

# 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 overthrow 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 21<sup>st</sup> Century are markedly different our scepticism over “expertise” appears to be fairly similar. The 2<sup>nd</sup> and 3<sup>rd</sup> 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.<sup>1</sup>

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

<sup>1</sup>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 (Kuhn 1962; 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*<sup>2</sup> 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*

<sup>2</sup> 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.<sup>3</sup>

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,

<sup>3</sup> 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 is 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”.<sup>4</sup> 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

<sup>4</sup> 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, equations, 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.<sup>5</sup> 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

<sup>5</sup> 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.<sup>6</sup> 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.

<sup>6</sup> 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.<sup>7</sup> 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.<sup>8</sup> 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

<sup>7</sup> Many of these arguments are presented in Behe’s (1998) *Darwin’s Blackbox*.

<sup>8</sup> 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.<sup>9</sup> 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

<sup>9</sup> The relationship between history, metaphysics and science is discussed more generally by Agassi (1975).

success of the dominant paradigm it seeks to replace.<sup>10</sup>

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 1<sup>st</sup> 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.<sup>11</sup>

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

<sup>10</sup> 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).

<sup>11</sup> “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 up. 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 17<sup>th</sup> 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 17<sup>th</sup> 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.<sup>12</sup> 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

<sup>12</sup> 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 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.<sup>13</sup> 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 were 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

<sup>13</sup> 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.<sup>14</sup> 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

<sup>14</sup> 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.<sup>15</sup> 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.

<sup>15</sup> 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 preceded 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.<sup>16</sup> 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*

<sup>16</sup> 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 States 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 we cannot rely on a methodological criterion of expertise to determine who one should listen to, 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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