can it mean simply that the idea or technique is new *to the legal system*, since this would mean that it could never get its foot in the legal door in the first place. It must mean, I assume, that scientific testimony should be excluded until it has gained acceptance in its field but, after that, be admitted as no longer "experimental" or novel.

[27] Kaminski v. State, 63 So. 2d339 (Fla.1953). On the history of the *Frye* Rule, see Paul C. Giannelli, "The Admissibility of Novel Scientific Evidence: *Frye v. United State*s, a Half-Century Later," *Columbia Law Review* 80 1980): 1197-1250.

[28] "While some courts have rejected the general acceptance standard, there remains considerable support for the *Frye* test." *Id.*, 1228.

(ii) *Judicial Screening of Expert Testimony: The Federal Rules of Evidence*

Section 7 of the FRE concerned opinion evidence generally. FRE 702 governed the admissibility of *expert* opinion testimony—whether scientific or not, whether novel or not. It made no mention of *Frye*, or of "general acceptance," but simply provided that an expert "qualified by specialized knowledge, education, skill, or training" might testify in the form of an opinion if his testimony would be helpful to the fact-finder, and was not otherwise legally excluded. Did this mean that FRE 702 had superseded *Frye*, at least in federal jurisdictions? Courts disagreed. Was FRE 702 best construed as requiring, simply, that such testimony be relevant to facts at issue in the case, or did being helpful to the fact-finder implicitly require something more, that expert testimony have some degree of reliability? Again, courts disagreed.

In *Barefoot* (1983),[29] for example, the Supreme Court brushed aside the suggestion that the defendant's constitutional rights were violated when psychiatric testimony that he would be dangerous in future was admitted—even though there was reason to believe that such predictions were wrong more often than they were right.[30] Both state and federal law, Justice White argued for the majority, anticipate that relevant evidence will be admitted, and its weight left to the jury to decide;[31] moreover, the defense had had the opportunity to challenge the reliability of the contested psychiatric testimony through cross-examination, and could have presented contrary witnesses.[32] But in *Downing* (1984) a federal court of appeals ruled that the lower court had erred in excluding psychological testimony about the factors that influence whether, and when, the testimony of an eyewitness is likely mistaken; and argued explicitly that judges should screen expert testimony not only for relevance, but also for reliability.[33]

By the early 1990s, the status of *Frye* under the new Federal Rules was still unresolved. Amid pressure for tort reform and complaints that bad science was flooding the courts, Peter Huber argued in *Galileo's Revenge*[34] that, while the

[29] Barefoot v. Estelle, 446 U.S. 880 (1983).
[30] *Id.*, 883, 898, 899.
[31] *Id,* 898. The testimony was undeniably relevant, since the Texas death-penalty statute required that, at the sentencing phase, jurors determine beyond a reasonable doubt that there is a probability that the defendant would be dangerous in future.
[32] *Id.*, 899.
[33] *Downing* (note 24 above).
[34] Peter Huber, *Galileo's Revenge: Junk Science in the Courtroom* (New York: Basic Books, 1993).

old *Frye* Rule had served to keep "junk science"[35] out, the more liberal Federal Rules of Evidence had opened the floodgates. Huber's legal history was all wrong; in point of fact the *Frye* Rule had virtually always been confined to criminal cases.[36] Nevertheless, his book struck a chord with those clamoring for reform of the tort system; and by 1991 then-Vice-President Dan Quayle was proposing, *inter alia*, adding a requirement of "widespread acceptance" in the field to FRE 702.[37]

In 1993, however, the Supreme Court stepped in, giving its first-ever ruling on the standard of admissibility of expert testimony, the status of the *Frye* Rule in federal jurisdictions, and the interpretation of FRE 702: *Daubert v. Merrell Dow Pharmaceuticals*.[38]

(iii) *Judicial Screening of Expert Testimony: The* Daubert *Trilogy*

Daubert was just one of many toxic-tort cases involving the morning-sickness drug Bendectin, which was alleged to cause limb-reduction birth defects in some of the babies born to women who took it.[39] Legally, however, *Daubert* was distinctive: it was a very rare instance of a civil case where the trial court had referred to "general acceptance," and the Court of Appeals had specifically cited *Frye*;[40] and hence provided the perfect opportunity to clarify the legal situation. FRE 702 *had* superseded *Frye*, Justice Blackmun wrote for a unanimous Supreme Court; but federal courts' responsibility to screen proffered exert testimony remained.[41]

This screening, Justice Blackmun continued—but now only on behalf of the majority—should ensure that expert testimony admitted is both relevant *and reliable*.[42] And how are courts to screen for reliability? The text of FRE 702 refers to "scientific, specialized, or other technical knowledge." But the

[35] By analogy, I assume, with "junk food," such as the burger that looks and smells like real food, but has no nutritional value. I don't know where the phrase originated, only that apparently it was Huber who made it famous.
[36] Kenneth J. Cheseboro, "Galileo's Retort: Peter Huber's Junk Scholarship," *American University Law Review* 42 (1993): 1637-1726.
[37] See Diana Culp Bork, "Reasonable Tort Reform," *The National Law Journal*, 14 (September 30, 1991): pp. 17, 18, 21.
[38] Daubert v. Merrell Dow Pharm., Inc., 509 U.S. 579 (1993) ("*Daubert* 1993").
[39] See e.g., Joseph Sanders, *Bendectin on Trial: A Study in Mass Tort Litigation* (Ann Arbor, MI: University of Michigan Press, 1996). Bendectin was withdrawn from the U.S. market in 1984; according to the manufacturers, this was because of the costs of litigation, not because the drug posed any real danger. It returned to the U.S. market (now under a new name, Diclegis, and made by a Canadian company) in 2013.
[40] *Daubert* 1993 (note 38 above), 583-84.
[41] *Id.*, 589.
[42] *Id.*, 591, citing *Downing* (note 22 above).

testimony at issue in *Daubert* was, specifically, scientific (epidemiological, toxicological, etc.). So, quietly dropping the phrase "technical or other specialized" from the text of the Rule, Justice Blackmun argued that such testimony should be genuine "scientific ... knowledge": i.e., really knowledge, not mere opinion,[43] and genuinely scientific. Being genuinely scientific, he continued, means arriving at your conclusions by the scientific method.[44] So courts should look, not to the conclusions an expert draws, but exclusively to the "methodology" he uses in arriving at those conclusions.[45]

In determining "evidentiary reliability," Justice Blackmun added, courts might consider these indicia of reliability (soon known as the "*Daubert* factors"):

- whether the theory or technique can be (and has been) tested;
- whether the theory or technique has been subjected to peer review and publication;
- the known or potential rate of error, and the existence and maintenance of standards controlling the operation of the technique in question;
- whether the theory or technique has gained widespread acceptance in its field[46]

The first of these—a result of Justice Blackmun's unfortunate confusion of "reliable" and "scientific"[47]—reflects a half-understood, quasi-Popperian misconception of the supposed "scientific method."[48] The second in part reflects the mistaken idea that peer-reviewed publication is a sign of the widespread acceptance mentioned in the fourth,[49] which is in turn a nod to the old *Frye* Rule. The third—though it looks potentially helpful with respect to, e.g., questionable forensic identification techniques, or those predictions of

[43] *Id.*, 589-90. Not surprisingly, potential scientific witnesses whose testimony is excluded by a judge who deems it not really scientific—the phrase is "Dauberted out"—are sometimes indignant at what they perceive as an insult to their professionalism.
[44] *Id.*, 590.
[45] *Id.*, 592-93.
[46] *Id.*, 593-95.
[47] See Susan Haack, "Trial and Error: Two Confusions in *Daubert*" (2005), in Haack, *Evidence Matters* (note 13 above), 104-21.
[48] See Susan Haack, "Federal Philosophy of Science: A Deconstruction—and a Reconstruction" (2010), in Haack, *Evidence Matters* (note 13 above), 122-55; "Just Say 'No' to Logical Negativism" (first published in Chinese in 2011), in Haack, *Putting Philosophy to Work: Inquiry and Its Place in Culture* (Amherst, NY: Prometheus Books, 2008, expanded second ed., 2013), 179-97 (text) and 298-305 (notes).
[49] See Susan Haack, "Peer Review and Publication: Lessons for Lawyers" (2007), in Haack, *Evidence Matters* (note 13 above), 156-79.

future dangerousness–is notably silent on the matter of *what* "known or potential" error-rate might disqualify expert scientific testimony as too unreliable to be admitted.[50]

Rather than make a final determination on the Dauberts' claim, after settling the question of the standard of admissibility the Supreme Court remanded the case to the ninth circuit. And in the course of this final ruling–once again deeming the Dauberts' expert testimony inadmissible, and once again granting summary judgment to the defendant company, Merrell Dow–Judge Kozinski added what is sometimes described as a fifth *Daubert* factor to the list:

- whether the work on which the testimony is based was litigation-driven, or undertaken independently of litigation[51]

–the idea being that scientific work undertaken for the purposes of litigation is inherently less likely to be reliable than work conducted in the normal course of scientific business. But in a footnote Judge Kozinski made an important exception: even though the testimony of forensic scientists is always litigation-driven, "the fact that [such an] expert has developed an expertise primarily for purposes of litigation will obviously not be a substantial consideration."[52] He doesn't say why not.

After *Daubert*, federal judges really did find themselves, as Judge Kozinski had observed, in a "Brave New World."[53] *Daubert* is much broader in scope than *Frye*; and it obliges judges, rather than deferring to the relevant scientific (or other expert) community, *to make determinations about the reliability of scientific expert testimony for themselves*–even though, as then Chief-Justice Rehnquist had pointed out in his partial dissent, they are untrained for such a task. The word "reliability" nowhere occurs in the text of FRE 702, Justice Rehnquist noted; Justice Blackmun's observations about falsifiability were baffling, sure to create confusion in the courts below; moreover, the stress on

[50] It is regrettable, in my opinion, that these dicta have now found their way into the entries under "scientific knowledge," "scientific method," and "falsifiability" in the most recent edition of *Black's Law Dictionary*. Bryan A. Garner, ed., *Black's Law Dictionary* (St. Paul, MN: Thomson Reuters, 10th ed., 2014).
[51] Daubert v. Merrell Dow Pharm., Inc., 43 F.3d 1311 (1995) ("*Daubert* 1995").
[52] *Id.*, 1317, n.5. See also Susan Haack, "What's Wrong with Litigation-Driven Science?" in Haack, *Evidence Matters* (note 13 above), 180-207.
[53] *Daubert* 1995 (note 51 above), 1315.

"scientific method" threatened trouble down the road about whether, and if so how, *Daubert* applied to *non-scientific* expert testimony.[54]

His dissent proved prophetic. Just a few years after constructing the new *Daubert* regime, the Supreme Court began quietly *de*constructing it. The first thing to go, in the second of the Supreme Court's trilogy of cases on expert testimony, *General Electric v. Joiner* (1997), was the distinction between methodology and conclusions that had played a starring role in 1993. The core legal issue in this case was the standard of appellate review of decisions excluding such testimony; which, the Court ruled, remained the same—abuse of discretion—even when, as happened in *Joiner*, excluding one side's expert testimony determined the outcome of the case.[55] Moreover, the ruling continued, the lower court had *not* abused its discretion in excluding the experts Joiner had proffered to show that his occupational exposure to PCBs[56] had promoted his lung cancer.[57]

But Joiner's attorneys had argued that their experts used precisely the *same* methodology that G.E.'s experts used—"weight of evidence" methodology—so that the lower court must have looked beyond their experts' methodology to their conclusions; which, under *Daubert*, was legally an error. Sidestepping this argument, Justice Rehnquist wrote for the majority that a court may legitimately conclude that "there is simply too great an analytical gap between the data and the opinions offered." He doesn't tell us what an "analytical gap" is, nor how courts are to judge when an analytical gap is "too great." But he does add, firmly: "[C]onclusions and methodology are not entirely distinct from one another."[58] As Justice Stevens observed in his partial dissent, this was already a significant shift away from *Daubert*.[59]

And, precisely as Justice Rehnquist had predicted, Justice Blackmun's dicta about "scientific ... knowledge" left federal courts puzzling over whether *Daubert* applied to non-scientific exert testimony and, if it did, whether those *Daubert* factors also applied. As one judge put it, federal courts were "balkanized":[60] some held that both *Daubert* and the *Daubert* factors applied

[54] *Daubert* 1993 (note 38 above), 598-601 (Justice Rehnquist, dissenting in part).
[55] Gen. Elec. Co. v. Joiner, 522 U.S. 136 (1997).
[56] Polychlorinated biphenyls (a class of man-made organic compounds). The production and sale of PCBs has been banned in the U.S. since 1977, after they were found to be seriously carcinogenic.
[57] *Joiner* (note 55 above), 143.
[58] *Id.*, 146.
[59] *Id.*, 151 (Justice Stevens, dissenting in part).
[60] Moore v. Ashland Chemical Inc., 152 F.3d 269, 280 (5th Cir. 1998) (Judge Dennis, dissenting).

to all expert testimony, scientific or otherwise;[61] some that *Daubert* and *a fortiori*, the *Daubert* factors, applied only to scientific testimony.[62] One court conducted a *Daubert* hearing as a result of which it determined that, since forensic document examination testimony wasn't science, *Daubert* didn't apply;[63] another seemed to fall into hopeless confusion: "[Mr. Bihlmeyer's testimony] will assist the jurors to understand whether or not there is a design or manufacturing defect involved in this case. So to the extent the *Daubert* case is applicable, it's applicable."[64]

In 1999, stepping in to settle whether, and if so how, *Daubert* applied to non-scientific experts, the Supreme Court continued the deconstruction of *Daubert* begun two years earlier in *Joiner*. The specialized evidence at issue in *Kumho Tire Co. v. Carmichael*[65]—the testimony of an expert on motor tires that the blowout that caused Mr. Carmichael's accident was the result of faulty design, not of the tire's having been abused—was by no stretch of the imagination scientific. The lower courts had been divided, with the trial court holding that this evidence was inadmissible because it flunked all the *Daubert* factors, and the appeals court reversing on the grounds that *Daubert* only applied to scientific testimony.[66] The Supreme Court split the difference: *Daubert* applied here, as to all expert testimony; those *Daubert* factors, however, might or might not be relevant, depending on the nature of the expert testimony in question.

Justice Breyer wrote for a (nearly)[67] unanimous Court:

> [W]e can neither rule out nor rule in, for all cases and all time, the applicability of the factors mentioned in Daubert, nor can we now do so for subsets of cases categorized by category of expert or kind of evidence. [...] Indeed, those factors do not necessarily apply even in every instance in which the reliability of scientific testimony is challenged. It might not be surprising in a particular case, for example, that a claim made by a scientific witness has never been the subject of peer review, for the particular application of it at

[61] See. e.g., Berry v. City of Detroit, 25 F.3d 1342 (6th Cir. 1994).
[62] See, e.g., Carmichael v. Samyang Tires, Inc., 131 F.3d 1433 (11th Cir. 1997).
[63] United States v. Starzecpyzel, 880 F. Supp. 1027 (S.D.N.Y. 1995).
[64] Compton v. Subaru of American Inc., 83 F.3d 1515, 1517 (10th Cir. 1996) (citing the District Court's ruling).
[65] Kumho Tire Co., Ltd. v. Carmichael, 525 U.S. 137 (1999).
[66] Carmichael v. Samyang Tires, Inc. 923 F. Supp. 1514 (S.D. Ala. 1996). *Samynang Tires, Inc., v. Carmichael* (note 62 above).
[67] There are two brief partial dissents, neither of which is relevant to present concerns. *Kumho Tire* (note 65 above), 158-59.

> issue may never previously have interested any scientist. Nor, on the other hand, does the presence of Daubert's general acceptance factor help show that an expert's testimony is reliable when the discipline itself lacks reliability... .[68]

He says nothing, however, about how a judge is to determine whether a discipline has, or lacks, reliability. And what, in the end, does this key passage tell federal judges about how to assess the reliability of proffered expert testimony?—That they should use any, all, or none of the *Daubert* factors, and/or such other factors as they deem appropriate; i.e., that they should use their judgment, and do the right thing.

It's true, and important, that what matters isn't (as Justice Blackmun had suggested) whether expert testimony is scientific, but whether it's really knowledge.[69] It's true, and important, that the sheer variety of kinds of expertise with which courts may have to deal makes it impossible to give indicia of reliability that will work for any and every kind of expert testimony. And it's true, and important, that widespread acceptance of an idea, theory, or technique in some field is no indication of its reliability unless the field itself is legitimate. All that said, however, the fact remains that, since this third ruling in the *Daubert* trilogy, federal courts are left with large responsibility and broad discretion in screening expert testimony in all its nearly limitless variety, but little substantive guidance about how to do this.

In 2000, FRE 702 was revised so as to say explicitly what, according to the Supreme Court, it had said implicitly all along. It was "restyled" in 2011,[70] but its content remains the same:

> A witness who is qualified as an expert by knowledge, skill, experience, training or education may testify in the form of an opinion or otherwise if:
> (a) the expert's scientific, technical or other specialized knowledge will help the trier of fact to understand the evidence or to determine a fact in issue;
> (b) the testimony is based on sufficient facts or data;
> (c) the testimony is the product of reliable principles and methods; and
> (d) the expert has reliably applied the principles and methods to the facts of the case.

[68] *Id.*, 150.
[69] *Id.*, 138.
[70] That is, re-written for style but not changed in content.

Arguably, by suggesting that courts need to look not only at the pedigree of the testimony, but also at its application to the case at hand, this makes a small epistemological step forward; but that ritual incantation of "reliable," "reliably," "sufficient," does nothing to guide judges as to how, in the specific, to screen those proffered expert witnesses.

(iv) *Judicial Education*

As Justice Rehnquist had observed, their training hardly prepared federal judges for the new responsibilities imposed on them by *Daubert*. The year after *Daubert*, however, the National Institute of Justice issued the first edition of its *Reference Manual on Scientific Evidence*.[71] A second edition appeared in 2000,[72] and a third in 2011.[73] By now a very substantial volume, the *Reference Manual* brings together chapters by different specialists, both scientific and legal, on various scientific topics (the nature of science, DNA analysis, epidemiology, probability theory, etc., etc.), likely to be encountered at trial. Naturally, the chapters are variable in quality and accessibility to a lay reader; naturally, they occasionally focus more on judicial rulings than on strictly scientific matters. Still, the manual is a useful tool for judges needing to mug up on some scientific topic pertinent to a case—though sometimes, probably, they use it, instead, as a source of authoritative-sounding quotations to bolster the conclusion they would have reached anyway. But of course, even this big brick of a book can't cover *every* scientific topic that might be legally relevant; and it doesn't touch non-scientific kinds of expertise at all.

There have also been various programs offering judicial education on various scientific topics. Some are now defunct: e.g., the "Science for Judges Program," held for several years at Brooklyn College of Law, ended in 2007; the Federal Judicial Center's educational programs, publications, and videos are no longer current on its website; the long-standing program Advanced Science and Technology: Adjudication Resource (ASTAR) lost its funding in 2013. Some are still active: e.g., in April 2015 the Law and Economics Center at George Mason Law School held a conference on Forensic Statistics. Others are in prospect: e.g., at the National Commission on Forensic Science—a joint effort by the Department of Justice and the National Institute of Standards and Technology (part of the Department of Commerce) created in 2013—one

[71] *Reference Manual on Scientific Evidence* (Washington, D.C.: Federal Judicial Center, 1994).
[72] *Reference Manual on Scientific Evidence* (Washington, D.C.: Federal Judicial Center, 2000).
[73] *Reference Manual on Scientific Evidence* (Washington, D.C.: Federal Judicial Center, 2011).

committee focuses on "Training on Science and Law," and will "explore mechanisms for judges, lawyers, and forensic scientists to engage in collaborative training ... "; the fifth planning meeting [!] took place in January 2015. And the newly-created National Courts and Science Institute (NCSI) includes a Center for Basic and Continuing Judicial Science Education that will oversee annual conferences and a judicial certification in "scientific method, tools and measures."[74] But, while there are, and have been, many admirable efforts to improve judges' education in the sciences, these are at best a drop in the bucket—and scientific testimony, remember, is by no means the only kind of expert testimony a judge may need to screen for admissibility.

(v) *Court-Appointed Experts*

Aware (like Justice Rehnquist) that, since *Daubert*, federal judges' new responsibilities for screening expert testimony posed formidable difficulties, Justice Breyer had urged in his concurrence in *Joiner* that they use their powers under FRE 706 to appoint expert witnesses of their own choosing.[75] Some did. But this process proved less straightforward than, perhaps, Justice Breyer anticipated—and *much* less straightforward than Judge Hand, who apparently thought it would handle all the main problems with expert witnesses,[76] imagined it would be.

By now, there have been many experiments with court-appointed experts, the best-known and most ambitious of which was Judge Samuel Pointer's National Science Panel of four scientists charged with sifting through the medical evidence in the thousands of silicone breast-implant cases consolidated to his court. In 1998, the panel reported that that there was no evidence that, as the plaintiffs claimed, these implants caused connective-tissue diseases. But Judge Pointer's experience revealed all too clearly just how hard it is to identify competent experts who have no connection to one or other of the parties. Indeed, despite all his efforts to ensure neutrality, *all* the panel

[74] See generally, John Cher, "Judges and Lawyers Work to Understand Courtroom Science," *New York Law Journal* 252, no.3 (July 2014), p.1, column 3. On the program at George Mason, see George Mason University Law and Economics Center, "The Value of Judicial Training in Quantitative and Scientific Methods," Draft White Paper, April 8, 2013, available at www.masonlec.org; on the NCSI, see http://ncsi.institute; on the Federal Judicial Center's programs, see www.fjc.gov. On the National Commission on Forensic Science, see p. 61 and note 101 below.

[75] *Joiner* (note 55 above), 149 (Justice Breyer, concurring). FRE 706 reads in part: "The court may appoint any expert that the parties agree on and may appoint expert witnesses of its own selection."

[76] Hand, "Historical and Practical Considerations" (note 2 above), pp. 56-57.

members turned out to have some such indirect connections; and one, a Canadian rheumatologist—who had been chosen in part because the U.S. Association of Rheumatologists had already taken a position on the safety of these implants—was found, while serving on the panel, to have solicited, and received, funds from one of the defendant companies to support a professional conference.[77]

The same year, with much less fanfare and at much more modest cost, Judge Robert E. Jones had appointed expert advisors to help him assess the expert evidence in the much smaller number of silicone breast-implant cases Judge Pointer had returned to his district for trial; after which he excluded all testimony to the effect that the implants cause connective-tissue disorders.[78] But this wasn't entirely reassuring, either, given that—unlike Judge Pointer, who had kept the selection process at arms' length by having a team of advisors choose his panel members—Judge Jones had asked his medical-scientist cousin to help him choose suitable experts to advise him; moreover, all but one of his experts came from the same university.[79] On top of which, in the end, Judge Jones simply disregarded the opinion of one of the experts he had himself appointed.[80]

We have also learned—as should have been obvious from the beginning—that, since the science involved in litigation is often controversial, there is absolutely no guarantee that court-appointed experts won't disagree among themselves.[81] None of this is to deny that, in some circumstances, the use of

[77] The conference was on an unrelated topic. However, the plaintiffs moved that Dr. Tugwell's appointment to the panel be vacated, and the panel's report be withdrawn; but Judge Pointer denied this motion. See: "Breast implant plaintiffs say scientific panel was tainted," CNN Interactive (April 13, 1999), available at http://www.cnn.com/HEALTH/990413/breast.implant.panel (last visited February 22, 2015); In re Silicone Gel Breast Implants Products Liab. Litig., MDL 926, Case No. CV 92-P-10000-S, Order 311 (denying plaintiff's "Motion for Relief from Prejudicial Bias"); and, more generally, Laural L. Hooper, Joe S. Cecil, and Thomas E. Willging, "Assessing Causation in Breast Implant Litigation: The Role of Science Panels," *Law and Contemporary Problems* 64, no.4 (Autumn 2001): 139-89, 170-71.

[78] Hall v. Baxter Healthcare Corp., 947 F. Supp. 1387 (D. Ore. 1996). Judge Jones used his authority under FRE 104 (a), appointing these scientists, not as witnesses, but as advisors to the court. See generally Hooper, Cecil, and Willging, "Assessing Causation in Breast Implant Litigation" (note 77 above). Joseph Sanders and D. H. Kaye, "Expert Advice on Silicone Implants: *Hall v. Baxter Healthcare Corp.*," *Jurimetrics* 37 (Winter 1997): 113-28.

[79] Hooper, Cecil, and Willging, "Assessing Causation in Breast Implant Litigation" (note 77 above), p.146-47 and n. 33.

[80] Judge Jones set aside Dr, Greenlick's critique of the idea that a showing of more than doubled risk is necessary for proof of specific causation. See Susan Haack, "Risky Business: Statistical Proof of Specific Causation," in Haack, *Evidence Matters* (note 13 above), 264-93, p. 284.

[81] See, e.g., Soldo v. Sandoz Pharm. Corp., 244 F. Supp. 2d434 (W. D. Pa. 2003).

court-appointed experts may be the best option; but it has not turned out to be the panacea Judge Hand hoped.

(vi) *Confrontation and Cross-Examination*

Justice Blackmun's rhetoric had suggested that *Daubert* was intended to liberalize the standard of admissibility of expert testimony. *Frye* was "an austere standard," he wrote, at odds with the "liberal thrust" of the FEE. [82] But in practice the effect of *Daubert*, *Joiner*, and *Kumho Tire* seems to have been to make the standard of admissibility not less, but more restrictive—at least, in civil cases.

The effect of *Daubert* on criminal cases, however, has been much less:[83] as witnessed, for example, by the numerous failed *Daubert* challenges to fingerprint-identification testimony.[84] And yet there's reason to suspect that some of the forensic sciences (such as hair analysis or bite-mark identification), and some of the psychiatric specialties (such as supposedly "recovered" memories, or predictions of future dangerousness) that have played a significant role in the criminal justice system are distinctly *un*reliable; and even DNA identification evidence, the "gold standard" of forensic science, is susceptible to the same kinds of human error—sloppiness, mislabeling or contamination of samples, confirmation bias, etc.—as other forensic sciences.

But when issues about forensic evidence in criminal cases came to the fore in *Melendez-Diaz* (2009), the Supreme Court's attention was focused, as in *Barefoot*, not on the minutiae of judicial screening for admissibility of expert testimony, but on constitutional matters, specifically on the implications for forensic evidence of the Confrontation Clause of the Sixth Amendment to the U.S. Constitution, guaranteeing defendants the right to confront witnesses against them.

Luis Melendez-Diaz had been convicted of drug-trafficking. At trial, the prosecution had provided three sworn certificates of analysis affirming that the substance the police had seized from him was cocaine. The defendant's objection that the analysts concerned should testify in person was overruled; the appeals court upheld the decision; and the Supreme Judicial Court of

[82] *Daubert* 1993 (note 38 above), 588 ("liberal thrust") and 598 ("austere standard").

[83] As, perhaps, Judge Kozinski's footnote on forensic science in *Daubert* 1995 prefigured. See, e.g., Peter J. Neufeld, "The (Near) Irrelevance of *Daubert* to Criminal Justice and Some Suggestions for Reform," *American Journal of Public Health* 95 (2005): S107-13.

[84] See, e.g., U.S. v Havvard, 117 F.Supp. 2d 848 (S.D. Ind. 2000); U.S. v. Mitchell, 365 F.3d 215 (3rd Cir. 2004).

Massachusetts denied review. The U.S. Supreme Court, however, reversed and remanded, ruling 5-4 that Melendez-Diaz had the right under the Confrontation Clause to have these analysts appear in court to testify and be cross-examined.[85] But four Justices, led by Justice Kennedy, worried that this ruling might have the consequence of requiring *all* the several technicians who may be involved in conducting a single forensic test to appear in court. Since "the defendant does not even dispute the accuracy of the analysts' work, confrontation adds nothing," Justice Kennedy notes; and yet, by obliging forensic scientists to set aside their real work in the laboratory to go testify in court, "for the sake of ... negligible benefits, the Court threatens to disrupt forensic investigations across the country ... ."[86]

Since then, the Supreme Court has been struggling to articulate exactly what makes a forensic report "testimonial" in the legally-relevant sense (and so, subject to the Confrontation Clause), and exactly *which* forensic analysts should be required to testify, and why. A couple of years after *Melendez-Diaz*, in *Bullcoming v. New Mexico* (2011), the judgment of the Court was that the Confrontation Clause required that the technician who actually completed the form reporting the results of the defendant's blood-alcohol test should testify; the testimony of his laboratory supervisor was insufficient.[87] The technician in question, Curtis Caylor, was on unpaid leave; his supervisor didn't know why; and Bullcoming's counsel had had no opportunity to ask questions that might have revealed whether he was removed from his work station for incompetence or dishonesty.[88] Once again, Justice Kennedy dissented. This decision, he argued, went well beyond *Melendez-Diaz*: in this instance an employee of the testing laboratory *did* appear in court to authenticate the findings and be cross-examined.[89] What would the presence of the technician who actually signed the form have added?—after all, the test in question was run on the gas chromatograph overnight, after everyone had already gone home.[90]

But the following year, in the plurality ruling in *Williams v. Illinois*,[91] Justice Alito—who had been with the minority in *Melendez-Diaz* and

[85] Melendez-Diaz v. Massachusetts. 557 U.S 305 (2009). U.S. Constitution, Amendment VI (Confrontation Clause).
[86] *Id.*, 339 ff. (Justice Kennedy, dissenting).
[87] Bullcoming v. New Mexico, 131 S.Ct. 2705 (2011).
[88] *Id.*, 2715.
[89] *Id.*, 2723.
[90] *Id.*, 2724.
[91] Williams v. Illinois, 132 S.Ct. 2221 (2012).

Bullcoming–wrote the judgment of the Court. At Williams' trial for rape, an expert witness had referred to the DNA profile submitted by Cellmark[92] as having been produced from semen found in the victim's vaginal swabs. Justice Alito echoes the language of the rule excluding hearsay evidence,[93] designed to combat the same evil as the Confrontation Clause–reliance on the word of an out-of-court declarant who can't tested under cross-examination: this witness was not testifying as to the *truth* of the claim that the DNA profile was produced from the victim's swabs, he argues; and so did not trigger the requirement that the technician(s) concerned appear in court to testify.[94] And this time it was Justice Kagan–who had argued in support of the decisions in *Melendez-Diaz* and *Bullcoming*–who wrote an impassioned dissent. Focusing on the power of cross-examination to reveal mistakes, incompetence, sloppiness, and dishonesty, she opens with an extraordinary excerpt from the transcript of a trial where a forensic witness realized only after cross-examination that, oh my God, she had the names on the DNA samples mixed up: "I'm a little hysterical right now, but I think .... the two names should be switched."[95]

The story is certainly disturbing. But, recalling that most criminal cases (up to 95% in some jurisdictions)[96] are resolved by plea-bargain and never go to trial, one wonders whether there aren't better ways to avoid this kind of forensic fiasco. An ounce of prevention, as the saying goes, is worth a pound of cure. Shouldn't the priority be to do what we can to ensure that such mistakes don't happen in the first place?

(vi) *Forensic Science Laboratories*

A thought much like this, apparently, motivated another important development the same year as *Melendez-Diaz* when, at the instigation of Congress, the National Research Council (NRC) of the National Academies of Science (NAS) produced a substantial volume entitled *Strengthening Forensic*

[92] Cellmark is a company (founded in 1987) based in the U.K. which also provides forensic DNA testing in the U.S. See http://www.cellmark.foresnics.us/welome-cellmark-forensics (last visited 2.9.15).

[93] FRE 801 (c), defining "hearsay," tells us that a hearsay statement is one that "a party offers in evidence to prove the truth of the matter asserted... ."

[94] *Williams* (note 91 above), 2240.

[95] *Id.*, 2264 (Justice Kagan, dissenting), referring to the rape trial of John Kozack, Tr. in no. SCD 110465 (Super. Ct. San Diego Cty., Cal., Nov. 17 1995).

[96] Murphy, Erin, 2014. "The Mismatch between Twenty-First Century Forensic Evidence and our Antiquated Criminal Justice System," *Southern California Law Review* 87: 633-72, p. 661.

Science in the United States,[97] suggesting ways in which the quality of forensic work might be improved *at the source*. The NRC notes, as I did earlier,[98] that the *Daubert* trilogy has done disappointingly little to improve the quality of forensic testimony:

> *Daubert* and its progeny have engendered confusion and controversy. [...] Federal appellate courts have not with any consistency or clarity imposed standards ensuring the application of scientifically valid reasoning and reliable methodology in criminal cases involving *Daubert* questions.[99]

Even a summary of the NRC's recommendations–which contain much good sense[100]–would require a paper of its own; the point I want to stress here is that this report looks for ways to strengthen the practice of forensic science before it ever gets to court, not to control the admissibility of forensic testimony or flush out mistakes and dishonesty in such testimony at trial. The report has prompted the establishment of the National Commission on Forensic Science referred to earlier (2013);[101] a report from the Subcommittee on Forensic Science of the National Science and Technology Council Committee on Science on what could be done to implement the NRC recommendations, including estimates of the cost of, e.g., implementing a proficiency-testing program (2014);[102] and, the same year, policy recommendations from the National Institute of Standards and Technology on the presentation of expert testimony.[103] But, as far as I can determine, the substantive changes that the NRC proposed remain largely prospective.

[97] National Research Council, *Strengthening Forensic Science in the United States: A Path Forward* (Washington, D.C.: National Academies Press, 2009).

[98] I mean, both earlier in this paper (p. 59 above) and several years before the NRC report. See Susan Haack, "Trial and Error" (2005), in Haack, *Evidence Matters* (note 13 above), 104-21, pp. 116, 120.

[99] *Id.*, p. 11 (citing Neufeld, "The (Near) Irrelevance of *Daubert* to Criminal Justice" (note 83 above). The NRC's somewhat naïve reference to "scientifically valid reasoning" and "reliable methodology" is worthy of note.

[100] As well as the predictable calls to set up and fund a new federal body, a National Institute of Forensic Science, to establish and enforce better practices at forensic laboratories, and for funds for more research.

[101] Department of Justice, *National Commission on Forensic Science* (2013).

[102] National Science and Technology Council Committee on Science Subcommittee on Forensic Science, *Strengthening the Forensic Sciences* (Washington, DC: Office of the President, 2014).

[103] National Institute of Standards and Technology, Presentation of Expert Testimony: Policy Recommendations (October 29, 2014). Some of the recommendations seem very sensible; others, e.g., that "[e]xperts should remain neutral, and attorneys should respect this neutrality," sound to me like whistling in the dark.

3. Lessons to be Learned: Shifting, and Broadening, our Focus

All these efforts to control the quality of expert testimony—though none could be described as an unqualified success—have brought to light what a tangled mesh of problems, theoretical and practical, courts' handling of such testimony involves, among them:

- the extraordinary variety of fields of expertise—some stronger, some weaker, and some so feeble as scarcely to constitute real fields of expertise at all;
- the existence of more and less competent practitioners in every field, even the strongest;
- the guild mentality that affects some areas, perhaps especially the weaker forensic sciences;
- the conceptual difficulties of distinguishing scientific from other kinds of expertise (or for that matter "hard" from "soft" science);
- and the squishiness of the idea of "methodology";
- the elusiveness of the contrast between the neutral expert and the biased, and the potential for tension between an expert's being "neutral" and his being competent to the task;
- etc., etc.

They should also have taught us a number of other important lessons.

Our experience with the Frye *Rule*[104] *should have taught us* that the very reasonable thought that consensus among scientists in a field is the best indication a lay judge can have that this theory or that technique can be trusted isn't quite as helpful as it initially seems. It should have been obvious long before Justice Breyer's ruling in *Kumho Tire* that the fact that, e.g., a psychiatric theory or a forensic technique is "generally accepted" in its field is little or no assurance of its reliability when the field itself is weak, small, cliquish, and/or self-serving.

Some purported fields of "specialized knowledge, experience, skill, or training"—mind-reading, say, astrological prediction, or phrenological

[104] Frye is still the law in a number of states. According to Demosthenes Lorandos and Terence Campbell, "Mental Health Experts: Science and the Law," *Cross Examining Experts in the Behavioral Sciences* (St. Paul, MN: Thomson Reuters, 2014), §1:16.1, notes 4 and 5, 36 states are now "*Daubert* or *Daubert*-leaning," and 12 continue to use *Frye* (in the text, however, the authors seem to have miscounted!).

diagnoses of criminal personality—are really no such thing.[105] But mostly it's a matter of degree, of more reliable forms of expertise, and less. DNA identifications, properly conducted, are much more reliable than hair analysis or bite-mark identifications; physical analysis of the canvas, paint, varnish, etc., of a painting probably more reliable than historians' intuitive judgment of its likely date and provenance; a chemical analysis of a drug likely more reliable than a sociological analysis of the roots of crime. Consensus in a "field" that is misconceived or fraudulent is *no indication* of reliability; more generally, consensus in a field is a *less robust indication* of reliability, the weaker the field in question.

And unfortunately—as we in Florida know from the long-running saga of Joseph Ramirez, convicted three times of a stabbing murder on the basis of a knife-mark examiner's testimony that he could identify this specific knife, to the exclusion of all other knives in the world, as the one that made the half-inch wound in the victim's neck[106]—it's all too easy for a tightly-knit guild of specialists in a relatively weak field, because *they* all agree it works, to convey a quite unjustified sense that their technique is sound enough for a jury to hear.

Our experience with the Daubert *trilogy should have taught us* that the preoccupation with the demarcation of science and the question of "methodology" was, at best, a distraction; that to ask judges to assess the reliability of any and every kind of specialized knowledge, technique, or skill imposes a burden they are ill-equipped to carry; and that, because of the huge range of types of kinds of expertise, guidelines for determining whether proffered expert testimony is reliable inevitably to end up leaving judges with plausible-sounding verbal formulae the effective application of which requires them—well, as I said earlier, to "use their judgment, and do the right thing."

[105] To give a more realistic example: in the 1980s testimony about supposedly "recovered" memories played a significant role in numerous cases of alleged sexual abuse of small children. Some events are so traumatic, the theory was, that all memory of them they will be blocked from consciousness; but the memories are still there, unsuspected, and may return spontaneously years later, or be brought to consciousness in therapy or under hypnosis. Whether or not this theory is true, it's not clear that, absent independent evidence of abuse, genuine instances of recovered memories can be reliably distinguished from false "memories" planted, consciously or otherwise, by therapists, or simply cooked up in the imaginations of the suggestible or psychologically disturbed. See generally Daniel Brown, Alan W. Scheflin, and D. Corydon Hammond, *Memory, Trauma Treatment, and the Law* (New York: W. W. Norton, 1998); and, on the handling of recovered memory testimony since *Daubert*, Robert Timothy Reagan, "Scientific Consensus on Memory Repression and Recovery," *Rutgers Law Review* 51, no.2 (Winter 1999): 275-321.

[106] The story is summarized in Ramirez v. State, 810 So. 2d 836 (Fla. 2001).

Justice Blackmun's confusion of "reliable" with "scientific" diverted courts' attention from what should have been an obvious fact: that *not all* scientific experts are reliable—some are honestly mistaken, some incompetent, some self-deceived, and probably a few outright dishonest; and *not only* scientific experts are reliable, either—I'm sure there are reliable experts in forensic accounting, the valuation of real estate, computer hacking, "lifecare" costs,[107] etc., etc., too. Moreover, by equating "reliable" and "scientific," *Daubert* focused courts' attention on whether proffered expert testimony is, or isn't, science. Some cases are clear: the expert testimony of an epidemiologist or a toxicologist is scientific evidence; the expert testimony of an art historian or a theologian is not. But where does a physician's differential diagnosis fall,[108] or a psychiatrist's testimony as to a defendant's fitness to stand trial, or a construction engineer's testimony that the building collapsed because the joists used weren't strong enough, or ...., etc.? Are social-scientific experts to be held to the same standards as natural-scientific experts, or should the bar be set lower for the "soft" sciences than the hard?[109] But their preoccupation with such questions didn't do much to help courts ensure the quality of expert testimony.

And by suggesting that what makes expert testimony scientific is that it uses the "scientific method" to arrive at its conclusions,[110] *Daubert* generated a fruitless, and sometimes laughable, preoccupation with "methodology." Looking at all the evidence and using your judgment about the degree to which it warrants a causal conclusion was elevated to the status of "Weight of Evidence Methodology";[111] fingerprint examiners' procedure of analyzing and comparing prints and then asking a second examiner whether he agrees there's a match became the "ACE-V [analysis-comparison-evaluation-verification]

[107] Experts who calculate the medical and related costs that a personal-injury victim will incur over his or her lifetime.

[108] See, e.g., *Moore v. Ashland Chemical* (note 60 above), 280 (Justice Dennis, dissenting).

[109] See, e.g., Nenno v. State, 970 S.W.2d 482 (Tex. Crim. App.1998).

[110] I have argued elsewhere that there *is no* "scientific method," at least if what that means is a method used by all scientists and only scientists, and responsible for the success of scientific inquiry. See Susan Haack, *Defending Science—Within Reason: Between Scientism and Cynicism* (Amherst, NY: Prometheus Books, 2003), especially chapter 4; "Six Signs of Scientism" (first published, in Chinese and Spanish, in 2010) in Haack, *Putting Philosophy to Work: Inquiry and Its Place in Culture* (note 48 above), 105-120 (text) and 278-83 (notes).

[111] See Susan Haack, "Proving Causation: The Weight of Combined Evidence" (2008), in Haack, *Evidence Matters* (note 13 above), 208-38.

methodology";[112] even Dennis Carlson, the tire-design expert in *Kumho Tire*, claimed to have a methodology: "visual inspection methodology"[113]—meaning, apparently, that he looked at the blown-out tire!

And now, since the Supreme Court abandoned the distinction between methodology and conclusions in *Joiner* and acknowledged, in *Kumho Tire*, that what's important isn't, after all, whether expert testimony is scientific, but whether it's reliable, the impossibility of identifying operationally effective indicia of reliability applicable to any and all of the host of potentially legally-relevant fields of expertise is unmistakable.

Granted, if judges were better-educated scientifically, they would be better able to use their judgment and do the right thing. But *our experience with scientific education for judges should have taught us* that—while it's certainly desirable that they have some understanding of, e.g., the ways in which epidemiological studies may be well, or poorly, designed and conducted, the basics of probability theory, how the scientific peer-review system really works, why a DNA identification is almost certainly more reliable than a fingerprint match to a latent print that amounts to 20% of one finger, ..., etc.—there's simply no way to bring (let alone keep) judges up to speed on every kind of expert testimony with which they may be faced. There are just too many potentially legally-relevant fields of expertise. It's impossible to solve the problem Judge Hand drew to our attention more than at century ago, that we set laymen to decide where experts disagree, by making judges experts on *everything*.

Our experience with court-appointed experts should have taught us that, while such experts are by definition "non-partisan" in the superficial sense that they weren't hired by one or other of the parties to a case, it's extraordinarily difficult to ensure even that these experts have no conflict of interest, let alone that they are neutral in an epistemologically robust sense. In fact, it's simply not realistic to expect to find someone competent to the task with *no* opinion, *no* tendency to conclude one way or the other, at the outset. In short, while it's common to hear "biased" experts contrasted with "neutral" ones, and sometimes assumed that this is co-extensive with the contrast between experts hired by a party and experts appointed by a judge, we should know by now that this is a big muddle.

[112] *Mitchell* (note 84 above), 221. It's also worth noting that the "verification" stage of this procedure is sometimes elevated to the status of "peer-review." *Id.*, 238.
[113] *Kumho Tire* (note 65 above), 146.

When the parties to a case choose expert witnesses, naturally they seek out specialists in the field who will testify in a way that favors their side; and naturally they prepare their expert witnesses to offer the strongest testimony they can. Moreover, expert witnesses often seem to become increasingly dogmatic as they testify over and over. But the fact that an expert is chosen by a judge, rather than by one of the parties to a case, doesn't guarantee "neutrality" even in the sociological sense of "having no professional contact, direct or indirect, with either party," let alone in the epistemological sense of "having no preconceived opinion."

In fact, it's not clear that this would be desirable even if it were feasible. In specialized medical-scientific fields such as those at issue in the silicone breast-implant cases and the like, anyone competent to offer an opinion will almost certainly have *some* professional interaction, direct or indirect, with others who have some professional interaction, direct or indirect, with one of the parties. Moreover, as I said, anyone competent to offer an opinion on some specialized matter will surely have *some* ideas on contested issues ahead of time. A supposed "expert" unaware that a drug with an atomic weight of less than 1,000 taken by a pregnant woman can cross the placental barrier, and so *might* harm the fetus,[114] for example, would surely not be competent to opine on whether a morning-sickness drug is teratogenic.[115] And when, as is almost always the case in litigation of this kind, the science at issue is thus far unsettled, even the most competent, honest, and conscientious experts may, quite reasonably, disagree.

Our experience with the ongoing saga of Melendez-Diaz *and its aftermath should have taught us* that, if several forensic technicians had to testify in court in every criminal case that goes to trial,[116] our already-overburdened forensic services might soon be even less adequate to their task than they are now; but also have prompted the thought that, while cross-examination *may* reveal crucial weaknesses in forensic testimony, there's not only absolutely no guarantee that it will, but also, when a case is resolved by plea-bargain, a guarantee that it won't.

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[114] Rock Brynner and Trent Stephens, *Dark Remedy: The Impact of Thalidomide and Its Revival as a Vital Medicine* (Cambridge, MA: Perseus Publishing, 2001), p. 12.

[115] That is, causes birth defects.

[116] An appendix to Justice Breyer's concurrence in *Williams* indicates that as many as 13 different analysts may be involved in producing a single DNA profile. *Williams* (note 91 above), 2253-55.

Am I saying, then, that we should just throw up our hands and admit that the problems with expert witnesses are insoluble? No. I *am* saying, though, that improving the quality of exert testimony will require a recognition that we face, not *one* problem, but a whole tangle of interrelated problems. It was never realistic to hope that any legal form of words, however carefully crafted, could by itself enable judges or jurors to discriminate reliable expert testimony from unreliable; nor that court-appointed experts would prove a simple solution to all the problems; nor that cross-examination would always flush out the weak or dishonest expert; nor, ..., etc.

But, once we acknowledge the tangled complexity of the issues, we might see how to make some headway going forward. Rather than focusing on how to tweak the rules of evidence or to boost the role of cross-examination, I suggest, we should think about what could be done (i) to reduce the incidence of bad stuff reaching the courts in the first place, and (ii) to increase the likelihood that it will get exposed quickly if it does. This would mean (as the sub-title of this section says) *shifting* our focus to earlier in the process, and *broadening* it to include more just legal rules and procedures.

Just as I reached this point of the paper, an article in the *Wall Street Journal* described the fallout from the discovery that one technician in the Boston drug-testing lab, the now nationally-notorious Annie Dookhan, had been faking her results: more than 40,000 convictions tainted, and now a big legal brouhaha, with the American Council for Civil Liberties (ACLU) asking that all the potentially tainted cases be reopened and the District Attorney arguing that each such case should be dealt with individually, plus a dispute over what to do about defendants who accepted plea-bargains.[117] Ms. Dookhan's malfeasance was discovered in 2011, when her supervisor caught her taking ninety samples from the evidence vault without signing them out; but it had begun even before she joined the lab nine years earlier: she had lied about her qualifications on her job application.[118] How, I wonder, might we have ensured that such gross dishonesty would be discovered sooner?

[117] Jennifer Levitz, "Lab Flaws Cast Doubt on Drug Convictions," *Wall Street Journal*, January 8, 2015, A3.

[118] Sean K. Driscoll, "'I Messed Up Bad': Lessons on the Confrontation Clause from the Annie Dookhan Scandal," *Arizona Law Review* 56, no. 3 (2014): 717-40. Brian Ballou and Andrea Estes, "Chemist Admitted Wrongdoing in Lab Scandal," *Boston Globe* online (September 26, 2012), available at http://www.bostonglobe.com/2012/09/26/chemist-annie-dookhan-lab-scandal-investigators-messed-bad/uORxid5JamieMK1wumq2uO/story.html.

You might think that Ms. Dookhan's misconduct would surely have been exposed if she had been cross-examined. But no: in the three years before her arrest she *was* cross-examined—in around 150 trials; and no defense attorney ever uncovered even one of her faked and skimped tests.[119] Moreover, the ongoing legal dispute over the disposition of all those plea-bargained cases in Boston,[120] and the similar recent scandals at forensics labs in New, York, Delaware, and Colorado,[121] all bring home the point that, even if cross-examination were better at uncovering forensic malfeasance than, apparently, it is, it would have been far better had the management of such labs been more attentive, their culture healthier, and their hiring practices more vigilant, etc., in the first place.

Well, yes, you may say; but surely this isn't a lesson that can be extrapolated beyond the forensic sciences. Not in any simple way, I agree; but the underlying thought—that it's better, so far as it's possible, to prevent a problem than to fix things later—applies here, too. Think of the storm of litigation over those silicone breast-implants, which had been "grandfathered in" when the FDA's (Food and Drug Administration's) remit was extended to medical devices, but then were banned when the manufacturers failed to submit the evidence of safety that they had been asked to provide by the date the FDA had specified.[122] There was no evidence that the implants were unsafe; but the announcement of the ban seems to have been handled, and reported, in such a way that many of the many women who had such implants panicked, and began to attribute every twinge to them.

You have to wonder: if the ban had been handled differently, mightn't the panic, and the legal fiasco, have been, at least, mitigated? For that matter, wouldn't it have been better if no medical journal had been willing to publish the *only* study that ever found even a small increased risk in women with the implants, given that it relied on the women's own reports?[123] Again: the year after Judge Pointer's and Judge Jones's panels reported that there was no

[119] Levitz, "Lab Flaws Cast Doubt" (note 117 above).

[120] These are many, because many of the tainted cases concern low-level drug offenses, which rarely go to trial. *Id.*

[121] *Id.*

[122] For the background story, see Marcia Angell, *Science on Trial: The Clash of Medical Evidence and the Law in the Breast Implant Case* (New York: W. W. Norton, 1996), chapters 1 and 3; for more details of the panels, see Hooper, Cecil, and Willging, "Assessing Causation in Breast Implant Litigation" (note 77 above).

[123] Charles H. Henneckens, "Self-Reported Breast Implants and Connective-Tissue Diseases in Female Health Professionals: A Retroactive Cohort Study," *Journal of the American Medical Association* 275, no.8 (1996): 616-21. Sanders and Kaye, "Expert Evidence on Silicone Implants" (note 78 above), p 127, n.87.

evidence that the implants caused the connective-tissue diseases they were feared to, a significantly larger panel set up by the Institute of Medicine (IOM) at the National Institutes of Health (NIH) reached the same conclusion. You have to wonder: wouldn't it have been better if the IOM had stepped in sooner?

My theme throughout has been that ensuring the quality of expert testimony involves a whole tangled knot of tricky problems; so it should come as no surprise that, rather than offering a panacea, I conclude by urging that we think harder and more imaginatively not only about legal rules and procedures but also about the many *other* ways in which we might, bit by bit, make expert testimony more often genuinely helpful to fact-finders, less often confusing or misleading.

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A Note To My Philosophical Friends About Expertise And Legal Systems

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ABSTRACT

This brief essay explores how understanding the treatment of expert evidence requires engaging with its legal and political contexts, and not just focusing on its epistemological aspects. Although the law of evidence and thus its treatment of experts is significantly informed by epistemological considerations, it is also informed by concerns over the organization of trials, larger issues of intelligent governance, social concerns, and enforcement issues. These five aspects to the law of evidence give rise to principles to guide the explicit structuring of the law of evidence that are identified here as well. This complexity helps to explain why the central issue of expert testimony is not the epistemological one of knowledge and belief but instead the conflict between educational and deferential modes of trial.

Keywords: testimony, expert testimony, reliability, epistemology, purposes of trials.

1. Introduction

The inclination of the philosophically minded seems to be to isolate and analyze the essence of the object, concept, thing, whatever, of interest. Sometimes that inclination should be resisted. The standard analysis of expertise and the legal system is a good example of this. The standard critique assumes or asserts that an important goal of the legal system is to admit or take advantage of scientific knowledge, and then descends into the seemingly endless (to the not-so-philosophically minded) wrangling over what is knowledge and how we can know that we possess it. Sometimes the

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intractability of these age-old and unresolved questions leads to justified belief as an alternative, or justified true belief as an amalgam of the two. Implicit in these efforts is the belief that the essence of expert testimony (and frankly testimony of any kind) can be identified and that its purpose is this or that. It is my belief, which I believe is both justified and true, that this background assumption is false. Indeed, it radically misconceives the object of inquiry as being "expert testimony" simpliciter and thus misses that expert testimony is embedded in a larger legal structure and implements whatever the objectives of that structure are.

"The objectives of that structure" are themselves immensely complex. The treatment of expert testimony is embedded in a theory of the trial, which itself is embedded in a theory of litigation, which itself is embedded in a theory of government, and often these theories are contested.[1] The relationship between trials and overall social welfare, as judged by a contested theory of government thus generates an incidence of the standard problem of the liberal state of how to structure things to assist people to muddle through in life in the face of disagreement about ultimate ends. The reduction of expert testimony to questions of justified belief or knowledge implicitly rests on a prior view that advancing accurate outcomes is the goal of a trial, but unfortunately it is not. It is one goal, and an important one (in my opinion the most important), but it is not the only goal. To be sure, overall social welfare is advanced by accurate decision-making because otherwise, generally speaking, rights are meaningless, but a litigation system is costly and the costs of accuracy may outweigh the benefits. A system can be monetarily costly and the transactions costs of litigation can outweigh the remedy in a particular case, but reducing the transaction costs may encourage more litigation and disrupt valuable forms of social interactions (I can just sue my neighbor rather than ask her to turn down the music).

The law of evidence in the various countries that I have studied is responsive to these kinds of concerns, and the rules governing expert testimony is no exception. In the liberal democracies, the pursuit of truth through knowledge is one but only one of the goals. And a good thing too. It is more than somewhat ironic to have the philosophically minded harangue about knowledge and truth when they have been telling us for centuries that they cannot agree on what "knowledge" is, nor when it obtains.

[1] For a good overview of the philosophical literature on expertise, see Selinger & Crease (2006).

To understand and to critique the role of experts in the legal system require that expert testimony be considered in the context of the complex social dynamic just alluded to. I do this in two different ways. First, I describe the five major problems that the law of evidence must accommodate, and then extract from them and from the implications of evidence reform movements worldwide eight principles that guide the fashioning of a sensible law of evidence, including provisions for expert testimony.[2] To be quite frank, the point is to show how the philosophical fascination with knowledge, while interesting, is a small part of the considerations at hand. I then briefly discuss the major conceptual issue that expert testimony does pose, which is captured by the distinction between trials as pedagogical rather than deferential events.

There are at least five sets of competing considerations to be reconciled by the law of evidence in the service of its social functions. Only one has to do with epistemology, although that is where I begin:

2. Epistemology and the law of evidence[3]

The connection between expert testimony and truth is part of the more general connection between facts and rights. Facts are prior to and determinative of rights. For example, ownership of clothes allows a person the "right" to possess, consume, control, and dispose of clothing, but what happens when ownership is challenged? A judge or jury will hear evidence about who bought, made, found, or were given the clothes in question. Whatever facts are found will determine who has the right. Rights and obligations are utterly dependent upon facts and are derivative of them. The significance of this point cannot be overstated. It inverts the normal way of thinking about liberal states– epistemology is prior to deontology rather than the other way around. In addition, tying rights and obligations to true states of the real world anchors them in things that can be known and are independent of whim and caprice and gives them solidity and stability so that they cannot be removed arbitrarily.

The conventional view, reflected in the standard discussion of expert testimony, is that the law of evidence largely resides here. However, the tasks

[2] As part of consulting with the governments of China and the United Republic of Tanzania on law reform over more than a decade, I have examined evidence reform efforts worldwide. See Allen et al. (2013 & 2014).

[3] This part is heavily indebted to Allen (2015).

of the law of evidence go far beyond the epistemological problem and involve at least four other matters that complicate both the structure of the legal system and efforts to analyze it.

3. The Organizational Problem

The law of evidence regulates the interactions of the various participants in the legal system: trial judge, jurors, attorneys, parties, and witnesses (both lay and expert) and constructs the framework for a trial. It allocates both power and discretion to each of the actors. By determining how much discretion the trial judge has, the law of evidence affects how much control the parties have over the trial process. The law of evidence also structures the relationship between trial judges and appellate judges. Should there be trial de novo in the appellate court, or is appellate review limited to the resolution of legal errors? Are small civil cases different from large commercial cases in ways that justify different treatment? What is unique about criminal cases?

The law of evidence also regulates the relationships among branches of government. Consider the choice between a complicated set of rules that restrict the power of trial judges and a series of guidelines that trial judges are expected to administer fairly. One may think that the primary implication of this choice has to do again with the epistemological problem, but that would be mistaken. The higher the discretionary threshold gets, the more power is passed down the chain of command to trial level judges. Discretionary rules insulate trial judges from control by appellate judges, but they also insulate the judiciary from control of the legislature. Categorical rules maintain control over the evidentiary process in the governmental organ that issues the rules, whether that organ is appellate courts or legislatures. Categorical rules also can be the means of educating trial judges of the risks of certain kinds of evidence.

The Organizational Problem does not end there. Complex rules of any sort give strategic and tactical advantages to certain groups in society, in particular those with the resources to master and employ those rules. This includes the wealthy and repeat players in the legal system, whereas simpler rules largely benefit those with lesser financial means. Complex codes of evidence law also contribute to the instability of decision making by encouraging appeals, which increase the transaction costs of litigation. Increasing the transaction costs of

protecting a right decreases its value, which may have detrimental social consequences. In any event, the law of evidence must be fashioned with all of these variables in mind.

4. The Governance Problem

Notwithstanding the importance of accurate fact finding, the public has other demands in addition to sensible trials, and consequently accurate fact finding competes with other social values, in particular through the creation of incentives of various kinds. Moreover, completely accurate fact finding is impossible, and difficult questions of how to allocate errors and correct decisions must be addressed.

The value of factual accuracy must be weighed against other policies that a government may reasonably pursue. The list of such policies is long and culturally contingent. For example, the law of privileges may foster and protect numerous relationships, including spousal, legal, medical, spiritual, and governmental. Perhaps settlement of disputes is preferred to litigation, which leads to the exclusion of statements made during settlement talks. In the United States and more and more in the world at large, a body of exclusionary rules is premised on the perceived need to regulate police investigative activities.

The Governance Problem also involves the relationship between primary and litigation behavior. Primary behavior is everyday behavior of the population. Litigation behavior is activity directed toward formal resolution of disputes. Regulating primary behavior involves what a society thinks is right and wrong, with creating the conditions for efficient economic behavior, regulating social interactions and institutions, and so on. Facilitating such behavior is the typical objective of social organization generally, and the law specifically. Litigation behavior, by contrast, involves parties attempting to resolve disputes that have arisen over claims about inappropriate primary behavior or to rectify social disruptions that have occurred through alleged violations of substantive law. Most current analyses focus on either primary behavior or litigation behavior as though they were separate spheres of influence with internal logics of their own. This separation, while analytically useful in many contexts, misses or distorts the central regulatory problem.

Primary and litigation behavior are not hermetically sealed off from each other. For example, there may be some types of litigation where behavior (both primary and litigation) is optimized by a low or zero cost litigation process. However, there may be other types of litigation that are optimized by infinitely high costs—in other words, cases that should not be brought. Perhaps family disputes are an example of this latter category. Other cases may be somewhere in between in that behavior is optimized by the impositions of some costs. The tasks for the legal system include responding intelligently in the face of such complexity—which cases should be encouraged to be brought, and which should not, and the law of evidence is an important tool in implementing whatever decisions are reached.

5. The Social Problem

Trials may serve yet many other social purposes, such as symbolic and political purposes. Both institutions and individuals can make statements through the means of trials, and impart lessons of various kinds. Trials also can be the means of vindicating reputations and obstructing governmental overreaching. Obviously, the law of evidence can impact all such issues. Principles of fairness and equity may also influence the law of evidence, although the precise effect of this variable is often hard to sort out from more overtly utilitarian motivations. Some think that the limit on unfairly prejudicial evidence reflects not only the concern about accuracy but also the concern about humiliation, as is also the case with character evidence rules. The limits on prior behavior and propensity evidence reflect in part a belief that an individual should not be trapped in the past. The hearsay rule reflects the values of the right to confront witnesses against oneself.

6. The Enforcement Problem

There is a critical distinction between the law on the books and the law in action. It is one thing to write laws and rules; it is another to enforce them in the way anticipated by the drafter of those provisions. The drafter of an evidence code may think that allocating discretion to someone, whether trial judge or attorney, makes sense, but the drafter will have in mind an approach to exercising that discretion that might not be shared by those being regulated by

the rule. More generally, it is hard to enforce complex codes in social events such as trials. The event itself, the trial, is often fluid and unpredictable, and it would be impossible to have every decision made at trial second guessed by some other authority.

A number of these variables interact. Another social value at the interstices of these various problems is the requirement that a legal system be perceived as fair. This has too many components to address here, but among them are the ideas that a litigant has the right to be heard and that the decision-maker comes to the task with an open mind. That means in part that dogmatic "truth" is to be avoided and that cases settle things only in light of what was presented at trial. That resolves the dispute between the parties, but does not resolve "truth" of very much in a more general way. If the next litigant has more or different evidence of some proposition, the tribunal is to consider it to see whether it changes anything. For a long time, separate but equal was equal, and then it was not. For a long time, cigarette manufacturers violated no duties to the consuming public, and then they did. For a long time, silicon was found to cause anti-immune diseases, and then it no longer did. The mutability of "knowledge" is well known in the philosophy of science, of course. It is a defining feature of legal systems. Its major consequence is the embracing of procedural notions of fairness of the kind just mentioned—the right to be heard by a disinterested fact finder.

7. Eight Principles

How does all this work out in the actual structure of legal systems? I have been working for over a decade with the governments of Tanzania and China in the reform of their respective legal systems, including the law of evidence. (Allen, et al. 2013, 2014) Out of that work has come what I call the eight principles to guide the writing or reformation of the law of evidence. (Allen, 2014, pp. 47-48) Collectively they indicate the complexities that emerge from the "problems" noted above.

1. Evidence law should facilitate the accurate, efficient, and fair finding of facts pertinent to legal disputes. Generally, all relevant evidence (evidence that would influence a reasonable person's inferential process) should be admissible. Otherwise relevant evidence should be excluded only if there is a

very good reason for doing so that outweighs, in the particular context, the value of accurate adjudication—or contributes to the probability of it.

2. The law of evidence does not determine the "facts" that may be found; the substantive law does. The law of evidence facilitates reliable investigation into those facts.

3. The evidentiary process should respect natural reasoning processes. It should not impose strained or artificial limits on testimony or the presentation of real evidence absent a compelling justification.

4. Evidence law exists to facilitate the rational resolutions of disputes and not as an end in itself, and should be so constructed and interpreted. Meticulous compliance with technical modes of proceeding that do not serve the ultimate ends of accurate, efficient, and fair fact-finding should not be demanded, whether emanating from evidence or procedural codes. Trials should be conducted as a rational search for truth, rather than games that require formalistic compliance with complex rules. Reversals on appeal should be limited to cases in which a significant violation of a right likely affected the outcome of the case.

5. Decisions at trial are always decisions under uncertainty, with mistakes being unavoidable in the long run. Evidence law should facilitate equal treatment of parties and the reduction of errors made at civil trials. Civil parties typically stand equal before the law and should not suffer discrimination due to their formal status (plaintiff, defendant, applicant, respondent, intervener, etc.). Deviations from that principle should be rare and justified (such as civil cases involving allegations of fraud). In criminal cases, the Government must prove each element of any charged offence beyond a reasonable doubt; affirmative defenses with differing burdens of persuasion are allowable in limited circumstances.

6. Evidence law should not discriminate among groups in society. For example, undue advantage should not be given to repeat participants in litigation. Its language should thus be as spare, nontechnical, and immediately comprehensible as the subject permits. Evidence law should always be administered to advance, rather than obstruct, the underlying purposes of a legal system.

7. To the extent possible, without significantly compromising any of the guidelines noted above, the law of evidence should respect the norms of the communities to which it applies.

8. There may be occasion to provide exceptions to any of the guiding principles noted above, but those exceptions should be rare, limited, clear, and justified. Examples may include privileges, as well as the structuring of incentives for other socially valuable purposes.

The point I am making here can be understood in another way. Most expert analyses, whether philosophical or legal, proceed as though the object of inquiry is like a closed deductive system. The legal system is not. It is organic, not static, just like society of which it is a part. So, no, the critical problem of expert testimony is not the philosophical problem of the conditions or existence of knowledge, or whether the system prefers or is satisfied with justified belief, or whatever. I have literally never seen a case decided on such a ground. The problem instead is how to manage all this complexity, and the basis of decision is invariably (although not always in these terms) the rather open-ended concept whether a rational human being could be influenced by the proffered testimony on a material proposition. This is more complicated than it appears.

The solution to this problem of taming complexity is essentially procedural—decision is by competent, disinterested individuals able to comprehend, process, and deliberate upon the evidence to reach a rational judgment as to what occurred—and thus as to the rights and obligations of the parties. The facts are to be found by the disinterested application of common sense by members of the community (whether judge or juror). After determining the most plausible account of what actually happened, (Allen, 1991; Pardo & Allen, 2008) liability is determined consistent with the formalities of substantive law.

All of this is accomplished by exploiting common sense and general experience. Everyone at trial—judges, jurors, witnesses—has enough in common so that effective communication, and more importantly comprehension, is possible. Fact finders come to trial with a vast storehouse of knowledge, beliefs, and modes of reasoning that are necessary to permit communication to occur simply and efficiently. Conventional beliefs about the nature of reality and the existence of causal relationships are just assumed to be held by all participants, and virtually never are the subject of evidence. Everyone is just assumed to engage in orderly reasoning, employing all the necessary forms—deductive, inductive, abductive, statistical—as necessary or appropriate. Given a common language, or translations if necessary, comprehension of witnesses is just assumed, as is the ability to perceive the

connection between the evidence and the trial. Everyone is assumed to know about the foibles of human testimony and the perverse effects of potential biases, and thus to be able to judge the credibility of the testimony. Less well known, everyone is expected to be able to fill in the evidentiary gaps at trial that result from many factors (including that individual witnesses always know more than they can express) by drawing inferences based on one's own experience. Indeed, one of the arguments for juries and multi-member courts is that the probability of all this being done well increases with the size of the body deciding a case, because each person added to the group brings a lifetime of experience and knowledge to judge the evidence.

To return to expert testimony, what if testimony can only be understood with knowledge or experience that the fact finder lacks so that the chances are virtually zero that the fact finder will understand what the spoken words are intended to convey or able to intelligently appraise the truth of what is spoken? This is the critical conceptual problem posed by expert testimony for legal systems, and there are only two possible solutions to it. Either the necessary background information must be provided or fact finders must defer to the judgment of others, not because of comprehension and agreement, but because the fact finder is simply delegating that decision to someone else. Virtually always when faced with this dilemma, the Anglo-American legal system, and most other liberal systems of which I am aware, has chosen to require that information be provided in a comprehensible fashion to the fact finder. If a witness speaks a foreign language, translations will be provided. When routine business practices or conventions matter, evidence is adduced on the topic so that the fact finder may judge what the actual routine practices or conventions are. Expert testimony at trial is often inconsistent with this normal conception of a trial. Experts often engage in years of specialized training, which can make it difficult to educate the fact finder about the relevant issues at trial. Although the controversies over expert testimony explicitly are typically about such things as knowledge, they in fact are controversies over supplanting the norm of education by deference when someone qualified as an expert speaks, and thus can only be resolved by addressing that issue.[4]

[4] The education-deference distinction was first introduced into the literature in Ronald J. Allen & Joseph S. Miller, (1993) and Ronald J. Allen, (1994).

The obvious first question to ask is whether deference is ever an absolute necessity, whether there are any cases that cannot be accommodated within the traditional model. Do some cases present issues for decision that defy the ability of fact finders to understand them? Perhaps the answer to these questions is "no". The deficits of juridical fact finders do not appear to be cognitive; they are informational. Judges and jurors lack knowledge about many things, like science and technology, but there is no reason that they could not adequately master the relevant fields. This does not mean that a fact finder would have to become an oncologist or radiologist, or whatever. The objective is not to understand any particular field in its entirety. Rather, the objective is to learn enough so that rational deliberation can occur. In this respect, multi-body decision makers—either juries or panels of judges—are again superior to single person decision makers. Not every member of a panel needs to understand deeply every issue. The question is whether the panel adequately understands. It would be astonishing if a legal case actually defied the cognitive capacities of a small group even randomly picked from society at large, let alone vetted as both judges and jurors are.

Obviously, there are examples of ideas and even fields of inquiry that may defy common understanding at present. Many ideas in physics seep only slowly into the general population, even the general population of scientists. Maybe it would be asking too much for a judicial fact finder to learn special relativity or quantum theory, but to my knowledge these theories are not pertinent to any litigation that has ever occurred. Admittedly, physics is not the only difficult subject matter to learn. Many individuals find higher mathematics difficult (which is probably why they find physics difficult). Examples of two areas of somewhat higher mathematics that are pertinent to modern trials are calculus and probability theory. Still, while some people do, others do not find mathematics at this level obscure—or more importantly would not find it impossible to learn sufficiently for intelligent decision. Here again is the value of a multi-body decision maker. What matters is not whether everyone understands but whether the body as a whole does or could learn what is needed for intelligent decision.

The real objection to educating the fact finder is not that it is impossible but that it would be costly. If statistics plays a role in the trial, it would have to be explained so that the fact finder can understand, which would require some considerable instruction. The same would be true of various areas of medicine, and so on. In some cases, this educational process would not be terribly

burdensome, but in others it would be difficult and require extensive instruction. So, yes, it would be costly, but I literally do not know of any cases actually litigated that would seem to defy this educational process.

If the aspiration of trials to rational decision-making is to be achieved, the parties must educate the fact finder in all instances. This would eliminate the legal problem of "expert" testimony, because the category would no longer exist. That may seem like solving a problem by definitional fiat, but it is not; the point cuts much more deeply than that. The lamentable consequence of conducting trials through deference is that mistakes will be made because fact finders choose to defer to a purported expert who is in fact not testifying reliably but instead is providing what is called in the United States junk science. Junk science and unreliable expertise exploit the informational vulnerability of the law, the necessary condition of which is the fact finder not understanding the basis of the expert's testimony. Making all witnesses, including what are now called expert witnesses, explain their testimony will largely eliminate this problem because false propositions resist comprehensible explanations. I do not say make them impossible, but the presentation of unreliable evidence would be made considerably more difficult.

But I need to examine the other side of this epistemological coin. Perhaps I am wrong that the primary limitation of fact finders is informational rather than cognitive; perhaps there are cases that involve "knowledge" in a strict sense—whatever that is—that judges and jurors are not able to comprehend. If such knowledge exists and cannot be conveyed at trial, then it is pointless to hold trials involving it in any legal tradition that emphasizes decision by disinterested individuals who rationally process the evidence; that simply cannot occur with a deferential mode of presenting evidence. Quite the contrary, if there are forms of expertise that are pertinent to trials but cannot be explained at trial, the solution is to not try those cases. If expertise exists and can be identified with the certainty that we know that Ohio is a state in the United States of America, its lessons should be embraced and the case so decided. How to do so is a different question. The form of trial but not its substance can be preserved through procedures like judicial notice or peremptory motions (summary judgment, directed verdict); alternatively, disputes can be resolved definitely by the state through legislation or regulation.

By contrast, maintaining a form of trial that involves expertise that is not comprehensible to the fact finder is, literally, nonsensical (but, as we shall see,

perhaps defensible). In trials involving deference, both sides offer expert opinions to which fact finders can defer; these opinions are virtually always diametrically opposed, with one favoring one party and the other favoring the other. If there are not opposing opinions, there is not a triable dispute, and the side with the unassailable (or at least unassailed) expert wins. If there are competing experts, fact finders in a deferential process do not grapple with the facts but simply decide which expert's opinion to accept. But fact finders cannot defer intelligently without understanding the relevant fields. To know which expert to believe requires knowing the field adequately enough to appraise the opinion in light of the facts of the particular case. Without knowledge of the field of inquiry, the fact finder has no rational basis to defer to either expert. This point reverberates over the use of expertise at trial, and emphasizes how much the deferential form of expert testimony is a reproach to deep aspirations of liberal legal systems. The mere admission by the trial judge of competing expert opinions without requiring an explanation of the experts' views, including testimony on the underlying field of inquiry, ensures that decision will be arational if not irrational. If, by contrast fact finders can decide intelligently about which expert to believe, deference to the expert is not necessary. The fact finders could see for themselves the progression of the expert's thought leading from the specialized knowledge through the evidence of the case to the conclusion being offered.

The struggle between education and deference certainly characterizes the American experience with expert testimony. The famous *Daubert*, 509 U.S. 579 (1993), case is a good example. It requires the trial courts to act as gatekeepers to expert testimony, admonishing them to admit only testimony based on "scientific knowledge".[5] As is well known, the Court did not get its philosophy of science quite right, and in any event did not show any comprehension, let alone effort to resolve, the deeper questions of "knowledge" lying behind the problem of scientific knowledge. Moreover, it structured a process that leads to the presentation of opposing opinions/inferences at trial, and left unaddressed the mystery how that could occur if each side's expert was in fact testifying on the basis of "knowledge". Last, while it was a step forward to require trial judges to engage with the underlying expertise, the Court did not require that the jury be presented with

[5] The Court subsequently generalized this to other forms of specialized knowledge, the point remaining that what is offered must be determined to be reliable by the trial judge.

the same level of explanation. In short, the Court converted the admissibility question into an educational event but left the trial as a potentially deferential event.

The key to unraveling what the Court did comes from a recognition that "knowledge" in a philosophical sense is not the foundation of the evidentiary regime. First, it competes with all the other interests noted above. Second, the problem of testimony, lay and expert, is about reliability, but it is not about much of anything that maps onto the philosophical debates about belief, knowledge, or truth. Testimony can be reliable but false, which is a common occurrence at trial, and one anticipated in great detail by the evidentiary regime that regulates examination of witnesses, permits credibility to be explored, and allows diametrically opposed visions of reality to be presented through testimony. The question is not whether a witness is testifying on the basis of knowledge but instead whether the fact finder can intelligently assess the witness's testimony and reach a reasonable judgment about what happened. There is a limit to be sure. A witness, expert or lay, may tell a coherent story that nonetheless no reasonable person could believe because it can be shown to violate too much of an informed view of the world (like a witness offered to testify about an event but is shown not to have been present; but if presence is contested, and a reasonable person could conclude that the witness was present, the fact finder gets to sort it all out). Generally, such cases will not proceed to verdicts. However, the cases that do proceed to verdicts cannot be demanding "knowledge" from experts because invariably their testimony will be opposed by another expert. Two experts cannot both be testifying from "knowledge" when they assert opposite conclusions. The typical, intensely practical, method of handling such scenarios avoids the deep philosophical questions by preempting them with a procedural solution that lets the parties do what they like to advance their interests, patrolled mainly by the requirement of a demonstration that a rational person could be influenced by an evidentiary proffer. If the parties want to roll the dice and not explain expertise to the jury (if there is one) that has passed the admissibility test, so be it. No one is required to do so; it is up to them.

The schizophrenic approach of the Supreme Court may appear to be problematic, with procedural fairness trumping knowledge and truth, but I think it reflects what I have been addressing in this essay, which is the complexity of the underlying dynamic of which expert testimony is a part. Requiring the trial judges to take some care in ensuring witnesses are testifying

on the basis of something reliable (which is really all that is meant by the Court's ramblings in *Daubert* about knowledge, science, and the scientific method) gestures in the direction of the epistemological problem of the legal system. Not requiring the parties to educate the jury in the same way as the trial judge gestures in the direction of party autonomy and fairness. It is up to the parties to choose whether to educate the fact finders or convince them to defer to an expert. They know their dispute and their resources better than anyone else and are in the best position to make choices that optimize their interests. So there is a form of deference occurring here but it is more to party presentation then to the specialized knowledge of experts.

At a more general level, decision in any particular case, even if it gets affirmed by the highest court with jurisdiction, does not establish any proposition in the case as true, except as between the parties themselves (in the sense that the end of the case ends that dispute). The parties to the next dispute are not bound by the prior decision and may litigate again any pertinent matters, including "scientific knowledge". If "knowledge" of most philosophical varieties were truly at stake at trial, leaving questions opened-ended would be a colossal waste of resources. Once things are known, they are known. Perhaps showing a more subtle understanding of the true nature of the problem than much philosophical discourse, or perhaps burned by making too many mistakes, liberal legal systems do not embrace this view. By leaving all questions open for reconsideration, the procedural context of litigation accommodates the lack of stability in "knowledge," expert or otherwise, that is such a philosophical irritant.

I am sure there are deep philosophical questions lurking in the description that I have given of the reality of litigation and its place in liberal legal systems, but they are not the standard fare of epistemology.[6]

[6] Except, to a limited extent, "reliabilism". See the entry, Reliabilism, in the Stanford Encyclopedia of Philosophy for an overview, available at http://plato.stanford.edu/entries/reliabilism/.

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Overcoming Expert Disagreement In A Delphi Process. An Exercise In Reverse Epistemology

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ABSTRACT

Disagreement among experts is a central topic in social epistemology. What should an expert do when confronted with the different opinion of an epistemic peer? Possible answers include the steadfast view (holding to one's belief), the abstemious view (suspending one's judgment), and moderate conciliatory views, which specify criteria for belief change when a peer's different opinion is encountered. The practice of Delphi techniques in healthcare, medicine, and social sciences provides a real-life case study of expert disagreement, where disagreement is gradually transformed into consensus. An analysis of Delphi shows that moderate conciliatory views are descriptively more adequate than rival views. However, it also casts doubt on whether the debate in social epistemology is explanatory relevant vis-à-vis real life cases of expert disagreement, where consensus replaces truth, and acceptance is more explanatorily relevant than belief.

Keywords: disagreement, experts, epistemology, Delphi process, acceptance.

1. Introduction

A central topic in social epistemology is the problem of disagreement among experts. What is rational for an expert to do when one of her peers does not share her opinion? Answers to this question that have been provided include the "abstemious view", according to which suspension of judgement is the only rational option when one acknowledges her peers' dissent (Feldman 2006, 2007; Christensen 2007), the "steadfast view" (one should hold fast to one's beliefs, dissenting epistemic peers notwithstanding) (Goldman 2010b, van Inwagen 1996, Kelly 2005), and moderate conciliatory positions aimed at

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specifying conditions in which belief revision is appropriate (Elga 2010, Lackey 2010, Christensen and Lackey 2013).

The problem of disagreement about experts is also a central topic in the life of scientific communities, governments, and organizations in general, where panel of experts are called in to report or even to decide on technical issues. Here the problem takes the form of reaching a final verdict in spite of the possible and actual disagreement within the panel of experts. The present paper discusses a widespread method for reaching consensus among experts, the Delphi technique. Introduced in the 60s, Delphi is now routinely employed in many areas, typically medical and healthcare services research, but also social sciences, in order to receive answers from expert peers who are likely to disagree, but whose consensus on a certain topic is important to reach (Dalkey and Helmer 1963; Linstone and Turoff 1975). This paper is an exercise of reverse epistemology in that the question "what should experts do when they disagree?" is confronted with "what do experts do when they disagree?" and the guiding principles of what is considered a good practice of overcoming disagreement are abstracted out and assessed, in a bottom-up rather than top-down approach. The guiding idea here is that the logical analyses that classical normative epistemology provides, can be tested with, or employed for real-life cases of knowledge formation procedures, and real-life problems of disagreement. This is not a widespread attitude in the literature yet, though Boaz Miller tested real-life cases with classical epistemological solutions to the novice-to-expert disagreement problem (Miller 2014), and Nathan Ballantyne suggests that the new challenges for classical epistemology crucially involve moving away from oversimplified, toy examples (Ballantyne 2014, see De Cruz and De Smedt 2013 for dissent).

The paper advances the following conclusions. On the assumption that the Delphi procedure is an *epistemically rational* practice, and that it can be considered a practice of *knowledge* formation, it appears that moderate conciliatory views are descriptively more adequate than the steadfast view and the abstemious view. However, Delphi techniques qualify as rational epistemic procedures vis-à-vis expert disagreement only if we concede that consensus - and not just knowledge - is an epistemic value in itself, or alternatively that consensus can be knowledge-based. Also, Delphi consensus practices involve the pragmatists' claim (from William James) that acceptance - and not just belief - is a relevant mental state in assessing knowledge and knowledge procedures.

2. Expert disagreement in classical epistemology

In this section I provide an overview of the main positions on expert disagreement in classical epistemology – the philosophical study of what should we do to achieve knowledge, conducted with the method of conceptual analysis. Differences among authors aside, classical epistemology is a normative, rather than descriptive discipline. Knowledge, the main epistemic goal, is generally conceived as connected with truth and justification – rather than as the mere acquisition and systematization of information (Goldman 2010a).

Within this tradition, the problem of expert disagreement is a particular case of the more general problem of disagreement among *epistemic peers*. Peers can be characterized as subjects who are as well-informed and well-disposed to react to the evidence as we are. This characterization stresses parity of intellectual and cognitive virtues, as well as shared evidence (Gutting 1982, Kelly 2005, Christensen 2007, Lackey 2008). A slightly different way to define epistemic peerhood involves expected outputs: a peer is someone who we think is just as likely to be mistaken in her judgement, as we ourselves are (Elga 2007). On this latter view, one's antecedent epistemic score, so to say, is relevant to peerhood. In the typical toy example often discussed in the literature – the simple maths case - my friend and me are plausibly epistemic peers when we confront with the problem of checking the bill that the waiter brings us at the restaurant after dinner, provided that we are equipped with same mathematical skills, same evidence, and in absence of defeating conditions of perception, cognition, and other physical abilities that may be relevant (Christensen 2007, Elga 2007).

Experts are a special kind of epistemic peers. Following Alvin Goldman's definition, an expert is "someone who possesses an extensive fund of knowledge (true belief) and a set of skills or methods for apt and successful deployment of this knowledge to new questions in the domain. Anyone purporting to be a (cognitive) expert in a given domain will claim to have such a fund and set of methods, and will claim to have true answer(s) to the questions under dispute because he has applied his fund and his methods to this question" (Goldman 2001, 146). Experts are not epistemic peers with respect to novices and non-experts, but they can be epistemic peers of other experts. In fact, expert communities and panels are (ideally) groups of people who are both experts with respect to novices and the general public, and peers among

themselves, that is, each of them ideally thinks of the others as being just as likely to be right as he or she is, in judgments pertaining a given area.

We can imagine sets of epistemic peers located at any level of competence and epistemic status, peerhood being a horizontal relation of equivalence holding between subjects (independently of the quality of the evidence or competence they possess). Novices can be peers, and experts can be, with respect to a particular judgement (Elgin 2010). Thus, the problem of expert disagreement is just a special case of the general problem of disagreement among peers, which can be stated as follows: Once that we realize that a peer does not share our belief (or belief confidence) in the face of the same evidence, what should we do? Do we have at least one reason (the peer's disagreement) to revise it, or rather we should withdraw judgement, or hold fast to our view? In the simple maths example, what should I do when I realize that adding up the bill I arrive at 46 euros, while your result is 48?

The debate on peer disagreement is now characterized by a continuum of positions ranging from Strong Conciliationism to Steadfast positions (Christensen 2009, Christensen and Lackey 2013). Conciliationist views in general hold that a peer's disagreement always constitutes for a rational subject a good reason to undergo a process of belief revision. When a disagreement about a certain proposition *p* is revealed, everyone should give some weight to her peer's judgment such that neither is justified in staying exactly as confident as she was before regarding *p*. The intuitive motivation behind Conciliationism is that to recognize someone else as an epistemic peer just is to assume that we both have the same chance of being wrong, and nothing is special (epistemically speaking) about me. According to Conciliationist, assuming peerhood makes one irrational if she doesn't modify her attitude upon disclosure of disagreement (Feldman 2006, 2007, Christensen 2007, Elga 2010, Ballantyne and Coffman 2012, Carter 2014).

One way – the strongest way - to reduce confidence in one's belief when disagreement is faced, is to suspend one's judgment altogether. This is what Richard Feldman famously argued for:

> One of us must be making some kind of mistake or failing to see some truth. But I have no basis for thinking that the one making the mistake is him rather than me. And the same is true of him. And in that case, the right thing for both of us to do is suspend judgment on p (Feldman 2007, 212).

This particular variety of Conciliatory view is now called the Abstemious view, for the idea is to abstain from judging about p, when your epistemic peers disagree (Aikin et al. 2010). As Feldman and Kornblith noted, it seems that suspending judgement is intuitively appealing in some cases, but not others. Perceptual cases are of that sort - if peers A and B look out of the same window from the same angle and A sees a pidgeon on a tree branch, and B does not, it seems rational for them to suspend judgement over 'there is a pidgeon on that branch'. Abstention seems appealing in this kind of case because, assuming peerhood, the epistemic viewpoint of the disagreeing peers looks like a dead end, there is no room for checking again, and try to locate the possible mistake. A and B may try again to establish whether there is a pidgeon on the branch, but they would better do that through a different epistemic route, changing the evidential basis or the procedure. For example, A may find out a telescope, and B could choose another angle from which to look at. The same goes with the simple maths case I mentioned above. If two peers at the restaurant disagree over the bill, it is rational for each of them to suspend their judgement, and change the epistemic settings completely, by using the mobile calculator, or asking a third party to check (Feldman 2006, Christensen 2007, Kornblith 2010).

There are two characteristics of the Abstemious position, which are of special relevance with respect to the concern of this paper, namely, an application of epistemological questions to a real-life epistemic practice such as Delphi. The first one is its relation with the Uniqueness Principle (Feldman 2006). As some philosophers have pointed out, Abstemious views on disagreement are appropriate if a Uniqueness Principle about evidence applies, stating that for any given proposition p and body of evidence, the evidence fully justifies just *one* level of confidence in the proposition (White 2005, Kelly 2005). Simply stated, the idea is that two peers A and B that disagree over p, can't be both right. Evidence cannot be interpreted in more than one way. Therefore, if neither A nor B has reason to claim a better epistemic status than the other one, suspending judgement over p comes out as a reasonable option. In the pidgeon-in-the-field perceptual case, and the simple maths case, Uniqueness seems appropriate, as the evidence is per hypothesis complete and sufficient for establishing whether p is true or not: adding numbers just gives one result, and checking perception with reality just gives one verdict. In Earl Conee's terms, "we have no better basis for discounting opposing summary impressions than we do for our own" (Conee 2009, 315).

The second characteristics of the Abstemious positions is that it leads to widespread skepticism, if for most (or maybe for all) possible opinions that *p* that one has, one can find an epistemic peer who disagrees, then for most (or maybe for all) possible opinions that *p*, one cannot says one knows whether *p* or not *p*. As Christensen puts it, there must be something wrong with a view that takes massive suspension of belief as the right thing to do (Christensen 2009). As other kinds of skepticism, Conciliatory Abstemious views on peer disagreement put too high a threshold to what counts as rational belief: in areas where disagreement among peers is pervasive (including politics, morality, arts, and science), almost nothing of interest would count as a rational belief. In fact, the typical cases (the simple maths and the perceptual case) that fuel intuitions in favour of this view are such that the value of having a belief at all, and reaching a verdict in that particular situation, is close to none (the bill can be easily checked later, or with a calculator, and nothing depends on whether there really is a pidgeon on the branch). Kornblith (2010) adds that to abstain from believing is more plausible, and more rationally defendable, when the beliefs in question are disconnected, or not much connected with the others one holds. If suspending my belief that *p* implies suspending my endorsement of a whole theory that is logically entailed or presupposed by *p*, then abstension is in conflict with an intuitive principle of conservation. Beliefs that God does not exist, or that the Earth is not flat – unlike "there is a pidgeon on the branch" are examples of beliefs whose withdrawal or suspension in the face of disagreement would cause a huge revision of a person's overall system of convictions.

The above problems of abstemious views, and the considerations of different kinds of examples, speak if favour of more moderate forms of Conciliationism. The common idea is that acknowledging one's peer disagreement makes it rational to change one's belief or level of confidence, not to withdraw one's judgment. One simple way to go is to reduce one's level of confidence in one's judgment by "splitting the difference". Thus, for example, experts A and B assess evidence, A reaches the judgment that p with credence 0.8, and B reaches the judgement that p, but with credence 0.2; then they learn about each other's verdicts, and compromise by both judging that p with credence 0.5, the average of one's own and the other's credence. Conciliationism of this sort is epistemically fair and founded on the principle that one's own judgement and reasoning and one's peers' judgment and reasoning have equal weight (Elga 2007). The possibility of each of the peers'

being completely irrational or wrong in that particular occasion makes this strategy epistemically risky – if B were just badly wrong in assessing the evidence, A's lowering her credence to 0.5 would be incorrect (Kelly 2005). So now Conciliationists tend not to adopt "splitting the difference" as a rule of thumb: a peer's disagreement is just one piece of evidence to be considered among others (Christensen 2009). A possible correction comes from complicating the examples: if disagreeing experts are more than one, then A's compromise is less risky, and more rational. Jennifer Lackey (2008) has proposed another sort of correction: a peer should compromise to other peer's disegreeing judgment depending on her level of "justified confidence" – the more A is justifiedly sure about believing that p, the less she should compromise. Suppose (Lackey's own example) that A and B are doctors, and p is "patient C suffers from lupus", where patient C shows many characteristic symptoms but no skin rush. Same evidence and level of expertise could still make room for doctor A being certain only to 0.5 credence, because she has never seen a patient with lupus with no skin rush. In that case it would be rational for doctor A to compromise to her peer's opinion.

At the opposite end of the spectrum with respect to Abstemious views, and in reaction to the difficulties of Conciliationism, classical epistemological positions about peer (expert) disagreement feature Steadfast views. According to the Steadfast view, when one expert discovers that another expert (or other experts) disagree over her verdict about *p*, she is still rationally permitted to hold fast to her belief (Goldman 2010b, van Inwagen 1996, Kelly 2005). The common strategy employed in favour of this view is to deny that one's personal point of view is the same as the other peers' point of view – in sum, to deny some aspects of peerhood, while granting sameness of evidence, competence and expertise. One straightforward way is to admit that vis-à-vis the same evidence (and granting the same competence and expertise), there can be different epistemic norms, i.e. methods, procedures, and ways to measure the importance of evidence that different agents can employ. Epistemic norms say that some sources or ways of forming a belief are reliable, i.e., likely to produce true beliefs, and examples may vary from particular diagnostic procedures in medicine, to meta analyses, genetic research, use of animals in testing, computer simulations, integration of different kinds multiple lines of evidence.

This is a form of epistemic relativity: what counts as rational, and rationally known, may vary as standards vary. Thus, expert A arrives at judging that p operating with norms N, then she might well realize that expert B arrives at

judging that not p operating with norms M, but given that A does not endorse norms N, she is rationally permitted to hold fast to p (Goldman 2010b). Epistemic relativity is incompatible with the Uniqueness Principle I mentioned above – that certain evidence commands only one possible verdict.

There is also a second way by which one may come to deny peerhood, and support the Steadfast view on disagreement, which is slightly similar to the first one, but more focused on a conceptual feature of beliefs and acts of judgment. It is the idea that even though one may admit or know that another person's reasoning is sound, and her principles are good enough, one's act of judging can be moved by one's own reasons only. So, for example, Bergmann (2009, 339) and Elgin (2010) argue that expert philosopher A may still hold her conviction that *p* even though she fully understand the reasons of expert philosopher B, who denies *p*, because understanding reasons is not the same as endorsing them. One's own reasons guide one in a way that other people's reasons do not, more precisely, one's epistemic reasons lead one irresistibly to form a belief given a certain evidence (Wedgwood 2010). Elgin on this point cites Bernard Williams' point that belief is something that happens to a subject independently of, and in spite of, her will or intention – it is not voluntary (Williams 1973). Thus, she argues, "since beliefs are not voluntary, an epistemic agent cannot, even through judicious assessment, bring it about that she retains, lowers her degree of belief, or suspends belief in the face of a disagreement (Elgin 2010, 12)". Thus, disclosure of disagreement or even a report of my peer's own reasons for disagreeing are sufficient to move a belief change in me; only if the other person's reasons become my own reasons (if I enlarge my evidential basis so to comprise them), they can come to affect what I believe. Note that this point is against any form of Conciliationism: by acknowledging another person's reasons no one can *ever* change her beliefs.

There is, however, an interesting turn in Elgin's argument against Conciliationism. She claims that when an epistemic agent acknowledges her peers' disagreement, and their reasons, she may, however, be able to affect her responses" (Elgin 2010, 12). How? By *accepting* other peers' judgment, where acceptance is a different mental state than belief (while possibly retaining the original belief). The difference between belief and acceptance is usually put in these terms: while belief is involuntary, acceptances are voluntary actions, so that one can accept that p at will, for practical or prudential reasons. To accept that p is to commit oneself to adopt it as a premise in inferences, or as a basis for action, as if it were true (Cohen 1992,

Wray 2010). One can accept that a certain person is trustworthy and capable – even without conclusive evidence – because she needs to employ her with a specific role in a plan of action. One can accept a scientific hypothesis and treat it as a premise, to see what follows, and check it. Acceptances are the attitude we bear to the antecedents of conditionals, when we reason hypothetically. The interesting point here is that if Elgin and Wedgwood are right, peer disagreement should affect what we accept. It should influence the inferences we are prepared to make and actions we are prepared to perform. I will say more on this in the next section.

3. How experts overcome disagreement in a Delphi process

This section confronts the positions in classical epistemology about expert disagreement, summarized above, with the Delphi process, a methodology aimed at issuing guidelines, advices, forecasts and consensus statements in general, in cases and fields where experts are likely to disagree, and/or purely statistical methods of analysis of the data are not possible, or impractical. Introduced around 1965 as a procedure intended to "obtain the most reliable consensus of opinion of a group of experts . . . by a series of intensive questionnaires interspersed with controlled opinion feedback" (Dalkey & Helmer, 1963, 458), the Delphi process is now widely employed in a variety of domains, prominently including medicine, healthcare, and the social sciences, but also finance, engineering, geography, geology, and many others (Gupta and Clark 1996 for a review, Powell 2003 for a review of criticisms and shortcomings of the method).

A Delphi process involves (at minimum) the following steps: definition of a problem for which a judgment, or a guideline, is required; selection of a panel of experts; definition of a series of questions that specify aspects of the problem, usually but not always formulated or assessed by the experts themselves; a first round of questionnaire, where each expert is sent or confronted with the questions so that she can give her answer anonymously; statistical analysis of the results; publication of results of the first round questionnaire; optional iteration of questionnaire; and final verdict. Here is an example of a Delphi process about the diffusion of Alzheimer's disease. More precisely, the study was aimed at establishing the value of dementia prevalence for all regions of the world, in 5-year age bands to 84 years, and for those aged

85 years and older (Ferri et al. 2006). Before the study, there was no agreement on such value, partially because of the lack of reliable epidemiological research from some world regions, so a qualitative consultation involving experts was required. As a first step, 12 experts were selected, who had published studies on the prevalence of dementia in peer-reviewed journals. Then, experts were sent the questions about prevalence in each area, together with a document combining the research evidence. Each one sent back his or her own estimates for dementia prevalence in the 14 WHO regions. After this first round, agreement on the value of prevalence of dementia for the region denominated "Afro E" was 0.62; the questionnaire was then sent again, together with the results, distribution of opinions, and other information, so that after the second round the agreement reached 0.80. As the authors write, "members of the panel who were invited to reconsider in the light of their colleagues' prevalence estimates and any accompanying comments. If they chose to alter their estimates, they could see the effect of this change on the group mean" (Ferri et al. p.3). Finally, the experts' judgments largely converged, and they were further statistically aggregated so that the final statement was issued: "We estimate that 24 million people have dementia today and that this amount will double every 20 years to 42 million by 2020 and 81 million by 2040, assuming no changes in mortality, and no effective prevention strategies or curative treatments. Of those with dementia, 60% live in developing countries, with this number rising to 71% by 2040" (p.4).

A number of observations are in place between tackling the issue of expert disagreement in a Delphi process. First, as a very general point, there is no a priori warrant that the outcome of such processes is knowledge, in the classical sense connected with true and justified belief. Simply put, scientific knowledge is fallible and approximates truth by trying to reach consensus (Miller 2014, Solomon 2007, Steup 2010). Moreover, even excluding culpable intentions, each step, from the selection of experts, to the formulation of questions, to the selection of input evidence given to panelists is prone to error, or may be altered by unconscious biases. The whole tradition of the sociology and philosophy of science following Thomas Kuhn (1970) reminds us that scientists' personal values, psychological dispositions, and social factors have an impact on their epistemic judgments and on the way they come to issue them. With such disclaimers in place, Delphi processes qualify as rational standards for knowledge acquisition in the enlarged scientific community, and

in this qualified sense they can be taken as a model of how experts overcome disagreement.

As the example shows, at the end the disagreement is in fact overcome. In Delphi processes, sometimes many rounds are needed, but a final judgment is always produced, though sometimes it may be complicated one, incorporating different views – as Miriam Solomon reminds us) (Solomon 2007). Assuming that this method of overcoming peer expert's disagreement is a rational practice, which one of the classical epistemological models describes it more adequately? What happens if we test epistemological models in a bottom-up way?

Let us focus on the transition between low agreement after the first round, and better agreement after the second. Here, Abstemious views and Steadfast views do not seem to capture what is happening. In fact, if all the experts were Steadfast defenders of their prior beliefs, no agreement could ever be reached. With respect to the Steadfast view, I think the problem can be located within the asymmetry first-person claim that the position assumes. A Delphi process is such that at each round experts are confronted with their peers' motivations and evidence for the judgments they gave. As consensus gets formed, each one incorporates at least some of the others' reasons in her own evidential basis or judgment procedure. Ideally, each expert learns in each round. So even though a plurality of possible judgments is assumed to be possible, it is also assumed that the disclosure of someone else's preferred methodology or evidence weighting may alter one's previous positions. Other people's reasons in this contexts are not motivationally inert with respect to one's judgment. The nature of the reasons in questions – scientific reasons – makes the transitions from objective to subjective reasons more feasible. Replicability and objectivity are the key concepts here: scientists are trained to assume and to require that the reasons they employ can and should become other scientist's reasons

Abstemious views are equally inadequate, for the epistemic principle they incorporate is at odds with the characteristics of scientific knowledge formation and theorization. As discussed in the previous section, one of the ways to defend judgment suspension in the face of peer disagreement is the Uniqueness Principle, stating that from a certain body of evidence, only one verdict is mandated. There is no room for underdetermination. Such a principle cannot be assumed without reservation in the domain of science – in scientific research, some underdetermination is generally accepted, implying that different theories and judgments can be compatible with the same evidence, at least at earlier stages of theorization in a given domain. Experts

know that other peers can reach different verdicts, typically if they give different values to certain methodologies and techniques, with respect to others. As said above, the mutual disclosure of specific methodologies and preferred techniques can eventually bring to consensus during the process, but the possibility of initial disagreement of correct verdict is assumed as normal.

So far, I discarded the Abstentionist view and the Steadfast view of expert peer disagreement as adequate rationalizations of the Delphi technique, because they incorporated objectionable principles. There is also another reason why views that recommend judgment suspension are not adequate vis-à-vis the Delphi example. Abstemious views are well-grounded in cases where nothing depends on our suspension of belief. Experts in a Delphi process, on the contrary, conform to William James' claim that "sometimes we are *obliged* to form beliefs on insufficient evidence, and that it would be a significant intellectual, prudential or even moral failure to do otherwise" (James 1896/1979, 298). The nature of this ought can be debated – it can be that consensus is evaluated as desirable and good, as a mark of a mature science. Or differently, the content of that specific agreement could be given a moral value, for example, an expert may believe that she ought to contribute to a consensus judgement about the prevalence of Alzheimer because without that, people who suffer from that illness would not receive appropriate care. Finally – as the sociology of science reminds us - the value of agreement can be of a egoistic, non-virtuous sort: to please a pharma sponsor, or to meet the expectations of the organizers of the survey. Whatever it is, the value of agreement makes it practically rational for each expert to contribute to it, in spite of the initial disagreement.

The practical value of agreement and consensus makes an argument against Steadfast views, as well. If consensus is what experts ought to reach, then a stubborn defence of one's own opinion becomes not valuable in itself, but only provided one's own epistemic reasons are very strong.

Then what is the best way to describe the actual process of belief revision that happens in Delphi rounds? What does each expert do? How is it that agreement is produced? A proper answer of this question would feature empirical psychological investigation. However, a projection from the example described above suggests that moderate Conciliatory positions may well equipped to provide a correct picture. Given that the distributions of answers and values of credence are disclosed to each member of the panel, it is plausible that processes such as those Lackey envisages are in play: each one

adjusts her own level of credence depending on how high it was antecedently, and on how many peers disagree, or alternatively, peers "split the difference". According to this description, experts actually change their beliefs, or the level of credence they attach to them, in the face of disagreement, by partially compromising to the other peers' belief when it is epistemically possible for them to do so.

However, in light of the above considerations about the value of agreement, there is room for an alternative description, according to which acceptance, rather than belief, is the right concept that describes what happens when consensus is reached. Given that the very production of a verdict has a value for all the participants, each one may be described as willing to adopt certain stances in order to converge with other people's opinions – to hold them as if they were true, in the future practice, even though they still deem the evidence insufficient. For example, if expert A and expert B "split the difference" of their respective credences, and reach a verdict that averages such values, the verdict is voluntarily issued, rather than spontaneously inferred from the evidence. This is to say that in Delphi processes, experts' rational behavior when facing disagreement conforms to Moderate Conciliationism, but what is produced is actually a state of acceptance.

Note that philosophers have also argued that acceptances, rather than beliefs, are more apt to describe what happens in group decisions, at the collective level (Wray 2010, Miller 2014) – here, I suggest that the Delphi example shows that acceptance may be an appropriate concept to employ also at the individual level, to describe what each expert does when facing disagreement, while holding some interest, drive or desire to overcome it. This is to say that practical rationality and epistemic rationality overlap in real-life cases of disagreement, in a way that is absent from the theorization of classical epistemology.

4. Conclusions

Delphi processes, in which disagreeing experts are brought to agree on a verdict, show that normative accounts of expert disagreement in classical epistemology are mostly descriptively inadequate. Neither Abstemious views, nor simple Steadfast views turn out to be tenable. Of course one may adopt a massive error theory, and claim that scientific practices of consensus are de facto irrational, thereby saving the classical epistemological views as normative

standards. However, such a stance would be implausibly revisionary. Moderate conciliatory accounts of expert disagreement, holding that a disagreement elicits some contextually modulated form of belief revision from the part of each expert, turn out to be illuminating, especially if they are reframed in terms of acceptance rather than belief.

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Philosophical and Psychological Accounts of Expertise and Experts

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ABSTRACT

There are many philosophical problems surrounding experts, given the power and status accorded to them in society. We think that what makes someone an expert is having expertise in some skill domain. But what does expertise consist in, and how closely related is expertise to the notion of an expert? In this paper I inquire into the nature of expertise, by drawing on recent psychological research on skill acquisition and expert performance. In addition, I connect this research on expertise to the larger context of psychological research on human cognition, as it will illuminate some of the differing elements of expertise. This allows me to then critique philosophical accounts of expertise, by showing how they make unwarranted assumptions about skills and expertise. Finally, I note the ways in which being credited as an expert can diverge from the possession of expertise itself. This can help us resist some of the power dynamics involved with those deemed to be experts.

Keywords: dual-process, expertise, philosophy, psychology, skill.

Introduction

There are many philosophical problems surrounding experts, given the power and status accorded to them in society. We think that what makes someone an expert is having expertise in some skill domain.[1] But what does expertise consist in, and how closely related is expertise to the notion of an expert? Although most of us have acquired several practical skills, few of us have achieved the level of expertise with regard to those skills. So we can be easily misled as to the nature of expertise, since it differs significantly from earlier

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[1] Expertise refers to the highest level of skill acquisition, for the possession of a skill is a matter of degree.

stages of skill acquisition. Furthermore, this potential for misleading characterizations of skills and expertise leads to philosophers implicitly working with different conceptions of skills. This can interfere with their attempts to solve related problems about experts.

In this paper I inquire into the nature of expertise, by drawing on recent psychological research on skill acquisition and expert performance.[2] In addition, I connect this research on expertise to the larger context of psychological research on human cognition, as it will illuminate some of the differing elements of expertise. This allows me to then critique philosophical accounts of expertise, by showing how they make unwarranted assumptions about skills and expertise. Finally, I note the ways in which being credited as an expert can diverge from the possession of expertise itself. This can help us resist some of the power dynamics involved with those deemed to be experts.

The first section of this paper provides an overview of the central features of expert performance from the perspective of the current psychological research on expertise. The main two features are automaticity and a recognition-primed form of decision making. Following this is a discussion of the implications of these features of expertise for the articulation and codification of expert knowledge. This section also briefly covers the distinction between System 1 (intuitive) and System 2 (deliberate) thinking in cognitive science, and how aspects of expertise draw on both systems. The second section focuses on how expertise is acquired. The main two features are deliberate practice and self-regulation. Following this is a discussion of the implications of the features for the role of motivation in acquiring expertise. The third section of this paper provides an overview of the Dreyfus model of expertise, and the fourth covers Julia Annas's account of expertise. Both of these philosophical accounts are critiqued from the standpoint of the recent psychological research on expertise. The final section brings out important distinctions between having expertise and being credited as an expert.

1. Expert Performance

Expertise can be thought of in two ways: with respect to a specific skill; or with respect to a domain, where expertise is a collection of related skills.[3] In either

[2] Hereafter, when I mention "research" I'll be referring to the psychological research on expertise.

[3] A skill can roughly be defined as a learned ability to achieve a desired outcome, though often it goes undefined even in the psychological literature. It's important to note that a skill involves some flexibility in

case, a description of expertise often proceeds by comparing the performances of experts with novices. A defining feature of expert performance is the ability of experts to act in a way that seems (and usually is) almost effortless. Experts do not need to devote much conscious attention to what they are doing, and this lack of conscious attention does not lead to any reduction in their performance. This phenomenon is referred to as automaticity in the psychological literature.[4] While automaticity is a defining feature of expert performance, it starts to appear at earlier stages of skill development. With practice, tasks can be accomplished more effectively and more efficiently. This allows a person to devote less attention to the tasks at hand without any reduction in performance, and to shift that attention to other matters. Being able to improve one's performance requires having the initial tasks becoming effortless, so one can devote attention and energy to more difficult tasks. This highlights the importance of Csikszentmihalyi's (1993) work on flow, where one is fully immersed in the task at hand. Being in this state means that you do not need to exert self-control to keep yourself from being distracted. Not only does flow free up one's attention, but it also makes one more unlikely to be disrupted by external distractions.

Another way in which automaticity enables effortless expert performance is by allowing the expert to operate well on the basis of intuitive (rather than deliberative) judgments, as intuitions are experienced as immediate and not as the result of any conscious deliberation. This intuitiveness is central to expert performance because it allows the expert to react quickly to situations. One important thing to keep in mind about the talk of intuition in expertise is that the ability of the expert to reliably act well on an intuitive level is due to having an immense amount of experience and practice.[5]

Expertise, however, is not the only source of intuitive judgment. Intuitions can also arise from the use of mental heuristics, which are basically short-cuts in reasoning, where you simplify a complex problem in order to come to a decision more easily. Since there are multiple sources of intuitive judgments, and they vary with respect to reliability, it will be important to cover a

how one goes about achieving that outcome (to cope with changes in one's environment), as well as a broad view of the outcome (as in learning how to speak a language, rather than a single phrase).

[4] Feltovich, Prietula and Ericsson, 2006, p. 53

[5] The psychological research "locates automaticity on the backend of development. It is the outcome of repeated experience, of instruction, intentional coaching and socialisation." (Lapsley and Hill, 2008, pp. 324-325)

distinction between two general types of cognitive processing: System 1 and System 2.[6] The distinction between System 1 and System 2 thinking is now fairly commonplace thanks to the work of the psychologist Daniel Kahneman. The distinction between these two Systems is important for understanding expertise, especially the intuitive side of it. System 1 refers to the intuitive side of our mental life, which is automatic and spontaneous. System 2 refers to the kind of thinking we identify with agency – making deliberate choices between options, engaging in various forms of analysis, and exerting self-control. Generally, our behavior is guided by System 1, unless we choose to stop and think about what we're doing, in which case System 2 takes charge. While it might sound from this that we ought to let System 2 take the reins most of the time, that turns out not to be the case. Kahneman's work shows that most of the time System 1 guides us efficiently and effectively, when you consider that by default most of our actions are guided by it. In addition, System 2 requires deliberate effort and attention, which is mentally taxing, and so it limits how often we can engage this system.

Kanheman's work focuses on the heuristics that are used in System 1 thinking to generate intuitive solutions to problems we encounter, especially the unreliability of heuristics. While heuristics provide us with good solutions in many circumstances, they are also the source of systematic biases or errors. The "availability heuristic", for example, is used when people judge the probability of an event occurring based on how easy it is to recall examples of those events. While an event being frequently mentioned might be due to it occurring often, it's also the case that more sensational events get mentioned more frequently (like shark attacks on swimmers which occurs rarely). What's most important for the present purposes, however, is to note that heuristics are not the sole source of intuitive judgments. As Kahneman himself notes:

> the accurate intuitions of experts are better explained by the effects of prolonged practice than by heuristics. We can now draw a richer and more balanced picture, in which skill and heuristics are alternative sources of intuitive judgments and choices. (Kahneman, 2011, p. 11).

Skills are context sensitive, and the accuracy of the intuitive judgments that arise in expertise is due to the great familiarity the expert has in operating in these kinds of situations. For example, the chess expert can have a reliable

[6] This is also known as dual-processing theory. For a critique of this theory, see Kren, G. and Schul, Y. (2009). For a defense from this critique, see Evans, J. and Stanovich, K. (2013).

intuition about what move to make in a situation because of her familiarity with being faced with this kind of board position before. The grounding of intuitions in this way is the reason why the intuitive judgments of experts are seen as highly reliable, in contrast to the inconsistent reliability of intuitive judgments produced by heuristics.

It is important to point out a limiting condition on the development of reliable intuitions arising out of experience and practice. Kahneman points out that we can't necessarily expect expertise to be achieved in all domains. He draws our attention to this in his overall description of what is required to develop accurate intuitive judgments:

> The acquisition of skills requires a regular environment, and adequate opportunity to practice, and rapid an unequivocal feedback about the correctness of thoughts and actions. When these conditions are fulfilled, skill eventually develops, and the intuitive judgments and choices that quickly come to mind will mostly be accurate. (Kahneman, 2011, p. 416)

As noted in other accounts of skill acquisition, practice and feedback are essential.[7] But in order to get useful feedback when one practices, there needs to be some predictability in the environment itself, in the sense that "there are stable relationships between objectively identifiable cues and subsequent events or between cues and the outcomes of possible actions."[8] Practice and feedback are what enable one to pick up on these cues at an intuitive level. However, if there isn't regularity between cues and subsequent events or outcomes, then recognition of those cues won't help you to figure out what to do next. For example, Kahneman argues that there doesn't seem to be enough regularity to the stock market environment to develop expertise in predicting stock prices.

Assuming there is enough predictability in one's environment to allow for the possibility of expertise; intuitive judgment can develop as you recognize cues from similar past experiences, and the outcome of actions that were taken in response. When you recognize that you have been in this situation before, and you have acted successfully in past situations like this one, then you do not

[7] "Whether professionals have a chance to develop intuitive expertise depends essentially on the quality and speed of feedback, as well as on sufficient opportunity to practice." (Kahneman, 2011, p. 241) Kahneman, as an example, compares learning how to drive a car with learning how to pilot large ships in a harbor. The latter is more difficult to learn in part because of the longer delay between actions and noticeable consequences, which leads to slower feedback on one's attempt to pilot.

[8] Kahneman and Klein, 2009, p. 524

need to stop and deliberate about what to do next. This lack of deliberation is supported by the recognition-primed decision model, which was developed with extensive research on the decision making of fireground commanders.[9] This is clearly System 1 thinking at work. All of that practice and experience shapes experts such that:

> experts see the world differently (Johnson and Mervis 1997; Myles-Worsley, Johnston, and Simons 1988). Because they have more and better organized knowledge in a domain, experts perceive things differently than do novices. They perceive different affordances. Perception of affordances is highly influenced by the amount of experience that one has with similar situations. (Lapsley, Narvaez, 2005, pp. 150-151).

Experience not only changes how experts view a situation, it also enables them to efficiently and effectively respond to the situation. A skilled chess player can know which moves to make because of her experiences in playing the game: being in a variety of situations, seeing the possible moves, and knowing which moves worked and which did not. What the player recognizes "includes the type of situation this is, what to expect from the situation (expectancies), suitable goals, typical courses of action (COAs), and relevant cues."[10] This allows her to have an immediate intuitive response about what to do next in the situation.

Part of what follows from this is that unfamiliar or unusual situations will require the expert to deliberate to some extent about what to do, because the expert recognizes that the current situation doesn't easily map onto a previous situation. The expert can run a kind of mental simulation on the initial course of action [COA] that occurs to her. According to the recognition-primed model of decision making:

> Mental simulation is the process of consciously envisioning a sequence of events, such as imagining how a COA [course of action] will play out. This allows a decision maker who knows enough to make accurate predictions to see what the consequences of a particular COA might be. . . If the first COA

[9] Fireground commanders are those commanding firefighters on the scene of a fire. They have to arrive at decisions about how to coordinate the activities of the firefighters to contain the fire and keep everyone safe, based on the behavior of the fire and the skills of their firefighters (amongst other factors). "Data analysis found that approximately 80% of the commanders' decisions were recognition-based. In fact, some interviewees said that they never made "decisions" at all." (Klein, Ross and Shafter, 20006, p. 407).

[10] Klein, Ross and Shafer, 2006, p. 406

> evaluated is found wanting, the expert generates a second and so on, evaluating each in turn but never comparing options against each other.[11]

Initially, one should not necessarily expect expert-level performance from an expert facing a unique situation, even if the experts will reliably perform better than non-experts in such situations. Expertise is limited to a certain background of experience.

Mental simulation is one of the places where we see the interplay between intuitive System 1 and deliberative System 2 in expert performance – System 1 provides the plan, and System 2 checks it.[12] It should be noted that while this kind of mental simulation is a form of System 2 thinking, it does not involve consciously comparing options or applying rules. Attempting to apply rules is what you do when you don't have any experience to draw upon. The courses of action that experts are simulating are drawn from their experience, and occur to them at an intuitive level. Furthermore, there's evidence "showing that when skilled decision makers abandoned their initial COA in favor of one they generated subsequently, the quality of that subsequent COA was significantly lower than their initial COA."[13] So while mental simulation can be useful for experts in some situations, it's not the case that engaging in System 2 thinking always improves upon the results.

While mental simulation engages System 2 thinking, it still operates on a course of action that was initially generated by System 1. So while experts might be able to articulate some of the process of mental simulation itself, they still cannot necessarily explain why they saw situations in a particular light, or why a particular course of action occurred to them.[14] The psychological research demonstrates that "experts often cannot articulate their knowledge because much of their knowledge is tacit and their overt intuitions can be flawed".[15] One reason for the difficulty in articulation is that intuitions arising out of expertise "are due to highly valid cues that the expert's System 1 has learned to use, even if System 2 has not learned to name them."[16] That is,

[11] Klein, Ross and Shafer, 2006, p. 406–407

[12] "The process involves both System 1 and System 2. In the first phase, a tentative plan comes to mind by an automatic function of associative memory – System 1. The next phase is a deliberate process in which the plan is mentally simulated to check if it will work – an operation of System 2." (Kahneman, 2011, p. 237)

[13] Klein, Ross and Shafer, 2006, p. 410

[14] Ericsson points out that "they cannot report why only one of several logically possible thoughts entered their attention, they must make inferences or confabulate answers to such questions." (Ericsson, 2006b, p. 230)

[15] Chi, 2006, p. 24

[16] Kahneman, 2011, p. 240

being asked to give an explanation of one's actions engages System 2 thinking. However, since the recognition of the situational cues and the resulting intuitive judgments are the work of System 1 thinking, an expert can't necessarily explain that part of her cognitive process.[17]

Even when experts are able to articulate an explanation, the explanations are often inconsistent with the observed behavior of the experts. These problems occur both when experts are asked about a specific task they just performed and when asked in general about their methods.[18] Of particular difficulty is getting an answer to the question of why the expert responded one way rather than another. It is important to note, however, that the research does not support the stronger conclusion that experts can never accurately articulate their reasons for action.[19] Rather, there are reasons why such articulation may be inherently difficult, and so articulation is not seen as a hallmark of expertise. In short, expertise is defined by performance, and such those with expertise have not been found to be able to reliably given accurate accounts of their decisions and judgments.

While there was an early hope in expertise research that the knowledge of experts could be extracted and rules could be developed that would greatly reduce the time it took to attain expertise, the problems with getting experts to articulate their knowledge reduced that hope.[20] Even if it was easier to get experts to articulate their knowledge, there's still a problem with trying to map

[17] Researchers involved with expert decision making maintain that "expert knowledge is largely tacit knowledge and can be difficult for the expert to share when asked. We cannot tell someone how to perform largely unconscious processes." (Klein, Ross and Shafer, 2006, p. 412)

[18] Ericsson notes that "When experts are asked to describe their general methods in professional activities, they sometimes have difficulties, and there is frequently poor correspondence between the behavior of computer programs (expert systems) implementing their described methods and their observed detailed behavior when presented with the same tasks and specific situations." (Ericsson, 2006b, p. 231)

[19] Despite these problems, there is a kind of reporting that experts can do about their thought process which does appear to be reliable. Instead of asking experts to explain their behavior after performing some task, experts are asked to 'think aloud' while engaged in performance of the task. While these verbalizations are far more accurate than after the fact explanations, they are not particularly detailed. The reason is that in 'think aloud' experiments "participants were not asked to describe or explain how they solve these problems and do not generate such descriptions or explanations. Instead, they are asked to stay focused on generating a solution to the problem and thus only give verbal expression to those thoughts that spontaneously emerge in attention during the generation of the solution." (Ericsson, 2006b, p. 228)

[20] Furthermore, the discovery of the complex of adaptations that mediate expert performance dispelled "the hope that it would be possible to extract the accumulated knowledge and rules of experts and then use this knowledge to more efficiently train future experts and, thus, reduce the decade or more of experience and training required for elite performance." (Feltovich, Prietula and Ericsson, 2006, p. 61) In addition, Polanyi is often credited as "the first critic who saw that nonconscious and intuitive mediation limits the possibility of eliciting and mapping the knowledge and rules that mediates experts' intuitive actions." (Ericsson, 2006a, p. 12)

out this knowledge, given the complexity of the mechanisms that mediate expert performance.

For example, Allen Newell (personal communication) described a project in which one of his graduate students in the 1970s tried to elicit all the relevant knowledge of a stamp collector. After some forty hours of interviews, Newell and his student gave up, as there was no sight of the end of the knowledge that the expert had acquired. As it may be difficult, perhaps impossible, to describe all the knowledge and skills of experts.[21]

It is important to keep realistic expectations of our ability to describe the knowledge of experts. Of course the research on expertise can extract some of the knowledge of experts, which helps to improve skill acquisition at all levels of performance, but there's no substitute for the role of experience in a variety of situations to achieve expertise.[22] This experience is what allows experts to reliably act well in an automatic and intuitive way.

2. Acquiring Expertise

While expert performance falls mainly within the domain of System 1 (intuitive) processing, there are a variety of ways in which System 2 (deliberate) comes into play in skill acquisition, which is what this section focuses on. Deliberate practice and self-regulating behavior are both essential parts of acquiring expertise, and these draw on System 2. Novices learning a skill will have to pay a lot of attention to what they are doing, and will need to expend a lot of deliberate effort in learning the basics of the skill. Since both self-control and cognitive effort draw on System 2 resources, and this is a limited pool of resources, the more you have to exert self-control to stay focused on the task at hand, the less cognitive effort you can expend on that task.[23] In order to make progress in learning a skill, the currently effortful tasks need to become effortless, in order to free up your attention to handle more complicated tasks.

[21] Ericsson, 2006b, pp. 235-236

[22] "All the paths to expert performance appear to require substantial extended effortful practice. Effortless mastery of expertise, magical bullets involving training machines, and dramatic shortcuts, are just myths. They cannot explain the acquisition of the mechanisms and adaptations that mediate skilled and expert performance." (Feltovich, Prietula and Ericsson, 2006, p. 61)

[23] Kahneman explains that you have "a limited budget of attention that you can allocate to activities, and if you try to go beyond your budget, you will fail. It is the mark of effortful activities that they interfere with each other, which is why it is difficult or impossible to conduct several at once." (Kahneman, 2011, p. 23)

The most obvious way to do this is by practice. Another way to free up resources is to minimize the self-control needed to keep your attention on what you're doing, by being in a state of 'flow', as described in the previous section. Flow, which is part of developing automaticity, reflects System 1 thinking. But such abilities cannot be achieved without a fair amount of help from System 2 thinking, to which we now turn.

Probably one of the most commonly understood aspects of skill acquisition is that acquiring a skill takes "practice, practice, practice". How much practice? Frequent estimates place the amount of time necessary to achieve expertise in any field at 10 years or 10,000 hours.[24] However, mere experience isn't sufficient for achieving expertise. People reach a certain level of acceptable performance, after which further experience does not lead to any improvement in performance. Additional experience may make performing at that level of skillfulness easier, but that is not the same as actually improving one's performance. Thus, the number of years of experience one has is not a sufficient predictor of performance. While having 10 years of experience may be necessary for expertise, it does not by itself guarantee expertise.

What more is needed? Research indicates that a particular kind of experience is necessary for expertise, as it turns out that the quality of the practice matters just as much as the quantity. Improving your level of skill requires not the mere repetition of things you already know how to do, but continually striving to do things that you currently cannot do. This kind of experience is referred to as 'deliberate practice', and it's roughly 10,000 hours of deliberate practice that's needed for expertise. Deliberate practice requires having specific goals in mind for improvement, rather than a more general goal of 'getting better'. There need to be specific aspects of your performance that you go about planning how to improve, which then structures the kind of deliberate practice you engage in[25]. As you engage in deliberate practice you seek out feedback about your performance, in the hopes of identifying and correcting errors. You keep monitoring your progress as you practice. If you don't seem to be progressing, you may need to redesign your practice sessions. If instead you keep up a steady progression, then at some point you reach your goal. At that point it's time to set out a new goal to strive to accomplish. This is how you improve upon your current level of performance.

[24] Horn and Masunaga, 2006, p. 601
[25] Horn and Masunaga, 2006, p. 601

Deliberate practice clearly involves System 2 thinking, as "the requirement for *concentration* sets deliberate practice apart from both mindless, routine performance and playful engagement, as the latter two types of activities would, if anything, merely strengthen the current mediating cognitive mechanisms, rather than modify them to allow increases in the level of performance."[26] This can also be seen in the fact that in addition to getting feedback from others[27], you need to learn how to monitor your own performance while practicing.

Self-regulation is important in acquiring expertise because feedback cannot come merely from others, as crucial as that is in the early stages of skill acquisition. "Because high levels of skill must be practiced and adapted personally to dynamic contexts, aspiring experts need to develop a self-disciplined approach to learning and practice to gain consistency."[28] Often there won't be a coach around when you are exercising your skill, and so you need to learn how to provide yourself feedback on your performance. Therefore, it is important for deliberate practice that you are able to monitor your own behavior during such sessions, so that you can provide feedback for yourself.

Experts need to not only monitor their own behavior, but they also must monitor the environment that they are working in for changes.[29] This is especially relevant when experts face situations that contain features they have little prior experience with. Because expertise develops out of concrete experience, experts will be at their best when facing relatively familiar situations. Thus, experts also need to be aware of when they are facing situations that include unique features, so as to adjust their performance. While they may not perform as well in truly unique situations, they will still fare better than novices. There is a bit of a balancing act that has to be performed between automaticity and monitoring one's environment, as experts still need to be aware of their situation in order to detect features that may be out of the

[26] Ericsson, 2006c p. 692

[27] This opens up the possibility that one makes a correct choice, but the feedback from other people is that the choice was wrong (and this may be simply a mistake or an intentional attempt to discredit the choice).

[28] Zimmerman, 2006, p. 706

[29] "This kind of monitoring prevents blind alleys, errors, and the need for extensive back-up and retraction, thus ensuring overall progress to a goal. In addition, these same kinds of monitoring behaviors are critical throughout the process of acquiring knowledge and skills on which expertise depends." (Feltovich, Prietula and Ericsson, 2006, p. 56)

ordinary. But even this kind of situational awareness can go on with little conscious effort.[30]

Furthermore, once expertise has been achieved in a skill, the same kind of deliberate practice and self-monitoring is necessary to retain expert performance. While everyday wisdom teaches us that once you achieve expertise you never really lose it, research on age and expertise shows that "maintaining skills is as effortful as acquiring them in the first place".[31] Expertise requires some level of routine practice to maintain it or the level of skill degrades over time.

Given the overall difficulty of achieving expertise, one of the most important factors for determining whether someone can attain that level of performance is motivation. "Unless a person wants to pursue the difficult path that leads to the development of talent, neither innate potential nor all the knowledge in the world will suffice."[32] Not only does an aspiring expert need to be strongly motivated to perform well, in the face of adversity and over a long period of time, but even after achieving expertise a high level of motivation is still required to maintain one's expertise.

Having the requisite motivation is not purely an individual affair though. A variety of social factors come into play in supporting expertise: "Becoming an expert in almost anything requires literally years of work. People will do this only if they have some initial success, enjoy the work, and are supported by the social climate. Expertise is not solely a cognitive affair"(Hunt, 2006, p. 36).

Social support can either help or hinder the development of expertise in certain domains (say by devoting public funds towards education and training in certain fields), as well as an individual's motivation to achieve expertise. One key group in the social support of expertise is parents. Since the development of expertise takes considerable time it helps to start young, and supportive parents can have a big impact on this stage of development. Teachers are another key group that can help to motivate students in acquiring skills. Not only is encouragement helpful, but also one's social class in terms of affording coaches and training.[33] Social support can of course work both ways, as there

[30] "Evidence exists, for example, that metacognition can be automatic (Reder & Shunn, 1996), thus avoiding Tulving's (1994) consciousness requirement for metacognitive judgement." Feltovich, Prietula and Ericsson, 2006, p. 57)

[31] Krampe, 2006, p. 733

[32] Csikszentmihalyi, Rathunde and Whalen, 1993, pp. 31-32

[33] "A child's acquisition of expertise in both common and more esoteric activities emerges from modeling, instruction, monitoring, and guidance activities by his or her parents, teachers, and peers within the social

are ways in which society can discourage individuals and groups from the achievement of expertise. It is important to note that there are several issues related to social support for expertise, such as social biases (such as gender, race, and social class) in who is publicly recognized as an expert; and in respect to supporting groups and individuals in their striving to attain expertise.[34]

3. Dreyfus on Expertise

The Dreyfus model of skill acquisition is the most well-known philosophical account of expertise. It represents a phenomenological approach to understanding expertise. While the psychological research in general supports their view, it also provides a more balanced picture of expertise than you otherwise find on the Dreyfus model, which emphasizes the System 1 (intuitive) aspects of expertise while neglecting the System 2 (deliberative) aspects. The following section presents a very brief outline of the Dreyfus model, and then points out those features of expertise that the model neglects.

The Dreyfus model divides skill acquisition into five stages: novice, advanced beginner, competent performer, proficient performer, and expert. At the initial stages of skill acquisition, novices follow simple and context-free rules, such as, in cases of driving, "shift into second gear at ten m.p.h," or use the two-second rule in judging how much space to leave between you and the car in front of you. Since the rules at this stage are context-free, however, they are apt to fail in a variety of different circumstances, such as when driving in the rain or in heavy traffic. As the novice gains experience, she discovers new features of situations, or someone else points them out, as relevant. Instead of relying only upon rules, the advanced beginner starts using maxims, which are not context-free like rules, but rather take into account the new features of situations of which the advanced beginner is aware. A maxim for driving might be "when the engine sounds like its racing shift up in gear." This maxim refers to the situational aspect of engine sounds, which it takes experience to recognize, and so this type of instruction is inappropriate for novices.

milieu of the family, the school, and the community. . . Bloom (1985) found that their parents not only nurtured the child's initial interest and provided or arranged high-quality instruction, they also emphasized the importance of dedicated practice: "To excel, to *do one's best*, to *work hard*, and to *spend one's time constructively* were emphasized over and over again" (p. 10)." (Zimmerman, 2006, p. 706)

[34] Further issues regarding the social dimensions of expert status are taken up in the last section.

Even these maxims have their limitations, however, for the number of situational factors can become overwhelming. Moving beyond maxims requires making choices about what the most relevant factor is in a situation, and this is done by adopting a specific plan or perspective. According to the Dreyfus model, the competent performer feels responsible for both the choice of perspective and the outcome of that choice, and thus becomes emotionally involved in the experience of the outcome. "An outcome that is clearly successful is deeply satisfying and leaves a vivid memory of the plan chosen and of the situation as seen from the perspective of the plan. Disasters, likewise, are not easily forgotten."[35] These outcomes provide the feedback that a person needs in order to improve her skill. The feedback, if positive, reinforces making that choice again in a similar situation. The feedback, if negative, prompts the person to make a different choice in that situation.

While the competent performer has to make up rules to help her decide what plan or perspective to adopt in order to focus in on the relevant features of a situation, the proficient performer no longer uses rules or even makes a choice about a plan. The proficient performer simply experiences the situation in the light of a certain perspective, without making a conscious decision about the most appropriate perspective to take in the situation.

The final stage is that of expertise. Dreyfus discovered that one of the hallmark features of expertise is an intuitive form of decision-making. By 'intuition', he is "referring to the understanding that effortlessly occurs upon seeing similarities with previous experiences."[36] The ability of the expert to act well intuitively is due to the expert's experience and familiarity with the situation in which she acts. The immediacy of the expert's judgment occurs because of repeated exposure to similar previous experiences, and the outcome of actions taken in those situations, so that:

> With enough experience with a variety of situations, all seen from the same perspective but requiring different tactical decisions, the proficient performer seems gradually to decompose this class of situations into subclasses, each of which share the same decision, single action, or tactic. This allows an immediate intuitive response to each situation. (Dreyfus and Dreyfus, 1991, p. 235).

[35] Dreyfus and Dreyfus 1986, p. 26
[36] Dreyfus and Dreyfus, 1986, p. 28

The expert knows what actions are required and how to perform them in that situation without detached calculation or having to weigh alternatives. An expert driver will shift gears when appropriate without even being aware of it. Dreyfus also found that experts frequently were not able to give an account of how they knew what to do. One might be an expert skier, but find it quite difficult to teach others how to ski. On the Dreyfus account, since experts generally act well without applying rules and principles, it is no surprise that experts often find it difficult to explain their actions by reference to principles. Of course some experts are articulate or are good at teaching others, but these abilities are not in any way necessary for expertise.

The Dreyfus account is at its best when discussing expert performance as involving automaticity and intuitive judgment, based on experience and pattern recognition. However, Dreyfus seems sometimes at pains to avoid talk of deliberation and choice with regard to expertise. It's mainly at the early stages of skill acquisition that Dreyfus acknowledges the role of deliberation and the need to make conscious choices between alternatives (i.e. characteristics of System 2 thinking). Dreyfus is correct to note that there's a definite change in performance past the stage of competency, where the higher levels of performance are characterized more by automaticity and System 1 processing. However, Dreyfus underestimates the degree of System 2 thinking in skill acquisition in two respects: 1) the degree to which practice must be 'deliberate practice' involving self-regulating behavior in the early stages of skill acquisition; and 2) that deliberate practice and self-regulating behavior carries over into advanced stages of skill acquisition (even though rule-following does not).

First, the initial stages of skill development on the Dreyfus model are characterized in terms of rule-following. A novice relies on context-free rules, at least until she gains enough experience that she can use more sophisticated rules that refer to situational cues that she has learned to recognize. While this is certainly a familiar aspect of learning a skill, what isn't mentioned is the role of deliberate practice and the need for self-regulating behavior. That is, the focus on the Dreyfus model is what performance is like at each stage of skill development, rather than the factors that enable one to improve (beyond needing more experience). Second, the main deliberative factor in expertise on the Dreyfus model is the rule-following of the novice and advanced beginner, which is something that needs to be left behind to progress to higher levels of skill. Thus, on the Dreyfus model, there seems to be no important deliberative

aspects that carry over into higher levels of performance. However, improvement in one's level of skill always requires deliberate aspects such as deliberate practice and self-regulating behavior. So while rule-following does drop out of the picture at higher levels of skill, not all deliberative aspects from the initial stages drop out. These are important features of expertise that are absent in the Dreyfus model, and certainly skew their view towards an overemphasis on System 1 aspects to skill acquisition and expert performance.

4. Annas on Expertise

Julia Annas frequently discusses practical skills, as she works in virtue theory and sees many structural similarities between the acquisition of virtue and that of practical skills. What is of concern here is the view of skills and expertise that she's working from, rather than her account of virtue.[37] She relies on an intellectual account of expertise, which portrays expertise as more of a matter of System 2 thinking. Her view can thus help correct for how the Dreyfus account underemphasizes the role of deliberative processes in expertise. Though, like Dreyfus, at times she overstates her case, and a few of her claims are not well-supported by the current psychological research. Annas's discussion draws mainly on Socratic ideas about the nature of skills and expertise. According to Annas, there are three necessary elements of a genuine skill: the skill must be teachable, there must be unifying principles underlying the skill that the expert can grasp, and that experts can give an account of skilled actions.

The first element is that the skill is teachable. Since the expert has learned something, she should be able to teach what she has learned to someone else. The expert has learned the theory behind the skill. This contrasts with what Socrates refers to as a 'knack', which is something that can be picked up merely by trying to do it yourself, or by watching someone else do it. Knacks lack the intellectual component that is found in skills. Rhetoric and cooking are putative examples of mere knacks. Genuine skills have a strong intellectual component, and this is what the expert is able to teach.

Annas is surely right that skills are teachable, as coaches and trainers can provide essential feedback and deliberate practice routines for improvement. However, Annas is also making a stronger claim, that the expert, in virtue of

[37] For an example of applying expertise to virtue, see Stichter, 2011.

her expertise, should be able to teach others. The psychological research does not support this stronger claim:

> Although it is tempting to believe that upon knowing how the expert does something, one might be able to "teach" this to novices directly, this has not been the case (e.g, Klein & Hoffman, 1993). Expertise is a long-term developmental process, resulting from rich instrumental experiences in the world and extensive practice. These cannot simply be handed to someone. (Feltovich, Prietula and Ericsson, 2006, p. 46).

Part of the difficulty in teaching is that expertise is not primarily an intellectual grasp of theory, but the development of a number of cognitive adaptations that result from experience and practice. There's no way to gain this kind of knowledge except by going through the same kind of process. Furthermore, one weakness of experts appears to be that they have trouble predicting novice performance, perhaps because they cannot easily take on the perspective of a novice attempting a task. Certainly a good teacher needs to be able to appreciate the perspective of a novice, in order to provide helpful guidance at that stage of skill development. Being an expert seems to carry an inherent disadvantage in that regard. However, one should not overstate the case, as experts can be good teachers. It's just not the case that expertise translates necessarily into being able to teach well. For example, many in academia should be familiar with professors that are good at research, but not so good at teaching.

The second element expands upon the intellectual component found in teaching. To possess a skill requires what Annas refers to as having "a unified grasp of its field." (Annas, 1995, p. 231). This implies that there are principles that unify the field of a skill, and that the expert has a grasp of these principles. There is no such thing as having expert knowledge of only part of the field. One could not claim to be an expert at something as narrow as only being able to fix Toyotas, or to claim, as Ion does, only to know Homer and not much of any other poet. Annas recognizes that:

> This probably surprises us. Surely, we think, Ion does have expert knowledge, only not enough: he knows only part of the field. But Socrates does not accept this way of looking at it. If there is such a thing as the skill that consists in mastering poetry, then it consists in grasping the principles which apply over the whole field. To fail to do this in one area reveals that one cannot do it at all. [...] In each case the skill in question is one that you do not have until you have mastered all the relevant elements in the field. (Annas, 1995, pp. 231–232).

Expertise requires understanding the principles that govern the entire field, and not just some parts of it. This unified grasp is what allows experts to deal with unfamiliar situations in the way that someone who has simply memorized a set of rules cannot, since it enables them to act well with regard to all areas of the field.

However, the expertise literature throws some doubt on experts having a 'unified grasp of the field'. It's not just the case that expertise is domain-limited, but it's also limited even within a domain. Although Annas relies on medicine as the main example of a 'genuine' skill that fits her account of skills:

> studies showed that the same physician can demonstrate widely different profiles of competence, depending on his or her particular experiential history with different types of cases. Indeed, in modern medical education, where assessment of clinical skill is often evaluated by performance on real or simulated cases, it has been found that because of the case-specificity of clinical skill, a large number of cases (on the order of fourteen to eighteen) are needed to achieve an acceptably reliable assessment of skill. (Feltovich, Prietula and Ericsson, 2006, p. 47).

Expertise arises out of experience, and one's experience places a limiting factor on which situations one can display expert-level performance. Psychological research emphasizes that experts rely on contextual cues, such that when the expert is operating in an unusual context, they lack information necessary to perform at an expert level.

> For example, in a medical domain, experts seem to rely on the tacit enabling conditions of a situation for diagnosis (Feltovich & Barrows, 1984). The enabling conditions are background information such as age, sex, previous diseases, occupation, drug use, and so forth. These circumstances are not necessarily causally related to diseases, but physicians pick up and use such correlational knowledge from clinical practice. [...] The implication is that without the contextual enabling information, expert physicians might be more limited in their ability to make an accurate diagnosis (Chi, 2006, p. 25).

Annas is correct in thinking that experts should be able to generalize to some extent from their experiences, such that they would have some idea of how to act well in unfamiliar situations. But it is important not to overestimate how well experts will react in novel situations, since their expertise is still linked to a

certain history of experience.[38]

The third element of a genuine skill further develops the previous intellectual components, by requiring that experts have the ability to 'give an account' of their actions. Giving an account, according to Annas means "that the person with a skill be able explicitly to explain and justify her particular decisions and judgements, and to do so in terms of some general grasp of the principles which define that skill."(Annas, 1995, p. 233). The expert needs to be able to articulate the reasons for her actions, and this explanation should draw upon the expert's grasp of the principles underlying the skill. Although this condition could be thought of as requiring merely that the principles are articulatable, rather than requiring that the expert can actually articulate the reasons herself, Annas explicitly describes this requirement in terms of the expert being able to articulate the reasons for her actions.

As discussed earlier, research shows that experts cannot be relied on to accurately articulate their reasons for action. However, in her defense, Annas is aware that the three essential elements form a high intellectual standard for skills that strikes people as counterintuitive. She notes that:

> This idea, that conveying and acquiring a skill requires articulacy, often meets resistance. This may take the form of pointing to skills where articulacy does not appear to be necessary; sometimes gardening is given as an example. In some cases, such as physical skills, the person outstanding in the skill may not be the best at conveying it (as with athletes and coaches). Many of these will be cases where what is at stake is really mastery of technical matters needed for the exercise of the skill, or where what is important is natural talent. (Annas, 2011, p. 19).

Annas admits that the requirement of giving an account is not true for a number of actual skills. For Annas, this result is not problematic, so long as there are some skills that do display these strong intellectual components, because it's those kinds of skills that she thinks share a structure similar to virtue. So her claims should not be read as applying to everything we might label a skill.

[38] "For example: "The experienced pilot who has never encountered or been trained for a particular anomaly will be challenged to process information in working memory to determine what is happening, and may be inefficient in searching for relevant information to solve the problem, in much the same way as when she was a novice pilot (although it is likely that she will not be as bad off as a complete novice). Most people do not operate at the level of novice all the time or expert all the time, but rather move around in between, using combinations of cognitive mechanisms depending on the situation at hand and the availability of key constructs (e.g, mental models and schema)." (Endsley, 2006, p. 640)

That there are skills that display the three intellectual components, however, is far from clear. One of the skills she does mention is medicine, but evidence from the medical field calls into question experts displaying all of these strong intellectual components. For example:

> Bias is probably one of the most serious handicaps of experts, especially in the medical profession. . . . my colleagues and I found the experienced physicians to manifest serious biases. We presented several types of cases to specialists, such as hematologists, cardiologists, and infectious disease specialists. Some were hematology cases and others were cardiology cases. We found that regardless of the type of specialized case, specialists tended to generate hypotheses that corresponded to their field of expertise: Cardiologists tended to generate more cardiology-type hypotheses, whether the case was one of a blood disease or an infectious disease (Hashem, Chi, & Friedman, 2003). This tendency to generate diagnoses about which they have more knowledge clearly can cause greater errors. Moreover, experts seem to be more susceptible to suggestions that can bias their choices than novices (Walther, Fiedler, & Nickel, 2003). (Chi, 2006, pp. 26-27).

The psychological research thus appears to temper some of Annas's claims about expertise, when she overstates the intellectual aspects.

Finally, there is one other reason to be concerned about overemphasizing the role of articulation in expertise. Patricia Benner[39] carried out studies of experts in the field of nursing. When studying nurses with a track record of life-saving decisions in emergency situations, she found that often the nurses could not fully articulate how they knew what to do. Benner quotes an expert psychiatric nurse clinician who is talking about her clinical judgments:

> When I say to a doctor, "the patient is psychotic," I don't always know how to legitimize that statement. But I am never wrong. Because I know psychosis from inside out. And I feel that, and I know it, and I trust it . . . One of the things that I am doing now is getting some in-service in to talk to us about language. But all I am really trying to do is find words within the jargon to talk about something that I don't think is particularly describable. (Benner, 2001, p. 32).

If we view articulation as a necessary component of expertise, then this nurse would seemingly not count as an expert because she is not able to give an articulate justification for her clinical judgment. But this would reach the

[39] Benner was applying the Dreyfus model in particular.

wrong conclusion, as Benner informs us that this is a nurse who has over 15 years' experience in the field and who is reliably correct. One of the most serious problems for the nurses is that their judgments were not taken as seriously as doctors because of an assumption that their lack of articulation signaled a lack of knowledge, and so they were also accorded less power and status within the hospital. So it matters a great deal that we get an accurate picture of what really goes into acquiring skills and expertise. There are important intuitive and deliberative aspects to both skill acquisition and expert performance. The psychological research helps to correct those philosophical accounts of expertise that overemphasize one aspect over the other.

5. Expertise *versus* Experts

In this final section, the discussion transitions from expertise to experts. It might be thought that an expert is just someone who has expertise, but it's important to distinguish between having expertise in a field and being credited as an expert. As discussed above, expertise refers specifically to the highest levels of skill that human beings have reached. Acquiring a skill is a matter of getting better at some task, and so the possession of skill is a matter of degree. Progression through the stages of skill acquisition is mostly a matter of experience and practice. While there are frequently social aspects to becoming skilled, such as who is encouraged to pursue different skill sets or who can afford access to training, acquiring expertise in a domain does not require being credited as an expert. For example, one could be a complete novice at fishing, be stranded alone on a desert island for 10 years, and with practice and experience during that time, develop a high level of skill at fishing, and even achieve expertise in it. So expertise, as I refer to it here and as it's studied in the psychological literature, is at its core a study of the phenomenon of how people get better at tasks.

Being credited as an expert is not the same thing as having expertise. There are a different set of questions that get raised when inquiring about who should be credited as an expert. Presumably the point of crediting people as experts is typically that there is something that we want from them. We hope that those we deem to be experts accurately tracks those who have achieved expertise in a field, but the two can come apart (perhaps due to bias or incomplete information). You can have people credited with expert status that lack expertise, and people with expertise may not be granted expert status. One of the main concerns with experts is that being credited with expert status can confer power and authority

that is distinct from the expertise itself. So while it's relatively uncontroversial that people can achieve expertise, what is controversial is our attempt credit people with expert status.[40] With respect to determining who to confer expert status upon, there are a host of social, political, and epistemological concerns.

While addressing all these concerns is far beyond the scope of this paper, in doing so it will be important to keep in mind not only what expertise is like, but also the difference between expertise and being credited as an expert. Deciding who to credit as an expert is not like describing a natural kind. We credit people with expert status because it serves a useful function, and we decide who to confer this status upon depending on what we want from them, and this also determines what power we choose to grant to them. So we want to avoid what some have termed an "immaculate" conception of the expert – someone who counts as an expert whether we like it or not, and to whom we must defer judgment.[41] While someone can have expertise without any social validation, the same is not true with regard to possessing expert status.

Not keeping this distinction in mind can lead to a very misleading picture of what it is to be an expert. No doubt some putative expert might want to push the immaculate conception on us, but we should resist it. While someone may be able to demonstrate their expertise, it doesn't follow from that demonstration that we should accord that person the status of an expert. If someone loudly declares "I'm an expert", then we can always reply "Only if we say you are". Expert advice can be ignored, and expert status can be revoked, since ultimately we're doing this (if at all) because we find it useful. In addition, seeing a few of the limitations of expertise, as mentioned above, can be helpful in reminding us of the fallibility of those who have achieved expertise. One hope for this account is that it not only helps to inform discussions of experts, but also to contest and challenge expert discourse.

[40] For these reasons, I try to talk in terms of crediting people with expert status, to emphasize that we confer this status on others (along with whatever power goes along with it). I try to avoid talking in terms of 'recognizing' or 'identifying' experts, as that may suggest people are experts independent of us conferring that status on them.

[41] See Turner, 2001.

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A Paradigm for Your Thoughts: A Kuhnian Analysis of Expertise

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ABSTRACT

It will be argued that the "problem of demarcation" and the defining of "expertise" share common structural features that can lead to either a type of strong relativism (everyone is an expert) or ultra-scepticism (expertise does not exist). Appropriating notions from Thomas Kuhn's (1996). The Structure of Scientific Revolutions it will be argued that an "expert" in a field that has a dominant paradigm is different to an "expert" in a field that has multiple competing paradigms. To illustrate my argument I will look at the field of economics and the competing claims of experts over the likelihood of a global recession circa 2005. To this I will apply Goldman's (2001) criteria for expertise assessment and by-way of a hypothetical non-expert show that this criteria becomes deficient in expertise assessment if we only hold to what I have called a "methodological" definition of expertise. I will also introduce the notion of the "anti-expert" who is an equivalent expert, but their whole field is dependent upon the dominant paradigm for its meaning. That is, its existence is parasitic upon the success of the paradigm, rather than as a "revolutionary science" which looks to overthrown or change the paradigm.

Keywords: expertise, economics, Thomas Kuhn, paradigm, normal science, demarcation.

Introduction

The problem is simple, how does the average person tell an expert from a non-expert, the professional from the charlatan? Upon whose advice should we act and whose should we steer clear of? This is an old problem going back to Ancient Greece, found in the Platonic dialogue *Charmides*. Here Socrates

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discusses the virtues of temperance and sophrosyne. He makes the point that in order to judge a real physician from a quack, one has to be qualified to tell the difference. If wisdom is the difference between knowing what you do know and knowing what you do not, no one would ever make a mistake, but as people do make mistakes, Socrates concluded that science (a way of determining knowledge) is impossible (Tuozzo, 2011). Whilst the "science" of Ancient Greece and the 21st Century are markedly different our scepticism over "expertise" appears to be fairly similar. The 2nd and 3rd waves of sociology of science studies challenged the notion of "expert" as not belonging solely to institutions, but extending to the public (Collins and Evans 2002). Yet intuitively we would feel uncomfortable about attributing "expertise" to everyone, even if "expertise" comes in degrees. The common notion of "expertise" is that it can be gained by way of specialised education, culminating in recognised qualifications from accredited awarding bodies or it results from a lifetime spent "in the field". Both notions, however, pre-suppose a "field" that one will have expertise in. What then makes something a "field"? It is not something private as people have to be able to share knowledge, allowing for opportunities of agreement and dissent. It is this ability to bring about agreement and dissent, I will argue, is abundant in a "field" that contains what Thomas Kuhn called a "normal science" by virtue of a ruling dominant "paradigm". Where this condition is lacking the ability to bring about consensus or meaningful dissent is minimal. What we are faced with here is two ways of doing science that *mean* something different to one another. That is, what it means to be doing science in a field that has a dominant paradigm/ normal science is not the same as doing science in a field that lacks one. Consequently, I argue that what it means to be an expert in each of these fields also has to differ.

Returning to the original problem: how does the layperson tell the difference between the expert and charlatan? One attempt to answer this question was a criteria given by Goldman (2001), where if met we could identify the expert and thus know who to listen to. This was not offered as a full-scale theory of justification by testimony, but that the "hearer's evidence about a source's reliability or unreliability can often *bolster* or *defeat* the hearer's justifiedness in accepting testimony from that source" (Goldman 2001, 88). Goldman asks the layperson to:

Examine the arguments presented by the experts and their rival(s).

> Look at the consensus between experts.
>
> Assess the independent evidence that the expert is an expert.
>
> Investigate whether the expert has any personal bias or investment in their claim.
>
> The track record of the expert. (Goldman, 2001, 93)

These are all very reasonable and as a general rule-of-thumb seem to work very well when applied to a domain that has a "normal science", but becomes unusable in fields that lack the features of "normal science" or a dominant "paradigm". Why this becomes a problem is that if we only have a definitional notion of expert in mind (an expert has the properties *x*, *y*, *z*...) when this becomes deficient we can be led down the road of saying that either expertise does not exist or that expertise can apply to everyone. This complication also arises in the "problem of demarcation" (how to tell a science from non-science). Here Karl Popper attempted to give what I call a "methodological" definition of science, that is, science is "falsification", the testing and refutation of bold conjectures. However, by examining the historical archive we can find instances that go against not just Popper's criteria of "falsification", but any methodological criteria that aims at consistency. The criticism that Kuhn made of Popper in forwarding "falsification" as the way of separating science from non-science is the same point I wish to make of Goldman's criteria of expertise.

Popper's "normal science" of bold conjecture and refutation was, for Kuhn, his "revolutionary science". Popper argued that everyday science works by challenging the deepest assumptions of theory, but Kuhn argued, this activity is only found during periods of paradigmatic change (Kuhn 1999, 5-6). Popper had taken the meaning of "normal science" and applied it across all historical variations of science. Equally, Goldman's criteria is only relevant to those fields that have a "normal science" and do not work in fields that, still contain experts, but no ruling paradigm. To demonstrate this point I will look at what a ruling paradigm and normal-science allows us to say (it's meaningful possibilities). Here not only does Goldman's criteria work extremely well, but it also produces a new type of expert, which I will call an "*anti-expert*". This is someone who has had all the training, is incredibly knowledgeable, but the "field" they are knowledgeable of or expert in is only meaningful because it is incongruent to the

ruling paradigm. It is parasitic off of the paradigm's success.[1]

In order to show that Goldman's criteria for expertise assessment breaks-down in cases of disciplines that lack a ruling-paradigm/ normal-science, I will look at an episode from economics centring around the 2006 International Monetary Fund seminar, where Nouriel Roubini and Anirvan Banerji made opposing claims over the likelihood of a global recession. I ask whether a hypothetical non-expert sat at that conference could, using Goldman's criteria, work out who to listen to. I argue that either one cannot decide or actually becomes worse off, because the sort of expert it is written about is not the sort that exists in economics.

To begin, however, I will introduce the structure of the problem and present its similarities to the "problem of demarcation". I will then explain the Kuhnian terms I will be using, applying them in an analysis of the "evolution vs. creationism" debate, and using Goldman's criteria seeing if we are able to tell which expert to listen to, here introducing the notion of "anti-expertise". Lastly, I will look at the exchange between Roubini and Banerji and the general state of financial expertise circa 2005 and see whether those same criteria hold good.

1. The Problem of Demarcation: Expert in What?

The "problem of demarcation" has famously alluded analytical philosophers as to what makes one thing a science and another not. The most famous answer to this problem was offered by Popper (1963; 2002) as the process of testing bold conjectures and accepting those that pass as "yet to be falsified" rather than as "true". What Popper sought to do was construct a totalizing methodological account of what science is, which today is how the vast majority of people understand it. "science" is falsification, hypothetico-deductive, empirical, evidence based, inference to the best explanation, and so on. A problem with this approach is that these are abstract, metaphysical, accounts of what science is – should be, rather than looking at what scientists have historically done. Those philosophers of science with a historical orientation quickly set about giving examples when criteria like "falsification" was, not

[1]Sorensen (1987) uses the same term, but it differs by instead of dealing in a methodological definition of "truth" it looks at how fields of practices are related and how they may become dependent or parasitic upon each other for their meaning.

only was ignored, but acted as a barrier to scientific progress (Kuhn1962; Feyerabend 1975). The point here was that what "knowledge" *is*, the "facts", is what science looks to overcome, which means potentially acting in "non-scientific" ways according to the standards of the time. In retrospect, however, we can accommodate any action "after-the-fact" by how we tell the history of science. We can look at the figurative footprints in the sand of time and retrospectively say how we arrived at where we are, as if the present was an inevitability of the past. The likes of Kuhn and Feyerabend sought about conclusively showing why one could not produce a definitive list of what makes one thing a science and another not. Their arguments, whilst varied, both hit upon historical critiques of those methodological criteria imposed upon things like "truth", "rationality" or "objectivity". Put simply, they asked what happens if you answer an historical question: *what is science?* with a methodological answer: *falsification* (or any criteria). What you get is either a failure to answer the question or seemingly radical answers – science is not about truth or operate rationally. This led to a period of academia known as the "science wars" where the problems of confusing historical and methodological questions and answers became amplified. How does a second order activity like sociology, history or philosophy know more about how science is conducted than a scientist? Or we could ask, who do we trust over matters of science? Who is the expert?

Intuitively we might say that scientists are the experts on science and the failure of the philosopher to demarcate science from non-science is the mark of a redundant practice that contributes little to science. Here there is the apocryphal quote from Nobel-Prize winner Richard Feynman who claimed that the "philosophy of science is as useful to scientists as ornithology is to birds" (Kitcher 1998, 32). This is normally taken to mean "useless" and a standard put-down by scientists, but anytime spent reading Feynman's thoughts on science we might see he is closer in agreement with historical philosopher's of science than some might be comfortable with. Four years after the release of Kuhn's *The Structure of Scientific Revolutions*[2] Feynman (2001) addressed the "National Science Teachers Association" with a talk titled, "What is Science?". Feynman rather than addressing the question directly, preferred to say what science was not, which among other things he states it is not its *form*

[2] Referred to as *Structure* from now on.

or *content*, as it is both of these things that science has to overcome in order to progress. Attempts to codify science as either a method (Induction from observation/ falsification – Bacon, Popper) or in terms of the knowledge it produces, Feynman says, "so what science is, is not what the philosophers have said it is and certainly not what the teacher editions [textbook] say it is" (Feynman 2001, 173). He argues that we confuse abstract scientific terms with the referent of experience, so that "when someone says science teaches such and such, he is using the word incorrectly. Science doesn't teach it; experience teaches it". (Feynman 2001, 187). Feynman also states that learning terminology or what things are called is also *not* science, they are the tools of a scientist, but not science itself. Ultimately, the nature of the question means it is easier for Feynman to *do* science than say *what* it is that he is doing. Feynman recognised that the actions of scientists do not conform to any prescribed method for doing science, for it is this, or "experience", that has to be overcome in order for progression. This critique is echoed by Kuhn and Feyerabend, where there are two notions of science at work (historical and methodological), but when an historical critique of this methodological approach is conflated for a competing methodological claim itself – we then get the worries of the rationalist over Feyerabend's "anything goes" ethos. Feynman's argument and additional notion of "cargo cult" science (Feynman 1992, 346) suggests that one could be mimicking everything that "proper" scientists do and still not be doing science, or good science at any rate. Moreover, there are instances of highly qualified professionals and Nobel-Prize winners having conjectured decidedly "non-scientific" ideas counter to the scientific community yet we cannot accuse them of not knowing the "rules" of science or not being expert enough.[3]

Why is it that if something like "climate change" or "evolution" as a phenomena is part of expert consensus, why can we find "experts" who are willing to disagree? The answer from the rationalist camp is that a microbiologist that does not believe in evolution or geologist that does not believe in climate change is just a bad scientist, yet this does not answer why a Nobel-Prize winner or Fellow Royal, who is demonstrably good at what they do,

[3] Nobel-Prize winner Brian Josephson takes seriously the matter of telepathy, parapsychology and "water memory" (Stogratz, 2004), Julian Schwinger suffered editorial censorship on his research into "cold fusion" (Mehra and Milton, 2000), and Professor of Pharmacology and Fellow of the Royal Society of Chemistry Arthur Ernest Wilder-Smith believed dinosaurs and humans lived together (Wilder-Smith, 1981).

is able to entertain non-scientific ideas. When we say "good" or bad" scientist, this is not a moral judgement, but the inability of scientists to conform to the rules and norms of their field. The "rules" of science make it almost impossible for a biologist to deny the veracity of evolution and still be a biologist in any meaningful sense of the term. To know these "rules" is to understand science through its methodological definition – which appears to work perfectly well and we can make the call of "good" and "bad" science when it happens. I will argue, however, that for the same reason Popper's attempt to demarcate science from non-science failed we cannot apply a blanket definition or criteria for "expertise" over all fields that contain experts, because in order to make the call of "good" and "bad" requires the field to have developed "rules" or a stability to the extent that a methodological definition can exist. Put another way, what it means to be an expert is relative to the maturity of the field one is suppose to be an expert of.

In order make my point I will take Goldman's (2001) methodological criteria for assessing the claim's of experts and show that for fields that, in Kuhn's language, have a ruling/ dominant paradigm Goldman's criteria holds good, but in other fields that have multiple-competing paradigms/ none, we are either unable to tell who one should listen to or actually become worse off by following those strict methodological guidelines. The point is not that Goldman is wrong, but that what it means to be an "expert" is relative to the field one is an "expert" in. That the expectations and demands of expertise should change according to state of the field they practice in.

Before I proceed, however, I will need to explain the Kuhnian terms I will be employing and how I am not using them. I will also introduce my term of "anti-expert" and how this differs from a "bad" expert.

2. Thomas Kuhn

Kuhn in *Structure* (1996) not only critiqued the abstract way in which science had been treated, but also developed a number of concepts that will help frame the issues at stake in discussing "expertise". An expert is implicitly linked to the thing they are an expert in, so any comment on the state of a field is also a comment on the people that practice it. Part of Kuhn's philosophy was to say that a "science" can only advance if it brings about a state of "normal science", which is dependent upon their being a shared agreement (tacit and explicit)

about what counts as the fundamentals of that field. A useful analogy here is to games, that if everyone if playing by different rules we have no coherent sense of what the game is or what the point of the different activities are. If, however, everyone is in agreement with what the rules are, not only can there be a shared sense of purpose, but we can also make calls of "good" and "bad" play. We know what "good" football looks like because we know what "football" is, however, an ad-hoc combination of golf, tennis and football makes it almost impossible to say anything about the state of play. Just as we "know" what football is and discuss and play it with relative ease the thing that performs this function in science is what Kuhn called a "paradigm". A "paradigm" provides everything for the scientist, from what problems to solve to what counts as an observation or evidence. It gives *meaning* to the empirical content of theories and at the same time allows those theories to be considered as candidates for explanation (Kuhn 1996, 188). Through the stabilizing affect of a paradigm, a field acquires a "normal science", which permits in-depth, almost esoteric levels of inquiry, that allows scientists to solve "problems that its members could scarcely have imagined and would never have undertaken without commitment to the paradigm" (Kuhn 1996, 24-25). A "paradigm" is not "normal science" but it is necessary for it to be practised. The level of scrutiny that "normal science" permits can bring about the paradigm's own downfall, which puts the field into a state of uncertainty, ambiguity or "revolution". Here what was agreed upon as the fundamentals of the field have been brought into doubt and new answers and sought.

Lots of early problems with Kuhn's work was trying to articulate what a "paradigm" was. A "paradigm" can be understood in both a "historical" and "methodological" sense – it is methodological in that it has exemplars (the Newtonian paradigm deals in mechanistic forces, aether, and so on), it is also historical in that those actions of scientists (Newton) can only be performed because they are meaningful given the time that they lived in. It is the paradigm that allows their actions to be meaningful, thus they can operate in relative freedom. When we are in a paradigm we do not see it or experience it, instead we talk unproblematically about "reality".[4] This coherency allows "normal science" to flourish and flesh-out the paradigm. It is only when the paradigm begins to fall apart do we experience it and its incongruity with the "world". A

[4] I have used the term "reality" several times already and if the reader glossed over these intuitively knowing what I am referring to, this should give you an indication as to how transparent paradigms are.

problem, however, is that philosophers have routinely confused these two notions, where a paradigm is an *interpretation* of the world, but not the world itself. Here we can think of "paradigm" as a background of meanings which when brought into doubt changes our relationship with the "world". In places where we thought we knew what we were measuring or observing it is now not so clear. In our attempts to deal with the problems generated by the collapse of the old paradigm and its replacement we have the act of "revolutionary science".

Another difficulty people have had with Kuhn's work is if we confuse "cutting edge" science for "revolutionary" science. This appeared to be Popper's problem with the idea that science should always be in a state of "revolution", never settling for the dogma of received wisdom. Popper argued that "everyday" science works by challenging the deepest assumptions of a theory, but this activity, Kuhn argued, we only find during periods of paradigmatic change (Kuhn 1999, 5-6). Rather than generating problems for the paradigm normal-scientists tend to be involved in "puzzle-solving", expanding the paradigm to cover more and more phenomena. The stability generated from paradigmatic commitment and its "normal science" then allows us to distil an abstract methodological conception of those activities. It allows a certain type of expertise to be known, where we can tell "good" from "bad" practitioner. However, during times of "revolutionary" science we do not know what counts as a "puzzle" so we have no way of telling if someone is any good or not at what they are doing. The fear here was that we descend into relativism if we have no way of telling who is correct, but what undergoes "revolution" is the *meaning* of things, objects, equation, theories, experimental results and so on. We do not cease to be scientists. For in the absence of a paradigm we only lose the dominant *meaning* of the "world", but not the world itself.[5] As we exist in the world before we learn to abstract it through "objectivity" there is a large tacit element that goes unchanged and which allows us to navigate even without a concrete set of scientific meanings. We are first always already in a world that is meaningful and then we abstract it using methodological tools like empiricism to create another set of meanings, which would not be possible if we were not already tacitly involved with the "world". This approach is hinted at in certain passages of *Structure* and Kuhn's

[5] In the more radical readings of Kuhn, "paradigm" becomes confused for the "world", so during times of paradigmatic shifts we find ourselves *literally* in a different world (Mayoral 2012, 276).

recruitment of Polanyi, but was not developed very far.[6] The problem is that "knowledge" by definition is explicit and so "none of these crisis-promoting subjects has yet produced a viable alternate to the traditional epistemological paradigm", and is not likely to as science only deals in what can be said (Kuhn 1996, 121). In our football analogy it is our tacit understanding of "sport", "games" or "play" (as situated by the world) that allows incongruent actions, such as someone handling the ball (whose is not the goalie), to be intelligible on the whole and only unintelligible according to the rules. This "unintelligible according to the rules of *x*" is another way of presenting Kuhn's notion of "incommensurability". This I understand as a simple disconnect of meaning, where someone else's actions are unintelligible, but *not* incomprehensible. It is this that allows those working in different paradigms to communicate because we share a world even if we interpret it differently.

These notions of "paradigm", "normal"/"revolutionary science", and "incommensurability" will all affect what we can expect of an "expert" from a science in its corresponding stage of development. Next, using those Kuhnian concepts described I will articulate the relationship between them and how they affect the meaning of "expertise" and its "normal-science" counter-part, the "anti-expert" .

3. The Good, The Bad and The Anti-Expert

If a field of study is ruled by a singular paradigm the "normal science" it enables means we can produce an abstract methodological definition of those involved within the field. Here something like Goldman's (2001) criteria helps the non-expert assess who to trust:

> Examine the arguments presented by the experts and their rival(s).
>
> Look at the consensus between experts.
>
> Assess the independent evidence that the expert is an expert.
>
> Investigate whether the expert has any personal bias or investment in their claim.

[6] Polanyi indirectly read Heidegger which in turn possibly affects Kuhn's notion of the "tacit". The postscript to *Structure* has a discussion of the "tacit" but by this time Kuhn had shifted to a linguistic and cognitive analysis of the same phenomena (Kuhn 1996, 191-198).

The track record of the expert. (Goldman, 2001, 93)

Taking the on-going "internet debate" between evolutionary theorists (ET) and intelligent design creationism (IDC), using Goldman's criteria could a neutral non-expert decide on which side of the debate to stand? As the literature is vast on the differences between these positions and this criteria was played out in the *Kitzmiller v. Dover Area School District* court-case I will spend more time on developing the original notion of the "anti-expert".

The *Kitzmiller v. Dover Area School District* (2005) case was to determine whether the teaching of "intelligent design" was in fact a competing scientific theory. One of the expert witnesses in favour of the proposition was the biochemist and creationist Michael Behe. It was eventually ruled that IDC is not a scientific theory because, among other things, it requires belief in the supernatural (against naturalism), it is not accepted by the majority of the scientific community, any evidence for IDC can be explained by other means, and Behe's track-record was dubious concerning his notion of "irreducible complexity" and argument that evolutionary theory could not explain the development of the immune system.[7] It would seem that a reasonable non-expert sat in the courtroom gallery would do well to follow Goldman's criteria and conclude with the judge that whilst Behe is an expert trained in biochemistry, he is in the minority. A common response here is that Behe and those like him are simply "bad" experts or scientists. Whilst we can make calls of "good" and "bad" practice within "normal science" I think someone like Behe goes beyond the category of "bad" expert to what I have called an "anti-expert". So what is an "anti-expert" and how do they differ from "bad" experts?

An "anti-expert" is not an amateur or novice who is simply mistaken, for like Behe, they are trained, received qualifications from accredited institutions and hold positions of authority.[8] Behe knows enough about biochemistry to be called an expert witness in court and certainly enough that the average person could not debate the details of his position. But why is he not just a bad biochemist? The problem is that the wider paradigm that gives meaning to Behe's world-view is parasitic upon the success of ET. The reason IDC can be taken as weak competing hypothesis is that metaphysically and historically it is

[7] Many of these arguments are presented in Behe's (1998) *Darwin's Blackox*.

[8] Behe gained his Ph.D. in biochemistry from the University of Pennsylvania, he completed postdoctoral work on the structure of DNA and now holds a professorship in biochemistry at Lehigh University.

dependent upon ET (orthodox science) being, approximately, correct.[9] How so? Metaphysically the IDC theorists are looking to replicate the methodological interpretation of science, i.e., science is falsification, logical inference, testing, evidence based, peer-review and so on, whilst trying to *prove* ET wrong. Yet we would not have the ET model if those things did not work. Moreover, the ability to interpret biblical passages *as if* they were empirical statements pre-supposes the methodological viewpoint, a viewpoint that did not exist in the pre-modern middle east. This methodological approach is a relatively new addition to human thought developing over the last 400 years. So in essence, the IDC person wants the metaphysics of science without any of its historical development making it an anachronistic practice. The anti-expert's field is dependent upon the paradigm it is looking to replace for its meaning, be it the practices they engage in or the phenomena they interpret. This, however, will in and of itself not generate success, as Feynman says – *science is not its form or content.* Science proper is the overcoming of these conditions, not the replication of them.

So if we can caricature science how do IDC theorists try to copy its form and content? IDC contains trained and qualified scientists, they publish in peer-reviewed journals, they do experiments, they have theories, explanations, and evidence for their position. Common IDC arguments are the lack of transitional fossils, that kind only begets kind, and that radio-carbon dating is unreliable. Now why we might be tempted to say these are just "bad" scientists for forwarding such ideas is to miss Feynman's point about overcoming the form and content of science as replicated in experience. Historically scientists have done what IDC members are doing, that is, offer up a competing hypothesis for the meaning of things, which by the standards of the day appear "unscientific". In the case of IDC, however, in order to view passages in the bible as *objective*, *empirical scientific* statements (age of the universe; order of creation; miracles) it necessarily requires the metaphysics of "objectivity" and "empiricism" that are intrinsic to "normal science". So historically, without the 400 year development of the scientific world-view we would not have modern IDC "alternative". It is not simply that IDC is run by incompetents for they are trained bio-chemists, microbiologists, geologists, immunologists, but their "expertise" and field of IDC science is *only* meaningful because of the

[9] The relationship between history, metaphysics and science is discussed more generally by Agassi (1975).

success of the dominant paradigm it seeks to replace.[10]

So the difference between an "anti" and "bad" expert is whether their "expertise" is governed by the standards of the paradigm, as if they were trying to play the same game as the other normal scientists, or their "expertise" is dependent upon its incongruity to the paradigm. Here they would be trying to play a wholly different game, but nonetheless are restricted by the concept of what it is to meaningfully "play a game". So regardless of how "revolutionary" or "alternative" they think they are, things like rules, players, equipment, and so on are needed in order for it to be recognisably a "game" and the same is for science. It is this restriction which confines "anti-expertise" to the rules and boundaries of the dominant paradigm, making them reliant upon it rather than unconstrained and independent of it, which is the epitome of "revolutionary science". For example, what a modern astrologer means by "planet" is not what a 1st century astrologer would have meant by it. Whilst we might feel that the referent has been preserved (the Sun), when an Ancient Greek astrologer looked to the sky they did not see what we see, for the Sun was bound up with mythology and cultural identity, which have been unavoidably altered by the modern developments of astronomy and cosmology (Himanka, 2005; Vrahimis, *2013*). Any attempt to be an astrologer has to anachronistically over look everything accomplished by modern science and any attempt to be an astrologer in the modern sense is dependent upon things like the shift from a geo to heliocentric universe, the discovery of extra constellations, the demotion of planets to dwarf planets, and so on. Due to this parasitism on "normal science" and the "ruling paradigm" they also get to share in the stability it brings allowing for claims of "good" and "bad" practice. This fits with Gordin (2012) who argues that the prevalence of pseudo-sciences are proportional to the health of a paradigm, so the stronger the paradigm the more alternative practices it can generate.[11]

The field of the "anti-expert" only exists because it is able to stand in some relation to the ruling paradigm – it gains its meaning from the success of the paradigm where it wants to replicate the metaphysics of "normal science", but without any of the historical development making it anachronistic. So what

[10] Professor of Pharmacology and Fellow of the Royal Society of Chemistry Arthur Ernest Wilder-Smith believed humans and dinosaurs lived together due his Christian fundamentalist view (Wilder-Smith 1981).

[11] "Quantum" is very much in fashion now as it lends itself as a fitting metaphor to what we might describe as esoteric or "new age wisdom". The metaphor, however, only carries credibility because we know the actual science of quantum mechanics works. See Chopra (2010).

happens when we have multiple paradigms competing for the meaning of things?

The way we generally acknowledge the lack (or multiple presence) of a paradigm is by uncertainty, ambiguity or confusion over the question. If a "paradigm" tells us what things mean, i.e., what counts as evidence, an observation, a theory, an answer, which allows us to minimise doubt, if this is removed the domain of "expertise" opens ups. For when someone is an expert, they are an expert in something. That "something" is given by the paradigm. If what that "something" means is widely contested then there is much more ambiguity over what they are an expert of. "Expertise" here is much more diffuse over a range of potential objects, but due to the lack of stability a ruling paradigm brings, the range of potential "things" is uncertain and is what has to be argued for. Thus it is much harder, if not impossible, to distinguish "good" from "bad" expert. Due to the range of possibilities there is nothing "concrete" to be incongruent with or stand in relation to, dissolving the basis for "anti-expertise". In our sporting analogy it would be equivalent to a bunch of people just being asked to play with no idea what game they are in. There are no obvious rules which one can break, so my picking up the ball or kicking it is not in opposition to anything, it is just one possible game and for the same reasons we cannot say if I am playing the game well or not.

"Anti-expertise" is always responsive to the developments of the paradigm. If however, a paradigm never comes to dominate, we are left in a kind of limbo fighting for the interpretation of reality. The "anti-expert" cannot survive here, rather they just become one voice among many trying to push for their interpretation of reality. For example, the 17th century naturalist Robert Plot felt it made more sense to interpret a large femur bone as that of belonging to a race of giant humans, as mentioned in the bible and other cultural-historical documents, than consider the possibility that it belonged to previously unknown massive animal (Plot 1677, 136-139). It was not that it was incomprehensible, but just unintelligible given that reality was filtered through religious ideology and science was in its infancy. From our current perspective we would say the hypothesis of "giant human" over "unidentified animal" is the practice of a "bad" palaeontologist, but given that the domains of palaeontology and geology had hardly begun Plot was neither "good" nor "bad", but rather a "naturalist" with a wide range of "expertise" in 17th century matters. Today, the "anti-expert" can exist because we have a fully fleshed-out scientific view of the world pre-human existence. Our "anti-expert" gains their

title by having their world-view exist because it is opposed by a dominant interpretation. Why invent "baraminology" if it were not for taxonomic systems used in evolutionary theory? "Anachronism" has been mentioned as a feature of "anti-expertise", and we see this with "baraminology" where the word itself derives from two Hebraic words (*bara* – *min*) that form a meaningless compound in the original Hebrew. For it is a modern term trying to invoke the authority of biblical history. Our "anti-expert" here would be skilled in "baraminology" opposing the views of experts trained in cladistics, both working off fundamentally opposed assumptions. If the "baraminologist" were playing by the "rules" of the evolutionary model we could consider them a terrible practitioner, but as their discourse is based on, amongst other things, the assumption that evolution is wrong, their "expertise" is defined by this conflict. For this reason IDC has a limited productivity, as it is constrained by their version of "normal science". Not only is it restricted in what claims it can make, but due to its parasitic nature it is dependent upon the dominant paradigm for how it can interpret reality. For example, the holes in evolutionary theory that IDC scientists are trying to exploit have not led to any major scientific breakthroughs. Rather it produces extended, weak criticisms of the evolutionary model, such as the lack of transitional fossils (Gish, 1979), but nothing other than diluted competing theories or alternative descriptions of *what is seen*. No full scale application of IDC in cancer treatment or bacteriology, no crucial experiment that renders evolutionary science null, no technology that can be built exclusively on IDC principles. This "what is seen" (methodological approach) is what undergoes revolution when paradigms change – but IDC is dependent upon the metaphysics of "normal science" for it to even be able to use the language of "observation" and "theory" let alone engage in empirical research, i.e., in order to flesh out a theory of a young aged earth one needs to criticise radio-carbon dating, the decomposition and formation rates of matter and so on.[12] This is all standard fare for "normal science", but IDC does it as if it were "revolutionary science". Rather the alternative to the paradigm becomes "inconceivable" as our imaginations are

[12] Interestingly, Dr Andrew Snelling, a trained geologist and mineralogist, published in highly respected peer-reviewed geology journals and worked as a geological consultant, also published in creationist journals as a geologist. Compare, for example, the concepts used in in Snelling's (1990) contribution in the huge "Geology of the Mineral Deposits of Australia and Papua New Guinea" monograph and "Limestone caves – a result of Noah's Flood?" (1987). One presupposes the veracity of the geological column (the Archean age – 2500 million year old rock strata, for example), the other its falsity.

limited to what our world currently allows as a viable explanation (Stanford 2006). The extent to which IDC and ET are incommensurable stems from the same source as they share in paradigmatic norms, so something like Goldman's criteria enables us to make a clear choice between them.

The "incommensurability" discussed in *Structure*, however, points to situations where we do not know which method or set of assumptions to take as there is no way of telling between them. Both are equally productive and credible in why they should be considered. "Incommensurability" here means an "equally justified claim to measuring or stating something about reality", and due to this ambiguity we cannot in advance say who is right or wrong. If a paradigm comes to dominance then we can say in advance who is correct as the meaning of those phenomena have been designated already. However, just because a paradigm determines the meaning of actions, phenomena, and so on it does not make all actions or potential observations incomprehensible, just unintelligible. This goes part way to explaining why most major scientific breakthroughs are made by mistake, as they had no good reason to consider it. A case in point would be Penzias and Wilson's discovery of "cosmic background radiation". Anyone who has owned a radio has heard static hiss, but it was not regarded as a daily falsification of "steady-state theory" or even some sort of "puzzle" because it had *no meaning* beyond random radio interference even though it had been predicted by theories (Gribbin 1978).

Today there still exist scientific practices that have no dominant paradigm, which entertain an equally wide variety of views and possibilities, equivalent to Plot's "giant humans". For these practices we struggle to tell "good" from "bad" expertise, for in a certain sense those categories do not apply. The argument here is that we should have different expectations of our experts that lack a "normal science" to those that do not.

So far it has been argued that Goldman's criteria remains useful for knowing whose side to take in debates that contain a "normal science" – however, if we remove this condition and the presence of a singular paradigm I argue that Goldman's criteria breaks-down. When we experience this "breaking down", if it is only viewed through the lens of the methodological definition of "expertise", it may appear as if "expertise" as a category does not exist, leading to the epistemic problems of extreme relativism or ultra-scepticism. Counter to this, I would like to say a different type of expertise emerges, but one that does not hold to the distinctions of "good" and "bad", but also does not give rise to its "anti" counterpart. In order to show that

Goldman's criteria breakdowns with those disciplines that lack a "normal science" I will be looking at the field of economics.[13] Primarily I will be focusing on the two opposing views of economic experts Nouriel Roubini and Anirvan Banerji over the prediction of a global financial crisis. Given Goldman's criteria could a layperson tell which "expert" they should listen to?

Whilst knowing who to listen to over medical or scientific advice is important, arguably the fallout of the toxic loan and hedge-fund scandals has had a more widespread negative impact than those who chose to have homeopathic treatments or follow their horoscopes. The response of austerity from governments, aimed at stemming the rise in national debts, has generated a number of adverse social side-effects. When the link between health (mental and physical) and wealth is so entwined, "austerity" combined with economic recession has seen the rise in job losses, zero hour contracts, youth unemployment, food poverty, pay-day loan companies, online gambling, cuts to public provisions and a general widening of the wealth gap between rich and poor (O'Hara, 2014). All of which contribute to an increase in poor health, depression, suicide, and other lifestyle related illnesses (Karaikolos, 2013). This link between the power of expertise and what they are supposed to be an expert of was one of the reasons Hayek (1974) thought the creation of the Nobel-Prize for economics was a bad idea.

4. The Economic Experts: Roubini vs. Banerji

Pre-2005 the arguments for a global financial crisis occurring where very hard to find with the majority of experts, regulating bodies and academics either naïve to or intentionally ignorant of the outcomes of superficially regulated financial practices. "Intention" here is hard to establish but according to Stiglitz (2010) the models used prior to the "credit-crunch" were intentionally designed to highlight the "triple A" rating aspects of sub-prime mortgages over their "double-A" counterparts. Even if our non-expert had been vigilant enough to find those "pro-financial collapse" arguments such as Keen (1995), Baker (2002), or Jones (2006), all these authorities cite different causes, effects, ranges of time, and arrived at their conclusions by different methodologies. So here not only has the

[13] This may also give some philosophical weight to Keen's (2001) *Debunking Economics* where he argues that suppositions at the core of economic theories, like "perfect competition", are unfounded.

non-expert got to weigh up the arguments of a handful of experts over the consensus of the majority, but then also assess the arguments presented by those experts, which may fundamentally differ. To help our non-expert let us hypothetically place them at the 2006 International Monetary Fund seminar where economics professor Nouriel Roubini outlined his prediction for an impending financial crisis (Roubini, 2010/2006). This was given to a room full of economic and financial experts. To help our non-expert further, during the same seminar the economist Anirvan Banerji gave a response to Roubini saying why he was wrong.

As part of Goldman's criteria we need to know the track records of those we are putting under consideration and assess whether they are indeed an "expert". Nouriel Roubini is a professor of economics at New York university, a "summa cum laude" and Ivy League graduate, a published academic and employee of the International Monetary Fund (IMF), Federal Reserve and World Bank. Within the industry he is known for having a pessimistic demeanour towards global finance, which earned him the nick name "Dr Doom"(Mihm, 2008). His approach to economics is also methodologically very different to most modern economists, where he makes "extensive use of transnational comparisons and historical analogies" by "employing a subjective, nontechnical framework" (Mihm, 2008). Roubini dislikes pure economics that only deals in equations and models, where for him John Maynard Keynes is one of his intellectual heroes, "the most brilliant economist who never wrote down an equation" (Mihm, 2008). As a reflection of this methodological preference it is noted that Roubini's book *Bailouts or Bail-Ins?: Responding to Financial Crises in Emerging Economies* does not contain a single equation (Mihm, 2008). Our non-expert might then ask, "is Roubini right to not trust the exclusive use of models and equations in economic forecasting?" One answer to this is given by Prakash Loungani (2001) who concluded that private forecasters that use traditional models are incredibly poor at predicting recessions.[14] Since then studies have shown that random chance or "monkeys" out-perform financial experts in selecting stock investments (Arnott, *et al* 2013; Clare, Motson and Thomas 2013). Whilst predicting "recessions" and "stock market fluctuations" are qualitatively different the idea of prediction is the same. Seeing that the majority of experts can be out-performed by random chance, we have to ask, is it them that are

[14] Loungani (2001) paper concludes that of the 60 case study recessions that occurred worldwide in the 1990's 97% went unpredicted by economists a year in advance. Of the 3% that did predict, all of them underestimated how severe the recession would be.

under-performing as financial experts or are the theories/ models they have to work with inadequate? Roubini's explanation is that built into econometric models is the assumption that the near future will be homogeneous with the recent past, or a strong inductive inference about future states. Tipping points exist which can spiral events quickly out of the remit of even the most robust economic models. A logic that is based on a similarity between temporally close events makes it very hard to predict any major disruption to short-term patterns. Yet, what we have seen with the recent global recession is the inadequacies in classical economic theories when applied to the "new world" of global finance and the advances of extreme capitalism. Moreover, given the "boom" periods of growth, it is in certain people's interests that models suggest that the near future will be like the recent past, retaining investor and market confidence. Roubini's methodology, however, was to look at historical cycles in multiple economies and use the markers of economic and social activity, such as borrowing, credit availability, housing prices, and so on as symptoms to the health of the economy and suggest where it is likely to head.[15] Roubini's track record, however, for predicting exactly when a recession would happen is, for some, not great. Eric Tyson (2011) claims that Roubini had a four year consecutive run (2004-2007) of failed recession predictions making him the "boy who cried wolf". I say, "according to some" because one of Goldman's criteria is whether the expert has any bias in their claim. This equally applies to the critics of those claims. Unfortunately, unlike physics or chemistry, the phenomena economists deal with is a completely human creation that grows and recedes as people collectively act. These actions are also responses to beliefs about how things are going to go, so one loud cry of wolf can spook the market. As those same economists and financial experts may have affiliations, sponsorships or personal investments in how the market goes, part of their job is to retain investor confidence by shouting down predictions, such as "recession", regardless of how legitimate the claim might be. Indeed, Roubini states that during times of economic exuberance irrational behaviours and irregularities are willingly overlook by people (Brockes, 2009). So how closely we can listen to the critics of Roubini is hard to determine given that capitalism is about invested interest and profit.

[15] This method of speculation is heavily based upon the Austrian Business Cycle Theory by economists such as Hayek (2001) and Von Mises (1996). This has been proceeded by the modern Credit Cycle Theory of which there are many proponents such as Minsky, Kindelberger and Fisher who argue that the modern Credit Cycle theories explain why financial crises occur.

After Roubini's 2006 IMF address Anirvan Banerji gave a response.[16] Banerji started by making two points, 1) that Roubini had been predicting a recession for a while and 2) he had no specific model for making those predictions. Banerji observes that Roubini, for no good reason, sticks to his prediction of recession even when the indicators that he cites remain stable. Banerji notes that when Roubini picks past episodes of recessions on which to base his analogies of future events how can he guard against subjective preference for patterns that may not be there? Banerji says that the "danger of such a subjective approach is that instead of letting the objective facts shape your views, you may be tempted to selectively emphasize the facts that support your views" (IMF Transcript, 2010). Equally, if you make enough predictions eventually one will come true making him a stopped clock. Banerji asks what the financial sector is to make of Roubini's prediction? What good is basing economic policy on a style of risk management that is not good at assessing risk? In order to avoid an on-coming recession one can artificially stimulate economic growth, but if the prediction is wrong in the first place this can then end up doing more harm in the long run. Banerji concludes that the best way to predict a recession is to follow the key indices that lead to a threshold for financial collapse. Only at this point can it be probable that a "real" recession is about to occur, rather than a false positive. Banerji then apocryphally states, "[a]ccording to the leading indexes we monitor, we are not there yet" (IMF Transcript 2010).

So why should our non-expert listen to Banerji? Well if we are looking at recession predictions and forecasting, Banerji is the chief research officer for the ERCI who successfully predicted American recessions and recoveries in 1990, 2001, and 2002 (Lakshman and Banerji, 2004), but what gave Banerji and those like him the confidence to ignore warnings of financial catastrophe? Around the same time people like Nobel-winner Robert Lucas claimed that depression prevention had been solved, the creator of the "Efficient Market Hypothesis" model Eugene Fama insisted that US housing bubble did not indicate a looming bust scenario (McNally 2011, 15). Then there were people like David Lereah (2005) publishing books with no-nonsense titles such as *Are You Missing the Real Estate Boom?: The Boom Will Not Bust and Why*

[16] Expertise credentials: Banerji is co-founder & chief research officer of the Economic Cycle Research Institute (ECRI). He was also a President of the Forecasters Club of New York and serves on the New York City Economic Advisory Panel.

Property Values Will Continue to Climb Through the End of the Decade - And How to Profit From Them, aimed at the general reader like our non-expert.

To help our non-expert we may summarize the situation as thus:

A) *Examine the arguments presented by the experts and their rival(s)* – as both experts were using different methodologies with different assumptions at their core, done in the knowledge of the weakness of each other's position, there is a sense in which both views are incommensurable. If the experts disagreed with each others positions how could a non-expert trump their knowledge? It would require our non-expert to make a judgement over whether they preferred a methodology that worked off a hermeneutics of historical-economic activity (qualitative) or relied on assumptions built into classical models of economic theory (quantitative). There is no evidence one could present that would definitively put one methodology over the other as they both allow for different considerations to be taken as *meaningful*. So the seemingly banal point that Banerji makes about letting "objective facts shape your views" elides something deeper. Whilst there is a trivial sense in which this is true i.e., two people could both agree or debate over the facts of key indexes and other considerations inherent to their models. There is a deeper sense to this. Whilst we use the term "fact" as a shorthand for "what there is" we are actually committing to a metaphysical idea of "what there is". Tallis says a fact is "not something like an object that is simply "there"" (2008, 263). A "fact" is dependent on how we notice the world and how we choose to divide it up. So even on an everyday level a room has the possibility for a number of facts, but that possibility is constrained by the "world" I occupy, or what I am allowed to acknowledge as being meaningfully "there". "Facts are the progeny of a three-in-a-bed between my consciousness, my language (and the habits of noticing and dividing dictated by my language), and whatever is intrinsically there, independent of my awareness (2008, 263). In the current case, it is the school of thought that each economist comes from that decides what can be taken as meaningful. So what gets picked out as a "fact" by Banerji or Roubini differs due to the lack of a "normal science" that determines what those phenomena mean.

B) *Look at the consensus between experts* – In this instance the consensus appeared to be against Roubini. The non-expert who chooses to go against the majority would be deemed either irrational or highly insightful negating their position as a "non-expert". Their ability to see further than the current

models, people and institutions that also missed the signs of recession are not the acts of a layperson. As former chairman of the United Sates Federal Reserve, Alan Greenspan said, "we all misjudged the risks involved. Everybody missed it—academia, the Federal Reserve, all the regulators" (McNally 2011, 15). Whilst this might be a bit of an exaggeration, for our non-expert the consensus is not under reasonable doubt. The non-expert also has to contend with the inaction of experts even when arguments are shown to be retrospectively wrong. Here I am referring to the Kenneth Rogoff and Carmen Reinhart model, which was then disproved by researchers finding a coding error in the author's original work (Herndon, *et al.* 2013). This error still did not prevent "austerity" measures from being implemented or rescinded by Governmental economists to this day.

C) *Assess the independent evidence that the expert is an expert* – over and above the claim that both were experts, there is an intractability to this criteria if one only sticks with a methodological ideal of expertise. As academia and scholarship are a part of a socio-historical system of referencing, which blind "peer review" is suppose to keep in check, every time an expert advances an argument based on the work of other experts, we invite a potentially infinite regression of expert and fact checking. Popper argued against a similar criteria in theory formation. That in order to check that every term in a proposition was meaningful one would have to define all terms, which means using other terms to give a definition, which then also require further defining and so on, until one is only left with tautologies (Magee 1985, 49). He thought this approach clearly irrational.

D) *Investigate whether the expert has any personal bias or investment in their claim* - while we might be able to see why a scientist who has affiliations with an oil company, for example, would be willing to put forward anti-climate change arguments, the "truth" of climate change remains unaltered. With economics, however, the objects it deals with have the property of being a uniquely human system that can change depending on how people believe things to be. So any scaremongering about a recession can actually cause one and likewise overly reassuring claims about GDP growth and market confidence can avoid one. Here it is hard to know if one is affecting the object of enquiry by simply asking after it. Also, in a profession where prediction is notoriously bad the expert risks very little in their claims, but given that certain economists and financial experts will work with particular interests in mind, everyone has a bias in their

position. This could range from the likelihood of financial reward in guessing market dynamics correctly, to outright propaganda on the state of conservative or liberal economics. Here there maybe ideological bias, whether at the level of methodology such as the "freedom" or "rationality" of markets, to values such as "profit is good". Another problem for identifying bias is where economic models that can be transposed into mathematical terms, which then shows x to be the case. These are much more politically desirable than studies that are inconclusive or deal in qualitative terms that can be scrutinised by non-experts (Saltelli and Funtowicz 2014, 82) such "working conditions". Here it is very difficult to distinguish between the biases of the system from that of the individual.

E) *The track record of the expert* – in a field that has clear and distinct criteria for what it means to contribute to that field, something like "track record" is easy to assess. The very term coming from "track and field" where whoever is the quickest around the track is the winner. Likewise looking at the past timings of other runners we can assess the state of competition. While "winning" and "losing" is something intrinsic to sport or games, the practice of science consists upon having a range of people that can contribute. From the "normal" science" drudge of everyday lab-work to the Nobel-Prize winning "revolutionary" breakthroughs of visionaries. If a scientist has not contributed to their field this limits what we can count as "track record", relying on things such as where they studied, who accredited their qualifications, who they work for and so on. This criteria of "contribution" becomes even more complicated when we consider fields outside of the "hard" sciences, where fraud maybe easier to commit. For example, prior to 2011 ex-Professor of social psychology Diederik Stapels" "track record" was impeccable, but after it was shambolic.

This summary should then raise the question: what is it fair to ask of an expert, given their field of expertise? If we say "prediction" is part of an economist's contribution to their field both Roubini and Banerji had known success and failure, however, Banerji's track record was a lot better than Roubini's. So if our question is about who we should listen to over forecasts of recession, based on the "track record", Banerji comes out the "winner". Yet, knowing that "prediction" in economics is so difficult it would seem unreasonable as a criteria for expertise. So what else could "track record" mean here or is it something that should not apply to the expertise of economists?

What the Goldman's example shows is that the we cannot rely on a methodological criterion of expertise to determine who one should listen too, given that the fields one can be an expert in differ in their structure. "Expertise" and the area they are an expert of can be probed philosophically to discern different sorts of practices and their meanings. An "expert" in a practice that contains a "normal science" is not the same as the expert of a practice that has yet to settle from its revolutionary or pre-normal state. There are abilities we can expect of one that we cannot ask of the other. As it is so lucrative that economists or financial analysts be able to "predict" events ahead of time it is a highly valued skill, but it is far from being a normal practice so it is referred to as "forecasting" or "speculation" rather than "prediction". The apparent need for the epistemic prowess of the natural sciences in a field with the ontology of the social sciences has led to "econometrics" (Klein, 1971). This melding has created an ad-hoc science where its epistemic foundations are continually shifting and every event is fraught with error, approximations, ambiguity and non-replication (Malinvaud, 1980). Indeed, economic data (what counts as data) and schools of economic thought (the theories that situate data as meaningful) are so varied that one cannot choose between competing economic theories (Manski, 1995). An econometrician might say they can predict events, but this cannot be meant in the same sense as celestial mechanics, but more in the sense of "forecast" the weather for months in advance. What-is-more, there are no laws of economics and the ones that we intuitively take to be true such as economic cycles, where a "bust" inevitably follows a "boom" may, arguably stem from Aristotelian metaphysics than anything inherent to modern economics.

Conclusion

It has been argued that there is a commonality between the "problem of demarcation" and the "experts" of those fields we designate as "science" and "non-science". That we can construct a methodological definition of a science and its corresponding expert if the field has a ruling "paradigm" and its associated "normal science". However, when a field has no dominant paradigm we are unable to provide a criteria for what it means to be doing that "science" and consequently the practising "expert" differs from the "normal science" expert. I argue that with the presence of a ruling paradigm we get a another phenomena, which I have called "anti-expertise". Here I tried to show that

when a domain is so successful people who think they are challenging the paradigm are actually sharing in its norms. For a field that has a ruling paradigm I argue that something like Goldman's criteria is highly useful, which we see with the evolution vs. intelligent design "debate", but could equally apply to the links between the MMR vaccine and autism. However, when a field displays no "normal science", such as economics, Goldman's criteria becomes defective. The lack of a dominant paradigm for economics results in the ambiguity of meaning for its objects, events and what it means to contribute to the field. So one can perform "economics" with a number of different, possibly mutually incompatible assumptions, and yet still be recognisably doing "economics". Here we do not find "alternative" economists like one may find alternative practices to orthodox medicine as the lack of paradigm cannot sustain "anti-expertise". A "normal science" has the meaning of objects and events already established so assessment of expertise and what "contribution" means is unambiguous. This also allows for "anti-expertise" where people's truth-claims are dependent upon the metaphysics of science proper i.e., treating a biblical passage *as if* it were scientific theory about reality. The direction of influence between the methodological abstract notion of expert, such as Goldman's, and the historical practice-bound version, is that we have to proportion our expectations of "expertise" relative to the maturity of the field one is suppose to be an expert in (i.e., pre, inter or post-normal science). Hence, one cannot assess "track record" or "consensus arguments" if the field itself is not sufficiently developed. Finally, as with the "problem of demarcation" if one is confined to only a methodological definition of what "science" or "expert" means, due to the availability of historical counter-instances we could conclude that no such separation exists between "science" and "non-science", "expert" and "non-expert". If however, we also include the historical perspective and consider the relative maturity of the field we can still have "science" and "experts", but have to concede they mean something different to their "normal science" variant.

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What Is An Expert That A Person May Trust Her? Towards A Political Epistemology Of Expertise

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ABSTRACT

I present a definition of expertise that involves both epistemic and political authority. I argue that these two forms of authority require different treatments and defend a political epistemology that articulates a division of cognitive labor between political and epistemic authority.

Keywords: epistemic authority, political epistemology, democracy.

The appeal to expertise is pervasive in contemporary societies. The need of experts has become a distinct feature of mature democracies, as if a new form of authority - epistemic authority - were slowly eroding and replacing the political authority of governments. Crucial decisions - as that of declaring war - are made dependent on expert reports about, for example, the presence of nuclear weapons or the violation of human rights in a particular country. An example of the new role of expertise in political decision, that I have analyzed elsewhere,[1] was the 2003 war against Iraq, declared by United States and Great Britain with the following mission: "to disarm Iraq of weapons of mass destruction, to end Saddam Hussein's support for terrorism, and to free the Iraqi people".[2] The political aim was thus submitted to the assessment of evidence about the presence of weapons of mass destruction in Iraq and the involvement of Iraq's regime with terrorist activities that could have harmed United States or Great Britain.

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[1] Cf. Origgi, 2008.
[2] Cf. http://georgewbush-whitehouse.archives.gov/news/releases/2003/03/20030322.html

The authority of politics seems thus dependent in many circumstances on the authority of knowledge, a paradoxical outcome of democracies whose authority should be based on the free expression of opinions of the many organized through appropriate and shared procedures (such as vote) and not on individual's special capacities.

The relation between expertise and democracy has always been problematic and it constitutes a central issue of political philosophy. Democracy is an anti-authoritarian regime, whose one fundamental tenet is the neutrality vis à vis of the opinions of people: no opinions expressed by a particular group should be favored by the State. In this respect, expertise is essentially authoritarian. It imposes itself as a better opinion that should be endorsed on the basis of the epistemic authority of those who hold it, in spite of public discussion and deliberation. That the Earth turns around the Sun and not vice versa, has to be accepted as a "better opinion" because it is endorsed by an elite of credible scientists whose epistemic standards cannot be questioned by the layman.

That is why many authors who consider themselves as advocates of democracy have condemned expertise as a threat to democratic regimes. Hanna Arendt writes against experts: "There are, indeed, few things that are more frightening than the steadily increasing prestige of scientifically minded brain trusters in the councils of government during the last decades. The trouble is not that they are cold-blooded enough to "think the unthinkable" but that they do not think".[3] What she means is the experts do not think critically about society and democratic life: they produce "cold truths" that can be manipulated by power to produce visions of society that are not what citizens want and need.

Another major criticism to the role of scientific expertise in societies is that of Jurgen Habermas. In his essay on "Science and Technology as Ideology"[4], mainly dedicated to a critique of Marcuse's thesis of technology as domination, he denounces the risks of new technocratic democracies: "The technocracy thesis has been worked out in several versions on the intellectual level. What seems to me more important is that it can also become a background ideology that penetrates into the consciousness of depoliticized mass of the population where it can take on legitimating power".[5]

[3] Cf. Arendt 1969, p. 8.
[4] Cf. Habermas 1968.
[5] Cf. ibidem p. 253.

This tension opens a series of questions that I would like to address in this paper:

Are knowledge and expertise essentially authoritarian?
On what form of authority the justification of power must rely on?
Is the idea of democracy compatible with the idea of authority?

These questions cannot be addressed without a preliminary analysis of what it means to be an expert. Is it possible to give some elements of an epistemology of expertise in order to work out a definition of an "expert"?

In his influential book on the politics of expertise, Stephen Turner distinguishes between five kinds of experts: 1. Experts whose expertise is generally acknowledged by everyone in a society (doctors, physicists); 2. Experts whose personal expertise is acknowledged by certain individuals (like authors of self help books, consultants, etc.); 3. Experts who are members of groups that are the only ones who acknowledge their expertise (theologians whose authority is recognized only by the members of the same sect); 4. Experts whose audience is the public, but who are supported by influential parties interested in the acceptance of their opinions (members of think tanks, researchers paid by private foundations with a political agenda); 5. Experts whose audience are bureaucracies with discretionary powers, who appoint themselves the experts on a specific administrative question and then implement the proposed solutions by selecting them through criteria and procedures that are typical of the bureaucratic decision making system.[6]

The problematic categories for liberal democracies are especially 4 and 5, that is, the expertise that is solicited, used and legitimized by the political powers. In these forms of expertise, that are so pervasive in our liberal democracies, there is a sort of "delegation of authority" from political authority to experts' authority in a way that violates the principle of neutrality that is at the core of liberal thought. The acknowledgment of the expertise of doctors is clearly not a threat to democracy, nor the submission of a tiny part of the population to the authority of psychoanalysts or other kinds of guru. What is problematic is the endorsement of expertise by bureaucrats who have discretionary powers to "use" this expertise to influence public policies and political decisions.

[6] Cf. Turner (2014)

What is an expert then? It is someone whose epistemic authority is granted and legitimized by political stakeholders who have an interest in delegating part of their authority to the cognitive authority of experts.

In this sense, the definition of an "expert" is different from that of a "scientist": experts, as scientists, 1. are people who hold a special cognitive authority on a subject matter, and 2. their cognitive authority may be delegated to in a process of decision making that has political, societal or personal stakes for the categories of people who defer to them.

To be an expert thus implies that someone defers to your expertise in taking crucial decisions. This second clause makes expertise potentially dangerous for democracy because it creates a bias for an opinion that will be adopted out of deference by some parties, thus violating the neutrality condition. In the case of science, deference doesn't have the same role in selecting and legitimizing a scientific idea. Science, as an organized activity, selects its own ideas through a process of peer review that involves insiders only. The scientific community is the ultimate producer, evaluator and consumer of scientific truths that are considered, as such, neutral. Although thus idealized picture of science can be challenged by a more fine grained sociological analysis of the forces at stake in the selection of scientific "truths", the distinction between experts and scientists holds: a scientist may become an expert when she fulfills the second condition, but she is not necessarily an expert. On the other hand, the category of experts can include people who have cognitive authority in extra-scientific domains. Connoisseurs have cognitive authority on art, wine, taste and their authority may be solicited in various contexts. Expertise may involve a form of "knowing how", a practical knowledge that gives authority to its beholder, but cannot be clearly spelled out in scientific terms. Take a dowser or a graphologist, or the identikit expert able to draw a sketch of a crime scene out of the description given by a witness: these kinds of expertise are not considered mainstream science, but they can be solicited by policy makers in certain situations.

Experts have knowledge that can be deferred to in order to take decisions and coordinate behavior in a society. Yet, their legitimacy as authorities does not depend on their community only, but on a societal consensus of stakeholders. Thus a series of crucial epistemological questions inevitably become political questions. Who does decide the level of expertise necessary to lead politics towards a certain choice that may have heavy consequences for the society? When the quest for "truth" should stop so that a decision must be

taken? Who does evaluate the experts? Are they politically responsible, that is, if their recommendations turn out to be harmful for the society, should they be considered accountable? A recent case that shows this tension was that of a series of experts meetings in Italy in order to estimate the probability of a disruptive earthquake in the area of the town L′Aquila, in the Abruzzi region, that ended up with a report that clearly underestimated the risks and a subsequent trial against the earth scientists involved in the expertise[7]. An international debate then burst among scientists to defend the neutrality of science and the fact that a scientific opinion cannot be subject to a post-hoc trial dependent on its success or failure. The defense of the scientific world was based on an apparently self-evident distinction for the scientists between the realm of "facts" with their associated probabilities, and the realm of "decisions". But each time experts are solicited for a political decision, the distinction seems easily blurred. What are the facts and what the decisions? Isn't an underestimation of a probability of occurrence of a certain fact in itself a political choice? Again, who decides the appropriate level of expertise that should be appealed to in order to make a wise move? Policy making is the realm of choices and sometimes a bad choice is better than no choice. Another example of the interaction between expertise and policy making, and of the difficulty to distinguish between them, was the case around the Ash Cloud in April 2010 that forced the ministers of transports in Europe to block the airspace for several days, apparently causing more harm than that which could have been caused by the ash cloud itself.[8]In this case, expert advice was clearly undetermined, given that the evidence available on previous cases of cloud ashes produced by volcanic eruption was not informative about the estimation of possible harm to flights. The decision of closing the airspace was a political decision taken at the European level on the basis of an extra-scientific principle, the Precautionary Principle, that is part of the Lisbon Treaty (art. 191) and lays the foundations of the European attitude on the relation between science and policy making, a principle that is highly contextual and not unanimously accepted across different countries[9].

[7] Cf. on this case G. Origgi: http://www.ilfattoquotidiano.it/2012/10/24/l%E2%80%99aquila-bastava-attenersi-al-principio-di-precauzione-europeo/392148/

[8] Cf. https://edge.org/conversation/the-ash-cloud

[9] Cf. on the Ash Cloud case: https://edge.org/conversation/the-ash-cloud

This poses a problem of "political epistemology" as Turner puts it: "In many cases people want guidance to solve problems that they consider important, and will embrace any credible expert who claims to have a solution or even some more fact relevant to the solution. But policy questions and the kinds of knowledge relevant to them is never a matter of "facts" of the sort that experts possess"[10]. Policy goes beyond the facts and, for many authors, the distinction between what is in the realm of facts and what is in the realm of politics, as they were two different spheres, is itself "political". According to Michel Foucault's political epistemology[11], each society has its own "regime of truth", that is, its way of prying apart what can be subject of scientific discourse and what cannot. This is eminently "political", that is, that it contributes to power as constituted by accepted forms of knowledge. The fact that a society accepts certain types of discourse and not others, and make them function as "true" is a political stance, and not only an epistemic one.

Thus, the core problem of political epistemology is to try to define the principles of a fair "division of cognitive labor" between two kinds of authority: epistemic authority and political authority.

The concept of "authority" is central both to political philosophy and to social science. It has to do with the legitimacy of power: a political choice has authority if it can "force" people to political obligation without coercing them. Science thus can be a way of legitimizing power by giving it the authority through the display of rational arguments that compel people to follow a certain conduct. If science says that CO2 emissions participate to global warming (the "fact") and that global warming has potential disruptive effects on our life on Earth, then, policy makers have a strong legitimacy in forcing policies that restrain citizens and industries' emissions of CO2.

Thus, epistemic authority (that is, authority over beliefs) may legitimize political authority (authority over conducts) but does not reduce to it. A democratic and pluralist society needs transparent procedures and rules to make the two interact in an acceptable way. That is why contemporary societies need to develop "political epistemologies" that is, rules and principles that legitimize the interplay between political and epistemic authority.

Authority is a very special sort of reason for action. To defer to authority is to refrain from insisting on personal examination and acceptance of the thing

[10] Cf. Turner, cit. loc 1203.
[11] Cf. Foucault, 1970.

one is being asked to do or believe as a necessary condition for doing or believing it. To cite authority as a reason for doing an act (or believing an opinion) is to put a stop to the demand for reasons at the level of the act itself and to transfer one's reasons to another person's will or judgment.

Most sociological and moral theories of authority fail to make the distinction between epistemic vs. political authority and present themselves as simultaneously accounting for the two concepts. The most striking example of this lack of distinction is the Foucaultian motto: "Knowledge is Power".

There are some obvious parallels between the notion of epistemic and that of political authority. Trust in authority poses a similar puzzle in both cases. How can someone - an institution or an individual - legitimately impose her/its will on other people's and have a right to rule over their conducts? How is this compatible with freedom and autonomy? And why should we trust an authority to impose us a duty to obey for our own good?

Much ink has been spilt on this apparent paradoxical relation between trust in authority and freedom. And of course an equivalent puzzle can be reformulated in the case of epistemic trust: How can it ever be rational to surrender our reason and accept what another person says on the basis that she is saying this? What does it mean to grant epistemic authority to other people? As the philosopher R.B. Friedman has rightly pointed out: "A person may be said to have authority in two distinct senses: For one, he may be said to be 'in authority', meaning that he occupies some office, position or status which entitles him to make decisions about how other people should behave. But, secondly, a person may be said to be 'an authority', meaning that his views or utterances are entitled to be believed"[12].

In both cases, the appeal to authority calls for an explanation or a normative justification of the legitimacy of the authoritative source, a legitimacy that must be acknowledged by those who submit to it. Still, I think that trust in epistemic authority and in political authority are two distinct phenomena that deserve a separate treatment.

As we have seen, epistemic authority poses the further problem of its legitimacy. Where does the authority over our beliefs come from? Why do we trust teachers at school, parents, experts of any sort? Why do we accept that even words in our language have the meaning they conventionally have on the basis of an act of deference to the uses that others make of these words?

[12] Cf. R.B. Friedman, 1990, p. 77.

Epistemic authority is pervasive in our cognitive life and yet, if we had to justify all our beliefs on the basis of reasons we hold for accepting them from others, our cognitive life would become too complex to be adapted to the complex tasks we have to solve everyday.

In order to make sense of our pervasive deference to epistemic authority, I will appeal to the notion of "division of cognitive labor", that was introduced in this domain by Hilary Putnam in his famous essay on the deferential uses of language (actually, he used the expression of "division of linguistic labor", later extended to scientific expertise by the epistemologists Hilary Kornblith and Philip Kitcher)[13]. I will try to argue that the division of cognitive labor has broader applications than the one envisaged by Putnam. The mechanisms of the division of cognitive labor should be at the heart of our "political epistemologies". That is the only way of avoiding paranoid attitudes towards the authority of science in our societies and making scientific and political authority supporting each other instead of eroding each other.

What is the division of cognitive labor? According to Philip Kitcher, who coined the expression in an influential 1990 paper, it is the social structure that optimizes the progress of science, that is, an optimal distribution of effort within the scientific community. It may be better for a scientific community to attack a given problem by encouraging some members to pursue one strategy and others to pursue another, rather than all pursue the single most promising strategy. The division of cognitive labor is thus the set of principles, conventions, role attributions that distribute knowledge within a community by allocating a reasonable effort to each mind according to its specificities and competences. Although Kitcher sees it as an "optimization" strategy, we may weaken his requirements to adapt his notion to the understanding of the role of expertise and epistemic authority in our political epistemologies. I do not think that the aim of a fair division of cognitive labor is an optimization aim. That would be another way of making the rational demands of science ruling our societies, an attitude that has revealed disastrous in many political experiences of the XX century (take socialism and its insistence on "objective, rationale and expert procedures").

A division of cognitive labor for a society that is the more and more epistemically dependent on expert authority means the mastery of a series of principles and rules of conversation that empower each parties of the society

[13] Cf. Putnam, 1975; Kornblith, 1993: Kitcher, 1990.

even when the asymmetries of knowledge are inevitable. As we have already said, there is not "factual knowledge" distinct from "political authority", at least not in the realms that matter for ordinary people such as health, security, ecology, life expectancy, etc.

Mature techno-scientific democracies have reintroduced a form of appeal to epistemic authority whose aim is a "rationalization" and optimization of political decisions. But a responsible political epistemology should not have this aim. Its aim should be that of the inclusion of most citizens, who are in a deferential position towards knowledge, in the assessment of the political impact of an expert-based decision. Expert advice is not neutral: assessing the "potential harm" of a certain fact means connecting some evidence based analysis with a world of values and expectancies. The decision process cannot avoid to take into account these values and hopes in choosing a line of action over another. Values and hopes are not a matter of expertise: we may value more the present generation than the future generations, thus discounting the consequences of our everyday actions on the survival of the planet, or we may be more far-sighted and value the future of our species more that our present interest. We may value precaution in health and ecology matters because we are committed to a vision of humanity we want to defend even when science tells us that it can become obsolete: a humanist vs. a transhumanist approach to human nature for example is not a matter of scientific expertise, but of a choice of value. We may value security against the technocratic arm-race in defense policies and thus collectively deliberate that the potential risks of new technologies of war, such as robots and drones in provoking resentment and future conflicts are more important than the potential benefits of the use of these technologies for our defense. We may value privacy and freedom more than security and thus oppose to the growing governmental demand in major democracies of surveillance of citizens. If we, as citizens, cannot enter on the subject matter of the installation of an algorithm of surveillance of the networks, we are wholly competent in deciding in which kind of world we want to live.

In conclusion, expertise is not just knowledge. It is delegated authority. And, as any form of authority in democracy, it needs to be legitimized without appealing to any special cognitive capacity of its citizens. Illiterate citizens, citizen coming from different religious and ethnic backgrounds, young and senior citizens, all have a voice not on the expert report, but on the way the expert report connects to the policies around which it has been solicited. That

is the division of cognitive labor that an expert-dependent society needs in order to avoid a technocratic turn that is presented sometimes as an inevitable evolution of our forms of life. Societies can improve and change themselves without "optimize" themselves: the room for debate, error, and revision is the main task of political epistemology.

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Responsibility, Expertise and Trust: Institutional Ethics Committees and Science

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ABSTRACT

This paper addresses what should be an important question for many institutional ethics committees: How might they justifiably trust external peer review of the scientific merit of research proposals under their consideration, since these committees are typically not constituted to review the science themselves?

Keywords: expertise; peer review; ethics committee; trust; authority; responsibility.

Introduction

Many institutions require that proposed research that would be conducted under their auspices gain prior approval from an appropriate ethics committee. This approval procedure complies with legal requirements in many countries in which institutional ethics committees based in hospitals, research centres, academic institutions and governmental bodies have the responsibility of evaluating proposed research for ethical approval before the research is permitted to proceed. In coming to their decisions, such committees can require external scientific peer review of research proposals that are subject to their ethical appraisal. (For convenience I shall refer to scientific peer review in a broad sense that includes physical and biological sciences, biomedical and veterinary science, and also areas of social science). I shall assume, I believe reasonably, that the use made by ethics committees of external peer review of the scientific merits of proposed research involves a type of trust in expertise. Precisely what type of trust it can legitimately involve is something that this paper will explore.

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An important question for many institutional ethics committees should be how they might trust external scientific peer review of the research proposals under consideration, since these committees are typically not constituted to review the science themselves. This question is the focus of this paper. Some of the considerations that I address here are relevant to critical issues of trust in other practical contexts where a person or a body that has responsibility for a substantive judgement or decision, as opposed to responsibility merely to ensure that due process is followed, must rely to some extent on external expertise in coming to that judgement or decision. An exploration of these wider issues lies beyond this paper's more specific concerns.

Why external scientific peer review can be necessary for ethics approval

An obvious preliminary question is why an institutional ethics committee would need external scientific peer review of research that is subject to the committee's ethical appraisal. If proposed research is ethically unacceptable for some reason (e.g., because it fails to disclose significant risks to the research participants) an ethics committee can reject it irrespective of its (purported) scientific merit. If, on the other hand, proposed research is ethically unobjectionable, then its scientific merit is not the ethics committee's concern. On this basis, members of institutional ethics committees are sometimes formally reminded that their role is to judge whether proposed research is ethically acceptable, as opposed to whether it is good science. This picture is simplistic, however.

Appropriate ethical appraisal of proposed research is not always independent of its scientific merit. There is a general question about the ethics of conducting (costly) research that is likely to be scientifically a waste of time and resources. This is an important question for research ethics, especially where the proposed research would involve human subjects or be publicly funded, although it seldom receives detailed attention in academic literature.[1] However, the responsibilities of institutional ethics committees are usually more narrowly confined to specific ethical matters and concerns such as

[1] Benjamin Freedman notes that scientific merit as a prior condition of ethical research has generated remarkably little discussion within the literature on research ethics, despite its recognition in various international codes and protocols. Freedman, (1987). See also Emanuel, et al. (2000).

consent of research participants, confidentiality, deception, and possible or predicted harm to researchers, research subjects or others.

A research proposal can raise specific ethical concerns that are not necessarily in and of themselves sufficient reason to reject the proposal, and here the scientific merit of the research can have an important bearing on whether it should gain ethical approval all things considered. In such cases, the central question for the ethics committee to answer is often whether the scientific significance of the proposed research is sufficiently important to justify the risks or harm that the research would involve. In relation to this, researchers seeking ethical approval for a project can be required to complete a *pro forma* that asks them to identify possible risks or harm involved in the proposed research and to explain how the scientific significance of the research would justify these risks or harms. The ethics committee must then assess what applicants say in this regard.

In deliberating the ethics of a proposal, members of an institutional ethics committee may need to come to a view about its scientific merit that is based upon external scientific peer review. For example, say as a moral philosopher Jane is a member of an institutional ethics committee that reviews research involving non-human vertebrates. A researcher submits an application in which he proposes using rabbits in developing a vaccine for human use. According to the application, the research procedures themselves will not cause the rabbits any pain or suffering but their immune systems will be significantly compromised and they will be humanely killed on the project's completion. In judging the ethical (un)acceptability of this research Jane needs specialist guidance about its scientific merit. In particular, she needs to clarify the following: Whether the proposed research methods and aims are well-conceived; whether the research is likely to deliver its aims and whether they are scientifically important; whether the use of rabbits is really necessary and, if so, whether there is a feasible better outcome for them on completion of the project; whether there are identifiable risks or harms that are not apparent in the application.

Assessment of the scientific merits of a particular research proposal is not necessarily outside the competence of some members of an institutional ethics committee who have relevant scientific expertise in respect of that particular research field. However, institutional research ethics committees are not constituted to review the scientific merits of the research under consideration even though, as I have said, this can be an important element in their

deliberations on the ethics of some proposals. Institutional ethics committees are perhaps not unique in this respect. All the same, their use of external scientific peer review can be distinguished both from the way in which an editorial board of an academic journal takes external peer review into account in its deliberations on the academic merits of submissions, and also from the deliberations of research grants awarding bodies that seek external peer review where members of the relevant panels are not themselves sufficiently competent assessors in the subject areas of particular grant applications. Editorial boards and research funding panels are constituted to review the academic merits of submissions that come before them; indeed this is their central remit. Institutional ethics committees, on the other hand, are charged with making a decision on the *ethics* of proposed research and typically these committees are explicitly not charged with reviewing its scientific merit. This is clear in the constitutions of many institutional ethics committees and also reflected in their composition where their specified membership includes people from outside the relevant research area who will bring an independent, disinterested perspective and understanding or expertise that is directly relevant to ethical, as opposed to scientific appraisal.

In judging whether proposed research is ethically acceptable it can be necessary for an institutional ethics committee to take external scientific peer review of the proposal into account. If, as I assume, this use of external peer review involves a type of trust, how and when is it warranted? In addressing this question in the next sections, I shall focus my discussion around the conceptions and conditions of justified trust that are the most relevant to this particular context.

Relevant Conceptions of Justified Trust

As recent philosophical discussions of trust emphasise, there are different types of trust. For instance, we can contrast personal trust with putting our trust in the health service; we can also contrast trusting a person to act in a certain way (e.g., to be punctual), as opposed to trusting what she tells us (e.g., that the train is on time). There are various philosophical accounts of the nature and requirements of trust and of trustworthiness.[2] For our purposes we need to focus principally on trust in another person's testimony, and in

[2] For a useful bibliography of recent philosophical writing on trust, see McLeod, (2014).

particular on an ethics committee's trust in external peer review of research proposals. As with all trust, this will involve some risk: as others have noted, if what another person tells us came with a cast iron guarantee of its truth or accuracy we would have every reason to believe it but no need to trust it. What we want to know is under what conditions this trust is justified.

Philosophical accounts of the conditions of justified trust invoke a range of elements. Nonetheless, these accounts usually take the task at hand to be a matter of identifying the conditions under which trust is *well-founded*. Certainly in asking ourselves whether we are justified in trusting what someone else tells us we usually have in mind whether or not our trust is well-founded. (For example, a student says that his essay is late because his mother is ill. Have I good reason to trust him on this? According to Wikipedia, Wittgenstein and Hitler could have known one other at school. Can I trust this source? I ask a stranger for street directions. Why should I trust what he says?) In considering whether our trust in what others tell us is justified we need to refer to the general conditions under which such trust can be well-founded and apply these conditions to the circumstances at hand. Philosophical accounts of the conditions of justified trust typically maintain that our trust in a person's testimony is justified when this person is trustworthy; these accounts usually go on to identify the competence and also the veracity of the person providing the testimony as two central conditions of her trustworthiness on the subject matter in question. (So, for example, the stranger who gives me street directions is untrustworthy if his knowledge of the surrounding area is poor (he is incompetent) or if he enjoys misleading passing strangers about their whereabouts – he lacks veracity.)

Clearly we need to think about the extent to which an institutional ethics committee's reliance on external scientific peer review could be well-founded, and this requires identifying the conditions under which peer reviewers and their reports (testimonies) are trustworthy. I shall take this up shortly. Before doing so, however, I want to draw attention to a distinguishable sense in which we can ask whether our trusting another person's testimony is justified. This distinguishable sense concerns when it is (il)legitimate for us to *entrust* a judgement on a particular subject matter to someone else. The following hypothetical example will help explain what I have in mind.

As a philosophy lecturer, Mary has the responsibility and the corresponding authority to grade her students' essays. Anne, a philosophical colleague from another university, is visiting Mary this weekend and they decide to go out for

the whole day on Sunday. Unfortunately Mary cannot do this and also complete all of her essay grading by Monday morning when her students' results are due. So on Saturday morning Mary and Anne agree to split the essay grading between them, taking half each. When Anne tells Mary that this particular essay is a distinction, that this one is a credit, and that this one is a borderline pass, and so on, Mary trusts Anne's judgement and she simply records the grades for those essays accordingly. Here Mary's trust in Anne's judgement about the appropriate grades for those essays can be justified in the sense of being well-founded. Anne is *trustworthy* in this particular respect: she is competent to assess the academic quality of the essays she grades; she is reliable in grading the work fairly; she reports her grades truthfully; and so on. But is Mary thereby justified in *entrusting* the grading of those essays to Anne in the way that she does?

To explain why the answer to this question is no, we need to attend to the relevance of notions of responsibility and authority to justified trust in the sense I have identified in the example of Mary and Anne. If a person's competence and veracity in relation to judging a particular subject matter (e.g., the merits of philosophy essays) are necessary for her (rightly) to have the authority to decide on this subject matter, they are not always sufficient for her having such authority. Notwithstanding Anne's trustworthiness as a judge of the merits of Mary's students' essays, Anne does not have the authority to grade those essays. In this case, this is because grading those essays is Mary's responsibility in her role as the course lecturer and assessor, and Mary has no role-related authority simply to delegate this task *via trust* to Anne in the way that she does. Even though we might agree with Mary that Anne is *trustworthy* in grading the essays, since Anne lacks the role-related responsibility and authority to grade those essays she is not what I shall call *trust-authorised* in this regard.

An institutional ethics committee's use of external scientific peer review of research proposals is not analogous to Mary's delegation of her essay grading to Anne. Rather, this particular example is intended to highlight the significance of the question of when a person is justified in *entrusting* a judgement to someone else, and to make the point that in some contexts this is not simply a matter of whether the other person is trustworthy on the matter in question. The salient point to take from the above discussion is that when a particular person or group carries the role-related responsibility for a making a

particular judgement, we need to consider both of the conceptions of justified trust that I have distinguished.[3]

From my example of Mary and Anne, I have emphasised the relevance of trust-authority in relation to Mary's unjustifiably entrusting some of her role-related judgements to another person. However, there are also circumstances in which we should consider the question of trust-authority in relation to our own judgements on particular matters. "Can I justifiably be entrusted with this judgement?" is not always exclusively a question about my trustworthiness in relation to making the relevant judgement; the question can also require that I consider whether I have the authority to decide on the matter in question. For instance, Anne should have asked herself this latter question before she undertook to grade Mary's students' essays, and her answer should have been no.

Having identified these two conceptions of justified trust in another person's testimony, I shall now consider both conceptions in relation to conditions of trust that are relevant to an institutional ethics committee's use of external scientific peer review.

Relevant Conditions of Justified Trust

Trustworthiness

Let's agree that an ethics committee's trust in external scientific peer review would be well-founded only if the reviewers are trustworthy.

As philosophical accounts point out, trustworthiness is relational notion: a person can be trustworthy in relation to *y* but not in relation to *z*.[4] This might be because she is competent in relation to *y* but not in relation to *z*, or because she is truthful in relation to *y* but not in relation to z. (For example, Anne is competent to assess philosophy essays but not chemistry exams; she might be truthful about her age and not about her income). Alongside a person's competence and veracity in relation to *y*, some accounts of trust invoke an additional condition of a person's trustworthiness which they identify as a

[3] It may be worth clarifying that alongside the fact that a person's trustworthiness in respect of *y* is not always sufficient for her having trust-authority in respect of *y*, so too a person's having institutional or role-related trust-authority in respect of *y* does not guarantee her trustworthiness in respect of *y*.

[4] A related claim is that *A* might trust *B* in relation to *y* and not in relation to *z*. (See, e.g., Hardin, Russell (2002).) It is a distinguishable matter whether *B* is actually trustworthy in relation to *y* and not in relation to *z*.

certain kind of commitment on this person's part: a motivation to act out of goodwill or in good faith as opposed to acting from ill-will, selfishness, or an ulterior motive. This commitment is said to distinguish a trustworthy person from one who is merely reliable. (If someone is truthful about *y* only because he will be subject to heavy sanction if he lies about it, or because he expects to be rewarded for being honest, although his testimony might be reliable in these circumstances, since it is conditional on an external sanction or reward we would probably not say that he is trustworthy in relation to *y*.) Arguably a condition of presumed goodwill makes most sense as a condition of personal trust, as opposed to trust in professionals or institutions.[5] All the same, as I shall outline shortly, an ethics committee's trust in the testimony of scientific peer reviewers needs to presume a particular type of commitment on the reviewers' part.

To what extent can an ethics committee justifiably regard external peer reviewers as trustworthy? In coming to a view about this we might appeal to the importance of having structures and procedures in place within the committee's operations that are designed to identify and select external peer reviewers who are most likely to meet the conditions of competence and veracity. As part of this process, potential reviewers can be required to state the grounds on which they are competent to review particular proposals and to declare any actual or perceived conflicts of interest. While such statements and declarations can be independently verifiable, nonetheless to some extent an institutional ethics committee might have to trust what a reviewer says about whether she meets the conditions of her own trustworthiness. It is here that a peer reviewer's commitment seems crucial: she must act with a certain motivation that requires that she both understands the role of an external peer reviewer and also that she endorses the norms of that role. A peer reviewer needs to act with a high level of professional integrity, central elements of which are conscientiousness and impartiality in reviewing the scientific merits of the research. A *pro forma* that scientific peer reviewers can be asked to complete as an initial step might prompt them to reflect on whether they are justified in trusting their own judgments about the merits of a particular research proposal. For example, they can be asked explicitly to state how the proposed research falls within their competence to review and also required to declare factors that could compromise their impartiality as reviewers.

[5] Baier, Annette (2013). What is Trust? In D. Archard, et al. (Eds). Reading Onora O'Neill. London and New York: Routledge, 175.

While all of the above steps are significant in selecting peer reviewers who are likely to be trustworthy, it is also the case that the role of a peer reviewer and its norms can be poorly understood by some of those who undertake it. Examples include people undertaking to review research in areas where their competence or impartiality is objectively questionable, and people reviewing the work of applicants with whom they have close personal relationships or animosity.[6] In such instances the relevant norms can be understood but nevertheless be flouted for personal, professional or other reasons: a reviewer can know that he is acting improperly in undertaking to provide a report and yet go ahead anyway, or he can be self-deceived about his suitability as an independent reviewer. Peer reviewers can also sometimes act with arrogance about their impartiality which they would not or ought not to accept from others in a similar position, and they can be insensitive or ignorant about what constitutes a conflict of interest.

Trust-Authority

An institutional ethics committee's use of external scientific peer review is complicated by the fact that the responsibility and authority for ethical approval reside with the institutional ethics committee. This might be thought to put the committee in the anomalous position of not itself being trustworthy or trust-authorised on what can be a significant element in its deliberations on some proposals, namely a review of their scientific merit. The need for external peer review of the scientific merits of some research proposals is of course why the issue of trust arises in this context. Nonetheless, we should bear in mind that while an ethics committee is not itself constituted to *review* the scientific merits of research proposals, in such cases it *is* charged with responsibility for making a judgement about the scientific merit of the research *based on* external scientific peer review.

In considering the situation of an institutional ethics committee in this regard, it will be instructive to consider a different context in which a body that is charged with the responsibility and authority for making a particular judgment draws upon expert testimony. Juries sometimes need to do this in the

[6] Peer reviewers typically remain anonymous to the research applicants (although not to the ethics committee). However, factors that identify a research applicant to a reviewer, such as the applicant's qualifications, experience, publications and reputation, can be highly relevant to reviewing the scientific merits of a research proposal.

course of their deliberations in a trial, for instance. (For convenience I shall refer to juries in the following discussion and note here that trial judges also sometimes draw upon expert testimony in cases or in legal jurisdictions in which it is their role to judge the facts.)

External Expertise: Authoritative or Advisory?

Legal theorists who discuss juries' reliance on the testimony of expert witnesses distinguish between what they call *authoritative*, as opposed to *advisory* testimony. This particular distinction refers to the differing degrees of deference that are called for on the part of a lay-person, a non-expert, and it corresponds to a difference in the degree to which a jury needs to depend upon what an expert witness says.[7] A jury's degree of dependence might be considerable where, for example, an expert witness testifies that a victim died of arsenic poisoning (authoritative), and its dependence not be as great where, for example, an expert witness says that in her opinion the defendant's anti-social conduct was influenced by his troubled childhood (advisory).

As understood by legal theorists, the distinction between authoritative, as opposed to advisory testimony refers to the *accessibility* to a lay-person, a non-expert, of the *reasoning* on which a particular expert's judgement is based. For example, whether a lay-person can reasonably accept the testimony of a pathologist as to the cause of a victim's death, or the testimony of an expert lip-reader as to what a defendant said on a particular occasion, must depend on *indirect* factors such as the pathologist's or the lip-reader's attested skill and record of reliability on such matters (which we take as evidence of his or her competence). By contrast, the reasoning that a psychologist uses in testifying that a person's anti-social tendencies stem from her violent upbringing can to some extent itself be *directly* understood by a non-expert, who might or might not find such reasoning persuasive.

I think that a distinction between authoritative, as opposed to advisory testimony might also be drawn somewhat differently however, and taken up in a second (related) way in which 'authoritative' means something like definitive or decisive. We call a person an authority on a subject, and we say that she speaks authoritatively on that subject when we think there is very strong reason to believe that *what* she says on that subject is very likely to be accurate or true.

[7] On this I am indebted to Ward, T. (2006).

We can contrast this with someone whom we regard as having (merely) advisory expertise on a subject, in virtue of which her opinion on that subject commands respect such that we should take it seriously into account in coming to our own view on the matter. An example of expert testimony that purports to be authoritative in the sense of definitive or decisive would be a pathologist's testimony as to the cause of death, given in the following form: "I can attest to the fact that the victim died of arsenic poisoning". By contrast, expert testimony that presents itself as (merely) advisory provides an interpretation of particular information. An example of this would be where a psychologist says that in her professional opinion a defendant suffers from particular antisocial tendencies that are due to his violent upbringing.

I have intentionally elaborated this second distinction in terms of expert testimony that *purports* to be authoritative (decisive), as opposed to testimony that *presents itself* as (merely) advisory. This terminology does not commit me to claiming that a pathologist's testimony, as given in the form above, *is* in fact authoritative (decisive) so that jurors must simply *defer* to it. Different pathologists could disagree about the cause of a victim's death, for instance, and each of them might purport to give authoritative (decisive) testimony on the matter.

Both of the senses that I have distinguished above, in which testimony might be regarded as authoritative, as opposed to advisory can and must allow for disagreement between expert witnesses on the same subject matter. Conflicting expert testimonies can each *be* authoritative in the first (the legal theorists') sense, since for testimony to be authoritative in that sense means that the reasoning on which it is based is not directly accessible to a lay-person, a non-expert. However if, for instance, two expert lip-readers disagree about what a defendant said on a particular occasion, although we can accept that both of their conflicting testimonies can *purport* to be authoritative in the second sense (decisive), we cannot ourselves regard their conflicting testimonies as *being* authoritative in this sense.

For the purposes of our present enquiry, the more important of the above two ways of distinguishing between authoritative, as opposed to advisory testimony is arguably the first (the legal theorists') sense that concerns the degree to which the reasoning on which expert testimony is based is accessible to a non-expert. However, the second distinction between expert testimony that purports to be authoritative (decisive) as opposed to (merely) advisory (an interpretation), is relevant to how external scientific peer reviewers should be

asked to frame their reports to the committee. The second distinction is also relevant to what we should say about cases where scientific peer review purports to be decisive or where the reasoning on which it is based is genuinely inaccessible to a non-expert. I shall now elaborate these points.

Like a jury in a legal trial, an institutional ethics committee is itself in a position of public trust in relation to decisions within its remit. Juries are charged with the responsibility and authority to judge matters of fact and to do this they must sometimes draw upon expert testimony that bears on these matters of fact; institutional ethics committees are charged with the responsibility and authority to judge whether research proposals are ethically acceptable and to do this they need to draw upon external scientific peer review when this bears on the ethics of proposals. Just as expert testimony given to juries is sometimes advisory in the sense that juries can directly access the reasoning on which it is based, so too institutional ethics committees can sometimes understand and assess the reasoning that underpins external scientific peer review. For example, if a peer reviewer says that proposed research would duplicate existing research, then the evidence that supports this claim can and should be provided in a way that is accessible to a non-expert.[8] Similarly, if a peer reviewer claims that the methodology of proposed research is flawed, a non-expert might be well able to understand the reasons why this is so if those reasons are clearly set out; and if there are risks involved in proposed research that are not apparent in the application itself, a peer reviewer who identifies these can explain how they might come about. For these reasons, external scientific peer reviewers should be directed to regard their role as advisory in both of the senses that I have distinguished above: this means instructing them to present their reviews as expert opinions that explain, as far as possible, the reasoning on which their assessments are based in a way that is comprehensible to non-experts.

Where the reasons upon which a peer reviewer bases her judgement about the scientific merits of research *are* accessible to members of an ethics committee, at least to some extent, the committee has a duty not simply to adopt a peer reviewer's conclusions *on trust*: it ought not simply *entrust* this element of the ethical review process to peer review reports. To be sure, an institutional ethics committee is not constituted to engage in scientific review

[8] Julian Savalescu and co-authors maintain that applications for ethical approval of health care research should require completion of a section entitled "Systematic review of relevant existing research", in terms that are accessible to ethics committee members (Savulescu, et al., 1996).

of the proposed research that is subject to its ethical appraisal. Nonetheless, just as a jury must reflectively consider, and not simply uncritically accept expert testimony about the facts of a case on which it must judge, so too an institutional ethics committee has a responsibility reflectively to consider, and not simply defer to external scientific peer review that it receives as part of its deliberations.

In this latter respect an institutional ethics committee's use of external scientific peer review is unlike the use that such a committee might make of independent legal opinion on whether proposed research would or could incur any legal liability or involve the commission of a criminal offence. Legal liability is relevant to an institution's approval of research to be conducted under its auspices but it is not a consideration that is relevant to whether proposed research should receive ethics approval. The criminality of proposed research is a sufficient reason for its not gaining ethics approval and this is something about which an institutional ethics committee should be aware. However, the legal permissibility of proposed research is a *precondition* of its evaluation by an institutional ethics committee, whereas the scientific merit of the research is an element that an ethics committee can need to take into account as part of its own ethical appraisal of the research.

We now turn to the question of what we should say about genuinely hard cases in which external peer review is authoritative in the first sense, where the reasoning, or part thereof, of a report is directly inaccessible to a non-expert. When this happens, should an institutional ethics committee regard a peer reviewer's assessment of the scientific merit of the research *as* authoritative (decisive), and thus simply defer to it? Here I think the ethics committee's responsibility requires that it obtain more than one report. It must also take responsibility for seeking independent clarification if external reviewers disagree. Where disagreement among authoritative experts persists, an ethics committee's decision on which report(s) to accept cannot be based on its own direct assessment of the veracity of reports in hand, since this would be outside the committee's own competence and remit. In coming to its own judgement, then, the committee must carefully consider *indirect* criteria that are relevant to assessing the competence and the veracity and commitment of its external peer reviewers.

What such indirect criteria can reasonably include raises further issues. More generally, Alvin Goldman distinguishes and endorses a number of possible sources of evidence that a lay person might have for trusting one

purported expert more than another where neither expert's evidence is directly epistemologically assessable to the lay person (Goldman, 2001). Goldman invokes the relevance of what he calls an expert's 'dialectical superiority' in defending a position; an appeal to 'numbers' in judging experts' relative credibility; evidence from interests or biases; and appeal to experts' past track records of correct decisions. All of these considerations can be relevant to conditions of warranted trust in (expert) testimony, it seems to me. Nonetheless, their applicability to an ethics committee's deliberations in hard cases must be shaped by such a committee's remit, its composition and responsibilities, and the way in which external peer review is appropriately conducted, presented to and received by the committee.

External scientific peer review normally comes to an institutional ethics committee in the form of written reports. In some circumstances where a particular research proposal raises significant ethical concern, scientific peer reviewers might appropriately be interviewed or further questioned by the ethics committee. (This could include asking them to comment on a position on the scientific merit of the reviewed proposal that runs contrary to their own appraisal, for instance). Expert witnesses who testify in court cases are of course subject to critical or cross-examination. However, unlike the use of expert witnesses in a jury trial in English-speaking jurisdictions, the context in which external peer review is used in the deliberations of institutional ethics committees is investigative, as opposed to adversarial. This is surely an advantage in the latter case. The investigative nature of an institutional ethics committee's deliberations has an important bearing on how external scientific peer review should be conducted and presented on behalf of the committee: peer reviewers should be advised not to regard themselves as advocates or as part of an advocatory process, for instance. It also has an important bearing on how the ethics committee should regard and interpret the reports of external peer reviewers: these reports should be taken seriously into account but not unreflectively so.

As suggested earlier in the paper, steps to identify and minimize biases and conflicts of interests can be incorporated into the institutional procedures for selection of appropriate peer reviewers for particular proposals. To be sure, the number of peer reviewers who then support a particular view of the scientific merit of a proposal, and also their established 'track records' of correct decisions about such matters can be relevant indirect evidence for the committee where disagreements among peer reviewers arise. The extent to

which an ethics committee could be obliged to garner and consider these latter types of indirect evidence is another matter.

When disagreement among experts persists and indirect criteria are summoned, it is important that an institutional ethics committee keeps in perspective that its responsibility is to form an overall judgment about whether the research proposals that come before it are *ethically* acceptable. This highlights two overarching considerations. The first is that the committee should address the level of confidence that it needs to have in the scientific merit of a particular proposal in order to be justified in deeming that research to be ethically acceptable. The required level of confidence can vary across applications in relation to a number of factors, the most obvious being whether and to what extent the proposed research involves significant (risk of) harm. The ethical acceptability of such research *depends* on its scientific merit being sufficiently important to justify any significant (risk of) harm. Where in such a case disagreement amongst external peer reviewers is sufficient to create reasonable doubt for the committee as to the scientific merits of the proposed research, this doubt must weigh more heavily against the ethical acceptability of that research than would be the case were the research to be relatively harmless or risk free.

The second overarching consideration is that the committee's decision (including any confidential aspects) on the ethical (un)acceptability of proposed research must be based on reasons that the committee can articulate and that would, in principle, be publicly defensible.

Concluding Remarks

Philosophical accounts of trust stress that all trust involves risk. Procedures, structures and protocols can be put in place, and also instruction can be provided that aims to strengthen the conditions under which external scientific peer reviewers are likely to provide trustworthy reports. That these measures would not guarantee the trustworthiness of external peer reviewers is clear enough. Less obvious is the point that these measures would not confer on external scientific peer reviewers what I have called trust-authority as far as an aspect of the deliberations of institutional ethics committee are concerned. This is because the responsibility of overall ethical appraisal of a research proposal lies with the institutional ethics committee and an important element

of this ethical appraisal can require that the committee itself form a view on the scientific merit of a research proposal. (Where individual ethics committee members disagree about this, or about any other aspect in relation to assessing the ethical acceptability of a particular proposal, appropriate procedures need to be in place to arrive at an acceptable and publicly defensible outcome.)

The committee itself must *take responsibility* for all of the elements that provide its reasons for its final decision. The committee's taking this responsibility is compatible with its drawing on the expertise of external scientific peer review of research proposals, and indeed such external review can be formally and ethically required. Here what the committee delegates, and what the committee cannot itself do and thus needs to rely upon external expertise to do, is a *review* of the scientific merits of proposed research. Measures that can increase the trustworthiness of such reviews do not justify the committee's then simply entrusting its judgement on this element of its deliberations to external experts.

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Experts Of Common Sense: Philosophers, Laypeople And Democratic Politics

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ABSTRACT

This paper approaches the question of the relations between laypeople and experts by examining the relations between common sense and philosophy. The analysis of the philosophical discussions of the concept of common sense reveals how it provides democratic politics with an egalitarian foundation, but also indicates how problematic this foundation can be. The egalitarian foundation is revealed by analyzing arguments for the validity of common sense in the writings of Thomas Reid. However, a look at three modern philosophers committed to the link between philosophy and common sense – Descartes, Berkeley and, again, Reid – shows that each assigns very different contents to the concept. This raises the suspicion that modern common sense is not only an egalitarian element, but also a rhetorical tool with which intellectuals attempt to shape the views of the lay masses. The last part suggests that the way out of the predicament is rejecting the supposition that common sense is a unified, homogeneous whole. An alternative is sketched through Antonio Gramsci's concept of common sense.

Keywords: common sense, Thomas Reid, Antonio Gramsci, democracy, egalitarianism, ordinary language.

1. Introduction

The series of popular uprisings that started in various places around the world in 2011 – often referred to as the Occupy Movement – brought to the fore the old question regarding the potential role of ordinary people in politics. Thus,

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for example, the social protest that swept Israel in the summer of that year was initiated by a small group of university students, far removed from the traditional image of professional politicians or social leaders. It succeeded in getting masses of people from various sections of the public involved in sociopolitical discourse by creating forums, discussion circles, and of course websites aimed at creating a popular basis for the protesters' demands and actions.

The protest's opponents tried to play its popular nature against it, portraying its young leaders as naive amateurs unaware of the complexities and implications of their demands. When the movement grew, the government decided to form a team of experts to contain the demands. In response, the protesters formed their own team which combined multidisciplinary experts with students and activists. Members of both teams announced their intention to listen to the public and represent its demands.[1] Is this irresponsible populism or democracy at its best? What is it that allows an ordinary person, who has never studied economics, to formulate demands regarding taxation and the national budget? At what point will that person know that she has exhausted her ability to judge in this matter, and had better consult an expert? One important albeit problematic answer to all those questions is common sense.

Common sense is usually defined as a set of obvious, self-evident beliefs and judgments, equally accessible to all.[2] Although the concept's long history is often associated with epistemological and anthropological discourses (e.g. Lemos, 2004; Stoler, 2010), it is always politically charged *ab initio*, as it sets the beliefs and judgments of laypeople against those of experts, professionals, scientists or philosophers, thereby involving a claim regarding the hierarchical relations between the masses and the elites. In this respect, the concept is charged with two opposite potentials. On the one hand, common sense implies that certain things are self-evident, beyond reasonable doubt, and hence has clear conservative potential in that it may be used to naturalize existing relations of power. On the other hand, it is intimately related to the notion of equality, thus having a significant democratic potential in that it can provide

[1] For the report of the government's committee headed by Prof. Manuel Trajtenberg, see http://hidavrut.gov.il/ [in Hebrew]; accessed Oct. 9, 2014. For the report by the alternative committee, whose education team was coordinated by the author, see Spivak and Yona (2012).

[2] See for example the Wikipedia article on "common sense", which is based on the Merriam-Webster and Cambridge dictionaries: http://en.wikipedia.org/wiki/Common_sense; accessed Oct. 9, 2014.

liberal-democratic politics with the philosophical and anthropological foundation needed to trust the individual citizen's judgment, allowing him to claim autonomy and oppose traditionalist privileges.

In a thorough study of the political history of common sense, Sophia Rosenfeld (2012) has recently showed how the concept was used to make two contradictory kinds of political demands, sometimes even during the same period: demands for change, and demands for the preservation of the status quo. She traces the way the concept of common sense emerged in the late 17th century as an attempt to deal with the political instability of post-revolutionary England by articulating the common ground shared by all members of the state, but soon became an oppositional tool in the hands of the people or their representatives. Common sense was seen as an apolitical platform for political discussions – "everyman's tribunal", whose authority everybody respects while opposing political parties' claim to be on its side. To be sure, the duality of conservatism and radicalism is never fully resolved: the concept of common sense implies a potential for pluralist-democratic politics that is based on the views of the masses, but this potential is in constant tension with a demand to narrow down the political arena in the name of self-evident truths. This tension is captured by the paradoxical figure of the "specialist on common sense", the expert whose field of expertise is that which requires no experts, who claims privileged knowledge of unprivileged knowledge.

The concept of common sense rarely appears in political theory, but the dominance of the ideas it expresses suggests that it is always implicitly present as a self-evident truism: common sense is part of the common sense of modern political thought. Hence, although the story of modern political theory famously stars reason, common sense is no extra, as it acts as the source of legitimation of the liberal right for minimal government intervention, as well as of the individual's democratic right to express her opinions on public affairs. These rights rest on the assumption that every person of common sense is capable of reasonable judgment, of telling right from wrong, good from evil, etc. This does not mean that every layperson is regarded as a proper arbiter in every matter: certain fields are seen as reasonably left to the judgment of experts. But from the liberal-democratic perspective every restriction imposed on ordinary people should itself be justified by common sense: in other words, common sense alone is authorized to limit common sense. For example, Mill demands in *On Liberty* to grant the right "to carry on their lives in their own way" to those who possess a "tolerable amount of common sense and

experience" (1977a: 270), while in *Considerations on Representative Government* he appeals to common sense to justify denying the right to vote to all those who do not possess enough education: "No one but those in whom an *à priori* theory has silenced common sense will maintain that power over others, over the whole community, should be imparted to people who have not acquired the commonest and most essential requisites for taking care of themselves" (1977b: 470).

In what follows I approach the concept of common sense and the question of the relations between laypeople and experts by examining the relations between common sense and philosophy. The analysis of the philosophical discussions of the concept will reveal how it provides democratic politics with an egalitarian foundation, but also indicate how problematic this foundation can be and mark the way for a new understanding of common sense upon which contemporary democratic theory can rely. First I contrast the traditional scorn philosophy pours on common sense with the modern approach that claims philosophy must keep in line with it. I then present the egalitarian basis the modern concept of common sense attempts to give democratic politics by analyzing arguments for the validity of common sense in the writings of the 18th-century philosopher Thomas Reid. These arguments, which are characteristic of modern philosophy, rely on the connection of common sense to everyday practice and ordinary language, and are independent of the specific contents of common sense. A look at three modern philosophers committed to the link between philosophy and common sense – René Descartes, George Berkeley and, again, Thomas Reid – shows that each assigns very different contents to the concept. This raises the suspicion that modern common sense is not only an egalitarian element, but also a rhetorical tool with which intellectuals attempt to shape the views of the lay masses. Finally, I suggest that the way out of the predicament that results from the importance of common sense for democratic theory on the one hand and its dangerous elasticity on the other is rejecting the supposition that common sense is a unified, homogeneous whole. In order to sketch an alternative I look briefly at Antonio Gramsci's concept of common sense, with which I rearticulate the political challenge of the democratic relations between experts and laypeople.

1.1.

Although every use philosophy makes of common sense involves taking a stance regarding the hierarchical relations that should or should not exist between professional philosophy and the views of ordinary people, the stance itself may vary from one writer to the next. Philosophical tradition has always been fraught with expressions of suspicion of the masses, whose views are contemptuously portrayed as not only false but dangerous. The most celebrated and influential representative of this attitude is Plato, whose writings – clearly influenced not only by Socrates' character but also by his trial – express persistent disdain of the masses and their opinions, the *doxa*. This is manifest, for example, in *Crito*, in which Plato has Socrates respond to his friend's claim that "one must also pay attention to the opinion of the majority" (1997a: 39), by saying that "we should not think so much of what the majority will say about us, but what he will say who understands justice and injustice, the one, that is, and the truth itself" (42). The majority, according to Socrates, "inflict things haphazardly" (39), namely follow inconsistently trends and eloquent speech (see also *Gorgias*, in Plato, 1997b: 800). Plato charges the philosopher – the expert for justice and injustice – with critically analyzing and correcting the *doxa*, thus creating a clear hierarchical difference between the prejudiced layperson and the philosopher. This is an expression of extreme elitism: it assumes that certain people do not really think and that their views are not really theirs, and in that legitimizes the prioritization of the views of the few over those of the many.

However, appeal to the use of common sense of the masses in modern philosophy also involves a conspicuous egalitarian moment. Descartes opens the *Discourse on the Method* by stating that "Good sense is, of all things among men, the most equally distributed; for every one thinks himself so abundantly provided with it, that those even who are the most difficult to satisfy in everything else, do not usually desire a larger measure of this quality than they already possess. And in this it is not likely that all are mistaken" (1985a: 111). That is to say, unlike philosophy that strives to point to the deficiencies and contradictions of ordinary ways of thinking, Cartesian common sense implies the existence of truths accessible to all. This view is not unique to Descartes, though: the main stream of modern philosophy accepts the validity of common sense, and believes that the scholar or expert should make it the starting point for their studies; namely that their conclusions must not

contradict certain things that are known to everybody. The modern philosopher emerges as the ultimate expert in common sense. As Clifford Geertz notes: "Common sense, or some kindred conception, has become a central category, almost *the* central category, in a wide range of modern philosophical systems" (1993: 76).

This modern approach to common sense is at the heart of liberal-democratic politics, furnishing both the political theories in which common sense is not explicitly mentioned, and the political usages of the kind discussed by Rosenfeld. Analyzing the worldview behind the modern concept of common sense sheds light on the grounds liberal-democracy has for trusting ordinary people, as well as on the relations it establishes between the expert and layperson.

1.2

Arguments in support of common sense are found in the writings of many modern philosophers (Lemos, 2004: 1-23), but nowhere are they more central than in those of Thomas Reid, father of the Scottish school of common sense. Although the first philosopher to turn common sense into a central concept was probably Claude Buffier (2009), Reid is undoubtedly the most influential philosopher to have done so.[3]

Reid's philosophy was developed in response to Berkeley's immaterialism – according to which the notion of material substance is contradictory, and perceivable objects do not exist outside the perceiver's mind (Berkeley, 1999: 25) – as well as in response to Hume's skepticism (Hume, 2011). Rather than confronting these views in the philosophical arena, Reid declares that Berkeley and Hume contradict "certain principles... which the constitution of our nature leads us to believe, and which we are under a necessity to take for granted... without being able to give a reason for them – these are what we call the principles of common sense; and what is manifestly contrary to them, is what we call absurd" (1983: 20). According to Reid, philosophy must accept the principles of common sense as its presuppositions, rather than mere beliefs the validity of which may be doubted (8). Although Reid writes that the specific

[3] Reid himself does not present his arguments in a systematic, orderly manner, and they can be assembled, assorted and classified in a variety of ways. For different analyses of Reid's arguments see Greco (2002), Magnus (2008).

principles of common sense cannot and need not be proven, he nevertheless offers some very interesting arguments in favor of their validity: "although it is contrary to the nature of first principles to admit to direct or *apodictical* proof; yet there are certain ways of reasoning even about them" (260). Let us now turn to Reid's main arguments, in order to present the way they link common sense to the social sphere, and analyze its importance for democratic politics.

1.3.

Reid's first argument rests on the distinction between theory and praxis, namely between speculative philosophy and the actual world: Reid argues that a view that denies common sense can exist on paper, but its absurdity is revealed the minute it is brought into the world. More specifically, Reid claims that it would be dangerous to act in accordance with views denying the principles of common sense; with regard to Berkeley, for example, he writes that it is one thing to deny the existence of the material world within "a philosophical inquiry" (27), but a person acting as though material objects cannot hurt him is bound to get hurt.[4]

The second argument takes the first a step further into the social world: the very presence of other people is enough to bring the skeptic back to the commonsensical world: the madness of skepticism, in Reid's words, "is apt to seize the patient in solitary and speculative moments; but, when he enters into society, Common Sense recovers her authority" (119). A person might doubt common sense when reflecting in solitude, but once that person steps out into the real world, he has no choice but to conform to common sense, lest he be "taken up and clapped into a mad-house" (86). Indeed, Reid notes that except for very few examples from ancient times, all skeptics lived and acted in public as if skepticism never occurred to them.[5]

The anchor the social sphere provides common sense is further expressed in Reid's third argument, according to which ordinary language reflects the principles of common sense, which can therefore be learnt and validated by examining how people use language. Reid is well aware of cases in which the

[4] This, as we will see below, is a complete misunderstanding of Berkeley, who claims that immaterialism has no practical implications on everyday experience.

[5] P. D. Magnus interprets Reid as claiming that practical behavior indicates what a person actually believes, and that from one's respect for common sense in everyday life we may conclude that they trust common sense in every meaningful sense (2008: 7).

obscurity of ordinary language is a source of misunderstandings, like the word "smell" that in everyday parlance indicates both something external (the power of an object to make us sense its odor) and an inner feeling (the sensation itself): this obscurity is one of the reasons for doubting the existence of an external world, as it leads to the mistaken belief that it is possible to sense without an external object being the cause of sensation (25). However, Reid does not think such cases are reason enough to doubt the adequacy of ordinary language, for in everyday use they are easily disambiguated thanks to the context: "every sensible day-labourer hath as clear a notion of this" (ibid.), if only because ordinary people simply do not derive the non-existence of matter from linguistic ambiguities.

The absurdity of all philosophical claims that deny common sense, on the other hand, is evident in the way their allegedly accurate formulations drift away from ordinary language and use words in an inadequate manner (like Berkeley's claim that "objects of sensation" do exist, but are immaterial). In other words, the fact that philosophers give words extraordinary meanings in an attempt to make their views appear plausible, testifies to the falseness of these views:

> If he [the philosopher] means by smell what the rest of mankind most commonly mean, he is certainly mad. But if he puts a different meaning upon the word, without observing it himself, or giving warning to others, he abuses language and disgraces philosophy, without doing any service to truth: as if a man should exchange the meaning of the words *daughter* and *cow*, and then endeavor to prove to his plain neighbour, that his cow is his daughter, and his daughter his cow (26).

Unlike Humpty Dumpty, then, the philosopher cannot make a word mean just what he chooses it to mean (Carroll, 2005: 60) – his professional expertise does not authorize him to alter the language that is normally used by everybody. Problems of ordinary language do not prevent the common people from understanding their world and act successfully within it. This real-life effectiveness of ordinary language is for Reid the crucial evidence for its adequacy. Hence all common languages reflect the common sense of mankind and demonstrate its validity.[6] In other words, our ability to understand each

[6] "...the general principle – that every distinction which is found in the structure of a common language, is a real distinction, and is perceivable by the common sense of mankind – this I hold for certain, and have made frequent use of it... I believe the whole system of metaphysics, or the far greater part, may be brought out of it; and next to accurate reflection upon the operations of our own minds, I know nothing that can give so

other and cooperate in public means that ordinary language can be a source for learning the principles of common sense.

This argument rests, therefore, on the acknowledgment that philosophical claims, just like any other claims, have to be formulated in language and understood by others, be they philosophers or laypersons. Here, Reid captures a point that would become central for 20th-century analytic philosophy: the bounds of meaning, which are also the bounds of reasonable thought, are congruent with those of common language. This does not mean that Reid believes that language precedes thought and determines it, but the linguistic stance Reid expresses, albeit not systematically, reveals extraordinary sensitivity to at least two essential characteristics of language: language is a practice, and this practice is inherently public.[7] According to this view the ultimate philosopher, the expert in common sense *par excellence*, is the philosopher of ordinary language – who accepts it as given and analyzes it either theoretically or empirically in order to study common sense and draw philosophical conclusions from it.

1.4.

We can now tie together Reid's arguments and see them as expressing the same argumentative logic. This logic establishes an essential link between two claims: first, that there are certain obvious things that everybody knows; and second, that if something is to be comprehensible in the public sphere, it must meet certain basic conditions upon which every common action rests – be it linguistic or not. Common sense is therefore not a random collection of principles or beliefs, but may be deduced from people's ability to act in the real world, as well as from the interactions between them, namely their ability to cooperate and understand each other.

This sheds light on common sense's function as supporting egalitarian politics and liberal democracy and the rights it confers on ordinary people. It establishes the assumption that people are capable of functioning properly in

much light to the human faculties as a due consideration of the structure of language". Quoted in Jensen 1979: 361.

[7] Reid is the first to attach such great importance to language in philosophy, foreshadowing 20th century philosophers like G. E. Moore, Ludwig Wittgenstein and J. L. Austin. See Jensen (1979) and Greco (2002).

the sociopolitical sphere on the fact that they actually function there – as indicated by their ability to speak meaningfully and act reasonably in everyday life. In other words, liberal democracy grants the *right* to speak based on the *ability* to speak, and the *right* to act based on the *ability* to act: one's ability to speak and act normally testifies to their ability to tell right from wrong, proper from improper. The philosophy of common sense thus plays an essential role in drawing the borders of meaningful speech and reasonable behavior, thereby purging the public sphere from disturbances that might turn into political deviations: it protects the relative stability of words used in ordinary language and constitute a linguistic space of shared meanings and values that provides liberal-democratic discourse with an irrefutable foundation.

Reid's arguments by no means suffice to defeat skepticism: the skeptic might dismiss the dangers involved in his behavior as imaginary, resist the social pressure, and simply refuse to present his claims in public. But even if the arguments fail to achieve their philosophical goal, they certainly achieve their political one: they make it clear that every public action must concur with common sense or else be meaningless. The philosopher may reject common sense, and theoretically he may be right, but the political agent cannot: politics is public by nature, and every action or speech must be meaningful to others in order to be effective. Reid's argumentation, therefore, not only founds the liberal and democratic rights of the masses, but also demonstrates the need for democratic politics to make sense to them.

These conclusions seem to contradict the historical evidence of the dynamism of common sense in both the epistemological and political contexts: what seemed self-evident in the 1st or 11th century is very much different from what seems self-evident in the 21st. Scientists and philosophers, politicians and activists, often struggle to change common sense rather than act within it. Indeed, 20th-century common-sense philosophy acknowledges the possibility as well as the need to change common sense in accordance with new discoveries and ideas. It argues that every claim must be open to criticism, even if it is seen as a solid part of common sense (Peirce, 1965: 308). However, such criticism must itself rely on other commonsensical claims, like a boat at sea every part of which can be replaced by its sailors only if most parts remain intact.[8] This approach, referred to by C. S. Peirce as "critical common sensism", was explained by A. J. Ayer as follows:

[8] The metaphor is borrowed from Neurath (1983).

> What the metaphysician would like to do is take up a position outside any conceptual system: but that is not possible. The most that he can hope to achieve is some modification of the prevailing climate... But if such a venture is even to be intelligible... it must have at least a rough correspondence to the way in which things are ordinarily conceived. Thus if a philosopher is to succeed... in altering or sharpening our vision of the world, he cannot leave common sense too far behind him (Ayer, 1969: 81).

This point clearly applies to politics as well: every moral or political belief is open to change, and transforming common sense may be one of the most important tasks of democratic politics; such action, however, must only challenge some parts of common sense while firmly resting on others.

2.

As endorsed by modern philosophers such as Reid, common-sense proves the validity of the vast majority of popular beliefs and judgments, and provides liberal-democratic politics with a solid egalitarian foundation. However, the arguments elaborated above are independent of the concrete contents of commonsensical beliefs and principles. Needless to say, philosophers take the contents to be self-evident, the acceptance of which follows from recognizing the validity of common sense. But a closer look shows that different philosophers, even around the same period, ascribed very different contents to common sense. Let us now see how three philosophers of the 17th and 18th centuries – Descartes, Reid and Berkeley (presented here in a non-chronological order) – insist their philosophies are in line with the common sense of the masses but make very different claims regarding its substantive contents.

This will naturally have interesting implications for the connection between common sense and egalitarian politics, and particularly the relations of experts and laypeople: the modern philosopher, the expert of common sense, presumes to work in accord with or even in service of the layperson. But every philosopher defines common sense in a way that suits her own philosophical interests. Every expert of common sense respects the lay masses, but imagines them from his perspective instead of actually listening to them. This raises the suspicion that common sense is used by the philosophers for rhetorical purposes, to appear as though they are on the people's side while attempting to

dictate to the masses how they are supposed to think and act. At the very least, the egalitarian trend committed to common sense and the one despising the commonsensical layperson are two moments coexisting not only in the same period but also in the very same thinker: the concept of common sense itself invites the establishment of hierarchies while at the same time challenging them.

2.1.

Cartesian philosophy is marked by the attempt to free itself from the yoke of traditional authorities. The challenge Descartes poses to the received intellectual and social hierarchies is based on common sense, defined as a cognitive faculty shared by all human beings, cutting across the various natural and social differences between them. This common sense – referred to as either *le bon sens* or *lumière naturelle* (natural light) – is a leitmotiv running through Cartesian philosophy, marking the starting point of philosophy as an egalitarian moment, blind to the differences between everyday thinking and philosophical knowledge. With this concept, Descartes attempts to rethink not only the contents and origins of philosophy, but also the identity and qualifications of those authorized to practice it.

In his unfinished *The Search for Truth by Means of the Natural Light*, Descartes presents an imaginary conversation in which Eudoxus ("one of sound judgment"), who serves as mouthpiece for Descartes' views, proves to his interlocutors – the scholar Epistemon ("knowledgeable"), and Polyander ("everyman"), described as someone who "has never studied at all" – that "a man with a good mind, even one brought up in a desert and never illuminated by any light but the light of nature, could not have opinions different from ours if he carefully weighted all the same reasons" (Descartes, 1984b: 405). Descartes' reservations about esoteric scholasticism and his support of epistemic egalitarianism are evident in the fact that the ideal participant in the philosophical discussion brings with him as little previous knowledge as possible, and must be equipped only with "natural light," which the essay's title guarantees is the proper means for attaining truth.

The clarity of natural light distinguishes it from reason, and provides it with an evident, hence solid, starting point. The division of labor between common sense and reason is clarified in the *Meditations*, where Descartes uses his reason to doubt everything that is uncertain, and then extricates himself from

the abyss of uncertainty by means of the natural light which is immune to every reasonable doubt (1984a: 27). The complex move Descartes makes in the *Meditations*, which involves not only intellectual challenges but also significant psychological difficulties (15-16), rests on that common, minimal cognitive faculty of which he says, echoing the opening sentence of the *Discourse*, that "I have no cause for complaint on the grounds that the power of understanding or the natural light which God gave me is no greater than it is" (42). The considerations raised throughout the *Meditations* are clearly ones every person can, perhaps should, be able to understand and reproduce – the voice Descartes adopts when writing in the first person is that of common sense, of Polyander. However, we must not identify it with the actual layperson of his time. The methodical doubt Descartes casts in the first meditation applies to what he calls "my opinions" (12), which include not only complex theories known to scholars alone but also the simplest beliefs about the natural and social world. Descartes eventually proves most of these to be true, but calling them into even tentative doubt is bound to seem to the non-philosopher as the complete opposite of common sense, indicating that what laypeople take to be self-evident truths are not necessarily common sense according to Descartes.

Indeed, the fact that all humans are endowed with common sense that enables them to attain certain, clear and distinct truths, by no means implies that no-one can err, or even that most people don't. In fact, the lion's share of humanity is often misled by prejudices or "preconceived opinions" that blur the natural light (Descartes, 1985b: 209; see also Morris, 1973). Descartes' mistrust of the scholars of his time, and his challenge to the traditional hierarchy between intellectuals and the laypeople, in no way give credit to the uneducated masses (see Descartes, 1984a: 21). The common, everyday beliefs held by scholars and laypeople alike are therefore the *object* rather than the *tool* of examination and critique (Frankfurt, 2007: 15). The advantage of the lay over the educated, according to Descartes, lies at most in their greater willingness to listen to the voice of common sense, but it by no means derived from their views. Put differently, Descartes turns his concept of common sense against the *doxa*, the common opinion that is commonly referred to as common sense. This means that there is nothing "natural" about the Cartesian layperson and the light that is supposed to guide him: they are both rhetorical figures crafted by the philosopher to provide an elegant solution to the problems reason becomes entangled with; and both are quite remote from what Descartes thinks of ordinary people and their ways of thinking.

2.2.

Let us now return to Reid, whose arguments for the validity of common sense we have examined earlier. As we have seen, the egalitarian moment plays an important role in Reid's criticism of skeptical philosophy, which he identifies with intellectual haughtiness. Reid explicitly declares his inability to find faults in the philosophical arguments of Berkeley and Hume, but claims that the obvious falseness of their conclusions is evidence enough that they rely on false premises. The most important premise in these philosophical systems, according to Reid, originates with Descartes – it is the belief that we do not perceive objects directly but rather through the mediation of ideas which exist in the mind (Descartes, 1984b: 27). Berkeley, who shares this belief, claims it is impossible to compare a mental idea with an extra-mental origin, and concludes that the concept of matter is self-contradictory (Berkeley, 1999: 27); but according to Reid this conclusion should be regarded as a *reductio ad absurdum* of the starting point (Reid, 1983: 20-1). The important point is that the "theory of ideas", identified by Reid as the point where modern philosophy deviates from the course of common sense, is presented by Descartes as a view that expresses the beliefs of the masses, and has been accepted in modern philosophy as an evident truth in no need of justification. Reid, therefore, endorses the Cartesian mission of standing knowledge on solid foundations, but what is clear and self-evident to Descartes he takes to be contrary to common sense.

Like Descartes, Reid locates common sense in the individual, claiming that it is "a part of the furniture which nature hath given to the human understanding" (1983: 118); at the same time Reid, even more than Descartes, fears that the ascription of common sense to every person could undermine the foundations of society.[9] He is preoccupied with explaining how some people fail to acknowledge common sense and behave accordingly. The main reason people think or act in an absurd non-commonsensical manner, according to Reid, is madness – a "disorder in the constitution" (118) that does not allow the patient to use his brain properly. However, Reid does not attempt to define madness independently of common sense. He find this unnecessary, since the borderline between common sense and absurdity, sanity and insanity, seems self-evident to him: "how does a man know he is not in a

[9] Rosenfeld (2012: 60-89) elaborates on the conservative impulse at the heart of the Scottish school, and analyzes the tension between it and the epistemological egalitarianism implied by common sense.

delirium? I cannot tell: neither can I tell how a man knows that he exists. But, if any man seriously doubts whether he is in a delirium, I think it highly probable that he is, and that it is time to seek for a cure..." (18). The limits of common sense are therefore determined ad hoc – what seems to Reid far removed from the alleged views of society is declared to be insane from the point of view of the individual's common sense.

Hence philosophy is given the role of revealing the principles of common sense and making them explicit. This is because although no sane person would deny them, their very self-evidence makes them difficult to formulate. Performing this task requires "reflective introspection", which involves careful attention and as we have seen, careful analysis of ordinary language only trained professionals can achieve (104-5). Among the principles Reid's analysis reveals are some of the most controversial issues in the philosophy of his time, such as the concepts of causality, substance and free will, as well as subjects under dispute in the general public like the existence of God. That is to say, not every person can simply look into herself and reach valid conclusions – reflective introspection is proper philosophy, and its findings are worthy of the label common sense, only if they concur with Reid's views. This means that Reid's criticism of intellectual elitism, as well as his praise of ordinary people, turn out to be quite the opposite: his appreciation for the lay and for their way of thinking is conditioned by the demand that their views be in line with what he takes to be common sense. At the same time, the introspection aimed at revealing common sense is a process only a trained philosopher like Reid can accomplish. Thus it would seem that like Descartes, Reid also tailors common sense to suit his needs – in this case, protecting society from skeptical philosophy.

2.3.

A look at the writings of Berkeley, one of the philosophers Reid accuses as dangerous skeptics, will demonstrate the force of the demand, in the 18th century, to fit philosophy to common sense, and at the same time, the variability of the specific characterization of common sense. In *Principles of Human Knowledge*, Berkeley explicitly argues that not only does immaterialism by no means conflict with ordinary thought and action, but that it accounts for them better than the materialist view. Already in the opening sentences, he voices his preference for common sense over skeptical

philosophy which seems to him – just as it would to Reid a few decades later – to be not only theoretically false but also a source of psychological difficulties:

> Philosophy being nothing else but the study of wisdom and truth, it may with reason be expected that those who have spent most time and pains in it should enjoy a greater calm and serenity of mind... and be less disturbed with doubts and difficulties than other men. Yet so it is, we see the illiterate bulk of mankind that walk the high-road of plain common sense, and are governed by the dictates of nature, for the most part easy and undisturbed... They complain not of any want of evidence in their senses, and are out of all danger of becoming Sceptics (Berkeley, 1999: 7).

Immaterialism is presented as antithetical to skepticism not only because Berkeley does not doubt the ability to know whether matter exists – he claims we can positively know it does not – but rather because the opposite stance is in fact the skeptical one. That is to say, Berkeley thinks that the view that accepts the existence of matter is not the popular view of the lay masses, but a philosophical error which is responsible for "several difficult and obscure questions, on which abundance of speculation hath been thrown away" (61) – pseudo-problems that will surely disappear as soon as the concept of matter is gotten rid of (28). He is certain that with the exception of philosophers and those influenced by them, whoever considers the question will realize that they have no need for the concept of matter in the first place (26). This view is also manifested in *Three Dialogues*, in which the two interlocutors – Philonous ("lover of spirit"), and Hylas (from ὕλη, "matter"), the uneducated layman who abhors skepticism – declare complete loyalty to common sense (1999: 108).

It is easy to see that what Berkeley calls common sense, or the views of the "uneducated", is quite different from what Reid and Descartes took it to be. All three, just like the vast majority of modern philosophers, are in complete agreement that philosophy must not contradict the dictates of common sense; they strongly differ, however, with respect to what these dictates exactly are. None of them, as it turns out, has given much consideration to what the masses actually think. And why should they, if the masses are bound to fall prey to prejudices no less than philosophers? In other words, each of them molds common sense in a way that suits his philosophical views, and they all do so by declaring that their philosophical views are derived from common sense. The reversal of old hierarchies and the promise of epistemic egalitarianism is in this sense merely a pretense: common sense is nothing but another tool with which the scholar imagines the masses, dictating to them how they ought to think.

3.

We have seen that democratic politics is committed to common sense and supported by it, but that the concrete contents given to common sense vary according to the philosophical – in fact the political – interests of whoever applies this term. This raises a political question: can common sense provide politics with an egalitarian foundation, or is it always open for manipulation by experts who consider themselves authorized to speak in its name? It is possible, of course, that only some of the contents given to the concept of common sense by those defining it are wrong, while others faithfully reflect common social assumptions. The philosophers of the 17th and 18th centuries may have asked too much of common sense, while it is wiser to limit it to a small number of very general beliefs. This conclusion can perhaps be extracted from G. E. Moore's discussions of common sense, in which he lists propositions like "there exists at present a living human body, which is *my* body" and "the earth had existed for many years before by body was born" (1959: 33). But even Moore's assertions are not necessarily uncontroversial (Berkeley, for one, rejected the existence of material body and world), and more importantly, as Nicholas Rescher clearly indicates, the more secure beliefs are, the less informative they are (2005, 137-8). Hence an indubitable common sense would be too vague and indefinite to provide politics with a solid foundation.

I suggest, therefore, a different answer to the political question. This answer rests on rejecting the presuppositions underlying Reid's argumentation for the validity of common sense, and reformulating the arguments in a way that articulates the challenge facing egalitarian politics regarding the relations of experts and laypeople.

3.1.

Reid's arguments rest on the presupposition that people do indeed understand one another, and normally manage to coordinate their actions. Mistakes and misunderstandings are regarded as rare exceptions that can simply be ignored. That is to say, Reid assumes that all meanings in the social sphere are shared in the same way by everybody; objective reality, the intersubjective sphere, and common, everyday language all amount to a closed realm of perceptions,

meanings, and practices that determine that which is apprehensible, perceivable and reasonable. The arguments in support of common sense are therefore inherently tied to a presupposed unified community of speech and action, in which all adult, sane individuals take equal part.

However, the social fact of cooperation and communication does not necessarily imply a unified, homogeneous social sphere. People can understand each other and live their everyday life together even if not everybody can understand everybody else all the time, and even if some or all meanings are shared only by some. Thus, while common sense is necessary for social existence, there is no need to assume the existence of a *single* common sense throughout the social sphere – there may be a heterogeneous plurality of "common senses", coexisting and making possible many different forms of communication and cooperation, not necessarily understood by all. This understanding of the social sphere – much different from the one relied upon by modern philosophy – can be found in Antonio Gramsci's *Prison Notebooks*.

3.2.

In his prison cell, Gramsci ponders the reasons for the failure of communist revolution in Italy, and concludes that Marxism was unable to collaborate with the masses and bring them into consciousness of their own objective interests, since it failed to understand the complex way in which they comprehended their world and gave it meaning. Gramsci believes that orthodox Marxism's assumption that the ideological worldview is completely false, and that all its elements contradict the real interests of the proletariat and only legitimize capitalist relations of power is simplistic and misleading, since the uneducated masses are not completely blind to their reality. He uses the concept of common sense (*senso comune*) to articulate a richer, more accurate understanding of the worldview of the masses, one that will hopefully be able to suggest intersections between it and the Marxist worldview, thereby helping to engage them in revolutionary action.

Gramsci is well aware of the uncritical manner in which common sense is acquired, and of the fact that many of its elements justify and perpetuate class domination. His version of the concept, which allows him to recruit it in the service of critical theory and revolutionary politics, is based on the insight that the worldview of the masses is not at all a unified, coherent whole, but rather a

heterogeneous ensemble of perceptions, ideas, customs and prejudices that have very different origins:

> When one's conception of the world is not critical and coherent but disjointed and episodic, one belongs simultaneously to a multiplicity of mass human groups. The personality is strangely composite: it contains Stone Age elements and principles of more advanced science, prejudices from all past phases of history at the local level and intuitions of a future philosophy which will be that of a human race united the world over (2005: 325).

Not all aspects of this ensemble reflect the prevailing relations of production and domination. Some of them represent a critical understanding of reality, which has genuine revolutionary potential (Manders, 2006). Gramsci calls these subversive elements of common sense "good sense" (*buon senso*): "this is the healthy nucleus that exists in 'common sense', the part of it which can be called 'good sense'" (239). Marxism, which Gramsci refers to as "the philosophy of praxis," must therefore apply good sense to the struggle against common sense: "philosophy is criticism and the superseding of religion and common-sense. In this sense it coincides with 'good' as opposed to 'common' sense" (327).

Note that such understanding of common sense implies an entirely new understanding of the sociocultural and linguistic sphere that makes everyday cooperation possible. According to this new understanding society is in fact an incoherent plurality of unreconciled yet coexistent discursive fields: "There is not just one common-sense, for that too [like religion] is a product of history... religion and common-sense cannot constitute an intellectual order, because they cannot be reduced to unity and coherence even within an individual consciousness, let alone collective consciousness" (327); common sense "is the 'folklore' of philosophy, and, like folklore, it takes countless different forms... even in the brain of one individual, [it] is fragmentary, incoherent and inconsequential, in conformity with the social and cultural position of these masses whose philosophy it is" (343). In other words, the entire social sphere, just like the individual, is not a unified whole but a split plurality that is never identical to itself.

However, Gramsci ascribes the heterogeneity of common sense to the existing relations of domination in society, and claims that a different kind of common sense, one that is homogenous and fully consistent, one that will reflect an egalitarian, free society, is indeed possible: "to criticize one's own conception of the world means therefore to make it a coherent unity and to

raise it to the level reached by the most advanced thought in the world" (326). Political action, that is, should use good sense to undermine common sense from within, thereby constituting a true, consistent collective consciousness. Thus, like that of the three philosophers discussed above, the egalitarian potential of Gramscian thought also falls apart: by assuming the possible existence of true consciousness, which fully understands the social reality, Gramsci in fact fails to shake off the hierarchical, authoritarian relations between the knowledgeable intellectuals and the lay masses.

What would happen, however, if we took Gramsci seriously and read his discussions of common and good sense as an invitation to think of the social and linguistic worlds as incoherent and disjointed? If we further pursued the line that takes common sense to be a fragmented field, thereby regarding it not as a temporary and undesirable but as a permanent, unavoidable situation? After all, the discussion of the origins of the modern concept of common sense – in Descartes, Reid, and Berkeley – has already suggested that homogeneous common sense exists neither in the minds of individual laypersons nor in the social sphere, but rather in the mind of the philosopher, who imagines it and tailors it to suit his needs. Such an understanding of common sense would call for a rethinking of political action itself, following the direction charted by Gramsci's critique of ideology – and going further.

3.3.

Realizing the coexistence of heterogeneous common senses makes it possible to reformulate the conditions for political action as well as the political relations between laypeople and the experts in common sense (or any other field). In line with Reid's arguments we will say that like every action in the social sphere, political action too must appeal to some common sense; but it need not make sense to all at the same time, or at least not the same sense. This does not mean that political action cannot be democratic and rely on a popular basis. The opposite is true: ridding ourselves of the illusion of a *total* common sense enables a fresh understanding of democratic politics and the legitimation that comes from the masses rather than the elites.

It must be made clear that the heterogeneity of common senses does not contradict the existence and importance of advanced professional knowledge, just as it does not deny the existence of shared popular knowledge (although it

may not necessarily be shared by all). Every common sense implies hierarchies, and may acknowledge the authority of experts and professionals, whose knowledge and experience appear relevant from its perspective. Democratic politics appears here as a struggle over which common sense(s) become(s) hegemonic, namely be recognized as reasonable and obligating by the majority of the political community. This is naturally also a struggle over which experts are recognized as relevant political authorities. In this sense, common sense is indeed an arena in which everybody is equal: for everybody can take equal part in the political struggles determining which common sense(s) are dominant, or in other words, under which circumstances the layperson's opinion can be considered relevant and in which that of the expert should prevail. In this view, everyone may be an expert in some context, and everyone is entitled to say which other experts should be listened to and when. The political challenge of egalitarian democratic politics, therefore, is first and foremost to keep the plurality of common senses relevant; not to let political space become petrified around a single total homogeneous common sense that will determine once and for all who is a layperson and who is an expert, who has a privileged voice in the political arena and who is only "a man of the masses".

This, I believe, has certain important implications for the social protest movement that has been taking place in Israel and around the world. One of the dominant cries throughout the various protest locations has been the demand for a fundamental change of the "system" – a refusal to accept it as self-evident. In the terms of our discussion, this cry challenges what is taken to be the common (non)sense of capitalism. But if indeed the protest has failed, as many now claim, I believe it is because it has attempted to express the voice of the popular masses while renouncing common sense altogether. To regain vitality and influence, activists throughout the world must look for the plurality of common senses, to try to understand the various ways in which different (groups of) people make sense of the world, in order to form political connections even in places that seem unlikely – between factors that seem to have no common denominator – based on communication practices that take as common and self-evident only the shared need to oppose the "system." On the one hand, this opposition should be commonsensical enough so as to express a clear voice and win wide support. On the other hand, the voices of protesters must not be silenced by those of experts, for every particular common sense implies the ability to decide which experts it wishes to consult with and how. The hierarchical relations between "ordinary people" and

experts need not disappear, but become dynamic, and work to undermine relations of domination rather than reinforce them. Such uses of common sense, I believe, are not self-evident at all.

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The Day After The Day Of The Experts. Lessons From J.M. Cattell, B.I. Gilman And C.S. Peirce

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ABSTRACT

In his 1914 paper "The Day of the Expert," Benjamin Ives Gilman expressed the hope that organizations would be ruled by experts instead of managers and politicians. My first part addresses his conception of expertise. Significantly, he referred to J. McKeen Cattell's article "University Control." In this paper, Cattell condemned "the transference to university administration of methods current in business and in politics." I thus examine university policy as a particular case and ask whether managers would do better than experts at the head of colleges. My last part investigates the possibility of a general science of reasoning, whose experts would properly be experts in taking steps, decisions and actions, an essential quality to managers. I follow the lead of Charles S. Peirce, who taught both Gilman and Cattell. I eventually suggest that boards should be mixed, and argue against leaving the whole power to managers, because they are not and cannot be experts in (good) reasoning

Keywords: experts, university, management, reasoning, Peirce.

Some of the most fruitful contemporary discussions of the problem of expertise bear on the expert-novice relation (under which conditions should a layperson defer to an expert?) and on the expert-expert relation (how to evaluate disagreement between peer experts?). In this article, I will focus on the relationship between experts and managers. Thus, my concern is not properly the tension between democracy and technocracy, which is often alluded to in philosophy of expertise; it is the tension between two kinds of technocrats: representatives of knowledge on the one hand, of management on the other. Should the direction of a group be left to experts of the domain relevant to this

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group, or to people experienced in policy and economy? I suggest that boards should be mixed, and argue against leaving the whole power to managers because they are not and cannot be experts in (good) reasoning. Benjamin Ives Gilman's 1914 paper "The Day of the Expert" (DE) will be the guideline of my discussion. I first expose Gilman's definition of experts and his view on their role in corporations' organization. I then examine university policy as a particular case and ask whether managers would do better than experts at the head of colleges. My last part investigates the possibility of a general science of reasoning, whose experts would properly be experts in taking steps, decisions and actions, an essential quality to managers. I conclude that such a science is probably a myth, so that instead of replacing scientific experts by managers, we should keep them in place or put them back onto leading positions.

1. Has the Day of the Experts Arrived Yet?

The attention given to expertise is not new, as shows a reflection displayed by a philosopher one century ago. In his 1914 presidential address given at the ninth annual meeting of the American Association of Museums, Benjamin Ives Gilman, the curator of Boston's Museum of Fine Arts, exposed some thoughts about the role of experts[1]. He started from a rather pessimistic analysis: the talks given by art historians and museum curators are fundamental for their practice, because they contribute to the development and exchange of ideas, but they weigh very light when questions of administration and political decisions are at play. "We have the voice here. How much voice have we at home?" (DE, 771) As a corporation, art experts hold relevant knowledge and should be at the steering wheel, but they feel powerless. Gilman divides his point into four questions: "What has been the position of the expert among us? What change suggests itself? What are the bearings of change? What are the prospects of change?" (DE, 772) These questions are almost a pretext for arguing that now is "the Day of the Expert," a phrase which gives the paper its title.

Gilman does not use these words carelessly. Here is his attempt to define them: "By expert will here be meant a person whose achievements demand

[1] He would later mention the point of view of lay people in a totally different context. See Gilman, B.I. (1927). Relativity and the Lay Mind (I) & (II). *The Journal of Philosophy*, 24(18), 477–486 & 24(19), 505–521.

special aptitudes long exercised; and by his day a time when these developed abilities are used to the best advantage of the community" (DE, 772). It may interestingly be compared with some of the many contemporary attempts to define an expert. Hubert Dreyfus zeroes in on the practical skills involved, contending that "the ability to make more subtle and refined discriminations is what distinguishes the expert from the proficient performer. [...] with enough experience in a variety of situations, all seen from the same perspective but requiring different tactical decisions, the brain of the expert performer gradually decomposes this class of situations into subclasses, each of which shares the same action" (Dreyfus 2002, 371–2). According to Alvin Goldman (2001, 91–92), the truth of the propositions stated by an expert in his domain are most significant, so that cognitive expertise can be defined in veritistic terms, even though an expert is not only someone who has more true beliefs about a field than the average. Goldman grants that the criteria for expertise also include the disposition to exploit his information to form beliefs in true answers to new questions that may be posed in the domain, especially about the principal questions of interest, or 'primary questions,' to the researchers and students. Some authors also try to define continuous steps from the novice to the experts of different kinds, such as contributory and interactional experts, according to their capacity to really contribute new results or only 'talk the talk' of inventors (Collins and Evans, 2007).

A retrospective glance at Gilman's earlier definition, "a person whose achievements demand special aptitudes long exercised," shows the following features: 1. it focuses on experts rather than expertise, which sometimes involves techniques, instruments or scientific discoveries (e.g. DNA analysis); 2. it requires some effective results – 'achievements' –, which can be either true propositions or successful actions; 3. it includes the possession by the expert of 'special aptitudes,' which seem to refer to dispositions, capacities and practical skills rather than information, even if such aptitudes may produce knowledge; 4. it also demands a long exercise. This last point is especially interesting, since it introduces a temporal criterion which often lacks in contemporary presentations. *Experitus* is the one who has experience and is well-versed in some activity due to her long acquaintance with the field and her long practice. An expert is reliable because she was more often right than wrong most of the time during a long period. It seems to rule out the cases of deviant expertise or 'cloistered experts' taking silly decisions, for instance illustrated by D. H. Freedman (2010).

This last point is fundamental. In effect, giving the power to experts is often seen as a threat for democracy. H. Landemore (2010) argues that cognitive diversity, that is, taking into account the larger number, is always epistemically more fruitful and reliable than the opinion of the few. Giving the power to a group of technicians is not only epistemically dangerous – many studies show that very often they do not produce better results that laypeople – but anti-democratic, not to mention the not so rare cases when they are victims of a conflict of interests.

It is also in terms of democracy that Gilman raises the problem of the experts' power, but with a different view. According to him, two models of democracy may be conceived. In the first one, all men are to be treated equal, whatever their differences. In the other, all men are to be treated equal as long as they show equal. One could add to support Gilman's view that, although the first model may seem fairer or more natural to us, it appears that we do not really want it to be applied; for instance, in reason of their deeds, we do not want criminals to be treated like anyone else. Thus, it makes sense, if not to reward 'better' citizen, at least to acknowledge individual differences of competence. Equal rights do not imply ignorance of differences, including differences of skills. It relies on the pragmatist maxim that real differences are equivalent to a difference in possible practical effects, as I will show in the third section of this article. This rational principle is intended to avoid mysterious, invisible entities; hence: "Equality proclaims the logical postulate that all real differences of human capacity are sensible facts of the present world [...]. The doctrine of equality affirms that only those persons who show themselves different should be treated differently" (DE, 774). Therefore, true democracy taking the individual differences of aptitudes into account is meritocracy: "It is the merit system generalized. Admitting all verifiable disparities of human capacity, and excluding all mystic disparities, the equality of the Declaration is simple common sense" (DE, 774).

In Gilman's words, it reflects a contrast between the 'colonial method', i.e. the English, and the American method. The so-called colonial method focuses on a purpose to be achieved. Consequently, regard is given to the special competence required to accomplish it. The national method of the US, conversely, focuses on the persons chiefly, who are held to all have the same general competences. It is supposed that someone able to fulfill a certain task will equally show ability for a totally different one. "We of the United States have been nurtured in the belief that a man who has distinguished himself in

any one direction will also distinguish himself in any other" (DE, 773). In this last model, experts are not needed. But it fits a society where no outstanding achievements are required, Gilman argues, where "surpassing excellence has not for the most part been essential." That 'the day of the expert' has come means that from now on "only the best has become good enough for us. The Jack-of-all-trades is master of none, and our progress calls for masters everywhere" (DE, 774).

As a consequence, it appears that relying on experts promotes a form of diversity. Specialization is a way of driving a homogenous group to a variety of directions and exploring new, diverse possibilities of knowledge and practice. "The new ideal is not that of a society of persons increasingly like each other, and hence increasingly sufficient each to himself, but of persons increasingly different each from the other, and hence increasingly necessary each to the other" (DE, 775). In this sense, the diagnostic is the opposite of Hélène Landemore's, who regards it as an impoverishment of the democratic diversity. Of course what she means is that a group of experts in a definite field tend to be more homogeneous than a group of laypeople taken at random, but the variety of expertise also must be taken into account.

2. University Policy

It may seem to us that the day of the experts that Gilman was calling forth arrived long ago. It is true that in politics, we still witness "the practice of naming any capable person for any office," and even the habit of naming anyone for any office. But in many domains (in fact, including politics), experts have at least part of the decisional power. Gilman was alluding to the question of how to run a museum, since he himself was a curator. He advocated for a "positive control by mixed boards." He suggested that "any corporation should include members embodying in their own persons the special types of skill essential in carrying on its work" (DE, 776). These experts should take part of the decisions, and even the most important part, in providing directions and aims to their institution, viz. museums: "the accumulation of our wealth has outrun our provision of knowledge and skill to utilize it. The positive system of control repairs this omission, now out of date. It supplements our present provision of means by providing also for ends. The men of means and

the men of ends must join forces in order to the best achievement of their common purpose" (DE, 776).

Gilman thus supports the presence of experts at the head of museums together with the usual political and economic leaders. But one can surmise he did not target only museums. His vision of society as having reached the time of excellence and the related need for experts implies a whole reorganization of social structures. Significantly, he referred in his paper to J. McKeen Cattell's article "University Control" published in *Science* in 1906. Cattell favored the idea that professors and alumni had a seat around the table of the directors of every university in the USA. Cattell described the situation he knew to be "between the Scylla of presidential autocracy and the Charybdis of faculty and trustee incompetence. The more incompetent the faculties become, the greater is the need for executive autocracy, and the greater the autocracy of the president, the more incompetent do the faculties become" (Cattell, 1912, 804). The solution to this vicious circle is a representative system.

Experts – namely, professors and alumni – are here viewed as a lever for democracy, not an impediment, due to the fact that for a university, the people to be represented are experts themselves. Are universities a special case of administration in this respect? It could seem that the society composing a university gathers people each of whom possesses a certain expertise in an academic field, and that for this reason the board of a university should be at least partially made of experts. But could not the same be said about a chocolate factory, a steamer, a farm, or a set of farms composing a village? In all these cases, a group shares a common purpose and each member contributes either same or complementary abilities. Thus, all corporative groups are made of experts and should be democratically represented by experts. The theoretical limit of this model is a group whose common purpose would be too vague or the abilities too poorly related to its end; it is basically what 'civil society' is, hence the idea that the experts model of government is not good for democracy.

Here is Cattell's plan for a new system of universities:

> There should be a corporation consisting of the professors and other officers of the university, the alumni who maintain their interest in the institution and members of the community who ally themselves with it. In the case of the state universities part of the corporation would be elected by the people. This corporation should elect trustees having the ordinary functions of trustees –

> the care of the property and the representation of the common sense of the corporation and of the community in university policy. (Cattell, 1912, 805)

Where Gilman did not go into the details of the mixed board, Cattell here makes it clear that the corporations of experts would elect trustees in charge of running the everyday routine and management of the university. The general function of the administrative part of the board would be, Cattell surprisingly grants, to represent "the common sense of the corporation." It may refer to the common financial, economical, strategic, etc. interests of the university. Or does it allude to the lack of common sense from the part of the so-called experts? It is not unlikely that academics are not the best people to run a university and that they need the help of specialists in budget, administration, management and economy.

Therefore, the picture does not involve experts vs. novices, but rather people belonging to the field (or rather, one of the fields) covered by the institution vs. people having expertise in auxiliary fields. Even if he does not understand anything to geology, an economist running a department of geology is not a layperson. What Cattell is unhappy with is in fact "the transference to university administration of methods current in business and in politics." But for the affairs to be run in a proper way, are not some competences in economy and politics also required?

Two logics are in conflict: on the one hand, excellence is sought and requires an increased role of experts, but on the other hand, economic constraints are increasingly significant. It is very much reminiscent of what we are experiencing nowadays. 'Excellence' has become a key term in academic mumbo jumbo: 'Centers for Excellence' in the US, the 'Research Excellence Framework' (REF) allocating funding in the UK, the 'Excellence Initiative' of the Federal German Ministry of Education and Research, 'Laboratoires d'Excellence' (Labex) in France, 'Scuole e Collegi di Eccellenza' in Italy... But in the same time faculties, or their representatives, are deprived of their decisional powers, transferred to administrators and managers. The facts seem to contradict the words.

This apparent paradox may be due to the appearance, in the end of the 19th century (e.g. London School of Economics, Sciences Po Paris) and more intensively after the Second World War (e.g. College of Europe, Escola Superior d'Administració i Direcció d'Empreses, École Nationale d'Administration, etc.) of schools whose purpose was to form the elite in administration and management. Taylor's influential *Principles of Scientific*

Management, published three years before Gilman's address, are not to be omitted. Does the notion of competence in general management make sense? When Gilman contrasted the forthcoming day of the expert to the past day of the novice, he meant that the dispositions someone developed in a field are not necessarily a sign for dispositions in a different field. But what if the skill consists in the ability to transfer one's abilities from one field to another? I will explore in the last section of this paper one possible way to interpret the art of being a specialist in management.

3. Can There Be Experts in Reasoning?

Auguste Comte used to say that a philosopher is a specialist in generalities. He also advocated the formation of a class of scientific politicians, who would mediate between people and rulers in forming expert opinions. What is the competence of a political expert? Plato's model of the king philosopher holds it to be the mastery of dialectics, the art of divisions. Aristotle's model of the *phronimos* sees it as a disposition to invent rules in situation. The enlightened king is a metaphysician, whereas Pericles has practical knowledge.

The question I would like to address in this third section is whether logical skills are a good candidate for this ability. The reason of this hypothesis is that the art of management seems to consist in taking right decisions according to the circumstances. It may be viewed as the art of drawing good inferences from a set of premises. It is not the only possibility and is probability a limited take on it, but I will not go any further within the present article. A more straightforward point against managers could be made (see e.g. Stewart, 2009), but it is not my purpose. My presupposition here is that if there possibly are good managers, they are able to manage anything, being experts in management in general. Their skill is indifferent to content. It must consist in some formal ability to proceed to a next step according to the information at hand. Such is the art of reasoning.

In order to consider and evaluate such a proposition, I will make a detour tying up Gilman and Cattell to a third historical figure. The man who connects them is the philosopher, logician and scientist Charles S. Peirce. Benjamin Ives Gilman was his student in logic and philosophy when Peirce taught at the John Hopkins University in the 1880s, and a rather active member of the Metaphysical Club. This is why I previously referred to Gilman as a

philosopher, although he worked in art history. He kept lecturing, writing and publishing papers in philosophy until his retirement from Boston's Museum of Fine Arts in 1925[2]. As for J. McKeen Cattell, he held the title of Fellow in philosophy at the same university during these years, and also took part to the meetings of the Metaphysical Club. Gilman published a chapter in the *Studies in Logic* collection directed by Peirce. In 1901, Peirce had Cattell elected as a psychologist at the National Academy of Sciences, against his closest friend William James. The three men were therefore strongly connected, and it is beyond doubt that Peirce influenced much the other two.

Although Peirce did not explicitly write about expertise, he made some points quite clear. Experts, and especially scientists, are of no use if they do not combine their specialized knowledge with a general sense of culture, that is, with a disposition for avoiding prejudices and judging and reasoning correctly:

> [...] the average scientist has become far more specialized, and instead of being the man of general and broad culture that he used to be, he is turned into an ignorant fellow, very little, if at all, intellectually higher than an average photographer, outside of his specialty. His peculiar narrow but deep training has made him a queer mixture of enlightenment and of what is equivalent to superstition (Peirce, 1911, MS 856).

To be a scientific expert does not prevent from bad reasoning. Far from being a gift for a few, the ability to draw conclusions in a valid way should be a universal capacity, Peirce believed. The power of reasoning should be shared among all people, although only some specialists make a good use of it. When sham reasoning is too threatening, or when complicated reasoning is indispensable, people sometimes "hire a specialist to perform it" (Peirce, c. 1896, CP 1.58). What is a specialist of reasoning? "For my part, I consider that the business of drawing demonstrative conclusions from assumed premisses, in cases so difficult as to call for the services of a specialist, is the sole business of the mathematician" (Peirce, 1894, CP 4.134).

One could be surprised that mathematicians, not logicians, are called forth when a problem of reasoning shows up. It is probably due to the idea that, according to Peirce, the business of logic is to analyze reasoning into as many steps as possible, whereas the business of mathematics is to draw inferences as straightforward as possible (Peirce, 1901, CP 4.373). I will not consider the

[2] Among his most significant papers are: The Logic of Cosmology; Reading the Kritik Afresh; The Dilemma of Darwinism; The Design Argument Survives Darwinism; A Logical Study of Law; Psycho-Anæsthesia.

question whether logic or mathematics is the most appropriate science for reasoning, since the uses of the words are different for us and in Peirce's time (e.g. his 'logic of probabilities' today belongs to maths).

Peirce's avowed purpose in teaching logic at Johns Hopkins was "to communicate the *logica utens*, and to make expert reasoners of the pupils, able to form clear ideas, to avoid fallacies, and to see in what quarters to look for evidence" (Peirce, W4.xxvi). He famously viewed his age as "the age of methods," and the university (and especially Johns Hopkins) represented for him the university of methods. It refers specifically to the idea that logic, in its part called 'methodeutics,' is to provide the "method of methods," that is, to give the power of discovering which method is right for which domain of knowledge. Abilities in logic are supposed to confer a sort of meta-capacity for all kinds of science.

After his years of lecturing at the university, Peirce even planned a wide course all over the United States, in order to bring his fellow people up to the required standards of sound reasoning. Although solitary and desperate, his enterprise had an educational purpose not very far from our schools of management. He wanted to teach the rulers-to-be of the country how not to fall into reasoning traps. His ambition for these "expert reasoners" was to be able to invent their own rational methods in the direction they would trace. That is why "a man needs to be more than a mere specialist; he needs such a general training of his mind, and such knowledge as shall show him how to make his powers most effective in a new direction. That knowledge is logic" (Peirce, W4.380).

Such a faculty has to be trained, for "powers of reasoning in any but the most rudimentary way are a somewhat uncommon gift, about as uncommon as a talent for music. Indeed, a much smaller number of persons actually attain to any proficiency in reasoning" (Peirce, 1898, CP 1.657). This endeavor should be inscribed in the broader debate on the characterization of logic as an art or a science. Peirce firmly rejected its definition as an organon for other sciences and as the art of thinking; he viewed it as a genuine science. Being a science, the ability to reason properly can be taught and improved. Peirce seems to have been sincerely convinced that it took only a good training to become an expert in reasoning, not a special disposition.

Peirce himself was called forth as an expert together with his personal trainer in (mathematical) reasoning, viz. his father, one of the leading mathematicians in the United States. The Peirces father and son were solicited

in 1868 as expert witnesses in the Howland Will Case. Howland's niece, Henrietta Robinson, was suspected of having forged a signature in order to invalidate the last will of Sylvia Ann Howland. The 'experts' decomposed Howland's signature in a number of downstrokes, and proved that the downstrokes of the signature in the suspect document and the genuine signature overlapped in a highly improbable way. Benjamin Peirce concluded that: "The coincidence which has occurred here must have had its origin in an intention to produce it" (quoted in Meier & Zabell, 1980, 499). Although, as Benjamin Peirce confessed himself, he had no expertise in handwriting, it is an outstanding case of forensic mathematics primarily based on the mastery of probabilistic reasoning.

Nevertheless, such an expertise in reasoning belongs to a specialized area. In this case, Peirce was an expert in the sense of the justice courts. It does not mean that he was a master in reasoning *tout court;* and were he, it does not prove that it was an acquired competence rather than a special, personal disposition. In teaching logic, Peirce manifestly supported the view that competence in general reasoning is a skill that could be developed. But empirical research has shown ever since that such optimism must be qualified. Not to mention abductive powers, which Peirce eventually regarded as an intuitive disposition and a stroke of genius rather than a scientific, repeatable process, even deduction has appeared to be founded on unstable grounds.

One of the most famous examples of an empirical study of deduction, namely the Wason selection task, turns out rather unsuccessful. Four cards are presented to the subject of the test, showing respectively two letters (say, A and E) and two numbers (say, 3 and 5). Each card has a letter on one side and a number on the other. Which card must be turned over in order to verify the proposition: "behind each E is a 5"? Most people fail to recognize that the correct answer is E and 3, not E and 5. The striking point here is that the proficiency of logicians (those who did not know the test yet, at least) and students in logic is no better than the average, although the selection task only implies the use of the logical rule of *modus tollens*, a basic principle familiar to every logician. It is a sign that training in logic does not improve the chances to make good reasoning in situation. Logicians do not 'think better,' nor are they experts in drawing the right inferences, even less in taking the good steps.

Other arguments, such as Harman's, dissociate reasoning and logic in showing that they obey different principles. Good reasoning involves a principle of "clutter avoidance" (Harman, 1986, 11–15), which goes against

the logical principle of closure under deduction. A clever reasoning does not necessarily consist in drawing a correct inference; it may result in revising the premises of the inference for instance. Such examples and others argue in favor of not reducing the process of reasoning, which is a mental activity dependent on a context, aiming at changing beliefs, to the universal laws of logic. There are professors of logic, but not of apt reasoning.

If there exists nothing like a general competence for good reasoning, there cannot be good deciders or managers in whatever area. In Gilman's mouth: "The belief that a man who has shown exceptional powers in any one direction will also show them in any other is such a beautiful theory, exposed by our political creed to slaughter by ugly facts" (DE, 774). To judge by the consequences is a principle which Gilman makes his: he only draws the consequence of the failed attempts to rule domains of knowledge without possessing this knowledge. Therefore, whoever fails in a domain cannot be said to be an expert in this domain. It explains the importance of the empirical, experiential element in Gilman's definition of the expert: someone whose long practice was successful in a domain.

It is manifestly transposed from Peirce's pragmaticist maxim, which amounts to the scientific method applied to conceptual analysis. Gilman applied it to the ruling of society in general: "True democracy is scientific method applied in politics". Peirce's pragmatism states that a possible empirical test gives to any concept its signification: "Consider what effects, that might conceivably have practical bearings, we conceive the object of our conception to have. Then, our conception of these effects is the whole of our conception of the object" (Peirce, W3.266). Gilman assumed that, applied to the problem of the relationship between superior experts and equal laypeople, it implies that equality should not be postulated as a general principle but considered in its consequences: "But whether verified or falsified, it is not the generalization itself, but the test of it, which is the sum and substance of the principle of equality". In terms very reminiscent of Peirce's pragmaticism, Gilman added that: "This is a doctrine of method, not a statement of results. It repeats in modern words the ancient injunction –'By their fruits ye shall know them'". The latter quote, extracted from Matthew's Gospel, was frequently used by both William James and Charles Peirce, so as to encapsulate the pragmatist maxim, like in the following text by Peirce:

> All pragmatists will further agree that their method of ascertaining the meanings of words and concepts is no other than that experimental method by which all the successful sciences (in which number nobody in his senses would include metaphysics) have reached the degrees of certainty that are severally proper to

> them today; this experimental method being itself nothing but a particular application of an older logical rule, 'By their fruits ye shall know them'. (Peirce, 1907, CP 5.465,)

Conclusion

Gilman's "The Day of the Expert" provides a pragmatist take on the role of experts in corporations and in society. It also sketches a sort of Peircean view on democracy, which contrasts the more famous developments by Dewey. Gilman supported a mixed participation of experts and managers in boards of directors and ruling institutions. Leaving the power to representatives of economical interests alone or to professional managers would not serve the scientific, content-related, specialized purposes of corporations. But letting experts manage would be equally inauspicious, since they would probably lack general competences of reasoning, like any other layperson. It does not mean that managers should be seen as experts in decisions, anymore than mathematicians or logicians. It has not been proved that training in theoretical reasoning improves the actual reasoning skills, and some empirical data rather seem to show they don't. Therefore, it is unlikely that education can produce experts in (good) reasoning, at least reasoning in situation, for instance when confronting moral dilemmas. To that extend, the possibility of a 'science of administration' should seriously be questioned. Yet, most of our executives were formed in such a supposedly consistent domain. In our days, excellence is sought everywhere but power tends to be transferred to non-experts, which appears as a manifest contradiction. The 'day of the expert' belongs to our past. The more the rules of economy lead the world, the more the word 'excellence' sounds like mere verbiage, dropped as solace for an insuperable loss. A possible solution to the situation could be to follow Gilman's suggestion and give a legal standing to experts, so that "the right based on capacity and the right based on law" do not conflict anymore.

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(Becoming) Experts In Meaning Ambiguities

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ABSTRACT

The discrepancy between the theoretical problems experts raise on polysemy, and the ease with which it is everyday understood by speakers, has been defined as the *polysemy paradox*. The same could be said for other forms of meaning ambiguity in the non-literal side, as for instance metaphor. A sort of *metaphor paradox* is raised by the fact that metaphor usually goes unnoticed for most people, even though experts claim that it constitutes a theoretical challenge for understanding human thought. In both polysemy and metaphor cases, people's intuitions clash with experts' intuitions. Moreover, experts seem to disagree on the very identification criterion of the linguistic phenomena. Deference to experts is anyway important in semantic applications, such as translation, where subtle distinctions in word meaning prove to be not only useful but also essential. However, the apparently wide gap between people's and experts' intuitions could be reduced once the paradoxes of meaning ambiguity are explained as a result of semantic underdetermination.

Keywords: Lexical ambiguity, semantic underdetermination, polysemy, metaphor, translation.

1. What's meaning ambiguity?

Ambiguity is pervasive in everyday language use and it can turn up in different shapes in our communicative encounters. A word is ambiguous when it has more than one meaning. The most widespread form of lexical ambiguity is *polysemy*, in which a term presents one (or more than one) literal meaning. Common words, as for instance the verb "cut", can be used in a number of different meanings. We can *cut* cloth with scissors when we separate into parts a dress, but we can also *cut* our hair when we shorten them, we can *cut* a line

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from a poem when we delete it, or we can *cut* whiskey with water when we dilute it. We describe at least four very different actions with the same word: "cut". This may sometimes be a source of confusion, but it normally raises no problem for everyday communication. As Falkum pointed out, «in normal circumstances, speakers can trust their audience to quickly and reliably figure out the meaning they intend to communicate when using a linguistic item that could take on a different meaning in a different context. On most occasions speakers and hearers are not even aware of the potential polysemy of the words they are using, and only upon reflection may they come to identify some of their other possible meanings» (Falkum 2009: 28). In fact, communication works even better if we can save our memory and use the same word, like a coin, to buy different things.

However, from a theoretical point of view, polysemy has proven to be a real challenge. Experts disagree as to how polysemy should be defined and represented in our mind: are all the different meanings of a polysemous word stored in our mental lexicon and how their relations are represented? The discrepancy between the theoretical problems which experts raise on polysemy, and the relative ease with which it is everyday produced and understood by speakers, has been described as the *polysemy paradox* (Ravin & Leacock 2000; Taylor 2003). Polysemy also poses a problem in semantic applications, such as lexicography and translation. For instance, how is a polysemous lexical item to be listed in a dictionary? This seems to be a problem just for experts, who categorize words and their meanings in dictionaries. In dictionaries, the meanings of the word "cut" are indeed classified under the same entry as literal meanings having a semantic relation. When the different literal meanings of a term have no semantic relation, we run into a rarer case of lexical ambiguity: *homonymy* (Frath 2001; Lyons 1977; Taylor 2003), whose meanings are classified in dictionaries as different entries. For example, the term *bank* is homonymous because it has two completely different literal meanings: "financial institution" and "riverside"; while the term *letter* is polysemous because it has two literal meanings, having a semantic relation: "symbol of the alphabet" and "written communication". In the case of the word *bank*, we will find two entries in dictionaries, $bank_1$ and $bank_2$, while in the case of the word *letter*, we will find just the entry *letter* with a list of meanings including "symbol of the alphabet" and "written communication".

However, in dictionaries, we can find also figurative meanings listed under a polysemous word. This is the case of *lexicalized* or *dead* metaphors, whose

frequent use has brought them to a status similar to that of polysemous, literal terms. For instance, the word *ghost* has the literal meaning "spirit of a dead person", but also the non-literal meaning "a slight or faint trace", when it is used in *a ghost of a smile* (Ervas & Gola 2013). In dictionaries, meanings such as the latest one are classified as frequent uses of language, as modulations similar to the lexical entries of polysemous terms. This is why they are called "dead" metaphors. As a linguistic phenomenon, metaphor is a transfer of a meaning from one source domain to a (usually more abstract) target domain, on the basis of some similarity between the source and target domains. In this transfer some properties are selected, while others are ignored according to some relevance criteria, which are based on the context and the conceptual frameworks of the domains involved. An example is the term *star*, which has two different meanings, the literal meaning "celestial body" and the non-literal meaning "famous actor", whose semantic fields partially overlap for some properties: being bright, unachievable, etc. As in the case of polysemy, the two meanings have a semantic relation represented by the shared properties. The shared properties are so fixed in the cultural/linguistic knowledge of native speakers, and so well-established in their mental lexicon, that they are easily grasped even when just a sentential context is given. Moreover, dead metaphors often represent so widespread a schema of properties associations that it is possible to find them in other languages and/or cultures, in exactly the same form (Bazzanella 2011; Handl 2011). For example, the English term "star" has a translation equivalent in Italian ("stella del cinema") and in French ("étoile du spectacle").

As in case of polysemy, we could talk about a *metaphor paradox*: metaphor represents an important theoretical challenge for experts, even though it is so common in language use that it usually goes unnoticed for people. Metaphor is for most people a poetic device and a matter of experts' rather than "the man in the street"'s language. Indeed, people are usually unaware of dead metaphors and do not need to know the original literal meaning to understand them. Moreover, metaphor is typically viewed as a linguistic phenomenon, as a matter of words, rather than thought or action. For all these reasons, most people usually think they can get along perfectly well without metaphors. However, according to the experts, they do not lose their impact on our thought and action (Lakoff & Johnson 1980), just because we are not aware of them or we think they are smoothly neglectable. On the contrary, experts think that metaphor is pervasive in everyday life, and that not just our language but also

our ordinary conceptual system is metaphorical in nature. Metaphors are not just a linguistic phenomenon, they are rooted in our experience and cannot be placed on the same level of any metaphorical linguistic expression in spoken or written speech. Conceptual metaphors, such as ARGUMENT IS WAR, SADNESS IS DOWN, LIFE IS A JOURNEY, TIME IS MOTION, are widespread in our way of thinking and acting. Many linguistic metaphors can be derived from such conceptual metaphors: for example, sentences such as "Your claims are *indefensible*", "Your criticisms were right on *target*" and "He *attacked* every weak point in my argument" are different manifestations of the same ARGUMENT IS WAR metaphor. Dead metaphors are so rooted in our experience that we would face difficulties in thinking and acting without them: «Imagine a culture where an argument is viewed as a dance, the participants are seen as performers, and the goal is to perform in a balanced and aesthetically pleasing way. In such a culture, people would view arguments differently, experience them differently, carry them out differently, and talk about them differently. But we would probably not view them as arguing at all: they would simply be doing something different. It would seem strange even to call what they were doing "arguing"» (Lakoff & Johnson 1980: 5).

The case of *live metaphors* - people usually have in mind - is somewhat different, because this class involves a completely new and creative use of language, not referable to a frequent (and already classified in dictionaries) use of language. Metaphors which suffer overuse, degenerate into stereotyped expressions, which is one process by which a living metaphor can expire and become lexicalized, dead. Whereas dead metaphors are not recognisable as metaphor by non-experts, live metaphor is metaphor which we are all conscious of interpreting. Live metaphors usually appear in literary contexts. For instance, in *I Have Often Met the Pain of Living* (1925), Eugenio Montale employs metaphors in the form of "objective correlatives", i.e. denotation of specific things/situations, to evoke a feeling. The well-known "pain of living" is thus evoked by "the strangled brook that gurgles", "the curling of the shriveled leaf" and "the collapsed horse". These metaphors are highly creative ways to represent the suffering of being-in-the-world in physical terms, without referring to psychological states (Bomprezzi 2014). Live metaphor comprehension requires a more demanding effort to find out the shared properties intended by the speaker and a finer knowledge of the context and its features (Glucksberg & Estes 2000; Indurkhya 2007). Therefore understanding a live metaphor depends on a very deep understanding of the

cultural-specific environment (Kövecses 2005). This is the reason why no well-established schema or patterns of shared properties are found in other languages and/or cultures (Callies & Zimmermann 2002).

2. Experts' intuitions on meaning ambiguity

Of course, meaning ambiguity is not confined to cases of polysemy and metaphor, but this is just to give an example of the most widespread forms of meaning ambiguity in both the literal and the non-literal side of everyday language use. People simply do not realize that they live by metaphors, as experts would say, or more generally by meaning ambiguities. In case doubts should arise, people usually rely on dictionaries. This would make even for the most unswerving speaker who wants to know the meanings of every single word, but it would not be enough for the experts in the field, who disagree on the very criterion of distinction among different forms of meaning ambiguity. Experts propose a number of criteria for the distinction between homonymy and polysemy, the most important ones could be considered the *etymological*, the *psychological* and the *translation criteria*. Moreover, each of these criteria run into experts' disagreement (Lyons 1977; Nerlich 2003).

According to the advocates of the *etymological criterion*, ambiguity is a mere historical accident, randomly causing a superposition of terms. On the one hand, homonymous terms such as *file*, present two meanings having different etymological roots: the French word *fil* as the origin of the linguistic form meaning "folder or box for holding loose papers" and the Old-English word *féol* as the origin of the linguistic form meaning "tool with roughened surface". On the other hand, polysemous terms such as *letter*, have meanings which share the same etymological root (Falkum 2011; Lyons 1977; Taylor 2003). While in the case of homonymy the meanings of a term, in general, do not share any property, in the case of polysemy a semantic overlap between the two meanings can be observed. The etymological criterion is a valuable tool in analysing the phenomena, however it is too relative to speakers' knowledge. For instance, the term *cardinal* has two meanings historically related: "leader of the Roman Catholic Church" and "a songbird". Experts know that the songbird inherited this name just because of its red coat, similar to the cardinal's mantle, but native speakers could ignore such a relation and the term *cardinal* could seem homonymous to them (Falkum 2009; Lyons 1977).

The *psychological criterion* precisely states that the polysemy/homonymy distinction is up to native speakers' intuitions: if native speakers judge a linguistic form as having unrelated semantic representations, then such a form is homonymous; if native speakers judge a linguistic form as having different but related semantic representations, then such a form is polysemous (Cruse 1995; Pinkal 1995). The difficulties with a complete agreement of a psychological criterion relate to the fact that it is not easy to identify the role of speakers' intuitions. According to experts, we cannot rely on speakers' intuitions, because there are no clear intuitions on 1) the "causal ancestors" of a word and 2) the "new usage" of a word (Lepore and Hawthorne 2011). For instance, for the word "dance", a linguistic community could have 1) performance standards of the dance, but also 2) an evolution of that dance, performed in different times, and 3) no agreement on what to consider as a new dance. After all, as Wittgenstein stated, this is anyway compatible with having an image of that dance: "in order to *want* to say something one must also have mastered a language; and yet it is clear that one can want to speak without speaking. Just as one can want to dance without dancing. And when we think about this, we grasp at the *image* of dancing, speaking, etc." (Wittgenstein 1953: § 338).

The psychological criterion also depends on the languages considered, as Falkum pointed out: "While the English word *open* is seen as exhibiting polysemy in "open the door" and "open the curtains", and hence the two senses are taken to be semantically related, a semantic relation between the Norwegian lexicalisations *åpne* and *trekke for* in the translations *åpne døra* ("open the door") and *trekke fra gardinene* ("open the curtains") is much harder to perceive" (Falkum 2009: 25). The *translation criterion* relies indeed precisely on the fact that ambiguity is usually not preserved in translation. As Kripke noted, «We can ask empirically whether languages are in fact found that contain distinct words expressing the allegedly distinct senses [...]. There is no reason for the ambiguity to be preserved in languages unrelated to our own» (Kripke 1979: 19). Therefore, if the translation of a term into a different language forces to choose among different translation equivalents, or if there is no one-to-one equivalence in translation (Ervas 2008), then that term is homonymous. For instance, the meanings of the English term "bark" – which denotes either the characteristic abrupt cry of a dog or the outer layer of a tree – could be disambiguated in the translation into Italian respectively with "latrato" and "corteccia". The term *bark* is indeed homonymous, as well as the

Italian term *credenza* which can be translated into Spanish with "creencia" (when the meaning is "belief") and with "aparador" (when the meaning is "piece of furniture"). However, the experts address some criticism to the translation criterion too (Zwicky & Sadock 1975). Against the claim that homonymy can be identified because it forces a choice among different translation equivalents, they point out that there are also polysemous words which are translated into different terms in other languages. Consider, for example, the word "fish", which could be translated into Spanish in either "pez" (live fish) or "pescado" (already caught fish): the term "fish" is indeed polysemous in English and other languages, such as Spanish, can codify subtle nuances of meaning not codified in English. The same could be said for some Italian polysemous words, such as "nipote", which can be translated into French by either "niéce" or "petite-fille", according to the family relationship (Ervas 2012).

According to Lakoff and Johnson, polysemy is strictly related to the conceptual network of metaphors: «the conceptual metaphor explains the systematicity of the polysemy» (Lakoff & Johnson 1980: 248). Metaphors is indeed considered one of the most important ways to give birth to new meanings (Bartsch 2002). A dead metaphor is just part of our ordinary literal vocabulary and not regarded as metaphor at all, as in case of a literal polysemy, but it can be "delexicalised" or "revitalised" (Pawelec 2006). Experts' distinction between dead and live metaphors faces indeed some difficulties, involving, in some sense, the "death" and the "resurrection" of a metaphor. There is an intermediate category, the *moribund metaphors*, which consists of expressions we use without being aware of their metaphorical nature, even though we can easily realize that they are unmistakable metaphors once we reflect on them. These metaphors are in the process of expiring, but they can be easily revived. As Grey wrote, «One notorious way in which their metaphorical character can be resuscitated is when they are used in conjunction with other metaphors, producing mixed metaphor. The conjunction of disparate metaphors is curiously prevalent in political rhetoric [...]. They are juxtapositions of ideas which might have been descriptively effective used separately but in conjunction produce an ugly result» (Grey 2000).

According to the experts, lexicalization is a necessary but not a sufficient condition for the death of metaphors, because: i) different dictionaries do not recognize the use, or ii) they could be "resurrected". The etymological

criterion has been put forth by experts in those cases as well. Terms such as *silly*, *pedigree*, or *daisy*, whose origins trace back to middle and old English, own literal meanings having a metaphoric etymological root. For instance, *daisy* is considered a corruption of the old English metaphor *dæges ēage*, "day′s eye": during the night the flower closes its petals over its yellow centre, the "eye", to unfold them again at dawn. Experts propose then *literalization* as the "real death" of a metaphor (Alm-Arvius 2003, 2006; Goatley 1997). The cases process of literalization can follow three main directions. In the case of *silly*, which is an alteration of the dialect *seely*, happy, and later *innocent*, *feeble*, the corresponding literal meaning is dead. In the case of *daisy*, a fusion of a metaphorical compound at both a phonological and a semantic level has happened. In the case of *pedigree*, whose origins are from late Middle English, from Anglo-Norman French *pé de grue* '*crane's foot*,' a mark used to denote succession in pedigrees, literalization is due to translation or linguistic loan from another language (Alm-Arvius 2006; Onions, Friedrichsen & Burchfield 1966/1994). Therefore, differences among cases are somehow flawed and seem a matter of degree. There is thus a sort of continuum from live metaphors, which are directly and transparently connected to the corresponding literal meaning, over moribund metaphors, which do not need to be interpreted in relation to their still existent source meaning, to dead metaphors, which are no longer connected with their original reading. As Alm-Arvius noted, «the difference between dead metaphors and merely moribund ones is thus that the latter retain a polysemous connection with some source contents, while this historical semantic link has been erased in dead metaphors» (Alm-Arvius 2006: 11-12).

3. Translators as intercultural experts in meaning ambiguity

Meaning ambiguity, in both polysemy and metaphor shape, also poses a problem for experts in semantic application fields, such as translation. Translation per se is already a complex process that involves many specific skills. Moreover, it is a quite common experience for translators, facing the problem of transposing meaning ambiguities into another language, to be forced to choose between two or more expressions which encode in different ways the lexical meaning of the words composing the original sentence. Sometimes, a semantic equivalence can be maintained in translation, but

sometimes it cannot (Ervas 2008). Kade's analysis of lexical equivalence in translation, given in terms of correspondence or lack of correspondence between two languages' lexical items, clearly shows that the one-to-one correspondence is just a fortuitous case. The most common translation cases are those of *facultative equivalence* (or one-to-many correspondence) and *approximative equivalence* (or one-to-part-of-one correspondence) (cf. Kade 1968. For a detailed analysis of lexical equivalence in translation, see also Bagge 1990; Hartmann 1985; Tomaszczyk 1976).

Legal translation offers a number of examples, where subtle differences in meaning can play an important role in law interpretation (Šarčević 1997; Alcaraz & Hughes 2002). Legal translation needs experts highly knowledgeable in legal terms and practices. Deference to legal translators is necessary for the translation of many things, including birth certificates, technical patents, application letters, financial statements, deposition records, litigation materials, evidence documents, and business contracts. Translators should not only possess general knowledge of legal terminology (Chromá 2004), they should also know the differences between legal systems which often entail the lack of equivalent terms (Legrand 1996). For instance, in the European case of DCFR (Draft Common Frame of Reference), it is possible to find many examples of interpretive choices translators make in order to make the target text equivalent to the source text (Ervas 2014). Legal translators sometimes need to draw out a term playing the same role in the target culture, even though differing in meaning when compared to the source term. As an example, the translation of the English term "agent" into Italian and French forces the translator to choose between a semantic equivalence, such as there would have been with "agente" in Italian and "agent" in French, and a pragmatic equivalence, such as "mandatario" in Italian and "représentant" in French, which maintain the same role of the source term.

As it has been pointed out, «instead of each term having a meaning of the sort necessary for deductive operations to go on in the first place, each term in a legal rule has a range of possible meanings, among which choices will have to be made. [...] The intellectual process of law is one of arguing and reasoning about which of them is to be preferred» (White 1982: 427). Sometimes the translator is forced to choose between a semantic equivalent and the conservation of the source term in the target language. This could happen when 1) the target language lacks an appropriate semantic equivalent (and/or the correspondent concept) or 2) when the target legal community needs to

borrow the source term (and/or the correspondent concept) for historical-political reasons. In the first case, the lack of an appropriate semantic equivalent entails a failure in translation, which might be highlighted by the fact that the translator is forced to add a footnote. The footnote is often used to explain the reasons why the translator decided to choose one translation instead of another, or why there is no "perfect" semantic equivalence between the source and the target words. Sometimes, as in the case of locution "Interpretation and development", the translator is forced to leave a gap in translation because a semantic equivalent of the English term "development" does not make sense in the translation into Italian, or the translator needs a paraphrase, such as "comblement des lacunes" in the translation into French, to avoid a complete failure of translation. In the second case, the translator needs to resort to calque or loan translation. As an example, "trust" itself is the translation of the word "trust" into Italian, because not only the English word "trust", but also the concept of trust is shared along different legal communities. In other cases, as for the French translation of "trust" with "fiducie", the legal tradition of the target community weighs on the translation, thus avoiding the loan translation or the concept use in another, historically opposed legal tradition (Kocbek 2008).

For most people failures in translation could seem just a problem for experts, whose occupation is grabbing polysemy nuances of a language onto the web of meaning ambiguities of another language. However, failures in legal translation could cause a huge loss to people. For instance, the concept of *compensation* cannot be understood without considering the concept of *charges*: «Therefore it must be determined whether the broader and related concept of *charges* includes *compensation for use*. If interpreted broadly, *charges* would include the latter, and the consumer might be asked to pay» (Pasa & Morra: 7). Cases like that would make people - and not just experts - sensitive to translation problems! People usually think that translation problems arise in literary texts: that is quite true, even though meaning ambiguities might cause practical - and more serious - consequences in other translation fields, as exactly in legal translation. This does not mean that literary translation does not deserve problems for translators, whose creativity is put on probation not only (and not so much) by polysemy, but rather by live metaphors. In translation, lexicalized metaphors could have a behaviour very similar to polysemy, probably because they share very similar conceptual frameworks in source and target languages. Many polysemies, for example,

may be translated using a corresponding polysemy in the target language, because both words are ambiguous in a similar way in the source and target language. For example the Italian term "appendice" might refer to both the last part of a book or to a body part and can be translated into English with the word "appendix" in both cases. In the same way, lexicalized metaphor might be fully translatable: for instance, "quadretto" in Italian can be translated into English with the term "picture", preserving both the literal ("little picture") and the metaphorical senses ("family") (Ervas & Gola 2013). Sometimes, as in case of polysemy, translators might not find an equivalent metaphor in the target culture and they are therefore forced to find a pragmatic equivalent, i.e. a different metaphor having the same function in the target language. For example, the Italian term "abbozzo", used in the dead metaphor "abbozzo di un sorriso", could be translated into English with the term "ghost" in the lexicalized metaphor "ghost of a smile".

Live metaphors pose instead more serious problems in literary translation. In Paul Valéry's poem *Le cimitière marin*, the live metaphor of the roof as the sea works because in Paris the roofs have a blue-slate colour under the sun, but the metaphor is not easily translatable in another context where the roof are imagined as red-coloured (Eco 2003). In such a case cultural-contextual cues could entail a failure in translation. In such cases, the translator has to resort to alternative strategies such as paraphrases, similes or completely new and creative metaphors. An example is represented by the spider as the metaphor of a man who captured a fly (a woman) in its cobweb in Paola Capriolo's *La grande Eulalia* (1988). The translation risks to loose exactly the image of human relationships evoked by the metaphor of the spider/fly. For instance, the feminine Italian term "mosca" ("fly") is translated into French with the feminine term "mouche", but also the masculine Italian term "ragno" ("spider") is translated with a feminine term, "araignée". Therefore the figurative man/woman relationship is lost in translation. The same problem is involved in the translation into German, where both the term "Spinne" ("spider") and "Fliege" ("fly") are feminine (Capriolo 2002). Another example is Eugenio Montale's translation into Italian of Emily Dickinson's *The storm* (1896), where the "Emerald Ghost" is the metaphor of wind having the green colour of a snake whose shiver provoked the movement of the grass on the earth. The metaphor contains a net of semantic associations and phonetic features which are not easily translatable into Italian. Montale decided to lose

part of the semantic content to maintain the same rhythm of the original metaphor in order to create a new poetic image in the target culture.

4. How to "become" experts in meaning ambiguities

In all the examples considered up to now, the perceived difference between the original and the alternative translations is the result of a change in the degree of explicitness in translation. What is crucial to translation is the fact that languages differ in the strategies used to make meaning explicit. This problem is rooted in the well-known "semantic underdetermination" phenomenon: when people use language they encode semantic representations which are just partial representations of their thoughts (Carston 2002). These semantic representations constitute a "guide" for the thoughts that the interpreter must recover to grasp the communicated thought. In Carston's words: «the linguistic semantics of the utterance, that is, the meaning encoded in the linguistic expressions used, the relatively stable meanings in a linguistic system, meanings which are widely shared across a community of users of the system, underdetermines the propositions expressed (what is said). The hearer has to undertake a pragmatic inference in order to work out not only what the speaker is implicating but also what proposition she is directly expressing» (Carston 2002: 19-20). One language may be equipped to encode very subtle nuances by means of specific linguistic devices, whilst another language may commonly express equivalent nuances by linguistic devices which encode very vague semantic constraints on the interpretation. This forces translators to resort to a sort of "enrichment" of the source text in the target text order to derive the original intended meaning. That is why there can be failures in translation: before the translator recovers fully determinate thoughts, she must engage in a process of development of the logical form which can be represented in different ways in different languages. As Wilson and Sperber comment: «although the logical form of an utterance is recovered by decoding, its fully propositional form is obtained by inferential enrichment of the linguistically encoded logical form. It is the propositional form of an utterance, not its logical form, that determines the proposition expressed» (Wilson & Sperber 1993: 6).

The process of enrichment, therefore, involves a completion of the logical form (i.e. the semantic representation encoded by the utterance). Enrichment

draws information, not only from the original sentence, but also from the context, to go from semantic representations to fully developed propositions (Rosales Sequeiros 2002). The translator may also choose to enrich the original text on some other grounds, as for instance by her own knowledge and expertise in a specific field. Some examples discussed above show that some interlingual enrichments are required on linguistic grounds. This is due to the way languages typically express some meanings. The consequence of not carrying out the enrichment in these conditions gives rise to failures in translation. Thus, the target language forces the translator to explicitly encode a meaning which was only implicit in the (semantic representation of the) original text. However other examples show that enrichment, in turn, may be due to a choice of the translator on some other grounds, i.e. a wider context, as for instance the cultural context and its differences from the original one. Culture, in simple terms, is viewed here as a set of assumptions shared by a given community. Shared assumptions are expected to be easily accessible and retrievable by members across that community, creating a shared cognitive environment (cf. Sperber & Wilson 1986: 38-46; Scollon & Scollon 1995: ch. 7).

From this perspective, even non-experts might realize that context is the key to understand both polysemy and metaphor in everyday language use. Thus the difference between homonymy and polysemy is based on different pragmatic processes which rely on the distinction between *narrow* and *broad contexts* (Bach 2012; Carston 2002; Perry 1997, 2001; Recanati 2004). In case of homonymy, the selection of the relevant meaning works by default on the basis of the pre-semantic context, or the *narrow context*. In case of polysemy, the selection of the relevant meaning involves a process of pragmatic enrichment on the basis of the post-semantic context, or the *broad context*. In a narrow, sentential context using both the meanings of a homonymous term, such as "bank", an anaphora would have the effect that "something does not work" not only for experts, but for most people. For instance, the sentence "He put some money in a bank and then he swam to it" puts together unrelated semantic fields and at best it could be interpreted as a joke referring to completely different readings of the term. On the contrary, a polysemous term such as "window" might be used via anaphora and might be read in both its meanings ("window of a house" and "window on the computer screen") preserving the impression that the overall sentence works in both cases. For instance, the sentence "He opened the window and then went through it", a

broader context is required to understand which meaning of "window" is used, otherwise both readings would be equally possible (Frazier and Rayner 1990; Garrod, Freudenthal and Boyle 1994).

In polysemy, indeed, the word contributes to an indefinite number of other meanings, which are the results of the enrichment process (Recanati 2004, 2010). There is a clear gap between what is literally expressible and what speakers may need to express, between the encoded concepts and the intended ones (Carston 2002; Wilson & Carston 2007; Hirst 1987). Enrichment is a pragmatic process that fills this gap by pragmatically inferring the intended ("ad hoc") concepts on the basis of the encoded concepts «in response to specific expectations of relevance raised in specific contexts» (Carston 2002: 322). The adjustment producing the "ad hoc" concepts, consists of narrowing or broadening the encoded concepts (on the nature of "ad hoc" concepts, see Allott & Textor 2012). In the case of narrowing, the semantic field of the encoded concept is reduced to a sub-set, as in the sentence "I do not like to drink when I have to work", where "drink" means "drink alcohol". In the case of broadening, the semantic field of the encoded concept is enlarged to a super-set, as in the sentence "This guy is crazy", where "crazy" does not mean that the guy has a psychiatric disease, but rather that he looks "strange".

This explanation of the explicit meaning of sentences challenges the traditional distinction between literal and nonliteral uses of language, as what is considered "literal" is the result of a pragmatic process of modulation (Carston 1997, 2002). Literal and nonliteral uses of language are just different solutions to the same problem: understanding in each communicative encounter and for each exchanged message, which its more relevant interpretation is, i.e. the interpretation optimizing the costs/benefits relationship between processing effort and cognitive effect. Therefore, in a relevance perspective, the dichotomy between literal and nonliteral uses of language is just an experts' invention. There is instead a "continuum" between literal and nonliteral language, metaphor included. Appealing to this "unified approach" to literal and nonliteral uses of language, Carston explained the case of metaphor interpretation in a way similar to the polysemy case: as in polysemy, metaphor interpretation is an enrichment process whose result is an "ad hoc" concept (Carston 2002, 2010; Vega Moreno 2004). For instance, the sentence "Leonardo is an angel" contains a metaphor which is not to be understood as if Leonardo were immaterial, had wings and feathers, etc. on the basis of the lexicalized concept of ANGEL. The interpreter builds an alternative

concept, the "ad hoc" concept ANGEL*, according to which Leonardo shares other properties with an angel, as for instance the properties of being good, calm, quiet, etc. Dead metaphors are therefore interpreted via a local pragmatic process of enrichment of the lexicalized concept, resulting in an "ad hoc" concept. In the case of live metaphors, the literal meaning would just be maintained in a more global pragmatic process resulting in a range of communicated affective and imagistic effects (Carston 2010; Carston & Wearing 2011).

In this perspective, an on-line pragmatic adjustment of the encoded lexical meaning is required in both polysemy and metaphor cases, on the basis of speakers' encyclopaedic knowledge, the available contextual cues and the mutually shared cultural environment. Such an interpretative process is not a prerogative of experts, but it is the common way people solve meaning ambiguities in both their literal and nonliteral shapes. The experts/non-experts divide remains, but it has been tone down by the fact that there are no substantial differences to be investigated among linguistic phenomena, all being part of the same literal/nonliteral spectrum. The polysemy and metaphor paradoxes are no longer such, if we think that both the phenomena are unawarely handled by people exactly because they shared a similar pragmatic-contextual cognitive process able to derive the intended meaning. Paying attention to the context and to interlocutor's intention, everyone can grasp the communicated meaning in case of meaning ambiguities. Experts still holds the ability to make this process more explicit by exploiting their knowledge and competences in a specific context, but the difference with "the man of the street" is just a matter of degree.

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