The Pseudo-Demarcation of 'Pseudoscience'

James T. Molesworth

B.A. (Hons.)

A thesis submitted for the degree of Masters of Philosophy at

Monash University in (2018)

Philosophy Doctorate Program

Copyright notice

Notice 1

Notice 2

I certify that I have made all reasonable efforts to secure copyright permissions for third-party content included in this thesis and have not knowingly added copyright content to my work without the owner's permission.

Abstract

The 'traditional' demarcation problem - the endeavor to devise some universal method for distinguishing true science from 'pseudoscience' - was first introduced in 1934. In this thesis, I establish both that none of the many subsequently proposed solutions to this problem are successful, and that no practically applicable solution to the 'traditional' formulation of, and approach to, the problem, is possible. However, rather than constituting a rejection of the demarcation problem and the objectives that render it significant, this conclusion indicates a new path to its resolution. Numerous important points of 'demarcation' should be recognized, referring to the degree of epistemic support required for a scientific proposition to be appropriately accepted for consideration in various practical contexts. Such 'demarcation' is directed towards distinguishing degrees of 'good science' from inferior contributions, rather than demonstrating what is 'not science at all'. Relevant determinations of epistemic merit must be established through standard scientific, rather than philosophical, arguments, focusing on evidential and methodological factors, and the term 'pseudoscience' should be abandoned as both unjustifiable and largely irrelevant.

Declaration

This thesis contains no material which has been accepted for the award of any other degree or diploma at any university or equivalent institution and that, to the best of my knowledge and belief, this thesis contains no material previously published or written by another person, except where due reference is made in the text of the thesis.

Signature: 

James Timothy Molesworth

Date: 14/07/2018

Acknowledgements

On a personal note, I must of course first acknowledge the assistance and patience of my family: my parents, Lee and Carolyn; my wife, Grace; and my children – Jacob, Elijah, Mykaela, and Isaiah. I would also like to particularly thank my brother Stephen for his extraordinary dedication to ensuring that this thesis was completed, and the assistance he provided to help me attain that objective.

To the late Homer Le Grand I am grateful for first introducing me to the Philosophy of Science, and for his stimulating advice in the earliest stages of this project. I must also thank Dirk Baltzly for helping me to recognize that I had to follow my arguments where they pointed, rather than vainly wrestling them towards the conclusion I initially intended.

Finally, and foremost, I must thank my longsuffering supervisor Graham Oppy, whose continuing encouragement and assistance was an invaluable constant throughout this process. For his boundless patience and seemingly unlimited capacity to listen to me rehearsing my sometimes nebulous and often lengthy arguments, and helping me to formulate from such beginnings a coherent position, I am inexpressibly grateful.

Dedicated to the Memory of

Homer Eugene Le Grand

3 September 1944 - 16 January 2017

Table of Contents:

1) Preliminaries - pg. 9
1.1) Introduction: The Demarcation Problem - pg. 9
1.2) Desiderata - pg. 11
2) Critical Assessment of Demarcation Proposals - pg. 17
2.1) Philosophers - pg. 17
2.1.i) Logical Empiricists - pg. 17
2.1.ii) Popper - pg. 17
2.1.iii) Contra Creationism: Ruse, Pennock, Gross - pg. 20
2.1.iv) Lakatos – pg. 23
2.1.v) Rothbart – pg. 25
2.1.vi) Lugg 1 – pg. 27
2.1.vii) Lugg 2 – pg. 29
2.1.viii) Derksen – pg. 32
2.1.ix) Pennock – pg. 34
2.1.x) Methodological Naturalism – pg. 37
2.2) Debunkers – pg. 41
2.2.i) Williams [Mousseau] – pg. 41
2.2.ii) Gardner 1 – pg. 43
2.2.iii) Gardner 2 – pg. 43
2.3) Recent Contributions: 2013 – pg. 45
2.3.i) Pigliucci & Boudry – pg. 45
2.3.ii) Pigliucci – pg. 47
2.3.iii) Mahner – pg. 49
2.3.iv) Ladyman – pg. 52
2.3.v) Hansson – pg. 53
2.3.vi) Boudry – pg. 55
2.3.vii) 2013 Contributions: Collectively – pg. 56
3) Against 'Demarcating Pseudoscience' 1: Guiding Belief – pg. 58
3.1) Demarcating 'Out of Hand' – pg. 58
3.2) Objectives, Units, Means – pg. 60
4) Against 'Demarcating Pseudoscience' 2: Examples – pg. 67
4.1) 'Pathological Science' – pg. 67
4.1.i) Cold Fusion – pg. 67
4.1.ii) Polywater – pg. 68
4.1.iii) 'Pathological Science' or 'Pseudoscience'? – pg. 68
4.2) A Paradigmatic 'Pseudoscience': Hynek's Ufology – pg. 70
4.2.i) 'The UFO Experience' – pg. 71
4.2.ii) The 'Condon Report' – pg. 74
4.2.iii) Ufology and 'Pathological Science' – pg. 75
4.2.iv) Ufology and Demarcation – pg. 77
4.3) Beyond Ufology: Addressing the Best Arguments – pg. 78
5) The Irrelevance of 'Pseudoscience' – pg. 81
6) Why Demarcation Looks Easy – pg. 83
6.1) Misrepresenting Science – pg. 83
6.2) Misrepresenting 'Pseudoscience': Debunkers on Ufology – pg. 83

6.2.i) Wynn & Wiggins – pg. 84
6.2.ii) Gardner – pg. 86
6.2.iii) Pigliucci – pg. 86
6.2.iv) Williams – pg. 92
6.2.v) Plait – pg. 93
7) Implications and Recommendations – pg. 95
7.1) Meaningful 'Demarcation' Possible – pg. 95
7.2) Demarcation as a Scientific Process – pg. 95
7.3) Demarcation as Disqualification Unsuccessful – pg. 96
7.3.i) Criticisms of Organized 'Skepticism' – pg. 96
7.3.ii) Partisan Disputes – pg. 98
7.4) The Benefits of Academic Demarcation – pg. 98
7.5) Relevant Philosophical Projects – pg. 99
8) Conclusion – pg. 100

Bibliography – pg. 101

1) Preliminaries

In this thesis, I will demonstrate that the standard, 'traditional' approach to the Demarcation Problem involves conceptual flaws that produce insurmountable obstacles, preventing the attainment of the objectives from which the problem derives its significance. Having introduced the problem, I shall first establish a set of desiderata for any successful demarcation method. The latter will then be employed in a detailed examination of numerous extant demarcation proposals intended to reveal not only their individual flaws, but also the nature of the 'traditional' approach and the major obstacles that confront such efforts. Subsequent discussion of the key issues highlighted in the foregoing will then clearly establish that no useful solution to the Demarcation Problem is possible without a significant revision of 'traditional' assumptions and expectations. The same conclusion will also be supported through the examination of a specific, 'paradigmatic' example of 'pseudoscience' – Ufology - and how it compares to sub-standard examples of science, referred to as 'pathological science'. However, as will be shown in the concluding sections, the guiding objectives of the Demarcation Problem can still be attained, and the difficulties that have until now prevented any successful resolution overcome, through the adoption of an appropriately revised, common sense approach employing standard scientific processes and conclusions.

1.1) Introduction: The Demarcation Problem

In the 1930's Karl Popper initiated a new discussion of what he called 'the problem of demarcation': "The problem of finding a criterion which would enable us to distinguish between the empirical sciences on the one hand, and mathematics and logic as well as 'metaphysical' systems on the other…" (Popper 1935, pg. 11). This problem was certainly not entirely new, and Popper examined how similar issues had been addressed by Kant, Hume, and the Logical Positivists (Popper 1935, pp. 11ff), but, for whatever reason, Popper's specific formulation, and his assertion that demarcation should be regarded as amongst the most important problems within philosophy of science, took firm hold in philosophical circles (Rothbart 1990, pp. 112-113; Pigliucci 2013, pg. 9; Hansson 2017; Shea 2017; Godfrey-Smith 2003, pp. 57-58; Thagard 1988, pg. 157; Van Rillaer 1991, pg. 235; Edmond & Mercer 2002; Pigliucci & Boudry 2013b, pg. 9). Popper's proposed solution – that all scientific hypotheses must be 'falsifiable'[1] – was ultimately rejected[2] by the great majority of philosophers, as even his supporters admit (Garcia 2006, pp. 1-2, 50ff), but that only ensured many philosophers of science would thereafter endeavor to devise some successful demarcation method of their own. Importantly, although demarcation can be applied, as Popper indicated, to distinguishing science from other, respectable endeavors, such as mathematics, the most important objective quickly became to demarcate science from 'pseudoscience' in particular, an objective which carried with it the firm conviction that the latter is always epistemically inferior to the former.[3] Thus the major focus of demarcation came to be distinguishing science from 'nonsense' (Gillies 1993, pp. 155-156; Pigliucci 2013, pg. 26; Lugg 1992, pg. 91; Godfrey-Smith 2003,

[1] This will be explicated below.

[2] At least in the form Popper proposed.

[3] This was also the primary focus for Popper himself, who explained that he had come up with the criterion of 'falsifiability' through comparing the arguments of Freud, Adler, and Marx with those of Einstein, but did not continue his analysis, in that initial instance where it seems his position was decided, to include as well those more respectable non-scientific fields he claimed demarcation must also address (Popper 1962, pp. 33ff.). Thus the emphasis was, from the beginning, the differentiation of well-supported claims from ill-supported claims (Hansson 2013, pp. 71-72; Godfrey-Smith 2003, pg. 58; Gillies 1993, pp. 155-156). As will be argued below, this basic approach – comparing unquestionably good science with manifestly ill-supported propositions - has since been adopted by many commentators, despite being, at the very least, ill-suited to the stated objective, as will be discussed below.

pg. 58), and the problem was approached as "…part of the larger task of determining which beliefs are epistemically warranted" (Hansson 2017).

Popper's presentation and proposed solution together formed a model establishing how philosophers would subsequently address the Demarcation Problem.[4] Accordingly, there is discernable within the vast demarcation literature an agreed basic approach and set of shared assumptions, which constitute what I refer to in this paper as the 'traditional' conception of the demarcation problem. The latter can be described, in short, as the endeavor to identify certain criteria or characteristics which universally differentiate all examples of science from all examples of 'pseudoscience', which latter is thereby to be justifiably excluded from scientific discourse. Alternatively or in parallel, demarcation is regarded as the proper positioning of any hypothesis upon a continuum, measuring scientific merit via the above mentioned criteria, which runs from firmly established 'science fact' at one end, through good science and bad science, to 'pseudoscience' at the opposite extreme, which latter is again to be excluded from scientific discourse because it is not, in fact, 'science' at all.[5]

The perceived importance of demarcating science from 'pseudoscience' arises primarily from its intended practical applications. "From a practical point of view, the distinction is important for *decision guidance in both private and public life*. Since science is our most reliable source of knowledge in a wide variety of areas, we need to distinguish scientific knowledge from its look-alikes. Due to the high status of science in present-day society, attempts to exaggerate the scientific status of various claims, teachings, and products are common enough to make the demarcation issue pressing in many areas" [emphasis added] (Hansson 2017). Philosophers have expressed expectations that demarcation will: improve scientists' productivity by determining which claims can justifiably be dismissed without detailed assessment (Rothbart 1990 pp. 113ff.; Wilson 2000, pg. 2; Fuller 1985, pg. 331); provide guidance to judges and juries in admitting and assessing expert testimony; assist policy makers in making science based decisions; provide essential criteria for educators; inform the public at large as to which claims should be legitimately afforded the respect due to genuine science, and so generally reduce irrationality, as well as having valuable theoretical applications (Pigliucci and Boudry 2013, pp. 2-3; Hansson 2017; Ladyman 2013, pp. 49-50, 52; Mahner 2013, pp. 35-36; Derksen 1993, pg. 19). Demarcation is also an essential means for the scientific community to preserve its intellectual authority by excluding individuals, claims, or fields that are judged to be, at minimum, sub-standard, and thus potentially damaging to science's credibility as a source of knowledge (Still & Dryden 2004; Gieryn 1983).

However, despite the endeavors of many philosophers and scientists over the course of some eight decades, no demarcation proposal has achieved anything approaching broad acceptance (Laudan 1996a, pg. 211; Mahner 2013, pp. 29-30; Hansson 2009, pp. 237-238; Lugg 1987, pp.222-224; Mousseau 271-272; Rothbart 1990, pg. 111; Ladyman 2013, pg. 50). Of course, widespread acceptance is generally not nearly as important to philosophers as it is to scientists. In this case, however, the intended practical applications render it highly desirable, if not essential. Demarcation must be undertaken in a context of dispute with the advocates of 'pseudosciences', who are bound to utilize any arguments critical of a demarcation proposal employed against their claims. This is illustrated by Creationists' responses to demarcation arguments, employed in legal cases, to establish that 'Creation Science' is not truly science at all. Creationists appealed to arguments by philosophers critical of the methods employed to dismiss the court's findings (Pennock 2011, *passim*). Thus, despite effectively universal agreement[6] among

[4] Popper's continuing influence is reflected in the extreme frequency with which papers on the topic of demarcation discuss his contribution. For example, the index of the 2013 publication *Philosophy of Pseudoscience: Reconsidering the Demarcation Problem* (Pigliucci & Boudry 2013a) lists 25 separate entries for Popper, representing 57 pages – considerably more than any other individual.

[5] These characterizations are supported in the numerous demarcation proposals discussed below (see also Lugg 1987, pg. 223; Hansson 2009, pp. 237-8; Mousseau 2003, pg. 271; Okasha 2002, pg. 16)

[6] Although the degree and significance of agreement among demarcators is sometimes overstated, as discussed below.

demarcators that 'Creation Science' and numerous other 'paradigmatic examples' of the class *are* indeed 'pseudoscience', and the ongoing application of that label by debunkers, such commentators possess no suitably uncontroversial method to justify their conclusions.

Moreover, the problem is not merely to determine which criteria can best achieve the demarcators' objectives. In 1983, Larry Laudan famously[7] argued that the demarcation problem, in the form I am referring to as 'traditional', is in fact an insoluble 'pseudo-problem' (Laudan 1996a[8], pp. 210-222). Laudan discussed the abject failure of all attempts to that date, noting that they all fell afoul of various counter examples contradicting the claimed significance of the demarcation criteria proposed (Laudan 1996a, pp. 218-221). However, his argument was centered on what he saw as the underlying reason for the unavoidability of such occurrences, arising from the requirement that any philosophically interesting demarcation method must establish the epistemic superiority of what it determines to be 'science' (Laudan 1996a, pg. 216).

Laudan argued that demarcators have thereby "…managed to conflate two quite distinct questions: What makes a belief well founded (or heuristically fertile)? And what makes a belief scientific?" (Laudan 1996a, pg. 222). Of the epistemic quality of what we call 'science', Laudan writes: "Many, perhaps most, parts of science are highly speculative... There seems good reason, given the historical record, to suppose that most scientific theories are false; under the circumstances, how plausible can be the claim that science is the repository of all and only reliable or well confirmed theories?". "…We have learned enough about what passes for science in our culture to be able to say quite confidently that it is not all cut from the same epistemic cloth". He thus asserts: "However we eventually settle the question of reliable knowledge, the class of statements falling under that rubric will exclude much that is generally considered 'scientific'" (Laudan 1996a, pp. 221-222). It is on this basis that he then concludes: "*The evident epistemic heterogeneity of the activities and beliefs customarily regarded as scientific should alert us to the probable futility of seeking an epistemic version of a demarcation criterion*. Where, even after detailed analysis, there appear to be no epistemic invariants, one is well advised not to take their existence for granted. But to say as much is in effect to say that the problem of demarcation… is spurious, for that problem presupposes the existence of just such invariants" (Laudan 1996a, pg. 221)

According to some demarcators, Laudan's paper was highly influential, and led to many philosophers accepting that demarcation was unattainable (Pigliucci 2013, pp. 9-10; Hansson 2017; Meyer 2000, pp. 19ff; Pigliucci & Boudry 2013b, pp. 1-2; Mahner 2013, pp. 29-30; Boudry 2013, pp. 79-80, 82). The relevant literature, however, does not really reflect this. There have subsequently been many published attempts to devise successful demarcation methods, regardless of Laudan's objections, and Laudan's paper is generally only cited by those opposing his conclusions.[9]

I believe that Laudan was essentially correct, and to some extent this paper can be regarded as supporting and expanding his arguments. I will also defend his claims against some of the criticisms advanced by demarcators. However, as will become clear, I am not primarily focused on developing the specific arguments Laudan employed, but will rather advance distinct (though related) arguments supporting the same core conclusions.

1.2) Desiderata

I will begin, as Laudan did, by first discussing the requirements that demarcation arguments must meet in order to be deemed successful. The suggested requirements that follow are in some cases

[7] At least among those interested in demarcation.
[8] Originally published in 1983 – Laudan, Larry. (1983). 'The Demise of the Demarcation Problem', *Physics, Philosophy, and Psychoanalysis*, Springer Publishing, Dordrecht.
[9] Numerous examples appear below.

controversial or negotiable, whilst others are clearly essential. I will not conclusively differentiate the two at this point, leaving that to be determined as we proceed or through later discussion. Thus I shall present these for the time being as simply 'desiderata'. For this section I have drawn significantly from Laudan's discussion of the same issue (Laudan 1996a, pp. 215-18).

To begin on a point of disagreement, Laudan holds that demarcation criteria should "Ideally… specify a set of individually necessary and jointly sufficient conditions", on the basis that necessary conditions supply no means of establishing what *is* science, whilst sufficient conditions alone cannot determine what is *not* science (Laudan 1996a, pp. 216-217). However, whilst necessary and sufficient conditions would certainly be 'ideal', it is uncommon for commentators to discuss sufficient conditions for science at all, and it does not seem to me that such are, in practice, strictly necessary. An argument intended to establish that a given proposition is 'pseudoscience' *could* succeed on the basis of sufficient criteria for 'pseudoscience' alone (whether individually sufficient, jointly sufficient, sufficient in certain combinations, or some mixture). Obviously, then, necessary conditions for science alone could equally well prove successful. Further, although characterizations of 'pseudoscience' should be universally applicable, a gradual collection of sufficient conditions (or their equivalent), each applicable to a particular sub-class of 'pseudoscience', is certainly a worthwhile project, and this will be reflected in my analysis.

A second condition raised by Laudan is that the outcomes of any demarcation process should "…at least in part…" match our current "…patterns of usage…" regarding the terms 'science' and 'pseudoscience' (Laudan 1996a, 215-216). This reflects the fact that, despite the current lack of any agreed demarcation method, there exists a list of widely accepted, 'paradigmatic' examples of 'pseudoscience', and similar agreement exists regarding which fields, claims, or hypotheses clearly count as 'science' (Shermer 2013, pg. 206; Pigliucci & Boudry 2013b, pg. 2; Hansson 2013, pg. 61; Rothbart 1990, pg. 111; Nickles 2013, pg. 114; Pigliucci 2013, pg. 24; Ladyman 2013, pg. 49; Ruse 2013, pp. 226, 228; Wynn & Wiggins 2001, pp. 1-2). As Laudan asserts, any demarcation method that does not broadly reflect these prior 'intuitive' determinations will almost certainly be rejected as failing to truly demarcate 'science' from 'pseudoscience' as those terms are employed and understood. There is, therefore, a need for ***Reflective Equilibrium*** - proposed demarcation methods are required to provide theoretical justification for existing, 'intuitive' demarcation determinations.

One significant aspect of 'current usage' that demarcation should therefore ideally reflect is the overwhelming tendency of commentators to demarcate topics and/or 'fields', rather than, for example, publications or individual propositions. Typical examples of 'pseudoscience', provided by both philosophers and scientists discussing demarcation generally, and 'debunkers' advancing specific demarcation claims, are: Ufology, Astrology, Creationism, Divination, Dowsing, Homeopathy, etc. (Shermer 2013, pg. 206; Thurs and Numbers 2013, pg. 138; Goode 2013, pg. 146; Wynn and Wiggins 2001, pg. 2; Mahner 2013, pp. 29, 30-31; Pigliucci 2013, pg. 17). If demarcation proposals cannot justify demarcating this class of target, there will exist a clear mismatch between the great majority of existing demarcation claims and the theoretical basis being advocated for them, which would obviously be problematic.

Simplicity is also an important requirement for any practically useful demarcation method. As discussed above, a major objective in resolving the Demarcation Problem is to inform the decisions of the judiciary, educators, policy makers, and the like, and to assist the general public in regard to the core issue of recognizing false claims to scientific authority. This requires that laypeople understand and accept any demarcation process, as it is their application of it that is sought. It is accordingly highly desirable that laypeople be able to competently employ any proposed method for themselves (Ladyman 2013, pp. 49-50, 52; Pigliucci 2010, pg. 2; Mahner 2013, pp. 35-36; Derksen 1993, pg. 19; Hansson 2017; Gardner 1952, pg. 7). For this to be possible, the criteria must obviously be relatively simple to apply, and how much knowledge is required to do so – the extent to which a layperson must become an expert - will be a crucial consideration. It is reasonable to expect that expert input would often be necessary to at least

outline key points and provide relevant data: to in effect outline the argument for demarcation. The lay-audience must then not only comprehend and properly appreciate the significance of such arguments, but also be able to assess their veracity relative to the counter-claims of proponents of targeted 'pseudosciences'. It is not difficult to see how, with any but the most straightforward arguments, such a debate could readily pass beyond the knowledge base of a non-expert audience. To the extent that, should this occur, laypeople would subsequently be forced to rely upon the determinations of experts, the key issue would *no longer be the demarcation of science and 'pseudoscience'*, but rather the social epistemological problem of establishing when a lay-audience is justified in accepting the contested assertions of any 'expert', in circumstances where the relevant role of scientific institutions, too, would be challenged (Gordin 2012, pp. 3-4). To what extent a successful method of demarcation would then be of any appreciable value is uncertain: the expert need only communicate that a claim should not be accepted, to which end scientific refutation is presumably sufficient, and thus demarcation and the term 'pseudoscience' not essential.

Precision*:* "…that we can know whether something does or does not meet the conditions" (Laudan 1996a, pg. 216), is likewise essential if demarcation is to be of genuine use in achieving its stated objectives. To the extent that some level of uncertainty may prove unavoidable, the degree to which this limits the practical capacity of a given method will be a major factor in determining whether it can possibly lead to attainment of the objectives that render demarcation of value. A particular focus of such assessments must be the outcomes attainable in regard to the most popular 'pseudosciences'. Further, the use of overly subjective criteria would invite arguments that may not only confound laypeople, but could prevent even experts from arriving at or clearly communicating demonstrably justifiable conclusions.

Laudan further suggests that demarcation arguments need to be 'especially compelling' (Laudan 1996a, pp. 217-18). Certainly, demarcation is considered necessary in part because 'pseudoscientific' claims can appear authoritative or otherwise convincing, and thus many people are resultantly inclined to entertain or accept them. As a result, the rationale behind demarcation determinations needs to be at least equally convincing, and it is an obvious objective that demarcation arguments should advance the strongest case possible. Demarcation arguments should therefore be assessed regarding their ***Strength*** relative to other arguments that could be directed towards the same objectives. Their capacity to convince those laypersons to which they must be addressed should be measured against the effectiveness of obvious alternatives to do the same - in particular Laudan's proposal to employ more direct empirical assessments, without regard for scientific status (Laudan 1996a, pp. 221-222).

Because I shall be assuming a 'realist' understanding of science throughout this paper, establishing the ***Epistemic Superiority*** of genuine science will be treated as an indispensable component of any demarcation program (See Laudan 1996a, pg. 216). Naturally, this immediately rules out demarcating on *purely* sociological grounds, and accordingly, proposals of this type will not be included for consideration below. This is not to suggest, however, that sociological factors are not of considerable significance within science generally, or that they may not play some important role in demarcation specifically: for example, as *indicators* of epistemic credentials.

Obviously the *raison d'etre* of any 'traditional' demarcation mechanism is to furnish ***Justification*** for its defining objective – the '*demarcation*' of '*pseudoscience*'. 'Demarcation' refers to delimitation: the fixing of a boundary or limits.[10] Thus it is an elementary stipulation that the specification

[10] According to *Webster's Third New International Dictionary of the English Language*, Encyclopædia Britannica Inc., Chicago, 1986, 'Demarcate' means: "1. To *mark by bounds*: determine the *boundary* of: DELIMIT. 2. To set apart *clearly or distinctly* as if by *definite limits or boundaries*: DISCRIMINATE" [emphasis added]. The *Oxford English Dictionary* (3rd Edition) defines 'Demarcation' as: "The action of *marking the boundary or limits* of something, or of *marking it off* from something else; *delimitation*; separation - Usually in phr. '**line of demarcation**'" [emphasis added]. 'Demarcate' is defined: "To *mark out* or *determine the boundary or limits* of; to *mark off*, separate, or distinguish from; to *mark or determine, as a boundary or limit;* to

of some *functional border* between science and 'pseudoscience' can be substantiated, and failure in this aspiration manifestly delegitimizes any 'demarcation' claim. 'Pseudoscience', meanwhile, denotes 'sham', 'spurious', or 'feigned' science: that which, though it may appear to be science, is in fact *not science at all*.[11] As Michael D. Gordin writes in *The Pseudoscience Wars*: "On the imagined scale that has excellent science at one end and then slides through good science, mediocre science (the vast majority of what is done), poor science, to bad science on the other end, it is *not* the case that pseudoscience lies somewhere on this continuum. It is off the grid altogether" (Gordin 2012, pg. 1; see also Gordin 2012, pp. 2, 12; Thurs and Numbers 2013, pp. 137-138; Hansson 2013, pg. 68; Mousseau 2003, pg. 271; Ladyman 2013, pp. 46-47).

It needs to be emphasized that its distinct title (the fact that one is used, and its specific meaning), and more importantly, its distinct outcome, both manifestly require that 'demarcation' can be differentiated from standard scientific processes. Scientists regularly criticize and dismiss *scientific* proposals, but this is emphatically not to be taken as indicating that the rejected contributions are *not science*. Indeed, it is not uncommon or at all improper for a small group of scientists to continue to pursue propositions that have been rejected by the majority of their peers, or to attempt to revive previously rejected hypotheses. The latter should not, therefore, be equated with 'pseudoscience', or their proponents declared to be 'pseudoscientists', both of which terms unambiguously indicate that such further pursuit *is* improper (see Ladyman 2013, pp. 46-47). Critical *scientific* assessments are explicitly the methods employed to justify decisions *within science*, and the 'demarcation' of a target as 'pseudoscience' therefore goes markedly beyond what such arguments can legitimately establish. Resultantly, any proposal in which routine scientific arguments are presented as sufficient to justify demarcation claims is highly problematic. An acceptable demarcation process *must* include some pertinent feature that legitimizes the defining move of declaring a target to be beyond the bounds of science.

Some contributors also consider whether demarcation should be ***Ahistorical***:[12] that no demonstrated 'pseudoscience' can ever have *been* genuine science, or, having once been dismissed as non-science, can ever *subsequently* be a proper subject of scientific endeavor. Such expectations are in accordance with, and probably arise from, Popper's seminal presentation of the problem, his proposed solution to which indeed meets this objective[13]. Certainly, the effective excommunication of 'pseudoscience' from the academy requires establishing that it could never possibly become genuine science. That what has previously been accepted as science could never justifiably be declared 'pseudoscience' is not so clear, and thus will be addressed further below. Regarding ahistorical determinations in general, if justifiable, such would serve as another major feature differentiating 'demarcation' from routine scientific 'rejection', as scientific conclusions are never entirely immune to revision.

---

define' [emphasis added] – "demarcation, n.". OED Online. June 2017. Oxford University Press. http://www.oed.com.ezproxy.lib.monash.edu.au/view/Entry/49595?redirectedFrom=demarcation [Accessed 17/09/17]; "demarcate, v.". OED Online. June 2017. Oxford University Press. http://www.oed.com.ezproxy.lib.monash.edu.au/view/Entry/49594?redirectedFrom=Demarcate [Accessed 17/09/17].

[11] Webster's (1986) defines 'Pseudo' as meaning 'sham, feigned, spurious', and 'pseudoscience' specifically as 'a system of theories, assumptions, and methods *erroneously regarded as scientific*' (emphasis added). The OED defines 'pseudo' as: 'Forming nouns and adjectives with the sense 'false, pretended, counterfeit, spurious, sham; apparently but not really, falsely or erroneously called or represented, falsely, spuriously'. It defines 'pseudoscience' as: 'a *spurious or pretended science*; a branch of knowledge or a system of beliefs mistakenly regarded as based on scientific method or having the status of scientific truth' [emphasis added] - "pseudo-, comb. form". OED Online. June 2017. Oxford University Press. http://www.oed.com.ezproxy.lib.monash.edu.au/view/Entry/153742?rskey=7oktpK&result=2&isAdvanced=false [Accessed 17/09/17]; "pseudoscience, n.". OED Online. June 2017. Oxford University Press. http://www.oed.com.ezproxy.lib.monash.edu.au/view/Entry/153794?redirectedFrom=pseudoscience [Accessed 17/09/17].

[12] Pigliucci & Boudry 2013b, pg. 2

[13] Mahner 2013, pp. 32-33; Rothbart 1990, pg. 112

Plainly it is also imperative that the demarcation process be correctly ***Focused***. Some contributors explicitly declare that their demarcation method involves comparing exemplary science with particularly obvious examples of 'pseudoscience' (Pigliucci 2010, pp. 1, 2; Gardner 2000, pg. 1). They may argue, for example, that: "Of course there will always be borderline cases hard to classify, but the fact that black shades into white through many shades of grey does not mean that the distinction between black and white is difficult" (Gardner 1952, pg. 7. See also Ruse 2009a, pg. 339; Boudry 2013, pg. 92). Others admit that they are employing an 'idealized' picture of genuine science (Derry 1999, pg. 163). More often, commentators apply such methods without explicitly describing them in such terms.[14] Such an approach to the problem is, I believe, transparently inadequate and, arguably, sophistic. Doubtless it makes the demarcator's task far simpler, but it also renders their conclusions philosophically uninteresting and functionally unhelpful - perhaps even detrimental.

Firstly, to compare something with exemplary or idealized science can technically establish only that it is not exemplary or idealized science: merely good science, mediocre (which is arguably typical) science, and poor science would necessarily fail the same test. Thus the outcome of this procedure falls far short of the essential goal of establishing that something is not science at all. To achieve that objective, 'pseudoscience' must be differentiated from *all* genuine science, and particularly those examples that it most closely resembles – flawed, preliminary, or controversial science.

Secondly, the more obvious it is that something is 'pseudoscience', the less it contributes to the problems demarcation is intended to resolve. The audience most likely to actually benefit from demarcation arguments - the group we may most reasonably expect to alter their beliefs and belief forming practices - are individuals who are rationally focused enough to appreciate and heed the significance of demarcation, but lack training in critical thinking and have only a basic understanding of science (neither of which indicates that they are 'irrational' - to the extent that this indicates someone who is of below average rational capacity - but rather that they are entirely typical[15]). There is also a particular need to inform the decisions of laypeople in positions of authority (policy makers, educators, the judiciary, and the like), most of whom are highly educated. Clearly, it is most unlikely that such an audience would benefit significantly from any method that applies only to egregiously flawed, 'black' claims that confuse only "the gullible" (Gardner 1952, p. 6-7).

A thorough examination of the literature on widely agreed 'pseudoscientific' topics such as ufology, poltergeists, parapsychology, cryptozoology, 'alternative therapies', and the like, reveals that many publications are, from the perspective of such an audience, *prima facie* rational and scientifically sophisticated contributions, produced by obviously intelligent and well informed advocates, some of whom are scientists respected for their work in other fields (for example: Heuvelmans 1958; Sturrock 1999; Vallee 1975; Hynek 1972; Salisbury 1974; Haines 1987; Roll 2004). Even leaving aside such 'best' examples to focus on the more numerous, less discerning contributions: these too are rarely as transparently erroneous as 'pseudoscience' is typically claimed to be by debunkers and demarcators - and the demarcators who rely upon debunkers' portrayals (for example: Kelleher & Knapp 2005; Coleman & Huyghe 2006; Hall 2014; Playfair 1980; Clarkson 2011; Fuller 1966; Good 1987; Keyhoe 1960; Storr 2006; Hall 1988; Bowen 1969; Lorenzen & Lorenzen 1967; Binder 1967; Carrington & Fodor 1951; Healy & Cropper 2006; Dolan 2002). Granted, 'pseudoscientists' often employ reasoning that involves common logical fallacies and cognitive failings, but the latter are *common* precisely because they are the type of error to which humans (including scientists) are most subject (Bausell 2007, pp. xv-xvi, 35, 38ff).

As I have repeatedly emphasized, the 'demarcation problem' is considered important precisely because it is intended to resolve very real practical problems. These problems arise precisely within, and

[14] This is evident in the literature discussed below, in which comparisons of 'pseudoscience' with exemplary sciences are common, and efforts to identify and accurately characterize the 'best' examples of those 'pseudosciences' discussed non-existent.
[15] Consider for example Carl Sagan's "Mr. Buckley" (Sagan 1996, pp. 3ff).

because of, legitimately confusing 'grey' areas, and it is therefore important to focus keenly on the functionality of proposed solutions in application to the most sophisticated 'pseudosciences'. Indeed, focusing purely on egregiously irrational 'pseudosciences' may do more harm than good, as acceptance of such a representation could potentially blind individuals to the fact that 'pseudoscience' can also come in the form of *prima facie* compelling and well supported claims, advanced by highly capable advocates.

Relatedly, although really it should not be necessary, we shall see that it is important to insist that characterizations of 'science' and 'pseudoscience' are ***Accurate***, and reflect a broad knowledge of the subject. In particular, it is essential to recognize the invalidity of demarcation conclusions based on criticisms of conspicuously inferior examples of the relevant 'pseudoscientific' arguments. Just as it would be incontestably improper to pronounce physics to be 'pseudoscience' on the basis of fringe claims advanced by authors claiming the title 'physicist',[16] or to likewise condemn all of modern medicine because of the sophisticated, deliberately misleading strategies employed (with alarming frequency) by certain powerful actors therein (Goldacre 2013), so too is it unarguably improper to utilize such inappropriately targeted arguments against any other topic, field, or hypothesis. Thus, in regard to 'pseudoscience' in general, any individual 'pseudoscience', or any specific 'pseudoscientific' argument, if a conclusion is intended to apply to anything broader than one particular iteration (as is almost universally the case), no demarcation claim can be truly justified if it fails to accurately represent and adequately address the *best* or *strongest* example(s) of the same.[17] Likewise, claims relating to 'science' in general are not accurate if they are in fact true only of exemplary or idealized science.

Finally, care should be taken to ensure that proposed criteria are ***Non-Circular***. Propositions that have been publicly dismissed as 'pseudoscience' thereafter persist in a context unique to that class and entirely different to that of accepted science. Criteria should not, therefore, refer to features arising directly or indirectly from this context.

[16] For example: 'Physicist' Dr. Joseph P. Farrell's *The Giza Death Star Destroyed: The Ancient War for Future Science* (Adventures Unlimited Press, Kempton, Illinois, 2005), as advertised in *Strange Times: A Nexus New Times Special Edition*, 2006, pg. 90 (Farrell in fact claims a 'doctorate in patristics' from the University of Oxford) (https://gizadeathstar.com/about/ - accessed 10/07/2017).

[17] Within reasonable limitations of availability etc.

2) Critical Assessment of Extant Demarcation Proposals

The objectives of the following analysis are not merely to demonstrate that each proposal fails individually. I also intend to establish the reality, and outline the nature, of the 'traditional' approach to the demarcation problem, and, further, to highlight the principal challenges confronting demarcation efforts. These latter will then inform the subsequent discussion regarding the demarcation problem generally, and the prospects for successful demarcation in the future.

2.1) Philosophers

The demarcation arguments that will be examined are obviously not exhaustive. However, I have sought to include a broadly representative sample, including: the most promising efforts, some of the best known examples, some of the most recent contributions, and a number of arguments drawn from the 'coal face' of demarcation – 'skeptical' or debunking publications. I shall begin, however, by examining the contributions of a number philosophers.

2.1.i) The Logical Positivists

For the sake of context, it is useful to begin very briefly with the 'Logical Empiricists' (or 'Logical Positivists') (see Godfrey-Smith 2003, pp. 25-38), such as Rudolf Carnap, Carl Hempel, and Otto Neurath, of the 'Vienna Circle', who were influenced in their views by Ludwig Wittgenstein (Gillies 1993, pp. 17-21, 156ff.; Godfrey-Smith 2003, pp. 22-25). This group forms something of a bridge between the modern Demarcation Problem and the more ancient, related problem of distinguishing true knowledge from mere opinion, as the Logical Empiricists were focused on language and establishing a general 'theory of meaning'. They held that all non-analytic statements (and *a fortiori* scientific claims) were either in principle empirically verifiable (testable), or else meaningless, such that "Knowing the meaning of a sentence is knowing how to verify it" (Godfrey-Smith 2003, pg. 27; Gillies 1993, pp. 171ff.). They also believed that inductive reasoning was characteristic of science, and so sought to develop a system of inductive logic (Godfrey-Smith 2003, pp. 29, 39).

From the perspective of demarcation of the sort relevant to this paper, the Logical Empiricists' 'Verificationism' is severely wanting. To meet this criterion "There just had to be the possibility of finding observational evidence that would count for or against the proposition in question" (Godfrey-Smith 2003, pg. 27). This is clearly correct as far as it goes – a claim that cannot be subjected to *any* empirical test, if advanced as 'science', can legitimately be dismissed - but it doesn't take us very far. Ruling out only those claims that tell us literally nothing whatsoever about the state of the universe will obviously not address the vast majority of declared 'pseudosciences', and thus fails dismally the test of 'Reflective Equilibrium'.

2.1.ii) Popper

Sir Karl Popper, the best known and most influential of all philosophers of science (Chalmers 2013, pg. xi; Godfrey-Smith 2003, pg. 57; Gillies 1993, pg. 22; Nickles 2013, pg. 101; Susser 1986, pg. 711; Mulkay & Gilbert 1981, pg. 389; Coady 2006, pg. vii; Williams 2000, pp. 273-274), developed his arguments in part as a response to the claims of the Logical Empiricists (Chalmers 2013, pg. 55; Garcia 2006, pp. 1, 40, *et passim*; Gillies 1993, pp. 21-22, 177ff.). Popper objected strongly to their endeavor to develop a system of inductive logic, arguing rather that the use of induction could never be logically justified (Popper 1935, 3ff.; Gillies 1993, pp. 27-29; Okasha 2002, pg. 27; Garcia 2006, pp. 8ff, 39). Popper noted that, although universal claims (such as scientific 'laws') could be tested, as Verificationism required, they could never be *proven*, as no amount of positive evidence could ever demonstrate that exceptions do not exist. However, such claims could, he argued, be *falsified* by a single, contrary example (Popper 1935, pp. 10, 19; Gillies 1993, pp. 29ff.; Okasha 2002, pg. 23; Garcia 2006, pp. 9, 15-16, 17, 40,

41). Accordingly, he proposed a normative theory of science based entirely on deduction, with the falsification of hypotheses being its core objective (Popper 1935, 9ff. *et passim*; Garcia 2006, pp. 1ff., 38-39; Gillies 1993, pp. 28-29, 205ff.; Okasha 2002, pg. 27). He also proposed 'falsifiability' as a necessary and sufficient condition for genuine science, which could be employed to differentiate it from non-science and pseudoscience (Popper 1935, pp. 17ff; Gillies 1993, pg. 177; Garcia 2006, pp. 9, 38ff.). Because Popper's purely deductive theory of science has been strongly criticized and long rejected (Garcia 2006, pp. 1-2; Godfrey-Smith 2003, pp. 57, 59, 63ff), the following assessment of 'falsifiability' as a demarcation criterion will, as much as possible, treat it independently of Popper and his other arguments.

'Falsifiability' comes in two basic forms, which I shall refer to as 'logical' and 'methodological'. 'Logical falsifiability' requires only that a *proposition itself* be empirically 'risky': that its logical form rules out some possible state of affairs such that there is at least one, in principle possible, observation that could reveal the proposition to be false (Popper 1935, pp. 18, 65-66; Garcia 2006, pp. 38, 40; Lakatos 1970, pp. 96, 98; Godfrey-Smith 2003, pg. 64). The benefits of this demarcation criterion are many. It appears to provide a simple, strong, precise, justifiable, epistemically significant, necessary and sufficient criterion of science, which refers directly to an ahistorical feature of the proposition itself. In this, it arguably serves as a model for the features expected of subsequently proposed demarcation criteria. However, this 'logical' form of 'falsifiability' is only *marginally* more restrictive than 'verifiability' (testability). It may well be possible to point to a number of claims that, through excessive vagueness or the inaccessible nature of their postulates, do not dictate any specifiable empirical outcomes that could be falsified. However, this is clearly not true of the great majority of declared 'pseudoscientific' hypotheses, which do involve explicit claims regarding the state of the universe which could, at least in principle, be shown to be false (Laudan 1996a, pp. 218-219; Mahner 2013, pg. 30; Ladyman 2013, pg. 54; Boudry 2013, pp. 83, 87; Gordin 2012, pp. 8-9; Rothbart 1990, pg. 112).

This criticism and others[18] have led Popper and his supporters to develop a more sophisticated, methodological account of 'falsifiability'[19]. Based on the idea that science proceeds through a series of 'conjectures and refutations', they require that a proposition be 'falsifiable' in the sense that proponents are required to explicitly seek to falsify their hypotheses, and strictly forbidden[20] to utilize '*ad hoc*' moves in order to defend them (Popper 1935, pp. 19-20, 32ff., 64; Godfrey-Smith 2003, pp. 61-63, 64-65; Garcia 2006, pg. 41; Lakatos1970, pp. 97, 103ff; Ladyman 2013, pg. 55).

It is important to immediately note the implications of this move for demarcation. Having begun with a criterion of science that referred to a feature of the proposition itself (Popper 1935, pp. 66-67; Mahner 2013, pg. 32), the discussion has now moved to the activities, perhaps even the 'attitudes', of proponents (Godfrey-Smith 2003, pg. 65). Given the basic fact that hypotheses do not dictate the methods by which they are pursued, this immediately creates a rational gap between the demarcation argument – focused on the behavior of proponents - and the 'target' of its conclusion – topics and fields (or, less accurately but more charitably, propositions or arguments). As will become evident as we proceed, rather obvious 'gaps' of this type are a common feature of demarcation arguments, although demarcators surprisingly often fail to recognize them.

In regard to Popper specifically, it is surely not a coincidence that the major examples of 'pseudoscience' he refers to as inspiring his demarcation criterion are *Freudian* Psychoanalysis, *Adlerian* Psychology, and *Marxist* History (Popper 1962, pp. 32-33; Pigliucci 2013, pp. 10-11), in which the use of

[18] My treatment of 'logical falsifiability' is admittedly truncated, as will be all of my discussion of Popper's and related arguments on this topic. Popper's contribution has been discussed so often, and in such detail, by other philosophers that there is clearly no need for me to undertake an entirely repetitive representation of the same here (Pigliucci 2013, pg. 12). Moreover, the arguments advanced against Popper's arguments are, I believe, entirely and manifestly successful. Popper's arguments have thus not been included here because of any great value they have in contributing to a resolution of the Demarcation Problem, but entirely because of their importance to the development of the 'traditional' demarcation problem, and because they continue to be so often cited by working scientists (Gordin 2012, pg. 7; Godfrey-Smith 2003, pg. 57).

[19] This too can be sub-divided as occurring in multiple forms.

[20] Or at least strongly discouraged. Popper recognized that scientists often did not behave in the manner he prescribed, and did not insist that this meant they were not scientists (Godfrey-smith 2003, pp. 61-62).

the eponym indicates that what is being referenced is considerably more than just a hypothesis, but rather a detailed *system* of commitments and arguments – something like a 'research programme' – which includes methodology[21]. This may perhaps have obscured from Popper the crucial distinction between proposition and proponent. In reality, 'pseudoscientific' hypotheses tend to be approached in a less methodologically homogenous manner than comparable scientific propositions, as 'pseudoscientific' communities typically lack broadly agreed sources of authority, and can disagree markedly regarding what evidence and methods they employ.

In order to apply 'methodological falsifiability' as a demarcation criterion, it seems that either the proponents of a hypothesis must *publicize* their unwillingness to accept falsification, or demonstrate it through continuing to support a hypothesis despite it having been 'falsified'. The former scenario establishes nothing specific regarding *the proposition* being advanced, and thus cannot justify its demarcation. The proponents' actions may or may not be prompted by some feature of the hypothesis, and there may or may not be other supporters of the proposition who support it in an entirely satisfactory manner (Derksen 1993, pg. 21). It is thus not possible in this case to bridge the gap between the claim, regarding a feature of particular publications, and the desired conclusion, regarding the relevant proposition or the research topic overall. However, it may be justifiable in such circumstances to demarcate the specific *publications* in question themselves.

Obviously, to claim that something is 'unfalsifiable' because it has been falsified yet proponents continue to advocate it, is contradictory: a proposition cannot be both falsified and 'unfalsifiable'. Leaving this aside, the claim also relies on a highly problematic concept of conclusive falsification, which faces a number of well known, major complicating factors. The 'Duhem-Quine thesis' establishes that no proposition can be tested in isolation. Scientific propositions are typically part of a larger system of hypotheses, and any empirical test must include various background or 'auxiliary' assumptions regarding the instruments being used, the initial conditions of the test, etc. (Godfrey-Smith 2003, pp. 31-32, 64-67; Pigliucci & Boudry 2013b, pg. 1; Boudry 2013, pg. 87; Garcia 2006, pg. 60; Lakatos 1970, pp. 100ff) There is also the problem of establishing the truth of the observations that are supposed to act as falsifiers. These too require acceptance of various 'auxiliary' assumptions, and in fact cannot readily be epistemically differentiated from theoretical explanations. The result is that scientists have many different options in how they can choose to respond to any apparent 'falsification' of a claim, and there is no simple, formulaic, or objective approach that would allow us to conclusively establish when 'falsification' has occurred (Lakatos 1970, pp. 97ff.; Godfrey-Smith 2003, pp. 65-67; Garcia 2006, pp. 45ff.). It follows that forbidding '*ad hoc*' arguments also does not constitute a useful demarcation method. Indeed, such are common within science, given that scientists do not typically give up hypotheses just because they appear to clash with some observation or experimental result,[22] and what may seem an '*ad hoc*' move to one scientist can appear entirely justified to another (Hunt 2012; Pigliucci 2013, pg. 12; Ladyman 2013, pp. 55-56; Boudry 2013, pg. 87; Samir 2002, pp. 15-16; Nickles 2013 pp. 109-110; Cleland and Brindell 2013, pp. 187-189).[23]

Moreover, any arguments involved in determining whether 'falsification' has occurred are clearly *scientific*. Assessing and debating the evidential support for a hypothesis is a key *scientific process* that is applied to *scientific propositions* at all levels of respectability, including debates at the leading edge of science (regarding which hypothesis is the 'best'). Asserting that such arguments can be used to demarcate would therefore justify scientists in dismissing as 'pseudoscience' any proposition that they regard as contrary to the evidence, even if it is accepted by a majority of their peers or constitutes an acceptable minority position. Even if scientists reserved the 'pseudoscience' label only for what they

[21] 'Research Programmes' will be more fully described shortly.

[22] Of course, defenders of Popper's arguments would insist that Popper was not attempting to provide a description of how scientists *do* behave or *have* behaved, but rather how they *should* behave (see Garcia 2006, pp. 50ff.).

[23] This *can* be overcome by giving the term '*ad hoc*' a specific meaning (see for example Chalmers 2013, pp. 70ff for a typical example), but as there are various moves available to 'save' any hypothesis from falsification, the problem then is that there is no reason to expect the same move to occur within all 'pseudosciences', and if any specified move were to be universally accepted as entirely improper to science, canny 'pseudoscientists' would simply employ the other options.

regard as particularly egregious cases, there is nothing within the argument itself, no specific, non-arbitrary line of 'demarcation', to justify such differentiation between 'falsified' science and 'falsified' 'pseudoscience'. Thus methodological falsification cannot possibly justify use of the term 'pseudoscience'.

2.1.iii) Contra Creationism: Ruse, Pennock, and Gross v. Laudan

It is worth noting here the use of Popper's arguments in the most celebrated practical applications of demarcation of the last century – the legal endeavors to keep Creationism out of U.S. classrooms. In 1982, Laudan published his objections to the arguments adopted by Judge William Overton in his decision on the *Mclean V. Arkansas* case.[24] Relying heavily on expert testimony supplied by philosopher of science Michael Ruse,[25] Overton ruled in part that 'Creation Science' is not science at all, because it fails to meet Ruse's five 'essential' (necessary) characteristics of science. To wit: "(1) It is guided by natural law; (2) it has to be explanatory in reference to natural laws; (3) it is testable against the empirical world; (4) its conclusions are tentative, that is, are not necessarily the final word; and (5) it is falsifiable" (Overton 2009[26], pg. 294). Points (1) and (2) were taken up again, much more forcefully and in improved form, in the *Kitzmiller V. Dover* case of 2005 against Intelligent Design Creationism, which will be discussed below. I therefore focus here on the dispute regarding (3), (4), and (5).

Laudan wrote that to claim Creationism is neither testable nor falsifiable is to assert that it makes "…no empirical assertions whatever…", whereas in fact Creationism involves various empirical claims, including, for example, that "…most of the geological features of the Earth's surface are diluvial in character…" (Laudan 2009a,[27] pg. 332). He therefore argued that the best way to combat Creationism is to point out that its claims *are* falsifiable, and *have been falsified* (Laudan 2009a, pp. 332-333). Laudan also asserted that, although some Creationist claims may not be testable in isolation, this is also true of many scientific hypotheses, and thus in this sense being 'testable' cannot differentiate science from non-science (Laudan 2009a, pg. 333)

Noting Overton's repeated insistence that Creationism is 'unscientific' because its advocates refuse to alter their views regardless of the evidence, Laudan argued that Creationists have in fact changed their positions over time, that dogmatism regarding core hypotheses can also be found within accepted science, and thus that whilst "…there may be subtle but important differences between the dogmatism of scientists and that exhibited by many creationists… one does not even begin to get at those differences by pretending that science is characterized by an uncompromising open-mindedness" (Laudan 2009a, pp. 333-334). More importantly, he objected that "…the ad hominem charge of dogmatism… egregiously confuses doctrines with the proponents of those doctrines". "What counts is the epistemic status of creationism, not the cognitive idiosyncrasies of the creationists" (Laudan 2009a, pg. 334).

In response, considering demarcation generally, Ruse notes that the issue has "…certainly tied philosophers of science in knots….", and that "Simple criteria that supposedly give a clear answer to every case… will not do. Nevertheless, although there may be many gray areas, white does seem to be white and black does seem to be black" (Ruse 2009a, pp. 338-339). He then argues that his five essential characteristics are "…not quite as irrelevant as Laudan's critique implies" because they can successfully distinguish between 'Mendel's first law' and the 'Catholic doctrine of transubstantiation' (it is significant that, in order to make this argument, Ruse had to treat (4), 'it is tentative', as meaning 'something

---

[24] The ACLU opposed a bill by the Arkansas Board of Education requiring 'equal time' for Creationism and Evolution (Overton 2009).

[25] Ruse subsequently indicated that he thoroughly approved of Overton's application of his arguments (Ruse 2009a, pp. 337-338). There is, therefore, no real need to differentiate Overton's arguments from Ruse's in the following discussion: they are, in terms of Ruse's view, effectively identical, although it may be that Overton did not understand the arguments in quite the same way as Ruse, as will be discussed below.

[26] Decided in 1982.

[27] This was originally published in 1982, but reproduced in Ruse's 2009 book, alongside that author's responses and Laudan's answering publications from the same period, such that the latter is the most useful source to reference.

empirical might change our minds', which of course renders (3): 'it is testable', and (5): 'it can be falsified', superfluous) (Ruse 2009a, pg. 339). However, as Laudan subsequently responded, the existence of one example, or any number of examples, to which the criteria apply is entirely irrelevant to the objection that the 'essential conditions' fail to apply in many cases of accepted science, and thus cannot be 'essential conditions' at all (Laudan 2009b, pp. 346-347). Moreover, in referring to 'black' and 'white' cases, and using transubstantiation as an example, Ruse is unarguably making the weakest of claims for his demarcation criteria, giving us no reason to believe they could be successfully employed in more problematic cases. Even Creationism is 'gray' compared to transubstantiation, which I doubt anyone has ever proposed as a 'scientific' hypothesis.

Ruse agrees that scientists 'frequently' cling to their views, but states that: "…although science may not be as open minded as Karl Popper thinks it is, it is not as close-minded as, say, Thomas Kuhn thinks it is" (Ruse 2009a, pg. 340). Again, this in no way supports his argument, but simply confirms Laudan's position that characteristics referring to open-mindedness are highly problematic.

Ruse then proceeds with further arguments referring directly to the activities of Creationism's defenders. Ruse insists that "…there is nothing tentative or empirically checkable about the central claims of creation science", because "Their publications (and stated intentions) show that, for example, there is no way they will relinquish belief in the flood, whatever the evidence. In this sense, their doctrines are truly unfalsifiable" (Ruse 2009a, pp. 340-342). He also insists that the revisions that Laudan referred to were not genuine (Ruse 2009a, pp. 343-344).

Laudan answered this by repeating and expanding his previous argument on this issue. Insisting that "…there is a difference between *ad hominem* and *ad argumentum*", Laudan grants that creationists refuse to change their minds, but asserts that while this "…tells us something interesting about the psychology of creationists…" it "…has no bearing whatever on an assessment of their doctrines" (Laudan 2009b, pp. 347-348). Laudan further correctly notes that in all other cases where a theory is refuted, "…we do not care whether the scientist who invented the theory is prepared to change his mind. We do not say that his theory cannot be tested, simply because he refuses to accept the results…" (Laudan 2009b, pg. 348).

That Ruse indeed seriously confuses propositions with proponent behavior is beyond doubt. For example, in his witness testimony, in response to the question "Do you think that creation science is testable" Ruse answers in part "…an attribute of creation science that distinguishes it quite clearly from science is that *it is absolutely certain about all of the answers*… Whatever the contrary evidence, *creation science never accepts that its theory is falsified*[28] (emphasis added)" (Ruse 2009b, pp. 277-278). Nor did Laudan have any impact on Ruse's confusion. In 1986 Ruse wrote: "The major reason why Creation-science is not genuine science is that its supporters have to believe, without question and without dispute, the literal truth of Genesis" (Ruse 1986, pg. 72).

Ruse appears to be attempting to defend this conflation of proposition and proponent when he writes: "Creation science is not like physics, which exists as part of humanity's common cultural heritage and domain. It exists solely in the imaginations and writing of a relatively small group of people" (Ruse 2009a, pg. 342). However, belief in Creationism is widespread, both in the West and in the Islamic world, and historically it enjoyed almost universal acceptance. Far more people believe in Creationism than are able to understand, and thus 'believe', the Standard Model of modern physics. Thus to claim that it is not "part of humanity's common cultural heritage" is patently ridiculous. More importantly, Laudan is clearly correct when he writes: "Creationists make assertions about the world. Once made, those assertions take on a life of their own" (Laudan 2009b, pg. 348).[29]

In 2003, Graham Oppy argued that the Ruse/Overton arguments are so egregiously flawed, if understood as "…philosophers of science will be naturally disposed to interpret [them]" (as necessary

[28] I can only feel sympathy for a theory that is expected to deny itself.

[29] This is not a controversial position. Imre Lakatos, for example, writes: "The cognitive value of a theory has nothing to do with its psychological influence on people's minds. Belief, commitment, understanding are states of the human mind. But the objective, scientific value of a theory is independent of the human mind which creates it or understands it. Its scientific value depends only on what objective support these conjectures have in facts" (Lakatos 1978, pg. 1).

characteristics of science that can be used to demarcate it from non-science), that it makes better sense to assume they in fact meant that Creationism is not "…good, well established science" or "…good scientific practice" (Oppy 2003, pp. 113-119). I am therefore not alone in holding that, in terms of a typical 'philosophical' interpretation[30], the Ruse/Overton arguments are egregiously and obviously flawed.

Nonetheless, despite clearly adopting the 'philosophical' interpretation of Ruse's arguments, both Barry Gross and Robert Pennock penned heated condemnations of Laudan's objections. Their arguments are significant here because of the philosophically problematic attitude they unambiguously reveal. Gross wrote (and Pennock repeated) that Laudan is "…a perfect example of a philosopher richly deserving exclusion from 'the conversation of mankind'" (Gross 2009, pg. 351; Pennock 2011, pg. 182). Yet Gross does not maintain that Laudan's arguments were incorrect[31], or that Ruse's were not flawed. Gross argues at length that the case required Creationism to be found to be 'not science', and that Laudan's contention - that Creationism is bad science - would not have been sufficient to ensure a win (Gross 2009, pp. 351ff). Referring directly to Ruse's essential characteristics of science, Gross admits: "Philosophically, these criteria may have been acceptable sixty or eighty years ago; but they are not rigorous, they are redundant, and they take no account of many distinctions nor of historical cases… One would not recommend to graduate school a student who could do no better than this" (Gross 2009, pg. 363). However, Gross nonetheless contends that the arguments are good enough for the purpose they serve "In this forum, confronted with creationism and religious 'know-nothingism' as adversaries…" (Gross 2009, pg. 363) He contends that "Laudan has confused the outlines of a constitutional conflict with a colloquium on philosophy." (Gross 2009, pg. 35), explaining that the function of the latter "…may be to seek truth…", but that this is not true of the former (Gross 2009, pg. 356). He argues that lawyers building a case "...must avoid epicyclical complexity and neurotic maneuvering against outré counterexamples" (Gross 2009, pg. 362). Then, referring to the strong objections of consulting philosophers (after lawyers had re-written their statements) being dismissed with the assertion "…philosophy should be kept to the seminar room", Gross insists that it is proper for philosophers to accept this, because the lawyers were "…aiming to win… and did win" (Gross 2009, pg. 362).

Further, Gross echoes Ruse's in writing: "Creationists' beliefs… constitute a faith and are thus immune to revision… The creationists are a monolithic community with a monolithic outlook that is immune to change because no member of the community will countenance any change and therefore no criteria for change exists" (Gross 2009, pg. 365). Both Ruse and Gross are shamelessly exaggerating - pretending that the Young Earth Creationists of the Creation Research Society are the only Creationists, and only type of Creationists, in existence. The truth of their assertions would entail that all Creationists subscribe to the same beliefs, and that there has never been, and could never be, any individual who was once a Young Earth Creationists but subsequently altered their position – either rejecting Creationism altogether or rejecting aspects of Young Earth Creationism. This ignores the fact that there *are* other Creationist groups – for example Islamic Creationists – and many different types of Creationist with

---

[30] And, of course, Ruse *is* a philosopher of science, and thus should have understood perfectly well how his arguments would be taken by other philosophers, even before Laudan made it explicit.

[31] Although he does offer some criticisms of Laudan's positions. For example, against Laudan's point that parts of science cannot be tested 'in isolation', Gross demands: "…in isolation from what is that claim that God created things not testable? In conjunction with what is this claim to be tested?" (Gross 2009, pg. 364). Gross is here merely presenting the claim in an unnecessarily problematic form, much like demanding to know how the claim 'Bigfoot exists' is falsifiable. The answer in both cases is simply to understand the claim as a proposed 'best explanation' of the evidence adduced in their support, which can be tested or falsified through assessing whether that evidence is, in fact, best explained via these proposals. To deny that evidence can be employed to assess such claims would require rejecting as irrelevant the good work of many individuals, including evolutionists, who have specifically sought to show that these proposed explanations are ill supported.
Gross then goes on to say: "Of course, creationists do make some empirical claims, but so do astrologers, flat earthers, clairvoyants, and magicians. Does Laudan require that any ensemble of doctrine which attaches to itself a set of empirical assertions is, by that token, science?" (Gross 2009, pg. 364). This is, frankly, an astounding attempt to flip the argument. Laudan insisted that 'testability' and 'falsifiability' should *not* be used as demarcation criteria *precisely because* they are too weak and would mean that any empirical claim qualifies a hypothesis as science (Laudan 2009a, pp. 332-333, 335; Laudan 2009b, pg. 345).

widely differing beliefs, including those who do not accept a young Earth and 'Theistic Evolutionists' (as well as ex-Creationists), some of whom began as Young Earth Creationists, but have since responded to some of the evidence, at least, in a manner that Gross and Ruse imply does not and cannot occur.

Pennock's 2011 attack (he refers to Laudan as "squinting" (202); "myopic"; and "petulant" (Pennock 2011, pp. 202-203); as having "…lost touch with reality in a profound a perverse way" (Pennock 2011, pg. 196); and asserts that his arguments are "…histrionic and ill-considered… (Pennock 2011, pg. 180), "merely provocative" (Pennock 2011, pg. 195), "ridiculous", and "dangerous" (Pennock 2011, pg. 203) - so 'attack' seems a fair description) is largely focused on Laudan's 1983 paper, and those portions will therefore be discussed later. Pennock is clearly frustrated that Laudan's arguments against Ruse had given "succor" to Creationists who repeated them (Pennock 2011, pg. 203). Regarding Ruse's arguments themselves, although Pennock soundly condemns Laudan's for his contribution, he nonetheless admits that "Many of his criticisms of Ruse's five criteria were correct if the demarcation task is taken to require the identification of an ahistorically exceptionless set of necessary and sufficient criteria to mark a pinline border between science and non-science" (Pennock 2011, pg. 202), having earlier admitted that this was *precisely* Ruse's intention (Pennock 2011, pg. 183). He insists, however, that even if Laudan is correct, the proper context is the discussion of constitutional matters in a legal setting, wherein philosophy should "…set aside its peculiarly abstract and rarefied interests and make a useful contribution in these other contexts for their more practical purposes" (Pennock 2011, pg. 181). He therefore asserts that Gross was "certainly right" in arguing that Laudan had confused a constitutional conflict with a philosophical colloquium (Pennock 2011, pp. 181, 202).

Gross and Pennock thus clearly agree with the editor of the S*keptical Enquirer* who, even prior to Laudan publishing his arguments, "…objected to those who criticize this whitewash of science "on arcane, semantic grounds… [drawn] from the most remote reaches of the academic philosophy of science" (Laudan 2009a, pg. 336). In doing so, they clearly reveal an attitude that appears to be common throughout the demarcation literature - that practical demarcation efforts *must* be pursued and defended *regardless of the philosophical issues*.[32] That Creationism had to be demonstrated to be 'not science' in order to win a legal case, as Gross and Ruse insist (Ruse 2009a, pg. 338; Gross 2009, pp. 351ff.) may explain why they advocate that claim, but it in no way advances the argument that this assertion is in fact accurate. If the issue was scientific rather than philosophical, surely no one would dream of attempting to justify, as Gross did, experts providing testimony they personally regard as inaccurate. Likewise, to dismiss philosophical objections as merely 'arcane', or 'abstract and rarified' quibbles, with no place in a legal setting is hypocritical when these same authors advocate deploying favorable philosophical arguments in pursuit of their preferred legal conclusions (see Laudan 2009a, pg. 336).

2.1.iv) Lakatos

The demarcation criteria proposed by Imre Lakatos arise from his efforts to improve on Popper's theory of science. In 1970, Lakatos argued that that we must focus, not on individual theories, but on series of theories (Lakatos 1970: 119), because "…the problem is how to demarcate between scientific *adjustments*, between rational and irrational changes of a theory" (Lakatos 1970, pg. 117). A 'progressive', scientific theory must have excess empirical content compared to its rival or predecessor, and some of this content must be corroborated (Lakatos 1970, pp. 116, 118). A theory that does not meet these criteria is 'degenerative'. "We '*accept*' problemshifts as 'scientific' only if they are at least theoretically progressive; if they are not, we '*reject*' them as 'pseudoscientific'" (Lakatos 1970, pg. 118). Importantly, this analysis requires that proposals must be 'content increasing' – they must include all the successful content of that which they are to replace, and add to it (Lakatos 1970, pg. 119). Lakatos thereby establishes the difference between acceptable and unacceptable uses of auxiliary hypotheses in response to contrary evidence (Lakatos 1970, pg. 117). Lakatos later framed his proposal in terms of 'research programmes': a 'hard core' of beliefs "…tenaciously protected from refutation by a vast

[32] Evidence of similar attitudes among other contributors will be noted below.

'protective belt' of auxiliary hypotheses", and "…a 'heuristic', that is, a powerful problem solving machinery…" (Lakatos 1978, pg. 3). As previously, these can be either 'progressive' science, making verified novel predictions, or 'degenerating' 'pseudoscience' (Lakatos 1978, pp. 3-4). Scientific progress occurs when scientists move from the former to the latter (Lakatos 1978, pg. 4). However, Lakatos repeatedly emphasizes that the assessment of a 'research programme' can require years. Seemingly 'crucial experiments' can only be judged as such in hindsight, and may be overturned "…a few years later…" (Lakatos 1970, pp. 173-175), while "One must treat budding programmes leniently: programmes may take decades before they get off the ground and become empirically progressive" (Lakatos 1978, pg. 4; See also Lakatos 1970, pp. 116, 173-175). Indeed, determining whether a theory's "…excess content is verified… may take an indefinite time" (Lakatos 1970, pg. 116). He also holds that"…it is not dishonest to stick to a degenerating programme and try to turn it into a progressive one" (Lakatos 1978, pg. 4): "…scientists frequently and *rationally* claim 'that the experimental results are not reliable, or that the discrepancies which are asserted to exist between the experimental results and the theory are only apparent and that they will disappear with the advance of our understanding" (Lakatos 1970, pp. 176-177).

This proposal includes a number of serious flaws. Firstly, holding that a research programme that does not advance verified predictions is 'pseudoscience' conflates demarcation with refutation, leading to the clearly unacceptable result that standard scientific arguments regarding the empirical merits of research programmes justify demarcation claims, and that refuted research programmes may be 'not science at all'. Lakatos holds that the outcome of a scientific revolution must be that one research programme is 'progressive' and successful, whilst the other is 'degenerating' and 'pseudoscience'. This is obviously improper, and confuses the issue of demarcation with that of scientific progress (to which refutation is, of course, key). Such revolutionary events are correctly characterized as *scientific* disputes regarding the empirical virtues of rival *scientific* positions (research programmes) in which opposing groups of *scientists* employ typical *scientific* arguments. Certainly there is nothing within such arguments to justify the conclusion that one set of advocates are 'pseudoscientists' and their research programme 'pseudoscience'.

That Lakatos has failed to grasp the true significance of the term 'pseudoscience' is reflected in the fact that his proposal allows a research programme to begin as science, become 'pseudoscience', and then become science yet again. This makes no sense if 'pseudoscience' is 'not science at all'. He also holds that scientists can properly advocate a 'degenerating' research programme for decades. This likewise makes no sense if such are truly 'pseudoscience', which ought to be excluded from scientific discourse entirely, and its advocates denounced as 'pseudoscientists'. It is thus apparent that Lakatos has incorrectly used 'pseudoscience' as meaning simply 'inferior' or 'refuted' science, rather than 'not science', and therefore that his proposal fails to justify the 'demarcation' of 'pseudoscience'.

Lakatos' focus on series of theories and the acceptability of changes is also epistemically problematic. For any research programme, it is entirely possible that different advocates could propose different changes, some of which may be acceptable, others '*ad hoc*' and unacceptable (Kuhn 1996, pg. 83; Le Grand 1988, pp. 125-126). The changes proposed therefore undeniably reflect the abilities of the scientists involved, rather than having any direct epistemic significance for the hypotheses of the research tradition. Thus to label a research tradition itself 'pseudoscience' because of the unacceptable nature of changes proposed by its advocates is improper (Derksen 1993, pg. 21). Nor is it accurate to assert that '*ad hoc*' alterations cannot be accepted within science. Science is replete with examples of scientists correctly employing '*ad hoc*' defense mechanisms to protect a hypothesis from refutation, as philosophers of science widely agree (Hunt 2012; Pigliucci 2013, pg. 12; Ladyman 2013, pp. 55-56; Laudan 1977, pp. 115, 117; Boudry 2013, pg. 87; Samir 2002, pp. 15-16; Nickles 2013 pp. 109-110; Cleland and Brindell 2013, pp. 187-189). The objection to '*ad hoc*' moves, after all, is not that they aren't justified by the evidence (which, by definition, they are) but rather only that they do not go *beyond* the evidence (see Laudan 1977, pp. 114-118). For example, the philosopher Fred Wilson criticizes the 'Extraterrestrial Hypothesis' (ETH) explanation for UFOs because the evidence in its favour is "…only the fact that, if true, it would solve the mysteries that it is designed to explain" (Wilson 2000, pg. 35). However, to the

extent that this is true, it would remain so even if a UFO landed and disgorged aliens on a university campus tomorrow. The fact that some proposed hypothesis involves too many unknowns to allow us to glean from it specific predictions does not establish that it is false, or that it should not in any circumstances be accepted as 'science'. Similarly, just as the hypothesis that Bigfoot exists may likewise be said to explain only the evidence that gave rise to it, a similar hypothesis regarding some much less sensational type of animal, based on exactly the same type of evidence - tracks and other traces, many (seemingly) corroborative sighting reports, and native lore - has exactly the same status, and yet may easily prove to be correct, and so cannot, reasonably, be simply dismissed as 'pseudoscience'. In relation specifically to subjects proclaimed to be 'pseudoscience', it is also likely that any proposal going *beyond* the available evidence would draw criticism from debunkers, who would object that the proposal lacks support.

Even ignoring the above objections, it is impossible to apply Lakatos' demarcation criteria to various paradigmatic examples of 'pseudoscience' because the latter cannot be reasonably characterized as 'research programmes'. The typically decentralized and heterogeneous nature of 'pseudoscience' ensures there is usually no agreed heuristic or set of auxiliary hypotheses, and any selection of a 'series of theories' would therefore be highly subjective (see for example: Mahner 2013, pp. 37-38; Wilson 1974, pp. vii-ix; Laursen 2015, pp. 222-225; Opit 2006, pg. 89; Bausell 2007, pp. 7-8, 10; Broughton 1991, pp. 36-37; Healy & Cropper 2006, pp. 158ff.).

It may also be circular to criticize 'pseudosciences' as 'degenerating' (i.e. not 'progressive'), as this may reflect only that their advocates have been denied essential resources, or that they have been focused exclusively on countering the 'pseudoscience' charge, and therefore have not sought to otherwise advance their research or propose novel predictions.

2.1.v) Rothbart

A very similar demarcation argument has been proposed by Daniel Rothbart, although without the explicitly Popperian context. Rothbart begins by proposing "…a metacriterion for every demarcation criterion", "stipulating that the dividing line between science and non-science is the fulfillment of eligibility requirements…" (Rothbart 1990, pg. 114). 'Eligibility requirements' refer to standards that must be fulfilled, "…before any testing…" in order for a theory to be eligible for testing (Rothbart 1990, pp 113-114). Rothbart insists that "…pre-testing standards are indispensable, since scientists are, in principle, barraged by an infinite number of possible theories…" and therefore "A rational procedure is needed for this a priori selection..." (Rothbart 1990, pg. 113). Rothbart then proposes two essential 'eligibility requirements': "(a) a viable hypothesis must account for all phenomena explained by its rival theory, and (b) it must empirically clash with its rival" (Rothbart 1990, pp. 115 ff.).

Rothbart's criteria thus differ from those of Lakatos in that the latter focused on 'research programmes' rather than hypotheses, and Rothbart is discussing 'eligibility requirements' that must be fulfilled prior to testing, and also does not require that novel predictions be verified.

Without doubt the most glaring problem with this proposal is the staggering suggestion that such complex demarcation criteria could be applied in any meaningful way 'before any testing'. Rothbart recognizes that scientists must in fact assess a hypothesis' 'promise' (Rothbart 1990, pp. 113, 114), and "…potential explanation[s] as opposed to successful explanation[s]" (Rothbart 1990, pg. 116), but an '*a priori*' judgement call on such a complex issue obviously cannot reasonably be accepted as the justification for a demarcation determination.

Rothbart insists that the controversial (see for example: Laudan 1996b, pp. 21-23, 77, 81-82, 113-122) 'cumulative' view of science he and Lakatos have assumed has been defended by numerous philosophers and scientists (Rothbart 1990, pg. 116). However, even if we accept this position as unproblematic, Rothbart is mistaken in believing that it permits the sort of simplistic comparisons he is advocating. Rothbart quotes William Whewell as writing "The principles which constituted the triumph of the preceding stages of the science, may appear to be subverted and ejected by the later discoveries, but in fact they are (so far as they were true) taken up into the subsequent doctrines…" (Whewell 1857, cited

by Rothbart 1990, pg. 121). Clearly the parenthesized clause is doing crucial work here – enough, certainly, to undermine Rothbart's proposal. Further, as Lakatos noted, "…budding programmes… may take decades before they get off the ground…" (Lakatos 1978, pg. 4). Thus, if at a given time, T^1 explains problems a – g, while its proposed replacement, T^2, explains d – m, there is no reason why T^2 should not be accepted as superior (and certainly not rejected as 'pseudoscience'), with the expectation that problems a – c will be resolved in the future. Indeed, in such a circumstance Rothbart's criterion would apparently and unacceptably require that *both* be rejected as 'pseudoscience' because *neither* encompasses all of the explanatory successes of its rival.

Moreover, it is not uncommonly effectively impossible to reach anything approaching an objective or agreed position regarding the empirical merits of competing theories while the dispute is ongoing (see for example Le Grand 1988, pp. 1, 92-97, 116, 126-127). Scientists advocating competing positions tend to sharply disagree on many critical issues, including, to use Continental Drift as an example: the nature of the evidence (do the edges of the continents 'match up', or not? Le Grand 1988, pp. 39, 41-43, 50, 56, 63, 65, 83, 203-205); the significance of criticisms (is the lack of a mechanism fatal for Drift, or merely a problem sure to be solved? Le Grand 1988, pp. 40, 55-56, 107-108, 109-117, 129); and the success of proposed explanations (do 'land bridges' successfully explain species distribution data? Le Grand 1988, pp. 26-27, 43-44, 56, 61, 86).

These highly complex arguments (utterly failing to meet the desideratum of simplicity) are undoubtedly scientific, and address scientific theories. For the reasons given above, employing them to demarcate 'pseudoscience', and holding that the empirically inferior of two competing theories is 'not science at all' improperly confuses the issue of demarcation with that of scientific progress, and unjustifiably equates 'pseudoscience' with refuted, or (comparatively) inferior, science. Rothbart's proposal would require that scientists supporting a majority position must dismiss as 'pseudoscientists' those who advocate a minority hypothesis, and that scientists engaged in a dispute must believe that their opponents are in fact 'pseudoscientists'.

As with Lakatos' proposal, it also proves very difficult to successfully apply Rothbart's system to existing examples of 'pseudoscience', and attempting to do so reveals precisely those complications just discussed. What, for example, should we regard as the 'competitor' with which to compare belief in Bigfoot, or the existence of (scientifically significant) UFOs, or the efficacy of Homeopathic treatments? It appears that these 'compete' only with their denials. Bigfoot researchers, for example, would insist that their hypothesis explains the tracks and other physical traces, and the many reported sightings, while debunkers counter with the argument that such evidence is explicable in terms of hoaxes and misidentifications, and cannot explain the absence of more conclusive evidence such as a carcass. Bigfoot researchers then attempt to demonstrate that hoax and misinterpretations are not reasonable explanations, and that the absence of a carcass is not really a problem, so concluding that the debunkers' hypothesis enjoys less explanatory success than their own, whilst debunkers, naturally, claim precisely the opposite. The issue of 'explanatory success' is therefore not immediately resolvable, and Rothbart provides no guidance regarding how we should assess these competing claims in the meantime, as each is being challenged and developed, because he expects the decision to be made prior to such activities taking place. One may ultimately conclude, after detailed assessment, that the debunkers' hypothesis is superior, but this does not initially manifest in the Bigfoot researchers failing to provide any 'explanations' for everything their opponents claim to explain.

Rothbart's 'metacriterion' for demarcation criteria is worth considering again for a moment. On the one hand, it seems naïve and excessively simplistic to hold that *a priori* assessments of hypotheses could, without exception, genuinely justify their demarcation as 'pseudoscience' against the objections of their advocates. However, if demarcation can, in fact, only be achieved through a detailed assessment of each claim, then the distinction between demarcation and scientific rejection will be eroded. If the assessment process and the resultant objections are the same as those applied to scientific hypotheses (and both *would* be if Rothbart's criteria were adopted) then, again, no meaningful difference between the 'demarcation' of 'pseudoscience' and the 'rejection' of flawed science would exist to justify the difference in outcomes.

2.1.vi) Lugg 1

Such objections do not apply, however, to a proposal by Andrew Lugg, which he presents as a departure from the 'traditional' approach. Lugg argues that the difference between science and 'pseudoscience' is *not* epistemic warrant. Having noted the failure of numerous proposed criteria, Lugg asserts that 'pseudosciences' display many different types of flaws, and he therefore doubts that criteria based on the "…assumption… that there is something that scientific theories possess and pseudoscientific ones lack…" could possibly be successful (Lugg 1987, pp. 222-225).

Lugg also explicitly rejects Laudan's arguments against demarcation (Lugg 1987, pp. 224ff). He notes that "…natural selection has been said to be truistic since it treats survival as a matter of fitness and takes survival to indicate fitness. Action theory has been criticized because it merely repeats what everyone knows in a misleading and confusing way. And psychoanalysis has been dismissed for the quite different reason that it incorporates a set of subsidiary claims… that shields it against refutation" (Lugg 1987, pg. 225). This, he argues, establishes that Laudan is wrong in claiming that pseudoscience is "…merely bad science…" because such objections "…have nothing to do with the question of how well [the claims] are supported by the evidence", (Lugg 1987, pg. 225). However, Laudan argued only that the truly interesting questions regarding hypotheses are those that relate to their epistemic merits (Laudan 1996a, 221-222), and the types of criticisms Lugg lists *are* relevant to this issue. Leaving aside 'action theory', of which I know nothing, consideration of the claims against natural selection and psychoanalysis (the latter characterized by Lugg as "…a device for reflecting refutations…" - Lugg 1987, pg. 225) reveals that they are important *precisely because* they problematize the relationship between those hypotheses and the relevant evidence, and are thus unambiguously germane to any assessment of the epistemic merits of those hypotheses.

However, Lugg argues that any hypothesis rejected on the basis of the type of argument he has referred to has thereby been found to be "conceptually unsound". He concludes that "'Pseudoscience' and 'unscientific'… mark the important distinction between theories that are structurally flawed and those that are merely ill-founded" (Lugg 1987, pg. 225). 'Pseudosciences' are "…radically flawed complexes of theories, methods and techniques… unworthy of serious attention" (Lugg 1987, pg. 228). Lugg likens demarcation to the rejection of fallacious arguments, arguing that, in both cases, other than to establish precisely what is being claimed, "…empirical analysis is neither required nor appropriate" (Lugg 1987, pp. 226-227). This, he maintains, explains how we are able to demarcate even without demarcation criteria, just as we do not need rules to identify fallacious arguments, and he insists that, in actual fact, most criticisms of 'pseudoscience' refer to their inclusion of logical fallacies.

Lugg argues that this allows us to revive Popper's claims, as we can understand them as not truly about methods as such, "…but rather as being about the specific methods integral to specific practices" "…not about the pseudoscientists themselves but about the manner in which the practices with which they are associated are structurally flawed" (Lugg 1987, pg. 229). He also believes that claims about sciences degenerating into pseudoscience make good sense as they reflect the fact that "…a practice may change even though its theoretical content remains much the same" (Lugg 1987, pg. 229).

Clearly, although Lugg is aware of the problem, there remains a logical gap between his focus on demarcating 'theories', and his appeals to 'practice'. Early in his paper he insists that the Extra-Terrestrial Hypothesis (ETH) cannot be rejected because its proponents hold to it dogmatically, and therefore "Even if advocates of extraterrestrial visitation were always open minded, disinterested and critical, nothing would change with regard to the pseudoscientific character of their view" (Lugg 1987, pg. 223). How can this be understood as other than referring to the irrelevancy of practice? It does not help to insist that such charges refer merely to the psychology of proponents, as the only relevant manifestation of such is the manner in which they defend their propositions, and thus charges of improper psychology must arise from charges of improper argumentation. This problem is also clearly reflected in Lugg's restatement of Popper's position, which does not in fact alter its logical standing because it falsely implies that theory dictates method. The suggestion that a science can 'degenerate' into 'pseudoscience' itself indicates the

impropriety of demarcating theories on the basis of 'practice', as it follows that one and the same hypothesis can be advocated by different proponents employing different practices at the same time (Kuhn 1996, pg. 83; Le Grand 1988, pp. 125-126; Derksen 1993, pg. 21), and thus, contradictorily, that the same theoretical content can be simultaneously both 'science' and 'pseudoscience'. The practice of advocates could also change in a positive way, and thus, unacceptably, 'pseudoscience' could, without any alteration to the 'theory' itself, become 'science'.

The impracticality of Lugg's contention that demarcation requires no reference to empirical matters, and the other flaws in his proposition, become yet clearer when he moves from 'structurally rich' 'pseudosciences' to those such as E.S.P. and Ufology "…which have little theoretical structure", but which he maintains can be accounted for "…if we widen our perspective to take into account the procedures which are characteristically used to defend them" (Lugg 1987, pg. 227). Most paradigmatic 'pseudosciences' are pursued by an entirely decentralized, unconstrained and heterogeneous group of individuals who recognize no authority, common method, or agreed set of assumptions (see for example: Kelleher & Knapp 2005, pg. 207; Mahner 2013, pp. 37-38; Wilson 1974, pp. vii-ix; Laursen 2015, pp. 222-225; Opit 2006, pg. 89; Bausell 2007, pp. 7-8, 10; Broughton 1991, pp. 36-37; Healy & Cropper 2006, pp. 158ff.). Thus, there is little basis, to say the least, for assuming that such 'characteristic procedures' exist. Nonetheless, Lugg claims that Ufology can be "…submitted to criticisms in advance of empirical [sic] because it typically involves the suspect strategy of shuffling forward new cases of extra-terrestrial visitation once old ones are shown to be problematic" (Lugg 1987, pg. 227). This assertion is problematic on three counts. Firstly, Ufology is not to be identified with 'extra-terrestrial visitation', which is merely one of multiple explanatory hypotheses. Secondly, if indeed Ufologists do continually appeal to new cases, this must be precisely because *empirical criticisms* of their previous examples have proven them to be problematic, and thus empirical refutation is essential to the existence of this 'structural flaw'. Thirdly, it is entirely false to suggest that Ufologists employ the practice in question. In reality, anyone with a genuine familiarity with ufological literature knows that the same cases are used again and again *ad nauseum*. A 2006 article, for example, appeals to commonly cited cases dating from 1954, 1956, 1957, 1965, two from 1976, 1977, 1981, 1989, 1994, and 1995: some over sixty years old, not one dating to within a decade of the publication ('New French Report on UFOs' 2006, pp. 51ff). Lugg's baseless assertion is thus an excellent example of a philosopher employing a highly inaccurate representation of a 'pseudoscience'.

In developing his ideas, Lugg, like Popper, has deceived himself through focusing too restrictively on what he refers to as 'structurally rich' 'pseudosciences'[33]: those which include a clear methodology and set of auxiliary hypotheses. Such examples are typically dominated by a single individual, and thus are effectively a single iteration of one hypothesis. Demarcation of specific iterations of a hypothesis (publications) through reference to unacceptable practice is doubtless defensible, and may perhaps justify dismissing a specific *argument* as improper to science. However, such a conclusion cannot simply be extended as though establishing the nature of all other arguments employed or employable in defense of the same hypothesis, and thus cannot justify conclusions regarding the hypothesis (or 'theory') itself. In regard to most 'pseudosciences', which are advocated by many, entirely independent and highly diverse individuals, it would therefore be necessary to identify structural flaws in *all* of the relevantly distinct arguments they advance. Thus Lugg's provision of a solitary criticism as the basis for the rejection of each 'pseudoscientific' 'theory' could not possibly provide sufficient justification for such theory-wide conclusions, which must rather address in some way *all* defenses of that 'theory'. Of course, this does not in itself render the project impossible, but it does ensure that it will be far more complicated than Lugg envisages, and Lugg's failure to recognize this necessity again reflects a failure to properly understand the subject he is addressing.

Lugg further argues that 'pseudoscientific' therapies, such as Naturopathy and Orgone Therapy, are to be rejected because their apparent successes establish only correlation and not causation – an

[33] Lugg never explicitly specifies what he means by this, but it is evident from his subsequent discussion regarding Popper that he had in mind the same targets that Popper was focused on – Adlerian psychology, Freudian psychoanalysis, and Marxist history.

assertion for which no support is provided. Clairvoyant predictions are 'pseudoscience' because, he asserts, the same predictions could be made without recourse to crystal balls and the like (Lugg 1987, pp. 227-228). Naturopaths and Clairvoyants, however, would doubtless insist that these criticisms are false, and thus Lugg is granting himself premises that can only be established on *empirical* grounds. Certainly it cannot simply be assumed that the audience to a demarcation debate would accept such unsupported assertions against the protestations of advocates of the 'theories' in question, and so an empirical debate is unavoidable. Ultimately, any conclusion can be (seemingly) justified if empirical matters are ruled out of consideration as irrelevant, as there is then nothing to prevent the use of entirely false premises. In reality, it is only because of the pressures arising from empirical realities that 'pseudoscientists' would ever be forced to resort to problematic practices.

Lugg has also provided no means of distinguishing between the problematic practices that supposedly characterize 'pseudoscience' and those that appear regularly within accepted science. Arguments in support of preliminary, controversial, flawed or bad science can always be similarly presented as involving 'logical fallacies' which blind their advocates to the significance of any justified objections. A critic of Continental Drift, for example, "... pointed to specific flaws in Wegner's palaeontological case but went on to assign those flaws to methodological failures" (Le Grand 1988, pp. 72-73).

Even when Wegner was correctly charged with 'conceptual flaws' such as cherry-picking his evidence (Le Grand 1988, pp. 62-63, 65, 71, 73), such behavior on his part did not disqualify Drift as a scientific hypothesis or establish that his defenses of Drift were not science at all. Medical research (especially regarding nutrition) is often criticized as failing to conclusively establish causation rather than mere correlation, just as Lugg says of Naturopathy, yet this is not taken to mean that further research on such hypotheses should be forbidden as 'pseudoscientific'. Ultimately, hypotheses supported by flawed practices may nonetheless be of scientific value, and indeed may become established science, and thus cannot be dismissed as 'pseudoscience' on such a basis alone.

2.1.vii) Lugg 2

Lugg made another attempt five years later (1992), in which he employed similar yet importantly different arguments. As previously, Lugg denied that there is any single flaw within all 'pseudoscience', and insisted that the difference is not just that 'pseudoscience' is not empirically well founded (Lugg 1992, pp. 93-94, 97). Rather, Lugg now asserts that 'pseudoscience' is "nonsense", "…far worse than bad science": that it should be regarded as "…transgressing the limits of intelligibility" in such a way that "…it makes no sense to speak of [it] as correct or incorrect" (Lugg 1992, pg. 91). He characterizes 'pseudoscientific' claims as "an empty play of words" that "…do not (for whatever reason) convey information" (Lugg 1992, pg. 94), and asserts that "…it is pointless to attempt to verify, confirm, compare them with the phenomena" because "…verification is out of the question, there being nothing in the case of such claims to be deemed true or false, to count as information" (Lugg 1992, pg. 95), and thus the difference between science and pseudoscience "…is the difference between saying something and saying nothing, between… making sense and lapsing into incoherence" (Lugg 1992, pg. 94).

However, Lugg emphasizes that he is not claiming the above to be true of 'pseudoscientific' hypotheses themselves, or "in isolation" (Lugg 1992, pg. 95). "The trouble is not that the pseudoscientist's utterances involve meaningless terms or combine meaningful ones in an illegitimate way…". Rather, "…it is the practitioners that are at fault…". "…Pseudoscientists put their ideas forward in such a way as to deplete them of sense…", and thus otherwise intelligible claims are "…meaningless when urged by pseudoscientists". "…We cannot understand what is being argued, what the individuals involved are driving at", because they "…fail to convey information", and "…do not as a matter of fact have a point" (Lugg 1992, pp. 96-97).

Lugg attempts to further explain his argument through maintaining that 'pseudoscientific' claims "…lack sense when considered in the particular contexts in which they occur" (Lugg 1992, pp. 95-96). The advocate of a pseudoscientific claim, confronted with scientific objections, "cannot allow it to be considered in this context, on pain of having it rejected as scientifically untenable…" and thus "…must demand that it be considered in a nonscientific context (and hence fail to make a scientific point), or demand that it be considered apart from any context (and hence fail to make a point of any sort)" (Lugg 1992, pg. 99). Thus, according to Lugg, pseudoscientists advance claims that only make sense within a scientific context, yet "To avoid submitting their views to scientific scrutiny they cannot but place them outside the confines of science…" (Lugg 1992, pp. 99-100). 'Pseudoscientists' therefore "…must pretend that their views are genuinely scientific whenever they can, and deny that they should be regarded as such whenever they are challenged on scientific grounds", in which case they "…resist any attempt to have what they are proposing evaluated scientifically" (Lugg 1992, pg. 100)

This latter assertion - that all 'pseudoscientists' shy away from any scientific assessment of their propositions - is simply and demonstrably mistaken, and thus again reveals a fundamental lack of understanding of the topic. Lugg fails to appreciate the manifest reality that the vast majority of 'pseudoscientists' are quite certain that their hypotheses are correct, and that this fact would be established beyond doubt if only they could prevail upon scientists to take the subject seriously (Shermer 2013, pg. 221; Gordin 2012, pg. 1). Therefore, far from demanding that their claims be assessed outside of a scientific context, they explicitly and repeatedly demand that adequate scientific resources be devoted to researching them (see for example Kelleher & Knapp 2005, pg. 207; Hynek 1972, pp. 207ff.; Jacobs 2000, pp. 1, 2, 3, 5, 6; Appelle 2000, pp. 10, 17; Vallee 1965, pp. 11-13, 116ff.; Long 1990, pp. ix-x; Gordin 2012, pg. 5). In the meantime, they do their best to undertake scientific research themselves, with the meagre resources available to them, often with the principle objective of enticing scientists (or *other* scientists, as they may see it) to examine the relevant data. They do *not* fear that allowing their claims to be assessed in a scientific context will lead to their being "…rejected as scientifically untenable…", and again, because they are clearly fully convinced of the veracity of the hypotheses they advocate, this is utterly unsurprising.

Lugg's contention that 'pseudoscientific' claims are incoherent, convey no information, and have no point, can be similarly dismissed. Without doubt there are many individuals who spout nonsense of one sort or another, but to maintain that all 'pseudoscientific' claims lack any point or informational content is simply not credible. There is no difficulty whatsoever in understanding the 'point' or the meaning of the claim that Bigfoot exists, or that extraterrestrials are abducting human test subjects, or that a particular medical intervention is efficacious, nor is there any difficulty in understanding the reasons adduced for regarding these claims as accurate (Ladyman 2013, pg. 53).

Of course, Lugg recognizes that 'pseudoscientific' claims at least *appear* to be meaningful, and argues that the lack of genuine content can be hard to demonstrate. "Generally", Lugg writes, "the task of showing that a pseudoscientist's remarks lead nowhere will be far from easy. Pseudosciences of the sort to which people devote their lives… cannot be disposed of by raising a few simple questions" (Lugg 1992, pg. 97). He maintains rather that to reveal their true nature, we must trace how 'pseudoscientists' try to give the impression they are saying something intelligible (Lugg 1992, pg. 97), and pay attention to the "…complex network of moves and countermoves to which they resort…" (Lugg 1992, pg. 98).

Although Lugg does not attempt to apply his argument to any actual examples of 'pseudoscience', there are numerous indications of what he specifically has in mind in insisting that the behavior of pseudoscientists renders their claims unintelligible. He writes that 'pseudoscientists' "…are to be blamed for treating key conceptions irresponsibly, for adopting a policy of dismissing recalcitrant facts as exceptions and for deploying other stratagems that preclude their saying anything of substance" (Lugg 1992, pg. 97). He refers to propositions that are: "…put forward in a manner that renders them vacuous", "…presented along with remarks that contradict them", or "…logically

disconnected from the evidence that is alleged to confirm them" (Lugg 1992, pg. 97). He further indicates that 'pseudoscientists can be criticized "…for having invoked techniques that ensure that the required results are obtained come what may, or for having asked their contemporaries to accept their claims, regardless of whether they make sense" (Lugg 1992, pg. 98). Lugg also repeatedly refers to a hypothetical 'meaningless' hypothesis, proposed by Moritz Schlick, in which it is claimed that there is within every electron an undetectable nucleus which has no observable effects (Lugg 1992, pp. 92, 97). Lugg maintains that, although the terms are intelligible, advocacy of such a position fails to convey any information (Lugg 1992, pp. 92, 97), and thus treats this as an example reflecting the meaninglessness of 'pseudoscience'.

It is evident from the last example, in particular, that Lugg's insistence that 'pseudoscientific' claims are 'meaningless' to the extent that they "…fail to convey information" etc., is overstated. Rather, what he apparently has in mind is that such claims, like the hypothetical electron nucleus, have no *specifiable empirical* consequences. However, other than propositions which truly are so vacuous as to have no comprehensible meaning (clearly not an accurate characterization of all 'pseudoscientific' propositions), none of the features referred to has the effect of rendering the relevant claims devoid of empirical meaning, but rather *require* cognizance of such meanings. References to 'contradictory' claims or 'logically disconnected' facts are themselves contradictory if we simultaneously hold that we do not know what is being asserted, or indeed that nothing is being asserted. Likewise, 'recalcitrant facts' and 'required results' are terms that cannot be applied to the hypothetical electron nucleus or any similar hypothesis. 'Key conceptions' are yet more clearly a non sequitur in the context of hypotheses that supposedly lack intelligibility. Finally, the proposed demand that claims be accepted, "…regardless of whether they make sense" is a baseless accusation for the reason previously noted – 'pseudoscientists' expect people to accept their claims because they firmly believe that the evidence, properly appreciated, supports doing so. Hence 'pseudoscientists' make no demands regarding claims that 'makes no sense', because they obviously do not accept that characterization of any claim they advocate.

Evidently, then, Lugg should be understood as arguing that 'pseudoscientists' employ 'stratagems' which result in their claims having no *fixed* meaning: that they alter and adapt their claims as necessary to avoid criticisms, and thereby render them at least ephemeral. Once again, this appears to reveal a lack of any broad knowledge of 'pseudoscience' on Lugg's part. There may be some 'pseudoscientists' who behave in roughly the manner he is discussing, but the details of most 'pseudosciences' are well established and actually change relatively little – hence the criticisms on that basis above and below. This is clearly evidenced by, for example, the entries in publications such as the *Encyclopedia of Pseudoscience* (Williams 2000), which provide a perfectly clear description of each 'pseudoscientific' claim, the major reasons advanced for believing it, and the common responses of debunkers, without the need for the many caveats that would be required if Lugg's description were accurate. Most of these entries are little different than they would have been if penned decades earlier, and remain accurate now, seventeen years later.

Overall, 'pseudoscientists' behave much as do scientists who are strongly convinced of the truth of the claims they espouse - they alter their core positions very little, as they are firmly convinced they do not *require* alteration. Bigfoot researchers, for example, find it literally *incredible* to suggest that so many reliable witnesses could have been entirely mistaken regarding what they have experienced, and yet so often report very similar details. This perceived strength of the supporting evidence ensures they feel little need to employ any 'stratagems' other than emphasizing that evidence. The arguments against their position also have not changed in any significant detail for decades, and are not regarded by the 'pseudoscientists' as particularly troublesome (they believe they have adequately answered them), so again, there is no perceived need for Bigfoot hunters to employ *any* 'stratagems'. Likewise, Ufologists are entirely convinced that their evidence is demonstrably on their side, and that the objections of skeptics are

based on ignorance and so simply unconvincing[34]. Thus again there has been little change in the *core* of Ufology for decades. Essentially the same applies to poltergeist researchers and other Parapsychologists, many 'alternative' therapists, and many other 'pseudosciences'.

Lugg this time explicitly states that he is *not* seeking to demarcate specifically *hypotheses*, as he did previously, but does not then reveal his intended 'unit' of demarcation. Given his focus on the manner in which claims are advanced by 'pseudoscientists', he is most charitably understood as aiming to demarcate the methods employed within their *arguments*, or specific iterations of such arguments, and so presumably individual *publications*, which are the publicly available representation of the same from which any evidence of the methods employed must be taken (Lugg 1992, pg. 95-97).

2.1.viii) Derksen

A. A. Derksen (1993) also rejects hypotheses as the proper 'unit' of demarcation, but turns instead to 'pseudoscientists' themselves[35]. Derksen argues that 'pseudoscientists' are the proper focus of demarcation "…because it is a person, and not a theory or field, who can have scientific pretensions, and who can be blamed for not making good these pretensions" (Derksen 1993, pg. 21). He further explains that, if we seek to demarcate a theory "…we would in any case have to relativize that judgement to the person who makes use of that theory, as the theory may be used and modified in any number of given ways". For example, Freud's theory, advanced by another theorist, could be modified such that there is "…nothing wrong with it…apart, perhaps, from its being false" (Derksen 1993, pg. 21).

Derksen maintains that it is difficult to discern the difference between science and 'pseudoscience' because the 'pseudoscientist' "…pretends to be, and often aims to be, a scientist" (Derksen 1993, pg. 19). However, the "scientific pretensions" of 'pseudoscientists' also mean that they acknowledge standards that can therefore reasonably be used to critically assess their claims. Specifically, two "yardsticks" - "reliable knowledge" and "human fallibility" (Derksen 1993, pp. 19, 37), give rise to the 'Seven Sins of the Pseudo-Scientist': S.1[36].'The Dearth of Decent Evidence'; S.2. 'Unfounded Immunizations'; S.3. 'The Ur-Temptation of Spectacular Coincidences'; S.4. 'The Magic Method'; S.5. 'The Insight of the Initiate'; S.6. 'The All-Explaining Theory'; and S.7. 'Uncritical and Excessive Pretensions' (Derksen 1993, pp. 21-37). Derksen discusses these criticisms as they relate to Freud and Bach-kabbalists[37].

Derksen is inconsistent regarding precisely how these 'sins' are to be applied in identifying 'pseudoscience'. Recognizing that all of the sins can occur within genuine science (Derksen 1993, pp. 19, 20, 37, 38), he twice states that 'pseudoscience' includes all seven (Derksen 1993, pp. 20, 38), but this position is then contradicted by a number of other statements. He writes, for example, that S.4 is only "often" applicable (Derksen 1993, pg. 30), and also that the "…sixth sin… does not occur in all pseudo-sciences", and indeed does not apply to Bach-kabbalists (Derksen 1993, pp. 35-6). He also suggests that S.7 alone "…may well be… the sin that in the end separates the pseudo-scientist from the scientist"

---

[34] See below

[35] Derksen directly addresses Laudan's 1983 conclusions, though only very briefly. He indicates that he agrees with Lugg that the difference between science and 'pseudoscience' is more than just their degree of epistemic warrant (Derksen 1993, pg. 18), presumably (he doesn't explicitly say) because he believes the methods employed by 'pseudoscientists' are key criteria. He also explicitly rejects the position that demarcation is a 'pseudo-problem' (Derksen 1993, pp. 17-38), but provides no response to Laudan's arguments themselves, rather simply noting that Popper, Thagard and Lugg do not agree (Derksen 1993, pg. 19), and again presumably because he believes his own arguments demonstrate otherwise. Finally, he asserts that, as everybody makes mistakes, we cannot reasonably expect to find necessary and sufficient criteria that distinguish science from pseudoscience (Derksen 1993, pg. 20). However, this latter point is in agreement with, rather than contrary to, Laudan's position.

[36] S.1-S.7 are my notations.

[37] This refers to the practitioners of 'Bach-kabbalistics', "…an alleged scientific approach aimed at the discovery of hidden, significant numbers in Bach's compositions" (Derksen 1993, pg. 20)

(Derksen 1993, pg. 36), and alternatively, that the two "yardsticks" represent "…two deadly sins, viz. the failure to have good reasons for one's claims and the failure to be critical about one's own achievements", while the 'Seven Sins' are merely ways in which these two failings can present themselves (Derksen 1993, pg. 37).

Leaving that issue aside, it is evident that there are significant problems arising from the excessive interconnectedness of the seven sins and their applicability to accepted examples of science and 'pseudoscience'. Derksen recognizes that the proposed sins are not independent, writing that S.1, 'The Dearth of Decent Evidence', is problematic only in the context of an "…excessive, uncritical pretension of reliable knowledge (the seventh sin)…" (Derksen 1993, pg. 23); that 'Unfounded Immunizations', S.2, "…are unfounded due to the dearth of decent evidence", S.1 (Derkseen 1993, pp. 24-25); and that S.5, 'The Insight of the Initiate', is an example of 'Unfounded Immunizations', S.2 (Derksen 1993, pg. 33). It is also evident that S.3, 'The Ur-Temptation of Spectacular Coincidences', which Derksen explains refers to the problem of determining when an abductive argument is reasonable, is problematic only if there is an alternative explanation which is as plausible as the one being advocated (Derksen 1993, pp. 27-30), and thus refers again to S.1, 'The Dearth of Decent Evidence'. Likewise, S.2, "Unfounded Immunizations', S.4, 'The Magic Method', and S.6 'The All-Explaining Theory' all refer in slightly different ways to the same basic criticism. Moreover, and critically, Derksen acknowledges that "…the question of how well a theory is confirmed is crucial for the judgment about its pseudo-scientificality: pseudo-scientific sins like temporary disrespect for experience, immunizations and the insight of the initiate do not damage a well confirmed theory. Put paradoxically, the sins of pseudo-science lose their sinfulness within science" (Derksen 1993, pg. 38). Thus, ultimately, it is the empirical virtue of a hypothesis that determines whether or not it is 'pseudoscience', and the seven sins are just different ways of referring to the same basic flaw or flaws – a lack of supporting evidence and the resultant responses of advocates, which are only significant if we already judge the hypothesis to be unfounded.

As a result, the arguments proposed to demarcate 'pseudoscience' (and thus the advocates of such as 'pseudoscientists') are yet again identical to those employed by scientists in their highly complex disputes regarding which scientific hypotheses provide the best explanations for the relevant evidence, and which should be regarded as refuted. The characteristics Derksen refers to as 'sins' occur regularly within genuine science, as Derksen recognizes, and may justifiably be applied by scientists to their scientific opponents, whilst the interconnectedness of the sins results in the likelihood that, if one is believed warranted, so too will many of the others.

Exceptions to the above – those sins that cannot be more or less automatically applied to any advocated hypothesis held to be empirically challenged – also fail to apply to numerous paradigmatic 'pseudosciences'. Derksen has been deceived by the fact that the two examples he exclusively utilizes – Freud and the Bach-kabbalists – are both process centered activities, whereas many other 'pseudosciences' are not. Derksen notes that 'The Magic Method' and 'All-Explaining Theory' do not apply to all 'pseudosciences', and indeed Ufology, Cryptozoology, poltergeist research and many other phenomena based 'pseudosciences' include nothing comparable to Freud's highly flexible heuristic. 'The Insight of the Initiate' is manifestly even less broadly applicable.

Even if none of these objections applied, however, there is a more fundamental flaw in Derksen's proposal - his intention to demarcate 'pseudoscientists'. *Prima facie* this makes some sense, particularly considering the logical gaps identified in some efforts to demarcate topics or hypotheses, utilizing arguments that refer directly to the activities of proponents. However, just as it is possible for the same basic hypothesis to be advanced by numerous proponents in importantly different ways, so too may a single individual advance significantly different arguments in support of the same, or multiple, hypotheses. It is particularly noteworthy that some paradigmatic 'pseudosciences' have been advocated by scientists who are successful and well respected in their professional lives. Dr. J. Allen Hynek,

professor of astronomy at Ohio State University, and subsequently chairman of the astronomy department at Northwestern University, and Dr. James E. McDonald, associate director and then professor at the University of Arizona's Institute of Atmospheric Physics, are among the best known Ufologists, whilst the editorial board of the *Journal of the Psychical Research Society*, which investigates, among other things, poltergeists and ghostly apparitions, spirit mediums who claim to be in contact with the dead, and the various manifestations of ESP (telekinesis, telepathy and clairvoyance), is a veritable alphabet soup of formal qualifications.[38] Even if criteria could be found to successfully establish that such individuals at times act as 'pseudoscientists' and produce 'pseudoscience', it cannot be reasonably denied that they also produce genuine science and act as scientists in their professional lives. Thus the most that could be established is that these individuals are 'pseudoscientists' when they are doing 'pseudoscience', and scientists when they are doing science. This is obviously unhelpful.

Moreover, the basis of any such demarcation must be the publications individuals produce: in order to conclude that an author is a 'pseudoscientist', it must first be found that their publication is 'pseudoscience'. The argument then moves to the demarcation of the author of the publication. However, we cannot *directly* believe or reject an individual, and must therefore return again to their publication, and, because the same individual can be responsible for both scientific and 'pseudoscientific' pronouncements, we cannot conclude that all of their publications are 'pseudoscience'. Hence, we are justified only in concluding that the publication which the initial assessment found to be 'pseudoscience', is 'pseudoscience', because it was authored by someone acting as a 'pseudoscientist' at the time they produced it. Plainly, then, the demarcation of individuals as 'pseudoscientists' gains us nothing beyond the original assessment of their publication, and so is again unhelpful in terms of guiding beliefs. If, as Derksen suggests, only 'pseudoscientists' can be demarcated, such that there is no justifiable application of the term 'pseudoscience', the same argument applies - we need only replace 'pseudoscience' in the foregoing argument with 'the product of a pseudoscientist'. In this case, the move to demarcating the 'pseudoscientist' would technically be essential for the justification of the terms employed as there is no other means of applying demarcation, but the *practical* applications remain the same.

2.1.ix) Pennock

Returning now to Robert Pennock (2011), mentioned above in relation to his 'defense' of Ruse's arguments[39], his paper contains two further sets of arguments: 1) those directed against Laudan's rejection of the demarcation problem, and 2) arguments defending his proposal that Methodological Naturalism (MN) can be employed to demarcate Intelligent Design Creationism (and any other 'pseudoscience' that appeals to supernatural explanations) from genuine science.[40]

Pennock's objections to Laudan's arguments reveal that he has, at best, grossly misunderstood the latter, so producing criticisms that are entirely misplaced. I believe the evidence, presented below, strongly supports the contention that Pennock is being less than honest. This would presumably, again, be regarded as acceptable because his objective is to combat the evil of Creationism, and so provides a

---

[38] For example, the Editorial Board for Vol. 74.4, No. 901, Oct. 2010 is: Prof. Deborah L. Delanoy, B.A., Ph.D., Carlos S. Alvarado, B.S., M.S., M.A., Ph.D., Pfof. Stephen E. Braude, Ph.D., Richard Broughton, B.A., Ph.D., Prof. Bernard Carr, M.A., Ph.D., Prof. David Fontana, B.A., M.Ed., Ph.D., F.B.Ps.S., C. Psychol., Alan Gauld, M.A., Ph.D., D.Litt., James Houran, M.A., Ph.D. Prof. Adrian D. Parker, M.A., Ph.D., Prof. John Poynton, M.Sc., Ph.D., Adrian P. Ryan, Stefan Schmidt, Ph.D., Paul Stevens, B.Sc., Ph.D., Prof. Donald J. West, M.D., Litt.D., F.R.C.Psych., Carl Williams, B.A., Ph.D., Robin Wooffitt, B.Sc., Ph.D. The Editor is Christopher Roe, B.Sc., M.Sc., Ph.D., A.F.B.Ps.S. The Review Editor is Tom Ruffles, B.Sc., B.A., M.A., Ph.D., A.R.P.S., and the Production Editor is David Ellis, M.A.

[39] You may recall that, despite his heated condemnations of Laudan, he did not actually argue that Laudan had been wrong and Ruse right.

[40] Pennock does also discuss other reasons for disqualifying Intelligent Design Creationism, specifically, from science, but these are of little relevance in terms of demarcation generally.

further example of the willingness of some combatants (as they clearly see themselves) to dispense with the ordinary rules of academic disputation when dealing with 'pseudoscience'. To what extent such attitudes are displayed by other authors discussed I will largely leave to the reader – such suspicions cannot generally be confirmed. However, I take the time to establish this point here because such activities seem to be a significant feature within the demarcation and debunking movements (the truth of this in regard to the latter will be more firmly established later).

Pennock has two complaints against Laudan upon which all his counter-arguments are based. Firstly, he repeatedly maintains that Laudan claimed, in 1983, that philosophers in general regarded the Demarcation Problem as 'dead'. To counter this, Pennock establishes that demarcation has remained a live issue, and provides supporting evidence in the form of lists of subsequent demarcation proposals and criticisms of Laudan's paper (Pennock 2011, pp. 202, 181ff). However, not only did Laudan advance no such claim, but it simply lacks credibility to suggest that anyone could reasonably gain such an impression from reading his article. It is an unmistakable premise of Laudan's entire argument that there then existed a 'New Demarcationist Tradition' (the heading, in large, bold type, of the penultimate section of the paper – Laudan 1996a, pg. 218) which he is arguing *should be* abandoned. The entire publication would make no sense if this were not the case, as it would be utterly pointless to argue against a position already abandoned. Laudan's conclusion makes this entirely clear: to write "…we *ought to* drop terms like 'pseudo-science' and 'unscientific' from our vocabulary…" followed by a suggestion of where our "…focus *should* be…" [emphasis added] makes no sense whatsoever if such terms had already been dropped and the focus already shifted (Laudan 1996a, pg. 222). Hence Pennock's complaint: "It would be one thing had Laudan simply been describing his own view or giving his judgement that philosophers *should* give up the demarcation problem as dead, but he wrote as though he were stating a historic fact" (Pennock 2011, pg. 181), is utterly without foundation – Laudan wrote *precisely* as Pennock says he ought to have, clearly stating his conclusion in terms of what 'should' occur.

Pennock also claims that Laudan's 1983 conclusion was supported by "…two main lines of argument", the first being "…to appeal to the *lack of unity* among philosophers regarding proposals for criteria of demarcation" [emphasis added] (Pennock 2011, pg. 191). This is a gross misrepresentation. Laudan *unmistakably* addressed himself to demonstrating that extant demarcation criteria *failed* to meet basic requirements for success. The fact that demarcators disagreed regarding which criteria to use had no real role in his argument (Laudan 1996a, pp. 215-221). Nonetheless, to support his claim, Pennock provides a quote, including in part "…Whatever the specific strengths and deficiencies of the numerous well known efforts at demarcation… it is probably fair to say that there is no demarcation line between science and non-science, or between science and pseudo-science, which would win assent from a majority of philosophers" (Pennock 2011, pg. 191 – citing Laudan 1996a, pg. 210). The lines immediately following where Pennock's quote ends unambiguously reveal Laudan's true focus: "Nor is there one which *should* win acceptance from philosophers or anyone else… What lessons are we to draw from the recurrent failure of philosophy to detect the epistemic traits which mark science off from other systems of belief?" (Laudan 1996a, pg. 211). Pennock nonetheless goes on to 'rebut' the significance of "…the lack of a 'settled consensus'", and so ignores completely Laudan's true contention that there are no *successful* demarcation criteria (Pennock 2011, pp. 191-192).

Moving on to Laudan's second "main line of argument", Pennock asserts that "Laudan's second, more substantive, approach is to ask whether *McClean* accurately captured how science works" (Pennock 2011, pg. 192). Pennock also refers to the 1983 article as part of a 'triptych' alongside the 1982 articles contra Ruse (Pennock 2011, pg. 191). However, Laudan's 1983 paper makes *no reference whatsoever* to his earlier dispute with Ruse or the *McClean* case. Thus, for Pennock to here present this as Laudan's second 'main argument' supporting his 1983 conclusion is again utterly mistaken, and rather inexplicable as an honest mistake.

Pennock then completes his presentation of Laudan's arguments, having made no mention of Laudan's true key contention (described in the introduction), with a fabricated conclusion: "These considerations lead Laudan to conclude that there is no sensible distinction between science and pseudo-science" (Pennock 2011, pg. 192). This constantly repeated assertion (Pennock 2011, pp. 182, 192, 193, 195, 196, 203) – that Laudan maintains there is 'no difference' between science and 'pseudoscience' - is Pennock's second major objection to Laudan's argument, and as baseless as the first. Pennock opposes this conclusion with a number of arguments. 'The Perversity Argument' is summed up with "To hold that there is no useful conceptual difference between science and pseudoscience is to lose touch with reality in a profound way" (Pennock 2011, pp. 195-196). 'The Pragmatic Argument' points out that, for example, we have separate departments in universities for science and religion, and know which employees belong to each etc., and thus there must be a difference which we can in practice recognize (Pennock 2011, pp. 196-197). 'The Empirical Argument' contends: "When we look empirically at what scientists and science educators themselves say science is, then we see immediately that they all ignore Laudan and clearly operate on the idea that there is a real distinction between science and non-science". Pennock then gives "…an outline of some of what Laudan had to ignore in his anti-demarcationism screed", going on to provide a number of statements relating to Methodological Naturalism, all of which post-date Laudan's article[41], and relate to a topic Laudan did not mention (Pennock 2011, pp. 197-198).

Of course, as previously described, Laudan's core argument was that demarcation confounded two non-coterminous issues – scientific status and epistemic virtue (Laudan 1996a, pg. 221-222). Laudan never denied that there is a difference between science and *religion*, as Pennock repeatedly insists (2011, pp. 195, 196, 199), nor did he deny that there is a difference between science and non-science – he directly discusses non-sciences (literary theory, carpentry, football) in terms of the features they share with science (Laudan 1996a, pg. 220). Laudan also explicitly refers to 'scientific status' (whether or not something is 'science') as a genuine issue that can be discussed by philosophers, and can, therefore, be confounded with another, discrete issue – he just judges it to be uninteresting *in the context of addressing empirically unsupported hypotheses* (Laudan 1996a, pp. 221-222).

Finally, Laudan's conclusion explicitly asserts that there is a *crucial* difference between those things Pennock has been referring to as 'science' and 'pseudoscience', and that we should *focus on that difference*. That he does not share Pennock's *vocabulary* in distinguishing them in no way indicates that he does not share Pennock's *interest* in distinguishing them, as he is at pains to explain in his concluding statements. Pennock presumably read Laudan's explanation that "I am manifestly not denying that there are crucial epistemic and methodological questions to be raised about knowledge claims, whether we classify them as scientific or not. *Nor, to belabor the obvious, am I saying that we are never entitled to argue that a certain piece of science is epistemically warranted and that a certain piece of pseudoscience is not*" [emphasis added] (Laudan 1996a, pp. 221-222). Pennock thus charges Laudan with contending that there is 'no difference' between science and religion, or good science and Creationism, entirely, and baselessly, because he held that "The 'scientific' status of those claims is altogether irrelevant" (Laudan 1996a, pg. 222).

Another argument Pennock offers as a counter to Laudan is a supposedly undeniable demarcation claim, which he titles 'The Dustbin of History Argument', and which he believes demonstrates that the demarcation of non-science *must* be possible. In particular, he believes it reveals the flaw of Laudan's argument that Creationism is science because it has been tested and refuted (not that this was actually something Laudan advocated – he made this point only as an objection to the claim that Creationism could be demarcated as 'untestable') (Pennock 2011, pg. 193). Pennock proposes of certain well-refuted

[41] It is hard to imagine a more unfair accusation – In writing his 'screed', Laudan has wantonly 'ignored' material published more than a decade later!

hypotheses from the past, such as geocentric cosmology, that, although they are historically scientific and testable, "…it is unscientific to continue to hold and teach [them] today", and that a scientist confronting someone doing so would be justified in telling that person "…you are not doing science…", because "The scientific picture of the world does not include claims that have been decisively refuted and effectively relegated to the dustbin of history", and "…sufficiently bad science is not science at all" (Pennock 2011, pp. 193-194).

However, we know that these examples are 'bad science' precisely because they have been *scientifically refuted*, via typical scientific processes, not 'demarcated' via any method that shows them to be truly beyond science altogether, and we would have to appeal to similar scientific arguments in demonstrating to anyone advocating them now that these hypotheses are false. Clearly the individual refuting the claim would thereby be 'doing' science, and it cannot be that *they* are 'doing science' in relation to a claim which is *not science at all* relative to the individual maintaining it – their dispute is clearly *scientific* and *about science*.

Regardless, the point is quite useless in terms of practical demarcation anyway, and irrelevant to Laudan's claims against the possibility of successful demarcation criteria, because Pennock has not actually suggested any *criterion* allowing us to demarcate these 'dustbin' hypotheses – he relies on the fact that it is common knowledge that they have been *refuted*. An appeal to common knowledge is utterly unhelpful in terms of *demonstrating* that a target, believed to be true by a large percentage of the public, and actively defended as true, is 'pseudoscience', and the argument that such a position is 'such bad science that it is not science at all' is certainly not going to sway its believers and advocates. The issue of maintaining that 'pseudoscience' is a class of especially bad science will be discussed further below.

2.1.x) Methodological Naturalism

Pennock's final major set of arguments are directed towards defending Methodological Naturalism (MN) as a demarcation criterion. Although Pennock devotes considerable time to establishing the widespread acceptance of MN (Pennock 2011, pp. 185ff), its true status is in fact subject to lively philosophical debate, with many contributors on both sides[42] (see for example Pennock 1999, pp. 181ff; Plantinga 1997; Forrest 2000; Fishman 2009; Fishman & Boudry 2013; Boudry 2013, pp. 83ff), and the force of the arguments clearly favours those opposing Pennock's position. This is illustrated in the discussion of Boudry, Blancke and Braeckman (hereafter B, B & B) (2010), who critically examine the five major arguments advanced in favour of what they refer to as 'Intrinsic Methodological Naturalism' (IMN) – the position that MN is an intrinsically necessary requirement of science. In opposition to this, they propose an alternative - 'Provisory' or 'Pragmatic Methodological Naturalism' (PMN) - according to which MN has been adopted because of its explanatory successes and the historical sterility of supernatural explanations. PMN, being based on scientific evidence, can in principle be suspended if a supernatural claim is deemed worthy of consideration, or even abandoned should any supernatural explanation ever prove well founded (Boudry, Blancke & Braeckman 2010, pp. 227-230).

The first argument for IMN is that science *by definition* cannot assess supernatural explanations: science deals only with natural laws and processes, and thus anything detectable to science must be natural. If any cause previously referred to as 'supernatural' has, therefore, verified empirical effects, then it *cannot* be 'supernatural' after all, regardless of any other considerations, including, for example, properties universally regarded as characteristic of 'supernatural' entities. Everything we traditionally refer to as 'supernatural' – ghosts, angels, demons, gods etc. – if they have any scientifically detectable

[42] Pennock is clearly aware of this, given the large number of relevant articles in the book he edited - *Intelligent Design Creationism and its Critics* - including his own debate on the issue with Phillip Johnson (Pennock 2001).

effect on the universe, would thus be held to have been 'natural' all along. This renders IMN trivial in terms of demarcation, as whether a proposed explanation is *truly* supernatural has nothing to do with whether the proponents use that term (and they would have to be using it improperly, given that they must be arguing that there *is* empirical evidence supporting the reality of their explanation), or whether the term has traditionally been applied to causes of that type, but depends entirely on whether it is a scientifically verifiable explanation or not, which of course cannot be determined without scientific investigation. Thus, in regard to Creationism, if God indeed intervened in the material universe, scientific evidence of this would be by definition evidence of a *natural* cause, and so God would be a natural cause, and Creationists therefore should not be understood as intending to propose a supernatural explanation after all (Boudry et. al. 2010, pp. 230-233).

A second argument for IMN maintains that to allow for the possibility of the supernatural within science undermines the assumptions necessary for its practice. The supernatural is that which can violate natural laws, and thus to accept the possibility of supernatural intervention compromises the assumption of lawful regularity essential to all scientific research. Further, it is argued that acceptance of the supernatural requires acceptance that anything logically possible could actually happen (Boudry et. al. 2010, pp. 233-234). As B, B & B point out, those raising such objections "…do not explain why *any* occurrence of supernatural intervention in the natural realm would necessarily frustrate *all* experimental work or automatically "destroy the stability of science". That anything logically possible can become an actuality is not as dramatic as it seems – in fact, it just restates the definition of logical possibility" (Boudry et. al. 2010, pg. 234).

To add my own interjection here, theists and others who actually *believe* in supernatural explanations also believe that we live in a universe by and large dominated by regular, lawful interactions – that supernatural interventions are rare exceptions. Thus scientist theists who believe in the existence of God, demons, and angels, do not on that account assume the latter's interventions will render any and all experimental processes pointless. If it were to be proven that they were correct - if we were to discover that supernatural entities existed and were both willing and able to *occasionally* interrupt the normal functioning of the universe - this would clearly be no reason to give up on science altogether. In many ways, it would be no different than accepting the fact that there are human saboteurs, hoaxers and mischief makers (including conceivably some supported by sophisticated agencies who could employ unknown technological devices to interfere in ways the experimenters cannot conceive of) who can and do occasionally interfere with experimental assemblies. Indeed, both the human and supernatural factors are merely additions to the existing reality of unknown numbers of potential confounding variables which occasionally lead to failed experiments and/or unreliable results. This is why all scientific work is already understood as necessarily limited by a *ceteris paribus* clause – a reality science deals with through experimental repetitions etc.

The third argument B, B & B address is the 'Science Stopper' assertion: the claim that allowing supernatural explanations puts an end to further scientific endeavor, as an 'easy' appeal to a supernatural explanation is always to hand. The authors argue that this need not be the case, as it would often be evident, at least to earnest inquirers, that further effort may produce a different, and true, explanation, given the track record of science in generally discovering a natural cause – a fact which would not be changed through the admission of a supernatural explanation in some few cases (Boudry et. al. 2010, pp. 236). Again, the fact that theist scientists, who believe supernatural causes are a real possibility, nonetheless do not typically appeal to them in their scientific work, demonstrates the unfoundedness of this concern.

Fourth, B, B and B discuss a 'Procedural Necessity' argument. Their discussion of this is rather complicated, however, by the fact that the interlocutors they discuss have adopted an ambivalent posture, indicating both that they believe science has provided evidence of the truth of Naturalism, through the

many successes achieved through assuming MN and, historically, demonstrating the failure of supernatural explanations, and yet also that the supernatural should be understood as that which is beyond the reach of science – returning to the first 'natural by definition' argument – and thus the evidence of science would be the same regardless of whether Naturalism were correct or not. However, this does not go very far to explaining the 'Procedural Necessity' argument.

The article B, B and B refer to by Barbara Forrest (2000) is intended to examine the relationship between IMN and Philosophical Naturalism (or Metaphysical Naturalism). Many defenders of MN insist that it is entirely distinct from Philosophical Naturalism, and so adopting it as a rule within science is compatible with accepting that supernatural entities truly exist and interact with the physical universe (see for example Ruse 2001). The abstract to Forrest's paper states in part "…philosophical naturalism… is the only reasonable metaphysical conclusion given (1) the demonstrated success of methodological naturalism, combined with (2) the massive amount of knowledge gained by it, (3) *the lack of a method or epistemology for knowing the supernatural*, and (4) the subsequent lack of evidence for the supernatural" [emphasis added] (Forrest 2000, pg. 7). Thus as B, B and B state, Forrest indeed embraces the success of IMN as providing evidence for the truth of Philosophical Naturalism, yet at the same time believes there is a "…lack of a method or epistemology for knowing the supernatural…". However, the question that interests me is her justification for this latter assertion.

Examining Forrest's discussion, it is sometimes far from clear *exactly* what her reasons are for insisting that there can be no knowledge of the supernatural - she provides various quotes in which it is simply asserted that there can be no scientific evidence for the supernatural – that science simply cannot admit supernatural explanations - and these do appear to be assuming this to be so by definition (Forrest 2000, pp. 8, 11-12). However, she does not only insist that *science* cannot permit the inclusion of the supernatural (although she certainly does insist on this), but also refers to "…the unavailability of any method at all for knowing the supernatural…", which clearly goes further (Forrest 2000, pg. 8). One quote she supplies, by Sidney Hook, ends with the statement (referring to science and Philosophical Naturalism): "…both hold that there is no evidence for the assertion of anything which does not rest upon some *observed effects*" [emphasis added] (Forrest 2000, pg. 9). Thus one 'procedural objection' she seems to subscribe to is that the supernatural cannot (or does not) have 'observed effects'. The next quote she includes, by Arthur Strahler, provides an entirely different 'procedural objection': "Supernatural forces… *cannot be observed, measured, or recorded by the procedures of science* – that's simply what the word 'supernatural' means" [emphasis added] (Forrest 2000, pg. 9). Thus far, then, we seem to be back to the definitional objection. However, the statement continues: "There can be no limit to the kinds and shapes of supernatural forces and forms the human mind is capable of conjuring up 'from nowhere'. Scientists therefore have no alternative but to ignore the claims of the existence of supernatural forces and causes". So now it appears that it is the unlimited nature of supernatural possibilities that is the problem (Forrest 2000, pp. 9-10). Another Strahler quote adds: "…the supernatural or spiritual realm is unknowable in response to human attempts to gain knowledge of it in the same manner that humans gain knowledge of the natural realm (*by experience*)…" [emphasis added] (Forrest 2000, pg. 10). Forrest then goes on to discuss statements by Wesley Elsberry, including: "Science is incompetent to examine those conjectures which *cannot be tested in the light of inter-subjective experience or criticism*" and "…humans cannot establish a supernatural cause by experimental reproduction of that cause… the only way to establish supernatural causation is *through the elimination of all natural alternatives*. This is simply another case of proving a negative, which is an intractable problem…" [emphasis added] (Forrest 2000, pg. 12).

Forrest thus advances a number of 'procedural objections' against the possibility of gaining any knowledge of the supernatural. What bothers me most about this discussion is that she simply ignores the fact that there are many intelligent, in numerous cases scientifically accredited, individuals, who believe

they *have* scientifically examined, to the best of their abilities, potential evidence for the supernatural (ghostly apparitions, poltergeist disturbances, demon possession, angelic interventions, miracles etc.) in the manner Forrest is here asserting *cannot* occur – through examining 'observed effects', which they have 'measured' and 'recorded' just as they would any (other) scientific data. Such individuals are thus endeavoring to gain knowledge of the supernatural through gathering evidence which can 'be tested in the light of inter-subjective experience and criticisms'. Leaving aside, as philosophically uninteresting and irrelevant to the issue of demarcating supernatural claims, the possibility of Forrest objecting that any such events must be natural by definition, consideration of the activities of such investigators clearly demonstrate that Forrest's contentions entirely fail to make any contact with the reality of existing supernatural claims and beliefs. From the perspective of individuals writing about investigations into the physical manifestations of what are believed to be supernatural (at least in common parlance) entities, Forrest's arguments would appear to reveal a complete ignorance of the relevant (and extensive) literature most germane to the topic she is discussing – the existence and scientific accessibility of the supernatural. Thus her claims would be regarded, somewhat ironically, as little more than a collection of baseless assertions contrary to the observed and recorded scientific evidence.

The objection that humans can just invent any supernatural claim 'from nowhere' has no real bite, as the same is true of non-supernatural hypotheses – the problem is not with the supernatural aspect of such claims, but that they lack any basis (i.e. come 'from nowhere'[43]). The objection that supernatural claims can only be proven through eliminating all natural possibilities fails to recognize that this is *technically* true of *all* proposed explanations – any specific explanation can only be *proven* true through conclusively ruling out all possible alternatives, which is indeed an intractable problem. Thus, rather than seeking such 'proof', scientists endeavor to establish a 'best explanation'. The only difference with supernatural claims is that we begin by assigning such explanations a very low probability, and thus cannot accept them as the 'best explanation' until all the more likely candidates have been discarded. This is not a *necessary* characteristic of supernatural explanations, and if the history of science had been markedly different, we could well often prefer them as among the *most* likely explanations, whilst any natural explanation that is deemed highly unlikely will elicit the same response. It takes little imagination to come up with circumstances where a supernatural explanation could be reasonably accepted as the most likely. Indeed, imagination is not really necessary – we need only look at the work of those investigating claims of supernaturally caused events, and ask what evidence would be sufficient to establish the truth of that interpretation.

One related 'procedural' type claim that may be advanced by defenders of IMN is that the supernatural, because it is not regulated by natural law, is *entirely unregulated*. Thus no knowledge of supernatural entities, for example, can be gained even if scientific research on their activities were attempted, because no patterns would emerge – all results would be effectively random. However, this would be rather similar to employing the argument that spiritual entities cannot possibly interact with the material world as a basis for rejecting claims regarding creators, poltergeists and demons. In both cases,

[43] Although Forrest provides no examples, and I have never come across anyone advocating a claim that they have simply invented from nothing – there is always some reasoning behind individuals' assertions, even if it is terrible reasoning – so I am not aware of any actual cases where this is a problem. Almost always, supernatural claims relate to entities with a specific title, for example 'poltergeist', which in turn indicates a recognized set of characteristics. Thus both the individuals advancing supernatural claims, and the individuals hearing those claims, can readily understand the differences between the various types of supernatural agents that may be referred to - Angels, Demons, Djinn, Poltergeists, Crisis Apparitions, Doppelgangers, and so on. This demonstrates that those who believe in or investigate supernatural agencies perceive them as a far more orderly, and to some extent already understood, class of beings than Forrest and those she quotes claim could even be possible. Of course, this is not to say that such entities exist or truly exhibit the characteristics ascribed to them. The point is that those who hold the beliefs Forrest is rejecting as necessarily baseless firmly believe that their beliefs *do* have a firm foundation in the *observed* and *measurable* effects of the supernatural on the material world. Forrest, however, writes as though she is entirely ignorant of this reality, and certainly offers no response to such claims.

the response again ignores the true positions held by those who maintain such beliefs. Whatever they believe about 'spiritual' beings, they *certainly* believe they can interact with the physical universe, and thus interpreting 'spiritual' in a manner that precludes this is simply sophistic. Likewise, such believers in various types of supernatural beings do not regard them as utterly unregulated or unconstrained, but rather believe they each have specific, limited powers and comprehensible proclivities, which ensure that their observable effects on the environment *do* reveal patterns and so can provide evidence that can be employed to gain further, more detailed knowledge of these entities' 'true natures', how precisely they produce the physical effects that they do, and perhaps as well their true intentions (for some typical examples, see: Sarchie & Cool 1999, pp. 6, 8, 9-10, 11, 12, 13-14, 15, 16-17; Martin 1976, pp. 9-26; Unger 1952, pp. 62ff.; Lightner 1998, pp. 163ff.; Hallowell & Ritson 2008, pp. 321-324). Thus, to define the supernatural as that which is utterly random and without regularity is another 'straw man' argument with no bearing on the actual beliefs of those advancing claims regarding supernaturally caused events.

The fifth argument B, B and B examine is that of 'Methodological Naturalism and Testability', which has much in common with the matters just discussed. They examine a number of arguments seeking to establish that statements describing the activities of an omnipotent and omniscient being are unavoidably compatible with any empirical evidence, and so concluding that supernatural claims cannot be falsified. The authors correctly object that this is to improperly apply an observation regarding one specific type of supernatural claim (those relating to said omnipotent and omniscient being) as though it is true of all supernatural claims. Moreover, they argue that even in relation to the theists' 'God', it is possible that claims could be made specific enough to entail potentially observable outcomes – thus the common argument from evil as an objection to the claim that a benevolent 'God' created the universe (Boudry et. al. 2010, pp. 239-241).

Finally, it is important to note the central contention of a further article by the same authors, in which they note that IMN is the favored position of Theists and Creationists, precisely because it insulates their beliefs against scientific findings that may otherwise count against them. More than that, the insistence that science *must* employ MN, combined with the argument that it may however be possible (so far as science is concerned) that supernatural agents both exist and interact with the material world, allows theists to argue that science is methodologically limited in its access to the full expanse of reality, and thus some alternative combination of science and theology is actually a more reliable guide to truth (Boudry et. al. 2012). Thus in terms of the strength of demarcation arguments, employing IMN to disqualify supernatural claims is a hollow victory, as it comes at the expense of denying the ability of science to disprove or combat such claims. PMN, on the other hand, admits the ability of science to address supernatural claims, and thus cannot be employed to automatically disqualify supernatural hypotheses, but does allow scientific examinations of the relevant evidence and arguments to be employed, and so for the use of far more powerful arguments, which are more likely to have some impact on the beliefs of interested individuals.

2.2) Debunkers

It will be instructive to next briefly examine some of the claims made about demarcation by those wielding the blade – the authors of debunking literature.

2.2.i) Williams [Mousseau]

Dr. William F. Williams, author of the *Encyclopedia of Pseudoscience* includes an introductory article in which he asks "What is it that characterizes pseudoscience and how does it proceed?" Williams provides two points in response (Williams 2000, pg. xix):

1. "All pseudoscience is irrational; that is, it disregards or contradicts rational principles."
2. "Pseudoscience does not proceed by trial and error but by revelation."

Williams further explains: "The first of these answers will be hotly contested by advocates of some particular doctrine or belief, their general contention being that, if we can imagine a possibility, then we must keep an open mind about it until it is disproved. That, they claim, is the rational approach" (Williams 200, pg. xix). This assertion does not survive even the briefest perusal of the 'pseudoscientific' literature (without bothering even to focus on the better examples thereof), the vast bulk of which is devoted to the provision and discussion of *evidence* which, it is claimed, support the 'pseudoscientists'' contentions. This immediately establishes that their hypotheses are *not* premised on nothing more than the assertion that they are not impossible.

As to the more general assertion that 'pseudoscience' "…disregards or contradicts rational principles", however true this may be, it is entirely unhelpful unless that truth can be demonstrated. If all 'pseudosciences' were manifestly guilty of this charge, there would be no practical need to demarcate them, as no rational member of the public would be inclined to accept them. Moreover, it must be established that 'pseudoscience' contradicts rational principles in a manner that is not also true of numerous *scientific* claims, which may also be based upon faulty logic and/or problematic evidence, or in some other way mistaken (Ladyman 2013, pp. 46-48). In other words, this rather predictable observation has not bought us any closer to a successful demarcation process. Perhaps, however, the truth of the second claim, if upheld, would be sufficient to establish the irrational nature of 'pseudoscience'.

Williams supports his second point through explaining that 'pseudoscientists' maintain their beliefs regardless of contrary evidence, and indeed are in general entirely oblivious to empirical evidence (Williams 200, pp. xix-xx). This is a common charge against 'pseudoscientists', and predictably so, given that in declaring something to be 'pseudoscience' the writer is effectively asserting that it is a baseless belief. From this it automatically follows that anyone advocating said belief must be exhibiting a wanton disregard for the evidence which establishes its baselessness. Of course, the problem is again to establish the truth of this accusation, and to do so in a way that differentiates 'pseudoscience' from any *scientific* claim that an author may also maintain lacks evidential support. As has been shown above, advancing such a claim through arguments intended to establish that individual 'pseudosciences' indeed lack sufficient empirical support requires treating them precisely as *scientific* hypotheses, to be opposed by *scientific* arguments assessing the relevant evidence, and such is clearly not 'demarcation'.

Thankfully, at least one author has tried a different approach – actually comparing the contents of 'fringe' journals devoted to 'pseudoscientific' subjects with those of scientific journals in order to determine whether the former indeed displayed the typical claimed characteristics of 'pseudoscience'. Marie-Catherine Mousseau reviewed the demarcationist literature to arrive at a substantial list of methodological criteria held to characterize 'pseudoscience' alone: "neglects empirical matters", "suppresses or distorts unfavorable data", "uses resemblance thinking (e.g. Mars is red, red is the color of blood, therefore Mars rules war and anger)", "formal background modest, little mathematics or logic", "over-reliance on testimonials and anecdotal evidence", "does not admit own ignorance and need for more research", "no overlap with another field of research" "falls back on authority", "practitioners oblivious to alternative theories (pseudo-scientists make little attempt to solve problems with the theory or evaluate the theory in relation to other alternatives)", "uses obscurantist language" (Mousseau 2003, pp. 271-274). Mousseau then compared the evidence for these characteristics in three 'mainstream' journals – *British Journal of Psychology*, *Experimental Physiology*, and *Journal of Physics B: Atomic, Molecular and Optical Physics* – and four 'fringe' journals – *Journal of Scientific Exploration*, *Journal of Parapsychology*, *Journal of Psychical Research*, and *Revue Francaise de Parapsychologie*. Contrary to

the claims of demarcators, she found essentially no evidence to support their characterizations of 'pseudoscience'. For example, "…43% of articles in the fringe journals deal with empirical matters, and almost one-fourth report laboratory experiments". "…almost half of the fringe articles report a negative outcome (disconfirmation). By contrast, no report of a negative result has been found in my sample of mainstream journals." "All the articles that aim to gather new empirical evidence, whether in fringe journals or in mainstream journals, use statistical analysis". "17% of fringe articles deal with theory and propose new hypotheses. Two-thirds offer explanations consistent with mainstream theories". "…29% of the fringe journal articles ('reflection articles'…) discuss progress of research, problems encountered, epistemological issues. This kind of article is completely absent from the mainstream sample." "Fringe articles include an average of 20 other-author citations. Sixty-four percent of the works cited are taken from scientific journals or edited compilations of articles, which are usually peer-reviewed", and so on. Mousseau thus contends "On all counts, this sample of fringe journals satisfies the methodological criteria for proper science" (Mousseau 2003, pp. 273-275). Mousseau did find some quantitative differences between the 'fringe' and 'mainstream' contributions, but not such as would be useful in seeking to establish demarcation criteria, or even forming the basis of major cirticisms (Mousseau 2003, pp. 276-278).

Overall, these findings are highly significant, both for the arguments already covered and those still to come, and for demarcation in general, as they show that the application of a relatively objective method or assessment does not uphold the typical contentions of demarcators regarding the nature of 'pseudoscience'. These finding reflect, rather, what we should *expect* to find once we recognize what many 'pseudoscientists' truly are – reasonable, intelligent individuals sincerely seeking to establish the truth as they see it (this will be discussed in further detail below). Of course, Mousseau's findings can only demonstrate that 'pseudoscientists' do not, for example, completely refuse to address the relevant empirical evidence or alternative possibilities. It obviously remains open to debunkers to demonstrate that they fail to properly appreciate the force of such evidence, or the superiority of such alternative explanations. However, this returns us to the problem of having to treat 'pseudoscience' precisely as one treats a rival scientific claim, and the associated inability to demonstrate a difference between 'pseudoscience' and contested or refuted science.

2.2.ii) Gardner 1

The next debunker to be considered, Martin Gardner, is a major figure within the organized 'skeptical' movement, and I will examine the statements included in two of his books, published almost half a century apart (Gardner 1952; Gardner 2000). In the first of these, *Fads and Fallacies in the Name of Science*, Gardner writes: "…the best means of combating the spread of pseudo-science is an enlightened public, able to distinguish the work of a reputable investigator from the work of the incompetent and self-deluded" (Gardner 1952, pg. 7). This reflects the expectation that demarcation should be something the general public can undertake for themselves. "This is not as hard to do as one might think. Of course, there always will be borderline cases hard to classify, but the fact that black shades into white through many shades of gray does not mean that the distinction between black and white is difficult. Actually, two different 'continuums' are involved. One is a scale of the degree to which a scientific theory is confirmed by evidence" - Gardner here briefly discusses a continuum running from theories that are 'almost certainly false' through to those that are 'almost certainly true' - "The problem of determining the degree to which a theory is confirmed is extremely difficult and technical, and, as a matter of fact, there are no known methods for giving precise 'probability values' to hypotheses. This problem, however, need not trouble us. We shall be concerned, except for a few cases, only with theories

so close to 'almost certainly false' that there is no reasonable doubt about their worthlessness" (Gardner 1952, pg. 7).

Gardner's statements thus far are highly problematic in terms of considering any practically useful demarcation process. The public Gardner hopes to benefit obviously does not see any 'pseudoscientific' claims to which they are sympathetic as clearly 'black' examples of non-science, and thus, as argued above, the focus of any debunking or demarcation publication needs to be precisely on the grey area that Gardner is here dismissing. Compounding this, Gardner correctly recognizes the extreme difficulty of actually establishing "…the degree to which a scientific theory is confirmed by evidence", but insists that this will not matter because no such demonstration is necessary in regard to such obviously 'black' cases as he will be discussing. Gardner is thus relying upon his audience already agreeing with his assessments of such claims, and thus, in effect, denying any need to actually provide any demarcation argument. Evidently, then, his objective is not really to provide any 'demarcation' arguments at all, but rather to discuss a series of utterly unbelievable claims with an audience who already regards them as such.

Gardner then writes: "The second continuum is the scale of scientific competence" – here Gardner admits that there are some complicated cases: individuals who are competent in some circumstances but not others etc. - "all this ought not to blind us to the obvious fact that there is a type of self-styled scientist who can legitimately be called a crank. It is not the novelty of his views or the neurotic motivations behind his work that provide the grounds for calling him this. The grounds are the technical criteria by which theories are evaluated. If a man persists in advancing views that are contradicted by all available evidence, and which offer no reasonable grounds for serious consideration, he will rightfully be dubbed a crank…" (Gardner 1952, pp. 7-8). This last sentence reinforces the point just made about the assumed beliefs of Gardner's audience – clearly no one is tempted to believe claims they recognize as deserving such a description. Further, Gardner then goes on to provide a lengthy description of the 'crank' - that they are paranoid and deliberately isolated, for example – so advancing a set of *ad hominem* arguments which immediately contradict the point just made- that 'cranks' are to be identified through "…the technical criteria by which theories are evaluated…", criteria that Gardner has just asserted are extremely complex and difficult to actually apply, and which he has stated he will not endeavor to employ.

2.2.iii) Gardner 2

Gardner's comments in *Did Adam and Eve Have Navels? Debunking Pseudoscience* are much briefer. The book actually begins with the following: "Most of the chapters in this collection are attacks on far-out cases of pseudoscience. I am aware of the difficulties involving what philosophers of science call the 'demarcation problem' – the task of formulating sharp criteria for distinguishing good science from bad. Clearly no such criteria are precise. Pseudoscience is a fuzzy word that refers to a vague portion of a continuum on which there are no sharp boundaries" (Gardner 2000, pg. 1).

As a description of the 'demarcation problem', this is again highly problematic. Although the book is explicitly devoted to 'debunking pseudoscience', Gardner asserts that demarcation is intended to distinguish 'good science from bad'. In a way this is only fitting, however, given that the process he describes - locating where a claims lies along a continuum measuring scientific virtue – is one that cannot possibly justify use of the term 'pseudoscience' as indicating something that is not science at all. The criteria used to determine each claim's position must be the same throughout the full length of any such continuum. Thus the process used to distinguishing strongly supported scientific claims from those only a little less well supported, which cannot be anything other than the application of scientific reasoning itself, must also be employed in establishing the placement of claims lying at the opposite extreme –

precisely the type Gardner is to address in his book. If, however, the criteria employed do not change, and their application itself constitutes doing science, there can be no basis for asserting that, at any given point, one has passed beyond the bounds of science. However bad the science may become, it is at no point non-arbitrarily 'demarcated', or subject to any criticism other than those applied to science, and hence cannot be justifiably described as anything other than bad science. Moreover, the positioning of any claim according to such scientific criteria cannot possibly be ahistorical – scientific hypotheses must be recognized as continually in motion up and down the line – and thus such criteria cannot establish that a claim currently located at the lower limits of the continuum will not at some later point be found to have made its way into undeniably 'scientific' territory. Thus such criteria cannot possibly justify disqualifying any hypothesis from future consideration as a genuinely scientific proposal.

2.3) Recent Contributions: 2013

Finally, I turn now to some of the most recent contributions to the demarcation problem, for which one particularly important source is the first section, 'What's the Problem with the Demarcation Problem?', of the 2013 publication *Philosophy of Pseudoscience: Reconsidering the Demarcation Problem* (Pigliucci and Boudry 2013a). It is worthwhile considering these contributions both individually, and as a collection indicative of recent trends.

2.3.i) Pigliucci & Boudry

The authors of the introductory article, 'Why the Demarcation Problem Matters' (Pigliucci & Boudry 2013b) begin by establishing the subject matter – the demarcation problem arising with Popper but left unresolved by his 'falsifiability' proposal, and the rejection of Laudan's 1983 argument (Pigliucci & Boudry 2013b, pp. 1-2) – and then go on to advance some highly instructive arguments.

It is firstly asserted that "Philosophers and scientists readily recognize a pseudoscience when they see one", and it is noted as significant that, despite the differences of their arguments, demarcators agree in their conclusions regarding what are 'paradigmatic' cases of 'pseudoscience' (Pigliucci & Boudry 2013b, pg. 2). However, as the authors implicitly admit with their subsequent comments (see below), none of the scientists or philosophers insisting they can 'recognize' a 'pseudoscience' have been able to successfully justify that assertion, and indeed the veracity (or otherwise) of such assertions is more or less the central theme, not only of this thesis, but of the 'demarcation problem' in general. As will be touched upon later, the extent of the claimed 'agreement' regarding what counts as 'pseudoscience' is markedly less than complete. Further, as I believe is clearly revealed in the discussion of demarcation claims up to and beyond this point, there is very good reason to doubt that many of the relevant commentators have any detailed knowledge of more than a few paradigmatic 'pseudosciences', and thus their competence to advance any truly significant judgement regarding the majority of them cannot be assumed. More importantly, the history of science is replete with examples which, as with Continental Drift, unambiguously demonstrate that scientists and others have repeatedly erred in their communal dismissals of 'unfounded' hypotheses (Turro 1999, pg. 2; Williams 2000 pp. x, 58-59). It is therefore evident that a determination regarding the significance of any asserted ability to 'recognize pseudoscience' must depend upon the ability of the claimants to *establish* some satisfactory justification thereof. Thus, in short, any assertion that *assumes* scientists' 'recognitions' of 'pseudosciences' *are justified* cannot legitimately serve as a premise opposing the contrary claim that such assertions have been found to be *un*justified, and unjustifiable.

Pigliucci and Boudry, however, insist that "To argue that philosophers can neither spell out which criteria we implicitly rely on to tell science from pseudoscience, nor are able to evaluate and further refine

those criteria, would be to relinquish one of the most foundational tasks of philosophy (what is knowledge? How do we attain it?)" (Pigliucci & Boudry 2013b, pg. 2). This claim, as an explicit response to Laudan's arguments, is entirely baseless – to reject the demarcation project's objective of 'demarcating pseudoscience' is in no way to indicate that philosophers cannot assess the nature of knowledge and its acquisition: this is effectively a repetition of the assertion that to reject the 'traditional' Demarcation Problem amounts to admitting that there is 'no difference' between true science and those claims labelled 'pseudoscience' – a position that simply ignores the clear distinction between the concepts 'scientific status' and 'epistemic virtue'. This apparently reflects a degree of conceptual inflexibility amongst some commentators, who seem to have somehow come to believe that the 'demarcation' of 'pseudoscience' is the only possible means of addressing scientifically problematic claims, forgetting that scientists daily reject refuted science, and are perfectly capable of performing the same feat in relation to supposed 'pseudosciences' via the same scientific processes. Thus, when the authors later insist upon the importance and urgency of demarcation through reference to the harms caused by various erroneous beliefs (Pigliucci & Boudry 2013b, pp. 2-4), it should be noted that these beliefs could be publicly denounced through scientific refutation just as effectively as through 'demarcating' them as 'pseudoscience', and without the attendant philosophical issues.

Taking note of the latter, the authors continue: "For too long, philosophers have been dwelling over technical problems and exceptions to formal demarcation criteria, only to rashly conclude that the demarcation problem is dead… A ballpark demarcation of pseudoscience – with a lot of blanks to be filled in – is not difficult to come up with: if a theory strays from the epistemic desiderata of science by a sufficiently wide margin while being touted as scientific by its advocates, it is justifiably branded as pseudoscience". (Pigliucci and Boudry 2013, pg. 2)

It is not clear that this statement can really be understood as an *argument* at all. The authors begin by recognizing the long-standing insolubility of the problems associated with providing the requisite details for a workable demarcation method, but then respond, if that is the right term, by insisting that demarcation is 'not difficult' if you don't include any specifics. This is actually little more than a re-statement of the problems posed by the unresolved need for 'technical' details, and certainly does not provide any reason to suppose the latter can be overcome. The basic sketch of demarcation they provide includes a vague reference to 'epistemic desiderata', a relative 'blank' which *must* be filled in if their suggestion is to be in any way practically applicable, so demonstrating one reason why 'technicalities' *cannot* be avoided and so remain an entirely proper focus for philosophers. Moreover, until such details *are* provided, it is impossible to judge whether indeed any target *can* be 'justifiably branded a pseudoscience'. There is no reason to simply assume that 'epistemic desiderata' can be employed to establish an identifiable border-line between science and 'pseudoscience' which will also properly justify use of the latter term, particularly given that this is precisely one of those 'technical problems' demarcators have failed to resolve. Further, there is simultaneously the 'technical problem' of ensuring that a 'sufficiently wide margin' is not *so* wide that the proposed method excludes a significant number of paradigmatic 'pseudosciences', and thus again, until the 'blank' is filled in, it is impossible to judge whether the proposed demarcation method will be of any practical value in attaining the key objectives of the Demarcation Problem. It is therefore ultimately impossible to determine whether the authors have in fact 'come up with' a workable demarcation method at all, and thus their assertion that doing so is 'not difficult' is highly questionable.

Of course, the 'technical problems and exceptions' referred to are precisely the sort that have been employed in this paper to establish that 'traditional' demarcation - including use of the term 'pseudoscience', the idea that 'demarcation' can be some simple, shortcut process distinct from normal science, or that it can justify ahistorical disqualifications – presents insoluble problems. As such, these objections do *not* establish that 'demarcation is dead', but only that 'traditional' demarcation efforts

cannot succeed. Given their recognition of the philosophical circumstances, it would surely be far more reasonable for Pigliucci and Boudry to re-visit their approach to the Demarcation Problem than to insist, as they do, that philosophers should somehow proceed regardless of unresolved 'technical problems'.

This argument, combined with the previous assertions, reflects the same attitude discussed above in relation to anti-Creationist arguments – demarcation claims must be advanced and defended regardless of philosophical 'technicalities' - in this case understandably associated with the aforementioned conceptual inflexibility that leads to an inflated perception of the importance of 'demarcating pseudoscience'. It seems to me inauspicious that the editors' introduction to a collection of modern demarcation contributions should include arguments as flawed as this, without even so much as an *attempt* to explore means by which the obstacles confronting demarcation proposals might be addressed.

2.3.ii) Pigliucci

The first article, also by Pigliucci, begins with a lengthy, critical discussion of Laudan's 1983 rejection of the 'traditional' demarcation project, the details of which are not really germane to the current discussion, and so can be ignored (Pigliucci 2013, pp. 9-21)[44]. Regarding Laudan's core argument, Pigliucci writes only that this "…has some truth to it, but its extent and consequences are grossly exaggerated by Laudan within the context of this discussion", and no more (Pigliucci 2013, pp. 16-17). The important point is that Pigliucci rejects Laudan's proposal that a demarcation method should provide a set of necessary and sufficient criteria, and argues that science should be understood as a 'family resemblance' (or 'cluster') concept (Pigliucci 2013, pp. 19ff.). Pigliucci argues that this would require the use of numerous connecting conceptual 'threads', some of which would be more important than others, although he does not propose to go into so much detail at this point. Instead, he proposes that "At a very minimum, two 'threads' run throughout any meaningful treatment of the differences between science and pseudoscience, as well as of further distinctions within science itself… 'theoretical understanding' and 'empirical knowledge'…" (Pigliucci 2013, pp. 21-22). Pigliucci then provides a graph, with 'empirical knowledge' as the vertical axis and 'theoretical understanding' the horizontal, in which Astrology, HIV Denialism and Intelligent Design are placed in the bottom left corner, complete with a line marking them off from the genuine sciences filling the rest of the space (Pigliucci 2013, pg. 23). He further proposes that the process could be made more precise through the use of 'fuzzy logic', although he does not actually attempt to demonstrate this – again, he is merely making suggestions regarding what *could* be done (Pigliucci 2013, pg. 25). Obviously, it is rather difficult to make an argument about what some future philosopher may do, so I will address myself simply to the process as Pigliucci has portrayed it thus far.

Assuming for a moment that there is no difficulty associated with the placement of various claims on Pigliucci's graph of scientific virtue, the proposal has much in common with Gardner's suggestion of a scientific continuum. The criteria are being employed to establish a quantitative rather than qualitative difference, and require that sciences receive the same treatment as 'pseudosciences', involving a process surely best described as 'scientific assessment' rather than 'demarcation'. Accordingly, this proposal supplies nothing to differentiate scientific dispute and refutation from 'demarcation', no justification for assuming anything currently huddling in the bottom left corner will not later be found strutting amongst the established sciences, and no means to establish any non-arbitrary point of differentiation marking the

[44] Much of Laudan's paper is devoted to a discussion of the history of demarcation, beginning with the ancient Greek philosophers. He also considers the requirements of a successful demarcation method, as discussed in the opening sections of this paper, and examines numerous failed, modern demarcation attempts (Laudan 1996a, pp 211-220). All of these are subject to Pigliucci's criticisms (Pigliucci 2013, pp. 9-21) (which I regard as unconvincing), but as they have little bearing on Laudan's primary argument, it would be pointless to comment on the specifics.

borders of science, fuzzy or otherwise. Moreover, the history of demarcation strongly suggests that this will prove true of *any* proposed criteria – it can be assumed that Pigliucci would not be suggesting quantitative criteria if he could think of anything better.

Further, the process of locating the proper place for each hypothesis will, in practice, prove *highly* problematic. Advocates of 'pseudosciences' obviously do not believe that their claims belong where Pigliucci wants to place them, and those who believe those advocates, or are unsure whether to believe them, are in such positions precisely because they agree with the advocates or are unsure whether to agree with them, and thus they will not automatically agree with Pigliucci's determinations. Pigliucci's graph does a good job of representing the *attitude* of demarcators, but it is essential that they be able to *demonstrate* that their judgments regarding the relative epistemic merits of highly diverse hypotheses are justified, and this, as argued above, is extremely difficult to do. As the debunker Gardner wrote: "The problem of determining the degree to which a theory is confirmed is extremely difficult and technical, and, as a matter of fact, there are no known methods for giving precise 'probability values' to hypotheses" (Gardner 1952, pg. 7). Pigliucci suggests, in regard to utilizing 'fuzzy logic', "For this to actually work, one would have to develop quantitative metrics of the relevant variables. While such development is certainly possible, the details would hardly be uncontroversial. But this does not undermine the general suggestion…". It *does*, however, undermine the suggestion that any such metric can *currently* be employed, and the future is notoriously difficult to predict. Even assuming that markedly increased precision *could* be developed, the combination of 'controversial' metrics and 'fuzzy' boundaries does not inspire confidence.

Laudan's *central* argument, if correct (and I make some further effort below to establish that this is so), establishes that on any *epistemological* measure, there will be unavoidable overlap between science and 'pseudoscience'. Importantly, Pigliucci has not made any attempt to argue that Laudan was mistaken, but has only asserted that the significance of this point was overstated. However, for a proposal such as Pigliucci is here advancing, Laudan's observation is fatal. Its consequence is precisely that any 'family resemblance' representation of sciences and 'pseudosciences' which is both accurate and properly focused (which does not focus exclusively on exemplary and egregious examples respectively) will reveal them as intermixing along *all* relevant conceptual 'threads'. The latter, moreover, is established not only by Laudan's and my own arguments, above and below, but more importantly, by the fact that, over many decades, demarcators have failed to discover any universal and exceptionless 'demarcation' criterion – a fact which Pigliucci clearly accepted in the previous paper (Pigliucci & Boudry 2013b, pg. 2).

Appealing to 'fuzzy' boundaries cannot resolve such issues unless the 'fuzzy' band encompasses the area of overlap, including the most problematic examples of science, which are the *least* like good science, and all the problematic 'pseudosciences', which are the *most* similar to good science. The latter are, naturally, also the most rationally believable 'pseudosciences' and thus the best and most important candidates for demarcation. And what are we then to say about hypotheses within the 'fuzzy' border area? They are not really neither science nor 'pseudoscience', but rather merely not *demonstrably* either. There is consequentially no justification for declaring them to be definitely non-science, or incapable of becoming fruitful science through the application of scientific effort, or following changes in the epistemic outlook, and therefore no basis for disqualifying them as proper subjects for scientific consideration. Thus the *important* line of 'demarcation' would simply become the furthest *edge* of the 'fuzzy' boundary, and as cluster concepts do not admit of *any* sharp edges, the edge of a 'fuzzy' boundary must itself be 'fuzzy', which means that both the location *and* the width of the 'fuzzy' boundary is indeterminate. Advocates of a 'pseudoscience' can therefore insist that their hypothesis is, at minimum, *not demonstrably beyond the borders of the relevant boundary*, whilst demarcators will be hard pressed to establish otherwise, as they cannot objectively demonstrate where the relevant boundary begins or ends, nor any uncontroversial and precisely specifiable position for the relevant 'pseudoscience'.

2.3.iii) Mahner

Despite such difficulties, the next proposal, by Martin Mahner, also employs a 'family resemblance' concept of science, and is thus subject to the same objections (Mahner 2013, pp. 39-41). Mahner, however, further compounds the difficulties by proposing that the 'unit of analysis' should be "…entire fields of knowledge, or epistemic fields", explaining that, "Roughly speaking, an epistemic field is a group of people and their theories and practices…", and that "Choosing fields of knowledge as a starting point allows us to consider the many facets of science, namely, that it is at the same time a body of knowledge and a social system of people including their collective activities" (Mahner 2013, pg. 36). Mahner argues that we should assess such 'fields of knowledge' via a "…checklist of demarcation criteria that is as comprehensive as possible: a whole battery of science indicators" (Mahner 2013, pg. 36), of which he provides a few examples. Some of the criteria he suggests are - of the individuals involved:

> "Do they form a research community, or are they just a loose collection of individuals doing their own thing?"
> "Is there an extensive mutual exchange of information, or is there just an authority figure passing on his doctrines to his followers?"
> "Does the domain of study consist of concrete objects, or does it contain fuzzy 'energies' or 'vibrations', if not ghosts or other spiritual entities?"
> "What are the philosophical background assumptions of the given field?" (Mahner 2013, pg. 38).

And regarding the logic and methodology of a 'field':

> "Does it accept the canons of valid and rational reasoning?"
> "Do the principles of noncircularity and noncontradiction matter?"
> "How important are testability and criticism?"
> "How important is evidential support?"
> "Does the field borrow knowledge and methods from adjacent fields?" (Mahner 2013, pp. 38-39).

A major problem here is the suggestion that 'pseudosciences' can always be appropriately treated as monolithic 'fields' (some objections to which have already been advanced above). It must at the outset be recognized that in various ways, the activities of 'pseudoscientists' will differ from those of recognized scientists *because they have been declared to be 'pseudoscientists' and cut off from the established academic community*. For example, if it were accepted that Bigfoot exists, then Bigfoot research would simply be one small part of Zoology, and certainly not a 'field' in itself. However, as they are *already* rejected by the broader zoological community, Bigfoot researchers' interactions with Zoology and 'other' Zoologists will obviously differ from those of researchers within an accepted specialty. Such differences cannot be employed to justify the rejection that produced them. In regard to Ufology, which *would*, if accepted, be a true 'field', Mahner is arguing that we should treat the work of relatively sophisticated Ufologists as part of the same undifferentiated 'field' as the contributions *they* dismiss as 'pseudoscience' (the suggestion that 'pseudoscientists' do not criticize contributions to their 'field' - Derry 1999, pg. 161[45] - being incorrect - for examples from Ufologists, see: Salisbury 1974, pp. 148ff., 225-226, 227-228; Wilkins 1954, pp. 64-65; Keel 1967, pg. 1; Keel 1970, pp. 32, 34 Vallee 1965, pp. 159ff.; Randles 1990,

[45] This is another example of a demarcationist employing an erroneous representation, and suggests (again) a lack of familiarity with the literature of 'pseudoscientists'.

pg. 11; Anderson 2007). A 'fair' comparison would then require that the field of Medicine, for example, be understood as a community that includes the bad actors associated with certain pharmaceutical companies (Goldacre 2013; Healy 2012; Gotzsche 2013) and the many advocates of 'alternative' interventions that the better researchers dismiss as 'pseudoscience'.

As will be discussed further below, any such analysis of a 'field' is, crucially, problematic in terms of establishing that laypeople (and scientists) should disqualify as 'not science' *all* publications arising from it. Even if, taken as a monolithic whole, the relevant 'community' is not adequately 'doing science', this does not mean that *all* contributions to the subject are beyond the pale of science, or that none of the *arguments* arising from that community establish that further scientific research is warranted, as is required if the 'field' is to be justifiably 'demarcated' *in toto*. Such an analysis cannot establish that there is not, for example, a small sub-set of contributors who *are* 'doing science' as best they can in the circumstances, and whose arguments fully deserve further consideration.

Like Pigliucci, Mahner is not actually endeavoring to present any useable demarcation proposal, but merely suggesting what one may look like. He suggests that the actual number of indicators to be assessed would be at least thirty to fifty, of differing degrees of importance, and that precisely how many (and of what importance) would have to be met for a 'field' to count as science remains to be determined (Mahner 2013, pg. 40). Mahner therefore writes "…to calibrate such a list of science indicators, and in particular to get an idea about the number of positive checks out of any such full list, several uncontroversial cases of pseudoscience would have to be carefully analyzed and compared to uncontroversial sciences. An ideal task for a PhD dissertation!" (Mahner 2013, pg. 40).

The objective here is to establish a purely *conventional* meaning for the term 'pseudoscience', given that, yet again, the proposed process can demonstrate no specific, non-arbitrary or qualitative distinction between bad science and 'pseudoscience' – the latter is merely science that is *so bad* it is (supposedly) *not science at all*. However, the results of such a dissertation would differ radically depending upon: which 'pseudosciences' were selected, whether the author recognized the need to compare 'pseudoscience' with *bad* science,[46] which of the latter were selected, and how the author resolved the problem of (artificially) homogenizing the often highly heterogeneous 'pseudoscientific' communities. Disputes regarding such extremely contestable results – which should be expected should the proposal be actively employed – would therefore most likely prove effectively irresolvable.

Moreover, Mahner is suggesting that a PhD level analysis then be undertaken of every proposed 'pseudoscience' to establish whether it does or does not meet the requirements established. A thorough *scientific* analysis would be a crucial factor in answering many of Mahner's proposed questions - how a research community responds to empirical evidence, for example, clearly cannot be determined without a detailed examination of that community's arguments and the evidence adduced in their favour. Mahner is then *adding* to this *expert* task a further thirty to fifty analysis points, most of which require highly detailed investigations. This obliterates any idea of 'demarcation' as a relatively quick and simple process of 'dismissal', and so contradicts Mahner's earlier arguments emphasizing that the general public must be able to reach demarcation conclusions for themselves (Mahner 2013, pg. 35).

Recognizing that it is questionable whether the term 'demarcation' can be justifiably applied to 'family resemblance' concepts (given that these are precisely such concepts as do *not* have identifiable boundaries – Pigliucci 2013, pg. 19), Mahner proposes that there are two options: either "weaker" terms such as "delimitation", "delineation" or "distinction" could be employed, or, as he prefers, 'demarcation' retained, but "redefined" in a "…new, weaker sense" (Mahner 2013, pg. 40). However, to employ *any* of

[46] If, as Mahner proposes, selected 'pseudosciences' were compared simply with "uncontroversial sciences", and not specifically (uncontroversial) *bad* sciences (Mahner 2013, pg. 40), then the outcome would be a comparison between good science and 'pseudoscience' that provides no justification *whatsoever*, conventional or otherwise, for application of the label 'pseudoscience'.

these terms whilst denying the existence of distinct lines or limits is unjustifiable and nonsensical.[47] The only apparent reason for such a radical 'redefinition' of 'demarcation' is the author's desire to avoid admitting that he and Pigliucci have, in fact, abandoned any attempt to 'demarcate' per the *established* meaning of that term.[48]

Mahner also advances a series of criticisms of Laudan's 1983 conclusions, which he presents as having the consequence that "…identifying and fighting 'pseudosciences' is a misguided endeavor" (Mahner 2013, pg. 33). This characterization is true *de dicto*, but certainly not *de re*.

Mahner suggests that, as there is no *unanimity* regarding how to distinguish between warranted and unwarranted beliefs, Laudan's proposal that we focus on such issues suffers from the same flaw as Laudan objected to in demarcation (Mahner 2013, pg. 34). However, as discussed previously, Laudan rejected the traditional demarcation project because he believed it faced insurmountable obstacles (arising from the confounding of distinct issues), and not because of the 'lack of unity'.

Agreeing with Pennnock,[49] Mahner argues that the full implications of Laudan's arguments are contradicted by the continuing existence of Philosophy of Science as a distinct discipline (Mahner 2013, pp. 33-34). However, to argue that 'A' cannot be defined in a manner that excludes all examples of 'B' is manifestly not to say that 'A' cannot be defined at all.[50]

Insisting that "…science and technology are still considered to be epistemically privileged due to their systematic and rigorous approach, as a result of which they produce the most reliable knowledge"[51] (Mahner 2013, pg. 34), Mahner goes on to discuss various reasons 'Why Demarcation Is Desirable'. Writing that "…citizens of a civilized and educated society ought to be able to make scientifically informed decisions in their personal lives and in their roles in society, politics, and culture", Mahner unambiguously indicates his expectation that demarcation decisions will be made by laypeople themselves (Mahner 2013, pg. 35). He regards this as crucial to ensuring, for example, that decisions regarding medical interventions, the admissibility of legal evidence, research funding, political matters, and what should be taught in science classes, are suitably informed (Mahner 2013, pg. 35-36). Mahner entirely fails to recognize that the crucial distinction in such circumstances is that between good, well

---

[47] According to the OED, to 'delimit' means "To mark or determine the limits of; to define, as a limit or boundary"; 'delineate' means "To trace out by lines, trace the outline of, as on a chart or map"; 'distinction' means "the action of dividing or fact of being divided; division, partition; separation; "the action of distinguishing or discriminating; the perceiving, noting, or making a difference between things; discrimination. With a and pl., the result of this action, a difference thus made or appreciated"; "the condition or fact of being distinct or different; difference. With a and pl., an instance of this, a difference"; "the faculty of distinguishing or accurately observing differences; discernment, discrimination"; "the condition or quality of being distinctly or clearly perceptible; distinctness"; "something that distinguishes or discriminates; a distinguishing quality, mark, or characteristic; a distinguishing name or title". Thus all three terms involve the identification and marking of specific (distinct) boundaries – although a 'distinction' is not *always* distinct, in which case this word would be applicable, so long as the lack of distinctness, which it implies, were noted. "delimit, v.". OED Online. June 2017. Oxford University Press. http://www.oed.com.ezproxy.lib.monash.edu.au/view/Entry/49402?redirectedFrom=delimit [Accessed 30/10/17]; "delineate, v.". OED Online. June 2017. Oxford University Press. http://www.oed.com.ezproxy.lib.monash.edu.au/view/Entry/49411?rskey=39zXdJ&result=2&isAdvanced=false [Accessed 30/10/17]; "distinction, n.". OED Online. June 2017. Oxford University Press. http://www.oed.com.ezproxy.lib.monash.edu.au/view/Entry/55674?redirectedFrom=distinction [Accessed 30/10/17].

[48] Dictionary definitions of 'demarcate' and 'demarcation' as involving the marking out of specific boundaries have been provided previously.

[49] As discussed above, Pennock also inaccurately asserted (in the same paper as here referred to) that a lack of agreement regarding demarcation method was the basis for one of Laudan's two main arguments, so his influence is apparent also in the previous point.

[50] Of course, it is problematic if one insists that 'B' be defined as 'Not-A', but Laudan is explicitly arguing that 'pseudoscience' is *not* in fact a genuine class of non-science, and so this problem does not, for him, arise. Mahner also objects that Laudan required ahistorical demarcation criteria (Mahner 2013, pg. 34). A response to this would require a more detailed discussion of Laudan's argument than is otherwise warranted, so I will note only that this has no relevance to Laudan's central contentions, and that Mahner does not otherwise argue that the objections raised against extant demarcation proposals can be dismissed.

[51] This of course simply ignores Laudan's relatively uncontroversial point that *bad* science often does *not* exhibit a 'systematic and rigorous approach', and is most certainly *not* properly regarded as 'epistemically privileged'.

established science and *everything* that is its inferior. In all the examples provided, to exclude only 'pseudoscience' and believe that the objectives of 'demarcation' had thereby been attained, whilst allowing in sub-standard and bad science (as *science* and so 'epistemically privileged'), would be disastrous. Mahner has thus succeeded only in establishing that the 'demarcation of pseudoscience' is unimportant compared to the task of identifying which hypotheses are sufficiently well warranted as to be recognized as *good* science.

2.3.iv) Ladyman

James Ladyman begins the next article by emphasizing the definition of '*pseudo*science'. Noting that "The history of science is full of mistakes and falsehoods…", he maintains that "None of the scientists responsible for promulgating these false beliefs seems to deserve to be called a pseudoscientist, and it would not be appropriate to call every erroneous scientific theory pseudoscience" and "Even very bad science that is advocated as good science is not necessarily aptly described as pseudoscience". He therefore argues that "It seems clear that the connotation of either fraudulence or some kind of pretense is essential to contemporary uses of the term 'pseudoscience'" (Ladyman 2013, pg. 46). He also insists that 'pseudoscience' is to be differentiated from typical science fraud (Ladyman 2013, pp. 47-48).

Ladyman's central contention is that 'pseudoscience' is analogous to 'bullshit' as defined by Harry Frankfurt. "Frankfurt makes a very important point about how bullshit differs from lies, namely that the latter are designed to mislead us about the truth whereas the former is not concerned with the truth at all. Plainly, this distinction is analogous to that between pseudoscience and science fraud" (Ladyman 2013, pg. 52). Although he recognizes that most 'pseudoscientists' are sincere in their beliefs (Ladyman 2013, pp. 47-48, 52), Ladyman insists that "…one can bullshit unwittingly, and pseudoscience is often akin to that. Just because one's first-order self-representations are that one is sincerely seeking the truth, it may be argued that, in a deeper sense, one does not care about it because one does not heed the evidence (Ladyman 2013, pg. 52).

From his conclusion and repeated assertions, it is evident that Ladyman believes 'pseudoscience' is 'bullshit' primarily because 'pseudoscientists' appear to be advancing specific empirical claims, but are not in reality saying anything.[52] He writes, for example, that "…bullshit and pseudoscience resist refutation by not making definite claims at all"; "…pseudoscience has a positive nature akin to that of bullshit, consisting in the production of epistemic noise that resembles propositions and beliefs only superficially" (Ladyman 2013, pg. 53); and "Practitioners of bullshit and pseudoscience both produce sentences intended to convince their audience that some factual claim is being made, when it is not" (Ladyman 2013, pg. 55).

Ladyman embarks on a wide ranging discussion in which he notes various other differences between science and 'pseudoscience'. He writes, for example, that: "Pseudoscience can be defined negatively in so far as it does not make precise and accurate predictions, while science in general does", and argues that 'pseudoscientists' use terms that appear to have scientific significance, but are in fact meaningless (Ladyman 2013, pp. 54-55). He also recognizes that 'pseudoscientists' often *do* make specific claims – for example, that particular 'alternative' treatments are efficacious - but responds that "…it is arguable that these false (or at least dubious) factual claims are not what makes pseudoscience pseudoscience", and proposes that we should focus not only on the 'product', but also the 'producer': "Clearly, we may give accounts that focus on the texts or theories that are produced, or on the mental states and attitudes of the people who produce them" (Ladyman 2013, pp. 53-54). Accordingly, he discusses differences within the relevant communities, including their attitudes towards their theories and

[52] This has much in common with Lugg's 1992 proposal.

interactions among their members. Ladyman maintains that the "…unity of scientific practice and its theories is missing in pseudoscience" and "Pseudoscience is largely about cult figures and networks whose relational structure involves a lot of chat, but lacks the integration with rich mathematics, material interventions, and technology that characterizes science" (Ladyman 2013, pp. 55-57).

Ladyman at no point makes any reference to how any of these points may actually be used to effect demarcation, and thus in terms of arguing that demarcation proposals cannot succeed, there is really nothing to respond to. Further, none of the points he makes are unique, and all have been dealt with previously, so I will not respond to them in detail here.

Regarding his major contention that 'pseudoscience' is 'bullshit': it is, firstly, somewhat confused by the various observations not related back to it. For example, there is no explanation of how the interactions of a research community contribute to this concept. The central argument is also in itself confusing, if not confused: 'pseudoscientists' are sincere, and thus unwitting, 'bullshitters', who are (sincerely) not really saying anything, although they do make specific claims, but it is not these that characterize 'pseudoscience', and we should examine the 'producers' and not just the 'product'… etc.

None of this really matters, however, as the key assertion - that 'pseudoscientists' are not truly concerned with the truth - is simply untenable. There is no shortage of 'pseudoscientific' contributions which exhibit absolutely no basis for the suggestion that their authors are less concerned with the truth than anyone else advancing a sincerely held hypothesis. Certainly accredited scientists who also advocate a 'pseudoscience' display no evidence of a reduced regard for truth (see for example the discussion of Hynek below, and also Sturrock 1999; Salisbury 1974; Rutledge 1981). Ladyman has indicated that 'pseudoscientists' *must* be unconcerned with the truth, as they do not 'heed the evidence', but precisely the same could be said of those advocating 'very bad science' as 'good science'. As for the assertion that 'pseudoscientists' are not really saying anything, this has been addressed previously, and is manifestly false in relation to, for example, the contributions just cited.

Ladyman offers no arguments against Laudan's central contention, but rejects Laudan's proposal that we focus on the epistemic warrant of hypotheses because it is not sufficiently simple: "…confronting pseudoscience in this way… consumes too much time and too many resources, is not useful when engaging in public debates that operate at a general level, and is too detailed for scientifically nonliterate audiences" (Ladyman 2013, pp. 49-50). Clearly, then, he must reject the two previous proposals (and the next) on the same basis (lack of simplicity), and it is far from clear that his own core arguments could be employed without a scientific analysis of the 'pseudoscientists'' claims. To establish that they do not take heed of evidence would certainly require that the evidence and its significance to their arguments be examined, whilst to demonstrate that they only *appear* to be sincerely advocating specific claims would presumably require a similar analysis of 'pseudoscientific' arguments and the evidence adduced in their favour.

2.3.v) Hansson

In the next article, Sven Ove Hansson proposes that "A statement is pseudoscientific if and only if it satisfies the following three criteria: 1. It pertains to an issue within the domains of science in the broad sense[53] (the criterion of scientific domain) 2. It suffers from such a severe lack of reliability that it cannot at all be trusted (the criterion of unreliability) 3. It is part of a doctrine whose major proponents try

[53] Hansson means by this '*Wissenchaft*', which "…includes all the academic specialties, including the humanities" (Hansson 2013, pp. 63-64). This rather erodes the distinction between Hansson's proposal and Laudan's recommendations, given that Hansson is using 'science' so broadly that it includes most any discipline to which epistemic warrant is significant, and his major criterion is effectively epistemic warrant.

to create the impression that it represents the most reliable knowledge on its subject matter (the criterion of deviant doctrine)" (Hansson 2013, pp. 70-71).

Clearly the second criterion is by far the most significant, as the first and third apply equally to genuine science.[54] In asserting that 'pseudoscience' "cannot at all be trusted" Hansson means to indicate that it is *entirely* "unreliable" and lacks *any* "epistemic warrant" (Hansson 2013, pp. 66-67). It is not clear that this can itself differentiate 'pseudoscience' from scientific hypotheses regarded as *refuted science* – Phlogiston and The Ether, for example, may be accurately described as 'entirely unreliable' *scientific* 'doctrines'. Indeed, opponents in a scientific dispute may well reject each other's hypotheses as refuted and entirely unreliable, yet not as 'pseudoscience'.

Hansson recognizes that his criteria could be said to apply to various examples which he agrees would *not* be properly termed 'pseudoscience' - scientific fraud and unrecognized scientific errors by legitimate scientists appear to meet all three (Hansson 2013, pp. 67-68). He argues, however, that "the crucial element that is missing in these cases is a deviant doctrine. All the typical cases of pseudoscience are cases in which a deviant doctrine plays a crucial role" (Hansson 2013, pp. 68-69).

This means that the concept of 'deviant doctrine' is absolutely fundamental to Hansson's proposal, yet he does not provide any explicit explanation of exactly what this means. What he does write indicates that a 'deviant doctrine' is one that does not have "scientific legitimacy", is "unorthodox", and contrary to the "…predictions of established scientific theories" (Hansson 2013, pp. 68-69). This is surely just another way of referring to rejected science, although Hansson clearly requires that it be more than that – as the *crucial* element that *differentiates 'pseudoscience' from mistaken science and science fraud*, a 'deviant doctrine' cannot possibly be 'science'. Yet it is advocated as though it were science, which means it must be 'pseudoscience' - and 'pseudoscience' is thus defined as always involving 'pseudoscience'. Once again, it is important to appreciate that numerous hypotheses have in the past been dismissed for meeting the description of Hansson's 'deviant doctrines', yet have subsequently become established science, and thus could not have ever been *justifiably* dismissed as entirely unreliable 'pseudoscience' (Turro 1999, pg. 2; Daempfle 2013, pp. 43-44; Le Grand 1988, pp. 3, 32; Powell 2015, pp. 94ff., 286; Greene 2015, pp. 479, 524; Bryson 2003, pp. 141-142; Oreskes 1999, pp. 3, 5, 124, 126, 316).

Hansson also recognizes that his proposal does not advance criteria that are "directly applicable" to demarcation decisions. He insists that this is essential, as "…direct applicability comes at a high price: it is incompatible with the desired exhaustiveness of the definition". He then explains that this is due to a lack of "methodological uniformity" within science ('in the broad sense'), which means that "…one and the same demarcation criterion cannot be both general and timeless and also be sufficiently precise to tell us how to evaluate the scientific status of specific investigations" (Hansson 2013, pp. 73-75). Hanson thus explicitly admits that no demarcation method can be both accurate and applicable, from which it follows that we *cannot in practice* 'demarcate pseudoscience'. Hansson began the article by remarking on the "striking unanimity" among those scientists who use "tacit knowledge" to "…draw the line between science and pseudoscience" (Hansson 2013, pg. 61).[55] If, however, no criteria specific enough to be

[54] The third criterion actually applies to anyone advancing what they believe to be a sound argument. 'Pseudoscientists' and scientists both may recognize that they are employing less than perfect evidence, and, in some readily imaginable circumstances, that others may have access to better evidence that is not, however, available to them. However, Hansson is not suggesting that this would never be true of 'pseudoscientists'. His point is rather that they present their *approach* as the best – that they present themselves as employing maximally effective methods relative to the evidence they have. In the case of 'pseudoscientists', as will be explicated below, Hansson believes such representations are being *falsely* made of 'deviant doctrines'.

[55] Such 'unanimity' is much less 'striking' when one considers that, as noted earlier, the 'tacit knowledge' employed, given that many scientists have not pursued direct knowledge of many 'pseudosciences', must be, quite often, simply that someone else with sufficient perceived authority has labelled something a 'pseudoscience' already. In his own contribution to this same volume, Ruse states explicitly the position that Pigliuuci, Boudry, and Hansson seem to be adopting: "It has proven very difficult to give a precise definition of the notion [of 'pseudoscience'], but I take it as an absolute that it does exist and that people

employed in drawing such a 'line'[56] *can* also be accurate, then the 'tacit knowledge' employed by such scientists *must* be *inaccurate*, and thus their conclusions *must* be unreliable.[57]

2.3.vi) Boudry

The final proposal to be examined here is by Maartin Boudry. Boudry's primary contention is that difficulties arising from "territorial demarcation" – distinguishing science from "other epistemic endeavors" - should not be accepted as legitimate objections to criteria intended to demarcate 'pseudoscience'. Boudry argues that Laudan's use of such criticisms undermines his argument, and that their rejection allows the use of criteria previously improperly dismissed (Boudry 2013, pp. 79-83). Laudan did make use of 'territorial' overlap in arguing against the accuracy of certain criteria, but as this is not relevant to his most important argument, I will not pursue the issue. As a general response, I will point out only that none of the objections I have employed above are 'territorial', and I am therefore unconvinced by Boudry's assessment that jettisoning 'territorial' objections is sufficient to make the 'demarcation of pseudoscience' possible.

Boudry rejects Laudan's[58] contention that arguments directed against proponents and their activities, rather than hypotheses, improperly confuse *ad hominem* criticisms with *ad argumentum* objections. Boudry contends that "In many cases, immunizing strategies have such a cozy relationship with a pseudoscientific doctrine that they are at least provoked by it", and that in such cases, since "…there is typically no procedure for separating the theory-in-itself from the cognitive and methodological behavior of its defenders", assessors have "…no choice but to get involved in the sociology of the discipline… and the psychology of those who are engaged in it" (Boudry 2013, pp. 90-91).

Now, the way it works is fairly simple - if a problem is encountered, supporters of a theory (scientists and 'pseudoscientists' alike) will propose some solution (*often* regarded by opponents as *ad hoc* or an immunizing strategy), which they naturally try to ensure is in as close (or 'cozy') a relationship with the theory as possible. To propose a solution - a 'behavior' – is to propose an addition to the theory, which may then become part of the theory.[59] If the solution employed is weak, then reliance on it weakens the theory and strengthens contrary arguments. Empirical evidence can thus erode the theory's epistemic warrant, and arguments can accordingly be advanced seeking to establish that the theory should be regarded as refuted (Pigliucci 2013, pg. 12; Samir 2002, pg. 16; Derksen 1993, pp. 24-25). Importantly, the objective is not particularly to convince the theory's *proponents* that it has been falsified (a claim proponents *by definition* deny). Rather, arguments are addressed to the relevant scientific community as a whole, and, if sound, are hopefully sufficient to achieve the true objective - majority approval. There is no

---

recognize it" (Ruse 2013, pg. 226). This is surely rather problematic, considering that their articles are presented as responses to opponents who argue that extant applications of the term 'pseudoscience' are inaccurate and/or unjustifiable, in which context simply assuming (as 'an absolute') the opposite is hardly appropriate. Ladyman, on the other hand, explicitly recognizes that demarcators' continuing utilization of the term 'pseudoscience' may indicate merely that it serves as a "…catchall for expressing disapprobation….", and should *not* be taken as establishing that they thereby "…pick out a true kind in the world" (Ladyman 2013, pg. 49).

[56] When Hansson discusses the agreement among scientists regarding where the 'line' is drawn, he cannot mean in terms of the criteria they employ, as this is explicitly something they cannot explicate. Thus he can only be referring to their 'unanimity' in regard to what they determine to be 'science' or 'pseudoscience', which therefore necessarily requires that they are utilizing practically applicable criteria.

[57] Hansson does suggest that demarcation could be achieved on a field by field basis, using criteria that are not universal to 'science' (or '*Wissenchaft*') (Hansson 2013, pp. 71-76).

[58] Which he notes is supported by a number of other philosophers.

[59] At least, the version of the theory advocated by those proponents – other proponents may prefer a different response, or may be advocating a version of the theory for which the problem does not arise, etc. (see (Kuhn 1996, pg. 83; Le Grand 1988, pp. 125-126; Derksen 1993, pg. 21)

means of ensuring truly *universal* agreement on any conclusion, scientific or otherwise, and therefore, if an inability to do so be taken as meaning a hypothesis is 'unfalsifiable' or the like, then this would be true of *all* hypotheses, and irresolvable.

Moreover, and thankfully, we do not need to pass judgement on ephemeral 'theories',[60] but rather must address specific *arguments*, made concrete as *publications*.[61] Scientists do not, and need not, go about *finding out* what a proponent or a 'research community' (assuming one can be suitably delineated) is thinking beyond what is reflected within their publications, and indeed there is no established way of doing so. Authors *must* be allowed to present their own arguments, and it is not up to anyone else to declare what they are thinking for them. A theory is thus 'separated' from the 'behavior of its defenders' by those defenders themselves, when they publish an argument. Hence, there is no need for any further 'procedure'. Conclusions must then be decided on the merits of such arguments – specifically, whether or not they are sound. To suggest that we might come to *different* conclusions on the *same* argument depending solely on who produced it and claims relating to activities not reflected within that argument itself, is, I believe, inappropriate.[62]

Boudry contends that 'pseudoscience' differs from science because "In the absence of the epistemic warrant that genuine science accrues, pseudosciences are confronted with the problem of surviving the day when prophecy fails and of creating a spurious *impression* of epistemic warrant" (Boudry 2013, pp. 87, 95), and thus 'pseudoscience' is defined as theories that employ *ad hoc* explanations, epistemic defense mechanisms, "…and other epistemic sins to a sufficiently egregious extent…" (Boudry 2013, pg. 95). This proposal neglects the fact, discussed previously, that most 'pseudoscientists' are entirely convinced that the epistemic evidence supports their hypotheses, and certainly do not perceive any need to guard against refutations (Ladyman 2013 pp. 47-48, 52; Shermer 2013, pg. 221; Long 1990, ix-x; Gordin 2012, pg. 1; Goode 2013, pg. 151; Derksen 1993, pg. 23). Boudry also fails to flesh out in any applicable way what 'sufficiently egregious' means, or to suggest how exactly we might differentiate between 'pseudoscientific' and scientific uses of defense strategies.

Far from justifying his use of 'pseudoscience', Boudry explicitly equates it with bad science (Boudry 2013, pg. 86), maintains that "…theories may move in and out of the domain of science as new evidence accumulates and conceptual progress is made…" (Boudry 2013, pg. 92), and against Laudan's conclusion that it is a 'hollow phrase', insists that "…we need *some* word…′and asks "…does he have a better term in store?" (Boudry 2013, pg. 95). As Katie Burk (2016) has argued, any *justified*, and ideally more descriptive, word or phrase would be better, and there are certainly other options! Boudry also maintains that "A twilight zone does exist, with theories that are neither scientific nor quite pseudoscientific, but we can readily come up with clear instances of both kinds, which is all that is needed for the viability of the normative demarcation project", repeating a position rejected in detail above (Boudry 2013, pg. 92).

2.3.vii) 2013 Contributions: Collectively

---

[60] Of which there can be multiple versions, and various proponents behaving in varying ways, so no *specific* judgement is possible if such must be taken into account (Derksen 1993, pg. 21).

[61] This position is defended below.

[62] Although of obvious value to the demarcator. No one can control who publishes on a 'pseudoscientific' topic, or who is perceived as a 'member' of a 'pseudoscientific' community. Scientifically unsophisticated contributors cannot be excluded, while market forces ensure that those who specialize in sensationalism tend to achieve prominence. Thus, focusing on the 'community' associated with a topic already dismissed as 'pseudoscience' is considerably more conducive to success than trying to demarcate on the basis of the *best* arguments produced by that community, which may have been authored by a trained and accredited scientist.

Taking these 2013 contributions as a group, it is surely significant that not one includes even an *attempt* to provide a full, workable method applicable to identifying examples of 'pseudoscience', and there is a marked move away from efforts to provide qualitative criteria or to 'demarcate' per the meaning of that term – marking out specific boundaries. With the exception of Ladyman, whose superiorly reasoned positions on the meaning of 'pseudoscience' and the significance of its use by demarcators seriously undermine the arguments of his colleagues, there is also a distinct absence of concern to establish that 'pseudoscience' is truly not science, combined with a dogged but unjustified insistence that extant determinations are sound. Finally, it is noteworthy that, despite the many comments regarding 'pseudoscience' in general, and the numerous specific examples advanced, not one of these articles cites a single example of 'pseudoscientific' literature, although they do cite many demarcators and debunkers. This strongly supports the contention that many demarcators' understanding of 'pseudoscience' is based largely on the representations of their fellow demarcators and debunkers – the 'pseudoscientists' opponents – rather than on any direct knowledge of such subjects as presented by their advocates.

3) Against 'Demarcating Pseudoscience' 1: Guiding Belief

The above examples are, I believe, sufficient to illustrate the serious obstacles confronting demarcation proposals, and the failure of demarcators to overcome them. I will now endeavor to demonstrate why, in fact, no 'traditional' demarcation proposal can possibly succeed.

3.1) Demarcating 'Out of Hand'

The requirements and desiderata applied to the demarcation attempts above arise largely from expectations regarding what demarcation is intended to achieve. One significant departure from the norm in this regard is the contention that demarcation is crucial to scientists deciding which hypotheses they should pursue (Rothbart 1990 pp. 113ff.; Wilson 2000, pg. 2; Fuller 1985, pg. 331), and this view of demarcation will therefore be addressed first.

Scientists are indeed theoretically confronted with an enormous number of hypotheses. However, this remains true even if all the 'pseudoscientific' propositions are removed: there are undoubtedly more genuinely scientific options than any individual scientist could possibly pursue. It follows, therefore, that the decision to pursue a particular hypothesis does not indicate that the 'rejected' alternatives are all 'pseudoscience'. Indeed, it does not necessarily indicate that the 'rejected' options are the less promising ones: a scientist may, for example, choose one option over another because it is a better fit with their particular specialized knowledge, and/or the resources or data available to them (Le Grand 1988, pp. 80ff). It is also quite clear that scientists do not believe that decisions of this sort should be understood as establishing that the rejected hypotheses are 'pseudoscience', or that anyone making a different decision has made a *wrong* decision. Accordingly, scientists do not typically refer to other scientists pursuing other hypotheses as 'pseudoscientists'. Rather, each individual makes a determination on the basis of their own particular predispositions, requirements, beliefs, etc., recognizing that such an obviously subjective decision has little, if any, normative significance. Obviously, then, these decisions do not constitute demarcation, and, indeed, need not involve demarcation at all. It is therefore clearly mistaken to argue that scientists *require* any demarcation method to justify such decisions.

However, such an approach may not really be what the relevant commentators had in mind. The focus here is specifically on the rejection of hypotheses 'out of hand' and 'prior to any testing' (Wilson 2000, pg. 2; Rothbart 1990, pp. 113, 114), and it may be maintained that it is precisely the *immediate dismissal* of a hypothesis as 'pseudoscience' that 'demarcation' refers to. Scientific propositions can only be *rejected* subsequent to careful scientific analysis, but some propositions, it may be postulated, do not require any detailed consideration, and should instead be simply and immediately *dismissed* as 'pseudoscience' (James 1990, pp. 31, 35). A clear distinction between demarcation and scientific processes is thus provided, and 'pseudoscience' can be understood as indicating hypotheses that are utterly unworthy of scientific attention.

There are a number of fatal objections to such a proposal. Firstly, it is not evident that a snap judgement regarding what is worthy of scientific attention can really justify maintaining that a given hypothesis should be disqualified as non-science, or that the evidential basis of such judgments may not alter over time such that what was once dismissed as unworthy of attention may later warrant pursuit or even acceptance. We must accordingly determine whether any 'out of hand' dismissal could possibly be justified, and precisely what it can justify.

It may initially seem that the claim being advanced is precisely that *no argument* is required, but this is clearly untenable, as it would indicate that there need be no particular reason for dismissing a hypothesis, from which it would follow that *any* hypothesis could be properly dismissed in this manner.

Therefore, if such a move is justified, it must be because a valid, supportive argument has been employed - it is just that we have not been given any indication as to what that argument is.

If a hypothesis is thus dismissed, but no reason is provided, then either the claimant has no argument – no valid reason – for their decision, and thus the decision cannot possibly be justified, or they *do* have a valid reason, but have chosen not to tell us what it is. In the latter case, they have opted out of publicly demarcating the target proposition. Obviously, in such circumstances we cannot judge whether the reasoning employed is sound. We may be intuitively inclined to believe that a decision is based upon a sound argument because we agree with the conclusion, and thus hold that the scientist in question has *correctly* identified an example of 'pseudoscience', and perhaps their doing so provides some further support for the justifiability of this demarcation conclusion. However, this would be a mistake. In fact, the argument they are relying on may bear no relation to our own reasons, and may be entirely erroneous or irrelevant, arising from complete ignorance of the relevant evidence and the true nature of the hypothesis they have precipitously dismissed. Really, there is no point discussing the matter, as the decision not to engage in public demarcation renders the personal decision irrelevant to rational demarcation altogether. Whether it is justified or not, a private decision to ignore a hypothesis is not truly part of any demarcation process, and does not require any objective justification, whilst the absence of an argument ensures that there is no contribution to public demarcation of the type that can guide belief or contribute to this discussion.

It may perhaps be maintained that 'out of hand' demarcation refers to scientists' claimed ability to recognize 'pseudoscience' 'when they see it' (Hansson 2013, pg. 61; Pigliucci & Boudry 2013b, pg. 2; Ruse 2013, pg. 226), and so reject it without any *detailed* argument such as is required when addressing genuine science. This may be made possible because a demarcation argument has already been advanced by someone else, and so the scientists in question need do no more than indicate their acceptance of that argument. In this case, however, the decision to dismiss the hypothesis is not itself demarcation, but rather based upon an earlier demarcation determination. The validity of the determination then rests entirely upon the merits of the prior argument and cannot be assessed unless that argument is explicated. If, moreover, the demarcation claim has been accepted uncritically – perhaps on the basis of respect for its author or their qualifications - then the scientist is effectively choosing to act as a layperson relative to that argument. Such uncritical acceptance of an argument is of no appreciable significance in supporting that arguments accuracy, and thus uncritical acceptance of a demarcation claim does not contribute anything meaningful to the demarcation process or to any other scientific or science related conclusion.

More reasonably, scientists may accept a demarcation claim without any detailed examination, but rather essentially because it seems right – the argument appears *prima facie* reasonable, and rejection of the hypothesis accords with their assumptions about it and their background knowledge. Indeed, they may instead come up with such a basic, short-cut type argument themselves, without knowing if anyone else has advanced any detailed demarcation claim (it is difficult to gauge how aware the average scientist is of specific demarcation disputes). Certainly, this appears to be roughly the basis on which the majority of scientists *do* dismiss those claims that are widely regarded as 'pseudoscience'. However, as such decisions are not in themselves directly relevant to demarcation, our focus should be on the significance of such arguments for public demarcation, which requires both that the disqualification of hypotheses as 'pseudoscience' is properly justified, and that we can be certain anything disqualified in such a manner cannot later prove to be genuine science.

Without proposing any specified basis for dismissal, which requires consideration of the arguments advanced in support of a hypothesis and the relevant evidence, it is difficult to see how such a process can be established as justifying the demarcation of 'pseudoscience' specifically, rather than merely varieties of flawed and empirically unacceptable hypotheses. Arguments have been provided above, and will be provided below, demonstrating the difficulties involved in establishing that hypotheses regarded as 'pseudoscience' are truly not science at all, and the use of truncated arguments clearly does not further advance that objective. The process under consideration is distinct from *detailed* scientific rejection, but there is no apparent reason to hold that unorthodox, unpromising, flawed, or rejected

science too cannot be 'dismissed': if more specific arguments intended to identify 'pseudoscience' often capture poor science as well, this is surely even more likely when less detailed arguments are employed.

The major objection to be raised here is obviously that it is highly problematic to assume scientists are correct in rejecting an unorthodox claim on the basis of their background knowledge alone. History abounds with examples of revolutionary or unorthodox propositions which were regarded by many contemporary scientists as complete nonsense, but which have subsequently attained consensus approval (Turro 1999, pg. 2; Daempfle 2013, pp. 43-44; Le Grand 1988, pp. 3, 32; Powell 2015, pp. 94ff., 286; Greene 2015, pp. 479, 524; Bryson 2003, pp. 141-142; Oreskes 1999, pp. 3, 5, 124, 126, 316)

Even if we are nonetheless fairly confident that the *conclusions* themselves are secure, and will not prove to be mistaken, the *arguments* employed are far less so. If the scientists advancing the arguments are ignorant of the pertinent details, then, even if they reach the right conclusions, it may be on an entirely erroneous basis. Ultimately, short-cut arguments of the sort under consideration are directed against conclusions alone, without seeking knowledge of the arguments made to support those conclusions, or the evidence appealed to. Clearly, however, to reject a conclusion without adequate knowledge of the evidence advanced in its favour is to unjustifiably (even if only technically) ignore half of the relevant equation. Yet to acquire adequate knowledge of proponents' arguments requires that their claims are not dismissed summarily, but rather assessed.

Finally, even if the arguments employed by short-cut demarcators are not actually erroneous, they will nonetheless certainly be criticized by proponents of 'pseudoscience' as simplistic and ill-founded. Unavoidably, in such circumstances, only a detailed argument which adequately addresses the assertions and counter claims of advocates will suffice. Thus, in terms of applying demarcation arguments to genuinely problematic cases of 'pseudoscience' in a manner that genuinely assists laypeople in recognizing the true nature of such claims, truncated or 'out of hand' arguments will be insufficient.

The idea that demarcation is crucial to scientists confronted by a mass of hypotheses competing for attention, and can justify their rejection of hypotheses 'out of hand' or before they have been 'tested', is clearly mistaken. The process of hypothesis selection is not truly relevant to, or in any way dependent upon, demarcation, and the suggestion that demarcation may usefully and justifiably be undertaken without any significant attention to the relevant details is likewise erroneous.

3.2) Objectives, Units, Means

We should focus, then, on demarcation methods intended to attain more widely agreed objectives.[63] Considering the details of the various proposed demarcation methods discussed above, it is immediately evident that there is no consensus among demarcators regarding either the means by which demarcation should be achieved – the type of features or criteria that are relevant – or the basic 'unit', or type of theoretical entity, to which demarcation should be applied. Any clarification which can be achieved regarding these details will obviously aid the discussion of demarcation in general, if only through limiting the possibilities that need be considered. Obviously, these two variables are closely interrelated, so that any determination regarding one will likely have significant implications for the other.

In regard to the potential 'unit' of demarcation, the possibilities can be reduced to: topics or fields, which are employed by virtually all debunkers and many other commentators; research programmes and similar historically linked multi-hypothesis entities; conclusions; arguments; specific iterations of a hypothesis in the form of individual publications; and authors or proponents themselves (Mahner 2013, pg. 32).[64] The potential 'means' can likewise be reduced to a small number of basic types: logical or inherent features allowing *a priori* disqualification; developmental features such as progress or the lack thereof; methods, including attitudes and the like; and empirical issues.

---

[63] As discussed in the introductory and 'desiderata' sections above.

[64] It is possible that some other type of entity consisting of multiple publications or hypotheses can be referred to, but this list seems to capture the major groupings, and minor alterations on that theme will make little difference to the arguments to follow. If there is any other basic unit type available to demarcators, it has not been referred to in any of the literature I am familiar with.

To begin with *a priori* arguments, I am not willing to rule these out as a possibility, although I cannot think of any unarguably acceptable examples. It certainly appears to be possible that some topics, for example, are not amenable to scientific treatment, or at least certain forms of scientific argument, and thus improper 'scientific' contributions to those topics may potentially be disqualified *a priori*. Any attempt to establish a firm conclusion on this matter would require extremely lengthy, extraordinarily complex, and undoubtedly controversial arguments regarding how exactly science is to be defined, and precisely how science relates to non-scientific cognitive endeavors. For example, a contribution concerning the implications of the findings of modern science for theistic beliefs may arguably be: science, a scientific contribution to a non-scientific field, philosophy, theology, or indeed pseudoscience. Rather than sort through such issues, I will simply accept that it *may* be possible to demarcate, *a priori*, certain claimed 'scientific' contributions of whatever 'unit', and hence possible to legitimately 'demarcate' such improper contributions as pseudoscience – both terms would be justified by such a process if properly utilized.

However, the number of significant 'pseudoscientific' claims to which this potentially applies is extremely limited. This is evidenced by the fact that almost any *major* 'pseudoscientific' claim you care to mention could, technically, be true - at least in an alternate universe - or, if that is too hard to swallow, may at least require different counter arguments in an alternate universe. ESP, UFOs, Bigfoot, the Loch Ness Monster, 'alternative' medical interventions, even invisible entities justifiably termed 'ghosts' or 'demons', and the like – none of these *necessarily* involves any inherent contradiction, and thus their acceptability cannot be ruled out *a priori*. If there are exceptions to this – forms of these arguments or hypotheses that *do* involve contradictions, they are not among the most attractive or scientifically sophisticated, and thus most important for our purposes, examples of 'pseudoscience'.

Significantly, acceptance of the possibility of some form of *a priori* demarcation does not indicate that any of the proposed *a priori* criteria discussed previously are applicable. The unsuitability of Methodological Naturalism has already been established. The remaining options – testability and falsifiability – have also been criticized. Further to this, in practice we never confront truly untestable or unfalsifiable hypotheses because the basic unit of science is necessarily the advocated proposition, which takes the basic form 'A, because X, Y, Z', or some equivalent or cognate. Hypotheses such as those typically advanced as an example demonstrating unfalsifiability – 'God created the universe' or the like, or Lugg's undetectable electron nucleus example - which simply take the form 'A', are never, in fact, encountered. To mention such a possibility, sans any reference to empirical significance or any reason to entertain it, is really just to meaninglessly name one of the effectively infinite number of details a universe may hypothetically include. Science cannot possibly deal with such ephemeral theoretical entities, nor could anyone have any reason to advocate one, until at least some hint of more detailed empirical significance is in evidence. Thus, such hypotheses can appear in science only when they are advocated for pursuit and development or acceptance, and this necessarily involves reference to reasons for taking the hypotheses seriously – the addition of something following 'because'.

This being the case - because every proposition with which we are actually confronted includes specified reasons why it should be at least entertained as worthy of pursuit – no such proposition is truly untestable or unfalsifiable: we can *always* assess the evidence advanced as supporting a hypothesis. If the supporting evidence is found wanting for any reason – whether because the evidence does not in fact exist, or because it is found to be irrelevant, or ambiguous, or more reasonably explicable through alternative hypotheses, then the claim can be found to lack support, and on that basis has been tested and falsified. It does not matter if we cannot provide any further reasons to reject the proposition. A lack of supporting evidence is sufficient to ensure it no longer warrants consideration. Even if we should find, as Popper maintained of Freud's propositions, that some advocated process can in fact accommodate any possibility, and therefore its claimed successes are evidentially meaningless, this itself is a rejection of the evidence adduced for accepting the process' claimed utility, and hence a falsification.

In regard to the assertion that God created the universe, when this is combined with evidence purported to establish its truth, it becomes, as previously argued, a proposed *best explanation* for that

evidence. It can accordingly be tested in terms of the accuracy of the evidence adduced and its true significance relative to the hypothesis, and is therefore falsifiable. Any argument successfully establishing that the hypothesis is *not* the best explanation of the claimed evidence falsifies the claim that it *is* the best explanation. The fact that we cannot *prove* the claim to be false is irrelevant – the same is true of many scientific claims.

Moving to the consideration of 'units' of demarcation, the 'pseudoscientist' as an individual is the most readily disposed of. As argued above, we know that the same individual can behave at one time as a 'pseudoscientist', and at another time as a scientist, and therefore the most that can be justified is the claim that someone is acting, or did act, as a 'pseudoscientist' at a given time. Further, the relevant argument must be based upon some specific example(s) of the individual's publication(s), which must, in order for the argument to work, be judged to be 'pseudoscience'. Because we cannot believe or reject an individual directly, but rather only their statements, any practical conclusion must then refer back to that same publication. We therefore move from the 'pseudoscience' that proves its author to be a 'pseudoscientist', to declaring the same publication 'pseudoscience' because it was authored by that 'pseudoscientist', an argument which is circular subsequent to the initial characterization of the publication. Lugg's argument that only 'pseudoscientists' can be demarcated, as it is they that hold scientific 'pretensions', is just another confusion of *ad hominem* and *ad argumentum*. We do not need to know who an author is or what they believe in order to justifiably reject their argument as failing to meet relevant standards, as evidenced by the fact that the anonymity of an author would have no impact upon an analysis of their arguments. We can therefore confidently remove individual 'pseudoscientists' from the list of acceptable demarcation 'units'.

The previous paragraph employs an important and surely uncontroversial, but thus far implicit, point: in order to be of any practical use, demarcation arguments must be applied, and thus applicable, to *claims*. It is not possible to *directly* believe or reject an individual, a topic or field, a research programme, or anything else of this sort, *in itself*. Whatever argument we employ, and whatever 'unit' of demarcation we use, we must be able to detail how the rejection of a 'claim' or 'claims' has been justified. The meaning of 'claim' need not be defined precisely at this point, as we are considering here a function rather than a specific entity. However, upon consideration it becomes apparent that everything that can be directly accepted or rejected includes a *conclusion* of some kind, whilst the entities just listed as examples of things it is impossible to directly reject do *not* include specifiable conclusions – they are all ambiguous in that regard.

With this in mind, it is evident that topics (e.g. 'UFOs') or fields (e.g. 'Ufology') are too broad to be of use as a 'unit' because they potentially include conflicting or directly contradictory propositions and conclusions. A topic includes both the arguments of claimed 'pseudoscientists' and those of debunkers seeking to refute the latter. Indeed, a debunker arguing that UFOs should be regarded as a 'pseudoscientific' topic would technically be disqualifying their own argument, which itself falls under that topic, as 'pseudoscience'. Thus, any demarcation argument (*a priori* aside) could not be directly applicable to an entire topic and yet justify a conclusion regarding the acceptability of any specific 'claim'. The situation is the same with 'fields', which are distinguished by topic: 'Ufology' simply refers to research on the topic of UFOs. To argue that 'Ufology' does not include the arguments of UFO debunkers is to attempt to use that term as a label for conclusions, and hence not as a 'field'.

When debunkers claim that a topic or field is 'pseudoscientific', we can most charitably understand them as intending rather to say that a particular prominent claim within that field is 'pseudoscience'. Thus the assertion that 'Ufology' is 'pseudoscience' is best understood as referring rather to some collection of hypotheses within that field, to which whatever argument has been employed is relevant – hypotheses grouped according to argument, or evidence type, or conclusion.

To the extent that a 'research programme' or 'research tradition' refers to something other than a grouping of hypotheses such as just discussed, it is as a historical entity upon which arguments relating to developmental features like 'progress' can be based. Such arguments refer not to hypotheses in themselves, but rather the relationships between them over time. Given that any useful demarcation argument must address the merits of some argument or conclusion, it can be seen that such developmental details are not fit for this purpose. When assessing the acceptability of an argument or publication, we do not need to determine who the author is or the process through which they came to advance the argument in its current form (the 'context of discovery' – Popper 1935, pp. 8-9; Garcia 2006, pp. 13-14). It would make no sense to insist that the merits of a given publication cannot be determined without knowledge of previous versions. Such a position would lead to a situation in which different conclusions may be arrived at regarding precisely the same argument, merely because it has been advanced independently by two authors, one *de novo*, the other with a long history of publications on the topic. The historical development of a proposition ultimately refers to the activities of its proponents, which are often not directly relevant to the status of that hypothesis. As with the proposed criterion of requiring progress, criticized extensively above, we should expect of any developmental criteria that many potential contributing factors will not be suitably relevant to the epistemic status of the hypothesis itself.[65]

If research programmes or the like are to be a suitable unit of demarcation, then, it is not as historical entities, but as just another way of referring to a collection of related hypotheses. A research programme can be regarded as the work of a group of individuals united by their shared assumptions and beliefs. However, the contributing individuals can potentially focus on different evidence, employ different arguments, and come to somewhat different conclusions (Le Grand 1988, pp. 5-6, 125-126). Thus a research programme is a rather loose and imprecise grouping (Derksen 1993, pg. 21), and as such is less useful than deliberate groupings of hypotheses according to demarcation-relevant features. It may or may not prove that the same demarcation argument can apply to all hypotheses within a research programme, but to the extent that such is the case, it is because the research programme is acting as a collection of hypotheses sharing some relevant feature, and thus it makes more sense to simply refer directly to that grouping. If different arguments are required to demarcate all the hypotheses within a research programme, then it is not serving as a unit of demarcation.

Thus in terms of 'units' we have so far eliminated individuals, topics or fields, and research programmes, and of possible 'means' we have rejected *a priori* arguments as inapplicable in most cases, and rejected developmental features. This leaves methods and empirical concerns as possible means, conclusions, arguments, individual hypotheses and various groupings of hypotheses as units.

I include the attitudes of 'pseudoscientists' under 'methods' because such attitudes are only relevant in as much as they influence behavior – the fact that a 'pseudoscience' proponent is perceived as dismissive of counter evidence, for example, is irrelevant if they address that evidence adequately in their publications. Hence attitudes and other psychological properties of proponents can be ignored in favour of addressing the methods they actually employ in advancing their arguments. Further, we have rejected arguments focusing on the relationships between series' of hypotheses, and therefore, in employing methods as demarcation criteria, must consider only the methods presented within an argument, as revealed within publications.

I have argued previously that many methodological arguments cannot be resolved without examination of the relevant empirical context. This is essentially because the methods being considered are present within genuine science, and thus are not improper in themselves, but rather only if employed in some unacceptable manner. This means that any demarcation argument intended to employ methods

[65] The effects of WWI and WWII on the development of Continental Drift are one clear example of this (Le Grand 1988, pp. 61, 117-118).

alone, without reference to empirical matters, must identify methods that are utterly forbidden within science. Clearly, if such genuinely disqualifying methods are utilized within all 'pseudoscientific' arguments, demarcators have thus far failed to successfully identify them.

Indeed, we should be surprised to discover such features within many 'pseudoscientific' arguments, simply because their advocates aspire to be (Shermer 2013, pg. 221; Gordin 2012, pg. 1), and sometimes *are*, scientists who respect scientific processes and seek to employ standard scientific arguments. Further, because 'pseudoscientists' typically believe their claims are true and that the evidence clearly reflects this fact (Ladyman 2013 pp. 47-48, 52; Goode 2013, pg. 151; Derksen 1993, pg. 23), they do not feel under any pressure to resort to improper methods. Therefore, if utterly unacceptable methods are employed, it is an accident: the advocates do not recognize their methods as improper. On the other side of the coin, scientists too are seeking to advocate propositions, and are ultimately subject to the same basic human failings and frailties as anyone else (Oreskes 1999, pg. 4; Bausell 2007, pp. xv-xvi, xiii, 35, 38ff., 69ff.). Thus, if 'pseudoscientists' fail to recognize the impropriety of a particular method, there is a decent chance scientists will occasionally make the same mistake. Further reasons for rejecting the idea that all 'pseudosciences' involve the use of methods never encountered within genuine science will be advanced shortly, in the form of an assessment of specific examples.

If we therefore reject the possibility of using methods in isolation, we are left with demarcation based on interrelated methodological and empirical arguments. Such a conclusion accords with the evidence within debunking literature: when debunkers are not referring to *ad hominem* and other clearly improper issues, they indeed typically employ standard evidence and method focused arguments (see for example: Hines 2003, pp. 213ff.; Klass 1983, pp. 135ff.; Klass 1974, pp. 74ff.; Jordan 2009; Wynn & Wiggins 2001, pp. 72-76; Peebles 1994, pp. 248ff.; Kelly 2004, pp. 12-19; Nickell 2001, pp. 276ff.). Indeed, it seems that the *best* examples of debunking literature, providing the strongest arguments, focus clearly and almost exclusively on such matters. Of course, it could be objected that these latter points are circular – that I am identifying as the 'best' examples of debunking arguments those that meet my expectations. However, the same conclusion can be established if we seek to identify the most highly researched and detailed arguments, which *should* accordingly be the rationally strongest.[66] In fact, because the great majority of 'pseudoscientific' claims fall within the subject matter of science – it has been shown that they cannot be dismissed *a priori* as improper to science – we should *expect* to find that the strongest arguments addressing such matters are scientific. This argument simply refers back to the basic position underlying the Demarcation Problem - the belief that science is our most authoritative tool for establishing claims about empirical matters.

What can we then conclude about the unit of demarcation? Arguments of the type now under consideration cannot legitimately be used to reject a conclusion alone. Whatever evidence we may offer to establish that a conclusion should not be accepted, unless the evidence advanced in *support* of that conclusion is also addressed, the argument is incomplete. It must be demonstrated that the evidence against the conclusion outweighs that offered in its favour, and this cannot be achieved without reference to the latter. So demarcation based on methodological/empirical arguments must address the methodological/empirical *arguments* of 'pseudoscientists'.

Debunkers and demarcators want to establish that certain claims should be disqualified - that they are not and could never be genuine science. Is there, then, any way to justify such a conclusion with the

[66] The best examples arise from medical topics such as 'Alternative Therapies' and the vaccination/autism controversy, wherein the literature is unmistakeably scientific and relies heavily on direct scientific research. See for example Goldacre 2008; Singh & Ernst 2008; Bausell 2007 an 'Alternative Therapies. Regarding the vaccination controversy, detailed research is presented in: Madsen et. al. 2002; Taylor et. al. 2002; DeStefano et. al. 2004; Mrozek-Budzyn, Kieltyka, & Majewska 2010; Smeeth et. al. 2004. Articles providing similar information to the public in a considerably more accessible form include: Farrington, Miller, & Taylor 2001; Gerber & Offit 2009; DeStefano 2007.

means and units available? This objective could be most easily achieved in regard to an individual iteration of an argument - a publication. It could be demonstrated that the argument in question, including particularly the specific evidence appealed to, fails to establish the stated conclusion, and so acceptance of that conclusion, on the basis of the target argument, is unjustified. This conclusion is ahistorical to the extent that it specifies the evidential context of the argument, such that any external change in the available evidence is irrelevant. The target argument therefore cannot itself become science simply because it is unchanging – any alteration produces a *different* argument.

Obviously such a process is rather artificial and unsatisfying as it cannot justify disqualifying a conclusion or claim. If demarcation must address arguments, then any determination about a *conclusion* must be built up from criticisms of *all* the arguments supporting that conclusion. However, in practice it would be sufficient to address the strongest arguments – if these fail, then any less satisfactory arguments will also. The caveat 'unless a significant argument has been missed' is not really problematic as it unavoidably applies to all scientific disputes with implications beyond a specific argument.

One major limitation, of course, is that the rejection of extant arguments cannot establish that no argument supporting the conclusion to be 'demarcated' could *ever* be worthy of scientific attention. Technically, the fact that we cannot dismiss a claim *a priori*, but must refer to established knowledge of the universe in order to justify its rejection, is itself enough to ensure we cannot entirely rule out that the claim could in the future warrant scientific attention. Such a reversal may require that our understanding of the universe is radically transformed, and I am not suggesting that we need always actively take such potentialities seriously – that 'anything is possible' should limit what we are willing to reject. On the other hand, to simply ignore this technicality would be an error.

It should have become obvious some time ago that the process under discussion will not be distinguishable from scientific disputation: to appeal to methodological/empirical matters in refuting an argument is to engage in scientific disputation. Again, this is exactly what we should expect. Science is the most powerful tool we have in deciding empirical matters, so to employ some process termed 'demarcation' *instead* would be reprehensible. Even if, contrary to all that has been said thus far, such a process could be developed to successfully justify the required conclusions in relation to the intended target claims, it could not be through use of a method 'better' than those of science: science automatically includes the best methods available to address issues within its domain, so any 'better' method would necessarily be incorporated into science. To demarcate using a non-scientific process would therefore be inappropriate, as it is undeniably improper to advocate a conclusion utilizing anything other than the best and most reliable methods available.

It may possibly be suggested that some significant difference still remains – perhaps that there is something about 'pseudoscientific' claims that warrants a more assertive response, or greater certainty in the conclusions reached than is typically the case in science. 'Pseudoscientific' claims certainly tend to provoke a distinct psychological effect. They are perceived by many as a challenge to science, a reversion to irrationality and superstition, and this leads to a degree of anger and/or disgust not induced by claims recognized as scientific, and a resultant desire to evict them from reasonable discourse entirely. However, I will provide arguments below to demonstrate that such perceptions are often mistaken, arising from misunderstandings, misrepresentations, and the partisan nature of demarcation disputes. Moreover, it cannot really be the case that 'pseudoscientific' claims are more completely refutable than erroneous scientific assertions. Are we truly any less certain that Contractionism is incorrect than that Bigfoot does not exist? Many of the scientific hypotheses championed in the past have been rejected with a high degree of certainty – effectively as much certainty as science can justify – so it is not actually possible for 'pseudoscience' to be rejected with *greater* certainty.

This in itself suggests yet another argument against the justifiability of demarcation claims. As has been repeatedly noted, to demarcate a target requires maintaining that there is no possibility of

revision of that determination or revival of the target's scientific fortunes. It *must* be *impossible* for 'pseudoscience', which is *not science at all* and has accordingly been disqualified, ejected from scientific discourse, and declared an improper subject for serious consideration (Gordin 2012, pp. 1, 2, 12; Thurs and Numbers 2013, pp. 137-138; Hansson 2013, pg. 68; Mousseau 2003, pg. 271; Ladyman 2013, pp. 46-47; Lugg 1992, pg. 91), to subsequently become science. However, this aspect of demarcation could only possibly be justified via arguments appealing to *a priori* or inherent features of a target, which have been revealed as inapplicable to most 'pseudoscientific' claims. To reach such a non-tentative conclusion through arguments including reference to empirical matters is widely recognized as impossible: our understanding of the universe is incomplete and evolving, and empirical claims are, accordingly, unavoidably subject to revision (Ruse 2009b, pg. 274; Rosenberg 2005, pp. 61, 65, 116-117; Derry 1999, pg. 92; Hitchcock 2004, pg. 5). Thus, even if demarcation could be achieved without relying solely on routinely scientific, empirical arguments, as long as such are significant in justifying the conclusion, that conclusion cannot be more unreviewable than science allows.

4) Against 'Demarcating Pseudoscience' 2: Examples

But perhaps the arguments advanced thus far are somehow incomplete, or I have made some mistake along the way that undermines the points I am seeking to establish. Another way to come to these same conclusions is through examining practical examples of demarcation: filling in the details which illustrate the difficulties confronting demarcation endeavors.

4.1) 'Pathological Science'

It will be instructive to examine at this point two claims that have occasionally been denounced as 'pseudoscience', but are more often discussed as cases of 'pathological science'. The latter term was introduced by Irving Langmuir in 1953, when he provided six identifying characteristics.

1) "The magnitude of the effect is substantially independent of the intensity of the causative agent."
2) "The effect is of a magnitude that remains close to the limit of detectability, or, many measurements are necessary because of the very low statistical significance of the results."
3) "Claims of great accuracy."
4) "Fantastic theories contrary to experience."
5) "Criticisms are met by *ad hoc* excuses thought up on the spur of the moment."
6) "Ratio of supporters to critics rises up to somewhere near 50 percent and then falls gradually to oblivion" (Langmuir & Hall 1989 pp. 16-17).

Langmuir did not intend that these be used as anything like demarcation criteria, but rather had noted that such features appeared to be characteristic of revolutionary claims that proved to be false.

4.1.i) Cold Fusion

The 'Cold Fusion' episode of the late 1980s has subsequently often been labelled as 'Pathological Science' (Turro 1999, pp. 3ff.; Rousseau 1992 pp. 60ff.; Huizenga 1993, pp. 206ff., 218, 274, 287). In 1989, electrochemists B. Stanley Pons and Martin Fleischmann claimed to have produced fusion at room temperatures using a relatively simple electrical cell apparatus. More specifically, they claimed to have detected the production of excess energy in the form of heat which, they believed, could only be explained through nuclear fusion, which was what they had set out to achieve. This claim was highly significant as, if true, it represented a clean and practically inexhaustible new source of energy that could replace fossil fuels or standard nuclear energy. Therefore, following Pons' and Fleischmann's public announcement of their results on March 23, 1989, there was a flurry of scientific research which continued for some years (Huizenga 1993, pp. 294-303). More than one hundred teams seeking to replicate the original experiment reported that they too had detected excess heat or other evidence of fusion (Huizenga 1993, pg. xi). On the other hand, a number of other groups were unable to replicate the results or find any evidence that fusion was truly taking place (Huizenga 1993, pp. 30ff, *et passim*).

The situation was also theoretically problematic. Most of the relevant experts believed that the fusion process was well understood, and that the results of Pons and Fleischmann simply did not add up in terms of theoretical expectations. In particular, the amount of heat claimed to have been detected required fusion to occur at a rate that should have produced enough harmful radiation to immediately kill anyone present. Such discrepancies led many physicists, in particular, to be highly skeptical of the Cold Fusion claims. The proponents of Cold Fusion, on the other hand, responded by suggesting that the

evidence indicated that a new, previously undiscovered form of fusion must be responsible (Huizenga 1993, pp. 108ff).

The counter evidence and contrary theoretical arguments proved insufficient to end the controversy as long as proponents were able to cling to seemingly positive experimental results which demanded explanation. Over time, however, the counter evidence continued to build, and the positive evidence was progressively shown to have arisen from various experimental errors. Ultimately, convincing arguments demonstrating that the original results were best understood as arising from a number of experimental flaws were advanced. This, to the satisfaction of the majority of experts, effectively removed any basis for continuing to regard the issue as worthy of pursuit (Huizenga 1993, 59ff. *et passim*). Nonetheless, a number of individuals continue to pursue the topic to this day (Ouellette 2012)

4.1.ii) Polywater

A similar set of events occurred in relation to the claimed discovery of 'Polywater', a substance produced from ordinary water through a method involving the formation of condensation within capillary tubes (Rousseau 1992, pp. 55-56; Huizenga 1993, pp. 189ff.). Polywater was found to have properties very different to those of ordinary water – for example: it was far more viscous, froze at -50 degrees Celsius, and boiled at around 300 degrees Celsius (Rousseau 1992, pg. 55). Originally 'discovered' in 1962, the substance, early on referred to as 'Anomalous Water', was little known outside the USSR until 1966, when B. V. Deryagin presented a paper at the Faraday Society (Huizenga 1993, pg. 189; Rousseau 1992, pg. 55). Scientists in the United States then employed infrared spectroscopy on samples of the Anomalous Water which revealed a spectrum entirely different to that of normal water. This result was interpreted as establishing a unique molecular geometry, and thus that the substance was a polymer of water – hence 'Polywater' (Rousseau 1992, pg. 55). Such claims led to a flurry of scientific activity lasting more than a decade, with around five hundred papers published on the topic between 1962 and 1974 (Huizenga 1993, pp. 190-191).

As with Cold Fusion, however, evidence progressively accumulated which indicated that Polywater was in fact no more than water contaminated by a large number of impurities. Initial attempts to explain such findings away – claiming that only some samples of Polywater were contaminated, etc. – were progressively overcome as the evidence mounted, until eventually even major proponents were forced to admit they had been mistaken (Rousseau 1992, pp. 56-57).

4.1.iii) 'Pathological Science' or 'Pseudoscience'?

Numerous commentators now regard Polywater (see for example Beyerstein 1996, pg. 8) and Cold Fusion (see for example Blair 2003, pg. 4) as examples of 'pseudoscience'. Obviously they cannot mean that *every* contribution to that subject was non-science. Scores or hundreds of articles on Cold Fusion and Polywater, respectively, were published by accredited scientists or teams of scientists in respected, peer reviewed, scientific journals. To maintain that these publications are all 'pseudoscience' requires holding that every individual scientist or member of a research team be regarded as having acted as a 'pseudoscientist', and also that the referees assessing the submissions were acting in an extremely problematic, improper manner. Clearly such a position is not credible, and thus to assert that Cold Fusion or Polywater is 'pseudoscience' produces the requirement of establishing where and how the line between scientific and 'pseudoscientific' contributions should be drawn.

As before, the apparent claim that these *subjects* are 'pseudoscience' is best understood as referring in fact to the relevant key claims or *conclusions*. The demarcators are evidently asserting that

Polywater and Cold Fusion are not legitimate subjects for ongoing research, and that any contrary conclusion should be dismissed as 'pseudoscience'. As previously argued, however, demarcation cannot justifiably address conclusions alone. As discussed by Nicholas Turro in his article on 'pathological science', that fact that the claims are revolutionary just means that they conflict with orthodox expectations, but so too have many propositions that have ultimately been accepted, and thus this point in itself cannot justify dismissal or rejection (Turro 1999, pg. 2). The evidence and arguments offered in favour of the contested conclusions must, therefore, be addressed, and so any determination regarding such conclusions must be built up through assessments of the relevant supportive arguments.

It may be contended that the research methods and/or the arguments of advocates were, we now know, significantly flawed, and this fact may therefore be employed as the basis for demarcation. Thus the claims of advocates could be individually rejected through reference to the flaw or flaws relevant to each case, such that the positive conclusions they advanced can be rejected overall as unfounded 'pseudoscience'.

This argument would immediately refute any claim that 'pseudoscience' can be immediately recognized as such by properly equipped individuals, as it would mean that hundreds of contributions were authored by scientists, and accepted for publication by referees, despite their being 'pseudoscience'. Further, because advocates employed various research methods and advanced a number of distinct arguments, the demarcation process would in practice necessitate identifying *various* specific flaws as sufficient to justify the disqualification of pseudoscience.

This suggestion does not appear to be credible. Perfection in scientific experimentation is very rare, and flaws are accordingly extremely common within both research efforts and arguments, yet we do not in other cases declare flawed science to be, therefore, 'pseudoscience'. Likewise, scientists arguing that a publication exhibits certain flaws do not automatically conclude that it is therefore not science and to be disqualified from further pursuit. To the contrary, the exposure of error is very often the basis for further research efforts. Flawed experiments and the conclusions they support are commonly *not* rejected, but rather it is determined that further effort seeking to correct the identified mistakes is warranted. This establishes that the identification of flaws is not in itself sufficient to justify complete dismissal of any proposition, finding, or conclusion: the wider scientific context must be taken into account in a manner not reducible to any specific formula. There is no immediately apparent basis for insisting that the flaws involved in the episodes under consideration were truly unique in any significant way, and demarcators have not succeeded in identifying any set of specific errors which characterize 'pseudoscience' and justify demarcation. Thus overall there appears to be no process whereby the identification of errors can independently justify a demarcation determination.

The most promising approach would seem to be identifying an evidential 'tipping point' beyond which continuing to advocate a positive conclusion is *so* unreasonable it justifies dismissal of the relevant argument as 'pseudoscience'. It would then be only 'positive' arguments advanced after a specific time (or occurrence) in a dispute that are 'pseudoscience'. In this scenario some specific publication presenting the relevant datum or data would need to be identified, subsequent to which failure to address the evidence within that publication constitutes an unacceptable exclusion of key data, whilst failing to recognize the true import of that data is likewise an unacceptably severe error.

This is all in perfect accordance with the conclusions reached previously, and subject to the same objections. Any discussion of such a 'tipping point' would necessarily involve detailed and above all *scientific* assessments of the applicability, strength and relative significance of the relevant data – thus methodological/empirical discussion relating to specific arguments. Moreover, although it may be possible in some cases to point to a relatively precipitous empirical event, in many cases there would be a far more gradual build-up of evidence on one side of the ledger. This latter circumstance more clearly reveals the fundamental nature of the claim under discussion: that, of a series of arguments regarded as

representing progressively *worse* science, there is a point at which they become such bad science that they cease to be science at all, with no possibility of revision or revival. Such an assertion is of itself manifestly problematic. Moreover, the process employed in criticizing the arguments does not change as the demarcating line is approached and crossed, and there is no *specific* identifiable feature that suddenly appears, disappears, or significantly alters with the advent of 'pseudoscience'. Thus 'demarcation' as a process is identical to routine scientific refutation and disputation. It is only the conclusion that differs, despite the process by which that conclusion is reached including no inherent justification for use of the term 'pseudoscience'.

If the latter term is to have any justifiable meaning in such a scenario, it could only be by convention. However, because there is no objective measure for the 'badness' of science, we can neither specify in objective terms where any conventional line of demarcation lies, nor objectively determine where any contribution lies relative to it. Finally, even if such obstacles could be overcome, such a method cannot justify the required insistence that this scientific determination, unlike all others, is not tentative – that there is *no possibility* of this determination being reversed, or even of revision being an acceptable topic of discussion. We may have a very high degree of confidence that such will not be the case, but technically, at least, there is no way to justify maintaining that such confidence may not prove to be misplaced.

The obvious alternative is to hold instead that Cold Fusion and Polywater are *not* properly regarded as examples of 'pseudoscience'. This position too places considerable pressure on the repeated assertions of unanimity regarding what is and is not 'pseudoscience', and the related claim that 'pseudoscience' is immediately recognizable as such. More importantly, it requires that there be some sufficiently important, epistemologically significant difference between all examples of science – including 'pathological science' – and all 'pseudoscience'. To test whether this is accurate, it is necessary to now examine an example of 'pseudoscience' in greater detail.

4.2) A Paradigmatic 'Pseudoscience': Hynek's Ufology

I will discuss an example that is universally accepted by demarcators as a classic, truly paradigmatic case of 'pseudoscience': Ufology (Thurs and Numbers 2013, pg. 138; Mahner 2013, pg. 31; Shermer 2013, pg. 206), as advocated by one of its most prominent proponents, Dr. Joseph Allen Hynek (Williams 2000, pg. 154). Hynek was the director of Ohio State University's McMillan Observatory when he was asked to serve as astronomical consultant to the United States Air Force's (USAF's) 'flying saucer' investigation, 'Project Sign', based at the nearby Wright-Patterson Air Force Base in Dayton, Ohio (Hynek 1972, pp. 15-16). 'Project Sign' had begun in September 1947, and would be renamed 'Project Grudge' in 1949, and 'Project Blue Book' in 1951 through to December 1969, when the USAF officially ended its UFO research efforts. Hynek would serve as the astronomical consultant, and the only scientific consultant on staff long term, for projects 'Sign' and 'Blue Book', but was not asked to be involved in 'Grudge' (Hynek 1972, pp. 15-17).

Hynek reports[67] that he was initially highly skeptical of UFO reports, which he had previously dismissed with "…many a heart guffaw…" for the "…naivety and gullibility…" they revealed. He notes that many of his colleagues were wary of his involvement with such an 'unscientific' subject, but he thought he may be able to do something worthwhile in "…helping to clear away 'nonscience'" (Hynek 1972, pg. 16). Hynek subsequently became extensively involved in the 'Blue Book' investigations: he reviewed every report that was submitted and personally undertook many on-site investigations,

[67] Obviously I must rely on Hynek's own account for the following details, but there is no obvious reason to disbelieve him, and even if there was it would make little difference to any final assessment of his position.

personally interviewing witnesses and examining the scenes etc. Obviously, what he found over time led him to revise his originally skeptical position (Hynek 1972, pg. 17). Hynek reports that the evidence from project Sign caught his attention, but was insufficient to convince him. There were a number of reports which, taken at face value, were difficult to explain in conventional terms, but the data was of poor quality and incomplete, and Hynek recognized that additional facts could potentially reveal that such cases were all misidentifications. It was only with Blue Book's accumulation of more complete reports, which appeared to include all the relevant information and began to suggest significant patterns, that Hynek began to believe that the evidence was suggestive of something more significant (Hynek 1972, pp. 213-216)

4.2.i) 'The UFO Experience'

Thus in 1972 Hynek published *The UFO Experience: A Scientific Inquiry*. In this book, Hynek seeks to justify his conclusion that UFOs, or more precisely UFO *reports*, are worthy of scientific study. He explicitly does not endeavor to establish any particular explanation, because he does not believe that the available evidence is sufficient to justify doing so (Hynek 1972, pp. 8, 26, 266-267). Of course, by 1972 many books and articles had been published, often advocating a particular explanatory conclusion – most often the Extraterrestrial Hypothesis (ETH) - or generally providing "…one UFO story after another, each more spectacular than the other" (Hynek 1972, pg. 7). Hynek accordingly dismisses much of the extant UFO literature, and explicitly rejects the sensational claims of 'cultists', 'contactees' and 'true believers' who insist that UFOs are alien spacecraft (Hynek 1972, pp. 19-20). His own book is intended primarily to present the UFO evidence which he believes deserves serious scrutiny.

Hynek is aware of the difficulties arising from employing witness reports as a primary evidence source, and recognizes that the great majority of UFO reports arise from the misidentification of mundane objects. Accordingly, he first proposes a definition of 'UFO' that excludes those that are readily identifiable, so ensuring that time is not wasted on the consideration of reports which are, at least from his perspective, irrelevant.[68] "We can define the UFO simply as the reported perception of an object or light seen in the sky or upon the land the appearance, trajectory, and general dynamic and luminescent behavior of which do not suggest a logical, conventional explanation and which is not only mystifying to the original percipients but *remains unidentified after close scrutiny of all available evidence by persons who are technically capable of making a common sense identification, if one is possible*" [emphasis added] (Hynek 1972, pg. 26). He then devotes a chapter to discussion of witness reliability, in which he examines the accuracy of reports in cases where the stimulus was subsequently identified, thus covering the extent to which witness reports tend to provide accurate information, and in what circumstances. He emphasizes the importance of background checks to establish the general character of witnesses, and the impact of relevant variables such as the duration and distance of sightings, the use of instruments, and the number and relative positions of witnesses (Hynek 1972, pp. 33-38).

Hynek applies an initial classification system to sort UFO reports according to two measures – a 'probability rating' arising from 'highly subjective' calculations regarding the credibility of witnesses and the accuracy of the details provided (Hynek 1972, pp. 35ff), and a 'strangeness rating' "...a measure of the number of information bits the report contains, each of which is difficult to explain in common-sense terms" (Hynek 1972, pp. 42). The 'credibility' score ensures that single-witness cases are largely ruled out, and thus the cases Hynek advances as evidence involve independent, corroborative witness reports, as well as visual reports supported by radar returns (radar-visual reports). The 'strangeness' rating

[68] Given that both sides agree on the fact that the majority of reports are attributable to misidentifications arising from the frailties of human perception, it is indeed a waste of time to focus on such cases. They have value in establishing the pitfalls of using witness reports as evidence, but this is not in dispute.

meanwhile serves to focus attention on those cases that are the most relevant to Hynek's contention that there is a genuine signal within the noise. Hynek then further sorts reports into categories according to their content, including in particular the distance of the UFO at closest approach and significant details such as interactions with the environment which may leave trace evidence, and in one category (C.E.III) the presence of 'occupants'. This is the system from which the term 'close encounter' (C.E.) arises - an indication of the extent of this book's influence within Ufology (Hynek 1972, pp. 44-49).

Having presented the evidence relating to each type of report and discussed discernable patterns within the evidence, Hynek concludes: "…it *has been established that*:

(1) There exists a phenomenon, described by the contents of UFO reports (as defined here), that is worthy of systematic, rigorous study. The extent of such a study must be determined by the degree to which the phenomenon is deemed to be a challenge to the human mind and to which it can be considered potentially productive in contributing to the enlightenment and progress of mankind.
(2) Even allowing for the unfortunate and disorganized manner in which the data have become available for study, the body of data points to an aspect or domain of the natural world not yet explored by science.
(3) For a directed, objective study of the phenomenon the available data require major organization, systematization, and the adoption of a uniform terminology for their description and evaluation. Such organization and systemization must be applied in the gathering and processing of new data.
(4) Investigations that have sought to disprove the above have failed to make a case. Blue Book and the Condon Report are the principal examples of such fruitless efforts.
(5) The probative force of the four uncontestable statements above strongly suggests that new empirical observations exist that describe a *new fact* – the existence of UFOs (as defined here) – which needs to be brought within an acceptable framework of concepts and, if possible, explained. Further work of an unbiased character is clearly the next step.

It is likewise important to keep clearly in mind what the previous chapters have not attempted to establish, prove, or show. It has not been shown:

(1) That the new fact implied in (5), above, requires a basic shift in our outlook on the natural world.
(2) What a *verifiable* explanation of the UFO phenomenon is. An organized approach to the problem must be formulated. In outline, the following steps should be taken:
    (a) The problem must be rigorously defined, and extraneous aspects must be clarified and set apart from the main problem.
    (b) Feasible, tractable methods of attack must be outlined, with great care being taken to avoid involved, prohibitively costly, and open-ended paths (for example, the establishment of thousands of manned or automatic highly instrumented observing stations)". (Hynek 1972, pp. 265-6).

Hynek then goes on to discuss what form further research should take, focusing on two basic approaches: statistical analysis and "…examination, in depth, of individual multiple witness cases…" (Hynek 1972, pp. 266ff.). Hynek briefly discusses some statistical research that has already been undertaken by Ufologists (with, from their perspective, positive results), and states that: "A serious scientific group engaged in such studies would, given access to the data in machine-readable form, soon demonstrate beyond any reasonable doubt whether there was anything substantive in the UFO problem" (Hynek 1972, pp. 267-270).

As well as advancing his own claims and evidence, Hynek criticizes arguments contrary to his own conclusion. He first addresses the arguments advanced by individuals who have not undertaken a detailed examination of the evidence, but rather dismiss the topic as undeserving of serious attention (Hynek 1972, pp. 22ff.). Hynek quotes the philosopher of science Thomas Gouge who, explicitly addressing the UFO problem in a personal communication with Hynek, argues that "…a necessary condition of scientific advancement is that allowance must be made for (1) genuinely new empirical observations and (2) new explanation schemes, including new basic concepts and new laws" (Hynek 1972, pg. 23). Hynek accordingly maintains that those scientists who dismiss the subject without due consideration of the facts are thereby acting in a scientifically unacceptable manner and advancing invalid, unjustified arguments (Hynek 1972, pp. 23-24).

He also emphasizes that both the scientific community and the public at large have been seriously misled regarding the nature of the problem by both the press and the Air Force (Hynek 1972, pp. 24, 212, 213ff), and contends that if the USAF had turned the problem over to a scientific body once the lack of relevance to defense had been ascertained, "…scientific respectability for the subject…" would have resulted, and "It might have been determined whether there was any 'signal' in the 'noise' (Hynek 1972, pp. 216-217). Instead, for reasons relating to defense and security, the USAF, rather than undertaking a scientific research effort as per its public pronouncements, enacted a dishonest public relations campaign focused on dismissing reports and minimizing public attention on the topic overall (Hynek 1972, pp. 210-212, 215, 216ff).

It is worth briefly including one example, taken from the Blue Book files, which Hynek emphasizes as illustrative of the latter claim (Hynek 1972, pg. 130). According to their report, on the night of April 16th, 1966, Deputy Sherriff Dale F. Spaur and 'mounted deputy' Wilbur Neff observed an airborne light-source at around 100 ft. elevation, which moved to hover in a position directly above their cruiser and emitted a humming noise. They were ordered to follow this 'object' as it moved away, and subsequently chased it, at speeds up to 105 miles per hour, for more than 70 miles (Hynek.1972, pp. 131-133). 40 miles East of where the chase begun, Officer Wayne Houston was positioned along route 14, along which Spaur and Neff were progressing, listening to their discussion with the dispatcher. According to his own report, when the other officers were roughly 5 miles away, he observed a lighted object which he estimated to be at around 800-900 feet, "…shaped something like an ice-cream cone with a sort of partly melted top. The point part of the cone was underneath; the top was sort of like a dome". This object approached from the West, passed directly overhead, and continued away to the East, followed shortly by the pursuing cruiser. Houston pulled in behind the other officers and so joined the continuing pursuit (Hynek 1972, pg. 134). A fourth police officer, Frank Panzanella, was in a location called Conway. As he was operating on a different frequency, he reports that he had no knowledge of events up to that point. From within his vehicle he observed a "shining object" which he took to be "…a reflection off a plane…". Exiting his vehicle, he had been observing the light source for 10 minutes when the two pursuing patrol cars arrived, and the officers asked him if he had seen it. According to Panzanella, the 'object', "…the shape of half a football… very bright and 25 to 35 feet in diameter" then "…moved out towards Harmony Township approximately at 1,000 feet high; then stopped and went straight up real fast to about 3,500 feet". The officers reported that at this point the object continued upward until it was a bright point of light hovering a little to the left of the moon. They also noted that there was another bright point of light to the right of the moon – the position of Venus at the time. All four then reportedly watched the light "…shoot straight up and disappear" (Hynek 1972, pp. 134-135, 137-8).

This sighting is listed in Blue Book as a misinterpretation of Venus (Hynek 1972, pg. 139). The head of Blue Book at the time, Major Quintanilla, maintained that the officers had first seen a satellite, and then transferred their attention to Venus, although Hynek (an astronomer who had explained many other cases as misidentifications of satellites) insists that there was no satellite visible over Ohio at the

relevant times (Hynek 1972, pg. 135). Note too that the time lapse between the various observations of the 'object' passing overhead rule out the possibility of it being the *same* satellite, thus requiring, in a period when very few satellites were in orbit, that the original satellite deceived the first two officers into believing an object was hovering 100 feet overhead and emitting a humming sound, before moving away to the East. Then another satellite fooled the third officer by passing overhead at just the right time and on the right trajectory to match the description of the officers now pursuing Venus. Finally, a fourth officer with no knowledge of these events happened to sight a third satellite, again appearing at just the right time and position, which he too then lost and, without realizing it, transferred his attention as well to the same entirely distinct stimulus – Venus – which gave the appearance of moving upwards, hovering, and then shooting away. Finally, the officers reported that the object they were observing took a position to the left of the moon, whilst another bright light source was observed *at the same time* to the right of the moon- the true position of Venus (see further Clarke 1998, pp. 461-462).

Regardless of what the true explanation for this case may be, it clearly demonstrates that Hynek was making a legitimate point when he asserted that Blue Book were not undertaking serious research and provided misleading information to the public and scientific community. This is highly significant to Hynek's argument, as the world's governments and scientific institutions were largely content to allow the USAF to take the lead investigating the UFO problem (Hynek 1972, pg. 186; Swords and Powell 2012, pp. 373, 461; Jacobs 1975, pg. 35; Jacobs 2000, pg. 2).

4.2.ii) The 'Condon Report'

It is evident that one of Hynek's primary objectives was to counter the conclusions of the 'Condon Report'. In 1966, the University of Colorado was contracted by the USAF to undertake an independent study of the UFO problem. A team, composed of scientists from various disciplines, and led by the highly respected physicist Edward Condon, spent two years and half a million dollars researching the topic (Condon & Gillmor 1968, pp. v, xvii-xviii). The report, *Final Report of the Scientific Study of Unidentified Flying Objects*, ran to 937[69] pages. The USAF was hoping for a negative conclusion that would provide them with a reason to cease their investigations, and that was, perhaps not coincidentally (Williams 2000, pg. 58), precisely what they received. The most important part of Condon's conclusion is the statement: "…nothing has come from the study of UFOs in the past 21 years that has added to scientific knowledge …further extensive study of UFOs probably cannot be justified in the expectation that science will be advanced thereby" (Condon & Gillmor 1968, pg. 1). The final report was approved by the National Academy of Sciences (Hynek 1972, pg. 252).

Hynek devoted a chapter to the 'Condon Report' in which he advanced criticisms on numerous fronts. He objected strenuously to the evidence selection process, which he insisted should emphasize reports previously assessed as 'unidentified', and at minimum include only reports that met Hynek's definition of a UFO: "a report that *remained* unexplained after severe screening by technically aware persons" (Hynek 1972, pp. 241-243). However, although there were roughly 12,000 reports available in the Air Force files, and 25,000 reports available overall, the Colorado team examined only around 90 cases, 40 of which were included solely because they arose in the course of the study, and 14 cases were assessed despite having already been 'identified' by Blue Book (Hynek 1972, pp. 241-243, 252, 254). Another objection related to the major problem addressed by the Colorado team, which focused on whether the evidence supported an extraterrestrial explanation (the ETH), rather than the potential scientific significance of UFO reports independent of any proposed explanations, and thus was not immediately relevant to the relatively conservative claims of advocates like Hynek. The small number of

[69] Or possibly 1,485 (Williams 2000, pg. 58)

cases covered by the research group also meant that claims relating to the presence of significant patterns could not be addressed, and Hynek advanced numerous criticisms relating to the methods that *were* employed (Hynek 1972, pp. 242, 247ff). He also raised concerns regarding the evidently biased and flippant attitude displayed by Condon himself from the beginning, as well as the significant ructions within the research group which led to the firing of two of its members. One result of this was that a statistical analysis of thousands of cases undertaken by one of the fired individuals was abandoned and excluded from the final report (Hynek 1972, pp. 248, 255-257, 258-262). An administrative assistant subsequently resigned in protest, writing an explanatory letter to Condon in which she claimed an increasing divergence of opinion between the team's leadership and a number of the active researchers among whom "…there is fairly good consensus… that there is enough data in the UFO question to warrant further study" (Hynek 1972, pp. 262-263). Finally, and relatedly, Hynek argued that Condon's conclusions and summaries of the evidence were at odds with the details and conclusions contained within the body of the report (Hynek 1972, pp. 241 258-263).

4.2.iii) Ufology and 'Pathological Science'

So, what, then, is the difference between Hynek's arguments and those employed in advocating Polywater and Cold Fusion? On what basis can it be established that Hynek's book is 'pseudoscience', and Hynek himself a 'pseudoscientist'? One immediately obvious difference is that Ufology has never been supported by scores of publications in peer reviewed journals. From very early on, Ufology has lacked respectability.

Hynek would doubtless argue that this is due to factors arising from a general ignorance of the evidence, and this position is rather difficult to refute. That many scientists publicly dismissing UFOs as nonsense were ill-informed or miss-informed is irrefutable.[70] The Air Force kept its investigations data secret for decades, and Hynek's claim that they undertook a deliberately misleading public relations campaign intended, in effect, to debunk UFOs, is well supported (see for example: Sagan 1996, pp. 82-83; Swords & Powell, 2012, pp. 84-85, 193-195, 199-200, 298, 468, 469; Jacobs 1975, pp. 89-107, 134-135, 136, 143-144). Hynek would further argue that the scorn and ridicule directed towards Ufologists ensured that many scientists who were interested in the topic kept their silence, and that there is at least some truth to this is likewise undeniable.[71] In short, given that one of Hynek's core claims is that the response of scientists to the UFO problem arises from ignorance, and if scientists were fully cognizant of the relevant evidence they would agree that UFOs are worthy of study, a response based upon the actions of those scientists is rendered invalid. Thus the negative response of most scientists, and therefore this difference between Ufology and 'Pathological Science', is clearly not *in itself* a sufficient basis for demarcation.

Nor can it be successfully maintained that Ufologists are at fault for failing to publish articles in scientific journals, rather than publishing books for public consumption or producing their own journals. Ufologists insist that they have submitted papers for publication, but the disrepute in which Ufology is (improperly, in their view) held meant that they were summarily rejected. Peter Sturrock, for example, emeritus Professor of Applied Physics and emeritus Director of the Centre for Space Science and Astrophysics at Stanford University, and author of over three hundred published scientific articles, writes of an attempt to publish a commentary critical of the 'Condon Report': "…I submitted the article to no fewer than six scientific journals. In each case, I received a reply almost by return mail. Although the Condon Report itself had been considered an appropriate topic for many journals, my analysis of that

[70] Of course, that does not mean their conclusions were wrong, but that is beside the point.
[71] Evidence supporting this contention, if any is necessary, is provided below.

Report was uniformly considered inappropriate… None of the journals ever sent my submission to a referee. This was an experience I had never had before… Taking UFO reports seriously was and is a heresy, and anyone guilty of doing so is a heretic" (Sturrock 2009, pg. 57).

Of course, any nut can *assert* that a particular conclusion, no matter how wild, is warranted by evidence that scientists refuse to properly consider. We need not accept that this means appraisal of the evidence would in fact reveal it to be even in the category of 'pathological science'. Could it perhaps be held that Hynek is just a crank, his arguments ridiculous, and his claims regarding the UFO evidence utterly baseless? In responding to this, Hynek would doubtless first point to the other scientists who publicly agree with him, including, for example, Michael Swords, Peter Sturrock, Jacques Vallee, James E. McDonald, Frank B. Salisbury, David Saunders and Harley D. Rutledge. Of course, this (incomplete) list is not very long, but then the number of individuals who have demonstrably detailed knowledge of the evidence and publicly oppose Hynek's position is also quite small. Hynek argues that the true number of scientists seriously interested in the subject is unknown. "I have positive evidence from personal correspondence and conversations with scientists that their interest is increasing but that it is still, in most cases, anonymous. There is truly a growing 'Invisible College' of scientifically and technically trained persons who are intrigued by the UFO phenomenon…" (Hynek 1972, pg. 208). In 1975 Peter Sturrock sent a questionnaire to 2,611 members of the American Astronomical Society, 1,356 of which were returned. The anonymous responses strongly support Hynek's contention. According to Sturrock, 13 respondents made critical remarks, 50 made positive comments, 34 offered to assist in research, and 17 indicated they were already investigating UFOs. Asked whether UFOs deserve scientific study, the responses were: 'certainly' – 23%; 'probably' – 30%; 'possibly' – 27%; 'probably not' 17%; 'certainly not' – 3%. Sturrock also reports a positive correlation between how much respondents know about the subject and a positive view of its scientific value, indicating either that those who regard the subject as significant tend to look into it, or that those who know the most about it tend to take it more seriously, but either way contrary to any claim that scientists well informed of the evidence perceive it as being contrary to Hynek's conclusions (Sturrock 2009, pp. 74-75).

Hynek also references a scientific institution which has openly argued that UFOs should be seriously studied. The American Institute of Aeronautics and Astronautics set up a subcommittee to examine the UFO evidence. In 1968 the subcommittee reported in the *Journal of Astronautics and Aeronautics*: "The committee has made a careful examination of the UFO issue and has concluded that the controversy cannot be resolved without further study in a quantitative scientific manner and that it deserves the attention of the engineering and scientific community" (Hynek 1972, pp. 272-3). A later publication of the subcommittee was critical of the 'Condon Report', particularly noting that Condon's contribution did not appear to be in accordance with the rest of the study, and objecting to the focus on refutation of the ETH. It concluded that "The subcommittee sees the only promising approach as the continuing moderate-level effort with emphasis on improved data collection by objective means and on high quality scientific analysis" (Hynek 1972, pp. 273-274).

Finally, leaving aside what conclusion it justifies, even the text of the 'Condon Report' can be cited as supporting Hynek's portrayal of the *evidence*. Despite the fact that they did not focus particularly on unresolved cases, the University of Colorado team were unable to identify causes for more than a quarter of the reports investigated (Hynek 1972, pg. 239). Moreover, Hynek cites in his book several significant selections from the analysis sections of the final report, including: "In conclusion, although conventional or natural explanations certainly cannot be ruled out, the probability of such seems low in this case, and the probability that at least one genuine UFO was involved appears to be fairly high". Elsewhere: "It does appear that this sighting defies explanation by conventional means". Of a case involving photographic evidence: "This is one of the few UFO reports in which all factors investigated, geometric, psychological, and physical appear to be consistent with the assertion that an extraordinary

flying object, silvery, metallic, disc-shaped, tens of meters in diameter, and evidently artificial flew within sight of two witnesses". Finally, of a sighting reported by astronauts: "Especially puzzling is… the daytime sighting of an object showing details such as arms protruding from a body having noticeable angular extension. If the NORAD listing of objects near… at the time of the sighting is complete, as it presumably is, we shall have to find a rational explanation or, alternatively, keep it on our list of unidentifieds" (Hynek 1972, pp. 241, 259, 260).

The overall picture, then, is really quite similar to the Cold Fusion and Polywater affairs. The number of scientists involved is less, but otherwise we have a circumstance in which a group of scientists sincerely maintains that certain evidence supports an unorthodox conclusion, and another group of scientists apposes that position, believing that the evidence best supports an alternative conclusion – a scientific dispute. Indeed, if the conclusion in question is Hynek's claim that the UFO issue is worthy of investigation – that Ufology is not 'pseudoscience' - then the extent to which the advocated conclusion is contrary to established science is significantly less than is true of Cold Fusion,[72] and there has as yet been no major revelation establishing that the original beliefs of the proponents regarding the key evidence were mistaken, as occurred with Polywater. Of course, there are also non-scientist advocates of Ufology, but that fact is not particularly relevant. If their arguments and conclusions are less acceptable or scientifically sophisticated than those of Hynek and his colleagues, criticisms of them have no relevance to the acceptability of the latter, or the basic claim that Ufology is not 'pseudoscience'.

4.2.iv) Ufology and Demarcation

What other basis may there be for disqualifying Hynek's arguments as 'pseudoscience'? *A priori* arguments are certainly out. There is no apparent basis for criticism of Hynek's attitude, or of Hynek personally at all – he may be mistaken, but he is sincere, he does not 'ignore' empirical matters, wantonly misrepresent the evidence, or anything of that nature. The arguments advanced are falsifiable, and Hynek explicitly discusses circumstances in which he would accept his position has been falsified (Hynek 1972, pp. 269-271).

The historical development of the argument does not seem to include any truly relevant, problematic features, and it would be very difficult to maintain that a subsequent 'lack of progress' is not significantly due to the subject having *been* dismissed as 'pseudoscience', rather than a legitimate justification for so doing. Ufologists have continued to gather and analyze evidence to the limited extent possible with their scant resources, and in the absence of any official or 'respectable' report collection mechanism, and continue to insist that increased resources are required to undertake research that could advance understanding of the problem. It would be better, perhaps, if they had undertaken considerably more detailed statistical analyses, which could potentially provide truly significant insights and should be within their capacities, but the failure of advocates to do more of this hardly establishes the illegitimacy of their arguments, and it does not seem credible to argue that they have avoided doing so because they fear the results may be negative – Ufologists thus far believe the statistical evidence is entirely in their favour (Hynek 1972, pp. 267-269; Hynek 1977, pp. 243ff.; Vallee & Vallee 1966, pp. 63ff *et passim*; Hall 2001, pp. 437ff).

In terms of methodology, it is difficult to maintain that Hynek's approach is 'pseudoscience' given that the 'Condon Report', approved by the National Academy of Sciences as a genuinely scientific effort, employed essentially the same methods in analyzing witness reports in order to determine whether any of them indicate a scientifically significant stimulus. Indeed, Condon and his team were doing just

[72] If no specific explanation for 'genuine' UFOs is proposed, then obviously it is impossible to say whether their existence is in any way contrary to orthodox science.

what Hynek recommends: 'seriously' examining the UFO evidence. Thus, if the Colorado team were not acting improperly, then Hynek's argument that such research should be undertaken cannot be rejected as improper. The investigation led by Condon establishes that, in this case at least, the USAF, Condon and his team, the University of Colorado, the National Academy of Sciences, and the scientific community broadly, determined that a claim widely regarded as 'pseudoscience' both could and should be addressed through a detailed scientific study. To this extent they agreed with one of the core arguments I have been advancing – that there are examples of so called 'pseudoscience' that cannot be adequately addressed save through the application of science. From this it follows that such examples cannot in fact be 'demarcated' as non-science, but must rather be rejected as flawed or failed science.

If Hynek's arguments are nonetheless somehow 'pseudoscience', then this must be either because he published his argument after the 'Condon Report' was released, or because he advocates a more positive conclusion. However, that Hynek's conclusion had not been adequately refuted, could not be dismissed and certainly had not been disqualified as 'pseudoscience', is effectively the background position that made the 'Condon Report' necessary. The Colorado team must have, at least technically, begun with the proposition that there was a body of proposed evidence, the scientific value and significance of which had not yet been adequately established, which is essentially the extent of Hynek's claim. Regardless of what conclusion they *expected* their research would justify, they implicitly accepted that such research was required to justify that conclusion, and thus, correspondingly, must have accepted that the contrary conclusion was a genuine scientific possibility. And again, the scientific community's response indicates their agreement.

To argue that Hynek's argument is 'pseudoscience' because it post-dated the 'Condon Report' it is necessary to maintain, clearly unjustifiably, that the report settled the controversy once and for all, such that even new evidence cannot justify revisiting the issue, and, also unrealistically, that it is somehow improper to even explore whether its methods and conclusions were problematic. The critical assessment of scientific research is a crucially important scientific process, so it cannot credibly be suggested that such is improper in this case. But then, any full consideration of the methods and findings of the 'Condon Report' requires that contrary arguments and claims, and the associated evidence overall, be taken into account. Therefore, the possibility that the evidence may indicate the existence of a scientifically significant phenomena must be addressed, and thus cannot be dismissed as 'pseudoscience'. Likewise, but more simply, if the Colorado Study was science, it cannot be that a replication of that study would be 'pseudoscience', yet in essence that is what Hynek is advocating, although he would naturally suggest that certain perceived flaws be corrected.

It is important to emphasize that I am in no way arguing that Ufology should be regarded as entirely respectable science, that it deserves attention and funding, or anything of the sort. My objective has been purely to demonstrate that it cannot be 'demarcated' through any process save scientific refutation, and is not 'pseudoscience' inasmuch as it cannot be permanently disqualified as something that is not science at all.

4.3) Beyond Ufology: Addressing the Best Arguments

An objection that may well be raised at this point is that I have tipped the odds in favour of my conclusions through the latter focus on Hynek's ufological arguments and my continued insistence on focusing on the 'best' examples of 'pseudoscience' throughout this thesis. Would it not be possible to justify the 'demarcation' of 'pseudoscience' in such cases as have not been discussed because they are not scientifically sophisticated and do not provide counter examples to the demarcation arguments examined above? The answer to this is, likely, yes: it cannot be credibly maintained that there are *no* arguments in print or on the internet which are presented as science yet include flaws so extreme that they are never

found within science. However, there are a number of important caveats. Firstly, it is clear that the heterogeneous nature of nonsense 'pseudoscience' would require the application of many different demarcation criteria. Any generalization intended to capture all such flaws is likely to re-introduce an overlap with science. Secondly, as even such indefensible examples *will* be defended by their proponents, the arguments will often become sufficiently complicated that a lay audience will be unable to benefit from them unless they either acquire sufficient knowledge of the subject to themselves judge the merits of the competing claims, or, as would doubtless more typically occur, trust the authority of scientists and the scientific institutions who support them when they assert their conclusions. Thus the idea of a universally applicable and relatively simple demarcation process that could improve the publics' ability to recognize 'pseudoscience' for themselves must be abandoned. And there are also far more serious objections to taking such a course.

I should first point out that Ufology was not chosen as an example because it is recognizably less flawed than other examples of 'pseudoscience', but rather because it is an entirely uncontroversial example of which everyone is aware. Likewise, Hynek was selected in part because he was a 'sophisticated' Ufologist, granted, but otherwise simply because he is undeniably among the most prominent Ufologists, and thus no one claiming any authority in discussing the topic can reasonably claim ignorance of his contribution. Thus, if Hynek's arguments are not 'pseudoscience', then no demarcator or debunker can justifiably assert that 'Ufology is pseudoscience'. Yet Ufology is central to our understanding of the common usage meaning of that term – it has effectively been part of the definition of 'pseudoscience'.

Nor is there any reason to suppose that a different outcome would result of focusing on numerous other paradigmatic examples of 'pseudoscience'. Cryptozoologists arguing that there are large, undiscovered ape-like creatures roaming the wilds may seem a more promising target, but then Sir David Attenborough has publicly declared that he believes the evidence supporting the existence of the Himalayan 'Yeti' should be seriously examined. I may be courting criticism for improperly appealing to authority, but it *is* hard to imagine anyone calling Sir David Attenborough a 'pseudoscientist'. More importantly, he has merely suggested that the reported evidence seems that it may be genuinely significant, because it is otherwise difficult to explain, and thus that the issue should be further examined. There is no immediately apparent basis for dismissing this as utter nonsense (Ellis 2013; Furness 2014).

Poltergeist research is also a seemingly quintessential 'pseudoscience', but again there are examples of clearly sincere individuals, apparently doing as much to uncover all the relevant data as can reasonably be expected in complicated field conditions, but confronted with a circumstance where inexplicable events seem, in the opinion of the witnesses and investigators, to be occurring. We can certainly reject their conclusions, but can only justify doing so through scientific arguments focused on the empirical data (Playfair 1980; Roll 1972; Rogo 1974; Clarkson 2011; Price 1945). Mousseau's analysis, discussed previously, indicates that Parapsychology more generally is approached with considerable scientific sophistication (Mousseau 2003).

It is, in short, possible to find 'sophisticated' arguments advanced by proponents of many 'pseudosciences'. To accept that such cannot be disqualified as science, and that only the more problematic arguments can be labelled 'pseudoscience', arguably justifies primarily simply ignoring those problematic arguments, just as we ignore crank arguments offered in support of clearly scientific propositions. The number of *conclusions* or basic *claims* subsequently subject to dismissal as 'pseudoscience' would also be *very* significantly reduced. To abandon the demarcation of 'sophisticated' contributions which can only be addressed through scientific arguments would require that many paradigmatic 'pseudosciences' be recognized as falsely labelled. This would require a marked departure from reflective equilibrium which may be less acceptable to debunkers and demarcators than replacing the term 'pseudoscience' with something more readily justifiable.

Of course, demarcation *would* be far easier if we ignored previous determinations and instead focused directly on where a line may most readily be drawn, if not for the fact that such a move would all but eliminate the practical value of demarcation, for two reasons. Firstly, as previously argued, the easier it is to apply demarcation arguments, the less point there is in doing so. The more a claim offends reason, the less likely it is to seem attractive to reasonable laypeople in the first place, and the less point there is in using reasoned arguments against it. Secondly, the 'demarcation' of 'pseudoscience' was hardly relevant to the objectives for which it was intended to start with, and is rendered progressively less so as the arguments it applies to decrease in scientific virtue.

5) The Irrelevance of 'Pseudoscience'

All the arguments advanced thus far have been intended to establish, in short, that the objective of disqualifying arguments, as not science at all, via a method legitimately termed 'demarcation', is far more difficult to achieve than has been recognized. I am now adding to this the observation that demarcation is remarkably ill suited to attaining the objectives for which it is intended. Thus demarcation is both too hard *and* too weak.

As previously discussed, demarcation is explicitly intended to resolve the problem of claims being improperly regarded as authoritative. The findings of science are widely regarded as warranting respect and acceptance, and it is believed that certain non-scientific claims, being erroneously identified as 'scientific', are on that basis incorrectly held in high esteem. 'Demarcating' these claims as in fact not science at all is therefore *supposed* to address this problem through removing the basis on which laypeople mistakenly afford them respect. In reality, however, as it is hardly controversial to assert, the bare fact that a claim is 'scientific' – that it has been advanced by a scientist, is based upon scientific research, or whatever it is that warrants that label – is *not* a sufficient basis for attributing to it 'scientific authority'. Such authority can be granted only by a consensus of the relevant section of the scientific community, and thus is properly attributed only to *well established* scientific hypotheses or theories. Thus the core problem to be resolved is the misunderstanding of the significance of scientific status, rather than its misattribution, and hence is not resolvable through demarcation.

The issue can be understood in terms of the confusion of entirely different types of scientific status, at opposite ends of the scientific spectrum, which require that entirely different 'bars' for inclusion be met. The term 'science' can be employed in both a *normative* and a *categorical* sense. Normative science, the type that warrants acceptance by the lay community, is science that meets the *highest* [73]'bar' within science – consensus acceptance. The categorical meaning of 'science', on the other hand, refers to those statements or hypotheses that meet at least the minimal 'bar' for inclusion within the subject matter of science, whatever that may be. Thus, because demarcation is concerned with establishing which claims meet the minimal 'bar' for inclusion within science – the issue of whether something is 'scientific' or not - it is relevant only to the *categorical* meaning of the term 'science'.

Demarcators, in advancing arguments intended to establish that 'pseudoscientific' claims should not be *accepted*, are thereby trying to establish that such claims should not be regarded as having any scientific authority, and accordingly they are *arguing* that 'pseudoscientific' claims do not meet the requirements for inclusion within established, *normative science*. However, in *concluding* that the target is 'pseudoscience' they are asserting that it does not meet the requirements for inclusion within 'science' in the categorical sense. Thus the distinct meanings of 'science', applicable at opposite ends of the scientific spectrum, have been seriously confused.

Indeed, so far as the core problem to be solved arises from a misunderstanding of the true nature of science, demarcation only contributes to the error. For scientists and other commentators to respond to this issue through focusing on whether something is genuine science or not, perpetuates the false belief that any genuinely 'scientific' claim *is* authoritative, and therefore that to establish a claim is *not* authoritative requires demonstrating that it is not science. What is really required is an education campaign establishing the opposite – that scientific status does not automatically indicate scientific authority.

Returning to the objective of providing useful guidance to educators, policymakers, the judiciary, and so on, it is clear that arguments referring merely to what should and should not be regarded as within

[73] This assertion is perhaps somewhat arguable – it is often said that some theories are accepted as more certainly established than others - but this does not alter the basic point being made here.

the subject matter of ‘science’ are largely irrelevant. There are an enormous number of unambiguously scientific hypotheses that should also be rejected as insufficiently established for such purposes. Noting which propositions are not science at all is barely a useful beginning, and becomes even less so as that determination is applied to increasingly flawed proposals. Insisting that the ‘demarcation’ of ‘pseudoscience’ is important to such decision making processes is rather like maintaining that nutritionists must always begin their deliberation by determining which substances are not food at all. Instead, the line of ‘demarcation’ relevant to the decisions of such individuals and groups will lie somewhere across the upper end of moderate to good science, marking off those hypotheses that are sufficiently well established, and thus reliable enough, to be accepted. Of course, the requirements for inclusion will also shift according to circumstances and what precisely is being decided, so there will be no singularly relevant point of ‘demarcation’.

To summarize the arguments thus far, I believe it has now been demonstrated that none of the demarcation proposals discussed succeed, or indeed come close to succeeding, in providing a useful demarcation method. It has also been shown that many paradigmatic examples of ‘pseudoscience’ in fact cannot be dismissed or disqualified, but must rather be addressed through scientific methods, from which it follows that they cannot be demarcated as ‘pseudoscience’, but must rather be rejected as flawed science. I have also established why shifting the focus of demarcation to more problematic examples further erodes its already scant significance to the objectives for which it is intended.

I have not appealed to Laudan’s argument is establishing any of these conclusions, although it is easy to recognize its relevance to much of the above discussion. None of the criticisms discussed previously succeeded in striking a fatal blow to Laudan’s core claim, which therefore remains to reinforce my own conclusions. Laudan was clearly correct in asserting that epistemic virtue and scientific status are neither the same thing nor coterminous, from which it indeed follows that establishing scientific status does not, as demarcation requires, establish epistemic authority or superiority.

6) Why Demarcation Looks Simple

My argument cannot be secure, however, until I have addressed how it is that so many commentators are so completely certain that demarcation *must* be possible. Clearly, a number of factors contribute to this situation. One of these is the continued insistence on employing the label 'pseudoscience' when discussing opposed unorthodox arguments, even though the philosophical difficulties arising from doing so are recognized and remain unresolved. No doubt commentators have reinforced their position on 'pseudoscience' through continual use of that label, but unless they can demonstrate that such usage was in fact justified, which I believe this thesis has shown they cannot, then this is nothing more than a strongly reinforced error.

6.1) Misrepresenting Science

Another source of error is the overly idealized picture of science employed by many commentators. We saw previously that some are quite explicit regarding their use of such portrayals, whilst many others openly employ examples of well established science, which they compare with obvious 'pseudoscience', thus obscuring entirely the overlap between science and 'pseudoscience' at their worst and best extremes respectively (see for example: Popper 1962, pp. 33ff.; Pigliucci 2010, pg. 2; Pigliucci 2013, pp. 10-11; Rothbart 1990, pg. 111). As just discussed, such discussions confuse the normative and categorical usages of the term 'science'. In order to successfully demarcate an empirical *claim* as not science at all, it is clearly minimally necessary to establish that the most sophisticated supporting arguments are epistemically inferior the least sophisticated scientific arguments.

The whiggish nature of many historical treatments of science are also an important contributing factor here. I do not have the space to explore this fascinating and complex issue in any detail here, and so will merely make a couple of observations. The author of a history of science knows in advance what will happen, and accordingly tends to focus on the events that are, with hindsight, the most significant. They have no choice but to largely ignore the countless mistakes, false starts and dead ends that litter the literature. The author obviously wants to explain what happened. Criticisms levelled at a hypothesis which came to be accepted, for example, may be a part of what happened, but they do not help to explain it, and therefore research errors and flawed arguments will often be passed over without comment. The overall result tends to be that the scientists who were proved right – the heroes - appear to be more skilled and rational than they really were, whilst the scientists proved wrong – the villains – appear to be more irrational and grossly in error than was actually the case (see Le Grand 1988, pg. 32).

Accounts of 'Pathological Science' can be seen to both reflect and contribute to this tendency. The authors of such treatments end up advancing the position that revolutionary claims must prove to be either correct, or 'pathological' (Turro 1999, pg. 2), and thereby errors within science are treated as anomalies. The Cold Fusion and Polywater episodes may have attracted a great deal of attention, and it may be that the scientists advocating both were somewhat more in error than is often the case. However, the same basic mistakes appear continually throughout scientific history, which is why scientific processes are designed to correct for such common human errors. In short, the use of the label 'Pathological Science' indicates a failure to accept that science is, and always has been, a human endeavor which reflects common human failings.

6.2) Misrepresenting 'Pseudoscience': Debunkers on Ufology

Most importantly, there is much evidence that commentators discussing 'pseudoscience' have been seriously misled by the misrepresentation of 'pseudoscience' in the debunking literature many of

them rely on. To illustrate this I will examine some of the discussions of Ufology in the literature of the organized 'skeptical' movement. These examples have not been selected because of their flaws, but roughly at random – they are taken from the 'skeptical' books I happened to have on my shelf.

6.2.i) Wynn & Wiggins

First is *Quantum Leaps in the Wrong Direction* by Charles M. Wynn and Arthur W. Wiggins (2001, pp. 49-68). The chapter 'UFOs and the Extraterrestrial Life Hypothesis' is divided into a number of short sections, beginnings with a brief discussion of the 'first' modern 'flying saucer' report by Kenneth Arnold, and the Roswell event, and ending with the conclusion that individuals advocating the ETH in response to these had made a 'quantum leap in the wrong direction' (Wynn & Wiggins 2001, pp. 50-51).

The next section is three pages covering the error prone nature of human perception and the special training pilots must receive to help them avoid common perceptual errors. The authors emphasis that "Objects observed in the sky by untrained observers are not necessarily what they appear to be". (Wynn & Wiggins 2001, pp. 51-54). This is followed by a discussion of hoaxes which the authors recognize 'do not play a major role in UFO reporting', but they outline an event wherein some investigators revealed a hoax, but the UFO group they were associated with refused to accept their conclusions (Wynn & Wiggins 2001, pg. 54). The next section continues the misperception theme, discussing, very briefly, the most common objects misidentified as UFOs, and a case of misidentification in space (Wynn & Wiggins 2001, pg. 55). The authors conclude this section with the following: "The J. Allen Hynek Centre for UFO Studies (CUFOS) keeps close track of UFO reports, and has on file ordinary explanations for 92 percent of all sightings. The balance could not be identified for lack of information".

If this section of the text were sent to me sans the final sentence, I would be unable to judge whether it had been written by a debunker or a sophisticated Ufologist such as Hynek. To that extent it reflects and perpetuates the false belief that simply recognizing the problematic nature of UFO witness reports is a sufficient response to the 'UFO problem'. There is no corrective reference to the fact that the statements being made are not in dispute – the vast majority of Ufologists agree that a very high percentage of UFO reports are caused by misidentifications (see for example: Clarke & Roberts 1990, pp. 17, 67, 75; Vallee & Vallee 1966, pg. xv; Williams 2000, pg. 359; Paris 2012, pg. 10; Hall 1964, pg. v).[74] Nor is there any recognition of the fact that Ufologists have accordingly focused on cases which they maintain are sufficiently supported by multiple witnesses or radar evidence etc. The true core of the dispute between debunkers and Ufologists is thus entirely neglected.

The final statement is particularly misleading in this regard. As written, it falsely implies that CUFOS has concluded that the eight percent of reports left unexplained can be dismissed as lacking sufficient data, which is emphatically not the case, and indeed is precisely the point on which they disagree with debunkers. Firstly, CUFOS, the USAF and other similar groups investigating UFOs maintained a separate file for 'Insufficient Data' cases, and the cases listed as 'unidentified' are therefore explicitly those which have been judged to include sufficient data to indicate a mundane explanation if such were applicable (Hynek 1977, pp. 248-249, 254, 257). Moreover, Hynek (representing CUFOS) explicitly argues that 'unidentified' cases should *not* be regarded as differing from 'identified' cases only to the extent that an identification has not been achieved, but rather that statistical analysis reveals that the probability of this being the case is "…less than one chance in a billion", and that the more reliable a report is, the less likely it is to become an 'identified' (Hynek 1977, pp. 262-267). The mass of explainable UFOs are, in other words, believed to be no more than the noise which obscures an entirely

[74] Wynn & Wiggins themselves refer to this very fact in relation to one major UFO research group (2001, pg. 56).

distinct signal. Thus, again, the primary point of disagreement within the UFO debate is dismissed by these authors with a single sentence in which they simply assert the truth of their position without any attempt to address the counter arguments of Ufologists.

The next section covers 'Roswell' in more detail and discuss an Air Force report explaining what really occurred (Wynn & Wiggins 2001, pp. 56-57). The authors fail to mention here, or in their previous discussion of the same event, that perhaps the most significant reason why many individuals made the 'leap' to believing a UFO was involved was because this is what the United States Army Air Force originally claimed in the *Roswell Daily Record*, July 08, 1947, in a story headlined "RAAF Captures Flying Saucer on Ranch in Roswell Region". Such an omission is explicable only as the result of a deliberate decision to exclude a crucial datum, or an unacceptable ignorance of the topic under discussion.

The authors also do not care to mention that most relatively sober Ufologists reject the Roswell claims (Swords & Powell 2012, pp. 44-45; Clarke 1998, pp. 119-122; Pflock 2001), and thus it is largely irrelevant to whether Ufology overall is 'pseudoscience'. The same is true of the two subsequent sections devoted to 'Ancient Astronauts' and 'Alien Abductions' (Wynn & Wiggins 2001, pp. 57-62), and their argument that 'UFO hypotheses' are 'nonfalsifiable', which latter considers only the claim that aliens wipe the memories of witnesses to abduction events (Wynn and Wiggin 2001, pg. 63).

In a section entitle 'Occam's Razor Applied to Overly Complex UFO Hypotheses', the authors state that "In the absence of strong observational data the invocation of extraterrestrial visitors or invisible Earth beings…" is "…unjustifiably complicated" (Wynn and Wiggins 2001, pg. 63). However, given that their discussion of whether or not there exists 'strong observational data' is so inadequate, they are again simply granting themselves a contested premise.

In the next section the authors assert that "UFO hypotheses require abandonment of well-tested scientific hypotheses, with no contrary evidence" (Wynn & Wiggins 2001, pg. 63). Again, whether or not there is 'contrary evidence' has not been reasonably addressed, and the arguments advanced – that faster than light travel is contrary to Einstein's theories of relativity, and that the sudden changes in direction reported by many UFO witnesses would kill any abductee on board – are relevant only to the ETH rather than 'UFO hypotheses' in general (Wynn & Wiggins 2001, pp. 63-65). More importantly, these are inadequate responses even to arguments that do advocate the ETH and alien abductions. It is simple enough for proponents of the latter to maintain that aliens, obviously, do not perform dangerous maneuvers when they have humans on board. Further, aliens clearly need not travel at or near the speed of light in order to reach Earth, even across vast distances. There are many possible alternatives – that the aliens are in suspended animation for most of the journey, or that the ufonauts are robots, and so on. The objections raised by the authors are thus relatively toothless.

The two subsequent sections deal with SETI and the Drake Equation respectively, and thus can be ignored (Wynn & Wiggins 2001, pp. 65-67). These are followed by an argument discussing the many different descriptions of 'aliens' provided by witnesses over the years (Wynn & Wiggins 2001, pp. 67-68). This is not an unreasonable point, but it is too simplistic and rather misleading presented. Providing a series of differing descriptions of aliens, each associated with a particular year, misleadingly suggests that reports for that year were fairly consistent in this regards, when in reality most of the descriptions listed are associated with only one or a very few reports, whilst other description appear consistently across many years (see Bowen 1969; Lorenzen & Lorenzen 1967). Thus a perfectly good argument is undermined by inaccuracy and insufficient development.

The final section of the chapter discusses the 'congeniality' of the conclusion that aliens exist, and argues that the possibility of alien visitation should not be completely ruled out. It then continues: "To date, however, there is not a shred of credible evidence to support the belief that ETs have already visited us. *Extraordinary claims require extraordinary evidence*. The extraordinary ***extraterrestrial hypothesis*** is not supported by ordinary evidence. It is therefore untenable" (Wynn & Wiggins 2001, pg.

68). These assertions are highly problematic for a number of reasons. Firstly, this conclusion, in relating exclusively to the ETH, is inadequate for a chapter that is presented as addressing UFOs. Secondly, to simply assert that there is 'not a shred of credible evidence' and no 'ordinary evidence', when no adequate basis for such a claim has been provided because the authors have ignored the evidential arguments of Ufologists, is also inadequate. Two of the quotes from the 'Condon Report' included above demonstrate that there are at minimum reports which credible scientists have not only failed to explain, but admit seem to defy explanation in mundane terms, and which also appear to indicate the existence of structured craft. Nothing included in this chapter in any way addresses such arguably evidential cases or justifies the assertion that they do not exist. Thirdly, the position adopted is particularly problematic if it is accepted that alien visitation is a genuine possibility, as this removes one of the major justifications advanced for rejecting otherwise potentially evidential reports as contrary to established science and thus best understood as having some alternative explanation.

Clearly, anyone deriving their knowledge of Ufology from this account would be severely mislead regarding the true nature of both the claimed evidence, and the significant points of dispute. They would come away with the mistaken belief that Ufologists universally support the ETH, and that all of the claimed ufological evidence can be dismissed through reference to the problematic nature of witness reports. Of Ufologists' efforts to rely only on well supported cases, relatively conservative conclusions, and the true nature of the 'best' evidence, among other things, they would be entirely ignorant. Thus any demarcation argument they may base on this portrayal of Ufology would be seriously flawed.

6.2.ii) Gardner

The next example is far, far worse. In the 'Contents' section of Martin Gardner's *Did Adam and Eve Have Navels? Debunking Pseudoscience* (2000), we find three chapters listed under the heading 'Part VII: UFOs'. The first chapter is entitled 'Claiborne Pell, Senator from Outer Space', and simply provides a discussion of the many wacky beliefs Senator Pell and his associates subscribed to. UFOs, which are barely mentioned, appear only as one of these beliefs, and there is no attempt to provide any argument against Ufology whatsoever (Gardner 2000, pp. 175-184). The second chapter is much the same, although UFOs are at least more central to the topic. 'Courtney Brown's Preposterous Farsight' discusses a book in which claims about UFOs, among other things, are advanced by an individual who claims his knowledge was gained through 'scientific remote viewing' (SRV), essentially a form of 'astral travel'. Again, there is no hint of any argument debunking UFOs (Gardner 2000, pp. 185-196). The same is true of the third and final chapter, which discusses the lives and beliefs of 'Bo' – Marshall Herff Applewhite - and 'Peep' – Bonnie Lu Trousdale Nettles – leaders of the infamous 'Heaven's Gate' UFO cult (Gardner 2000, pp. 197-208).

It is hard to imagine that anyone could think they had gained an understanding of Ufology from this contribution. However, they could certainly form the opinion that all Ufologists are cranks, and the subject itself utterly ridiculous. After all, if there is any argument about UFOs in this book, it can only be that individuals who believe in UFOs are likely to believe a lot of other ludicrous things as well, and thus the subject is utterly disreputable.

6.2.iii) Pigliucci

The next author begins his section on UFOs with another discussion of the Heaven's Gate cult. Writing in *Nonsense on Stilts: How to Tell Science from Bunk*, Massimo Pigliucci (2010, pp. 68-70) concludes that discussion with the observation that "even UFOs can kill…". He then links the preceding section to the subsequent discussion with a comment on Ufology in general: "…the same twin elements

of lowered critical thinking and heightened emotional dissatisfaction with the world as it is play a role in the cult[75] at large" (Pigliucci 2010, pg. 70). This speaks for itself in terms of what it indicates of Pigliucci's attitude towards Ufology and the 'believers' that one may otherwise have assumed he was trying to reach with his arguments.

Pigliucci further states: "…a typical defense of ufologists is that no matter how hard the skeptics try, they have not succeeded in explaining *all* the alleged UFO and alien sightings… the idea is to shift the burden of proof from the person who makes the extraordinary claim (to whom such burden logically belongs) to the person who simply asks for the evidence before accepting the belief" (Pigliucci 2010, pg. 70). However, whether a belief should be 'accepted' is beside the point: debunking Ufology as a 'pseudoscience' requires establishing at minimum that it is utterly unworthy of scientific attention. One can deny that the evidence is sufficient to establish any particular conclusion, and yet maintain that the matter deserves scientific scrutiny.

Moreover, debunkers like Pigliucci are not simply 'asking for evidence' that supports Ufology, but rather advocating an alternative conclusion: that the relevant evidence is best explained in prosaic terms. As such, there is as much a 'burden of proof' on them as there is on Ufologists, as anyone advancing a 'best explanation' of the evidence is required to provide support for their contention.

Finally, Pigliucci is explicitly arguing that Ufology is a 'pseudoscience', yet to do so through advancing criticisms of arguments manifestly weaker than numerous well known alternatives is undeniably sophistic. Whether the argument Pigliucci referred to above is 'typical' or not (and I would argue that it is not), it is certainly not typical of sophisticated Ufologists like Hynek, and Pigliucci has no business writing on Ufology at all if he is unaware of Hynek's contribution. The alternative, which, as will become evident below, seems most likely, is the Pigliucci is uncritically repeating the arguments of others, without having taken the time to familiarize himself with the literature of both sides.

Pigliucci goes on to examine three cases "…not only because they are representative of this vast literature, but more importantly because they will teach us some generalities about ufology as a pseudoscience" (Pigliucci 2010, pg. 70). The first case was a 'cloudlike UFO' witnessed by thousands of Americans observing the Perseid meteor shower, which was quickly identified as caused by a Japanese rocket. In Kentucky a witness reported to the police that they "…heard an explosion during the event", but this turned out to have been caused by illegal fireworks. Pigliucci gleans from this episode that: "…thousands of inexperienced observers of the sky can be wrong about what they are actually seeing. This by itself goes a long way towards explaining most UFO cases. Second, the Kentucky explosion shows how easy it is to causally associate two events just because they take place in rapid succession… the *post hoc ergo propter hoc*… invalid conclusion" (Pigliucci 2010, pp. 71-72).

A number of comments are warranted here. Firstly, it is unlikely that this case would have met Hynek's definition of a UFO, and in this regard its relevance to sophisticated ufological arguments is slight. Secondly, there seems to be no real basis for Pigliucci's assertion that the thousands of witnesses were 'wrong' – they reported seeing something that they could not immediately identify, which is entirely accurate, and I strongly doubt that thousands of them, as Pigliucci implies, declared it to be "…an extraterrestrial intelligent machine…" (Pigliucci 2010, pg. 71). There is essentially no discussion here of the accuracy of the witness reports, although Ufologists would insist, with considerable merit, that such data is the only truly valuable information that could be recovered from such a case. Similarly, Pigliucci's account did not indicate that the person reporting the explosion asserted it to be associated with the aerial event. If, as Pigliucci describes, they merely reported "…having heard an explosion during the event"

[75] It is important to recognize that Pigliucci is not here applying the term 'cult' to Heaven's Gate – he is explicitly asserting that Ufology in general is similar to Heaven's Gate, and thus referring to Ufology at large as a 'cult'. There is undeniably a cultish element within Ufology, but to use this term to describe even the more sensational, 'popular' section of Ufology is quite unreasonable. To assert that Ufology as advocated by Hynek and his ilk is a 'cult' is ridiculous and combative in the extreme.

(Pigliucci 2010, pg. 71), then this too was entirely accurate, and there is no basis for attributing to them any fallacious reasoning.

The 'Flatwoods Monster' case which Pigliucci next discusses is primarily a 'monster' report only peripherally associated with UFOs. Pigliucci provides a glowing account of a "…clever and painstaking investigation…" by debunker Joe Nickell, in which the latter "…reconstructed the story from a less extraterrestrial perspective, while accounting for every detail reported in the press or recalled by witnesses" (Pigliucci 2010, pg. 72).

An important early account of the incident by Gray Barker (1956, pp. 23-35), the contents of which Nickell does not dispute (Nickell 2000; Pigliucci 2010, pp. 72-73), reveals the following significant details. There were numerous meteor sightings in the area at the time, many of which were perceived as crashing into nearby hills (Barker 1956, pp. 27, 33). Upon climbing the hill to locate a 'saucer' they believed had crashed there, the witnesses reported that they first observed a "*huge* globular mass" roughly *fifty feet away* on the other side of the hilltop. "It was just like a big ball of fire…*as big as a house*", and periodically dimmed and brightened. A *thumping* noise "like someone hitting on canvas" was heard, "and there was another noise, *half-way between a hiss and the noise made by a jet plane*". One witness, carrying a torch, thought he saw animal eyes in a nearby tree and shone his torch towards them, so illuminating the 'monster'. "Fifteen feet away, *towering over their heads*, was a *vast* shape something like a man. The face, everyone agreed, was round and *blood red*. No one noticed a nose or mouth, only eyes, or eye-like openings*, from which projected 'greenish-orange' beams of light*. These light beams pierced through the haze pervading the scene… Around the red 'face' and reaching upwards to a point was a dark, hood-like shape. The body was seen only from the 'head' down to the 'waist'. It appeared dark and colorless to Nunley, though some said it was green, and one child drew a picture with an outline of fire. Mrs. May said it *lighted up* when the flashlight beam touched it as if there were some source of illumination inside it. She also saw *clothing-like folds around the body*, and *terrible claws*. No one is sure whether the shape rested on the ground or was floating". "The 'monster' could not have been more than fifteen feet tall, for *it was under the overhanging limb of a tree*, and the limb was that height" [all emphases added].[76] The witnesses also reported that the 'monster' glided towards them immediately before they fled moments after first sighting the apparition (Barker 1956, pp. 23-26).

In brief, Nickell argues (Nickell 2000; Pigliucci 2010, pp. 72-73) that the witnesses first misinterpreted a meteor as a UFO crashing into a hilltop – this is a reasonable explanation for the initial UFO sighting. They then mistook, he contends, an airplane alert beacon located on the hill for a house-sized, fiery, pulsating UFO on the ground. This is certainly not an *entirely* implausible explanation, but only if the witness accounts were substantially unreliable. It should also be noted that, according to Barker, an individual investigating the site the next morning found not only 'skid marks' leading from the tree to the position where the 'UFO' had been, but also that "Where the globe had rested, a huge area of grass appeared to have been crushed down". Further, the witnesses were all locals living in the immediate vicinity, and thus should have been familiar with any 'beacons' in that location.

Regarding the 'skid marks' Nickell states that one Max Lockhart explained them as having been caused by his pickup truck when he attended the site that night to investigate. Barker, however, reports speaking to the same individual on the same topic, and gaining the impression that Lockhart's vehicle was *not* the cause of the marks (Barker 1956, pg. 29).

Finally, Nickell proposes that the witnesses were startled by a *barn owl* sitting on a branch, with a pile of brush beneath it, which they, in their highly suggestible state, perceived to be a monstrous entity. He argues that certain barn owls have a reddish-brown tint around their faces, accounting for the

[76] The witnesses also reported a strange, chocking nausea inducing odor.

coloration reported. The owl's feet were, in his opinion, mistaken for claws, and the owl also caused the hissing noise, and flew towards the witnesses, causing them to flee the scene.

For Pigliucci to assert that this proposal accounts for *all of the details reported* by the witnesses is obviously problematic. As revealed in the italicized portions of the report above, the 'monster' was described as 'towering' over the witnesses and having a 'blood red' face. The face of an owl would be extraordinarily small on a toweringly tall creature, and no owl has a 'blood red' face. An owl's talons are also far too small and close to its face to be reported as 'terrible claws' on such a large 'monster', and if the owl's claws had been observed, it seems very likely the branch they clung to would also be visible. Instead, the 'monster's' head was reported as located immediately *beneath* an overhanging branch. The descriptions of the body, including 'cloth-like folds', do not correspond well with either a void beneath a branch or a pile of brush (which none of the investigators seems to have noticed when examining the scene, including measuring the height of the branch). Nor does Nickell's proposal explain the beams of light emanating from the 'eyes', the 'hood-like' shape around the face, or the 'thumping' sound heard. Finally, the hissing noise reported as 'half way between a hiss and the noise made by a jet plane' does not accord well with the suggestion that this was simply the owl hissing.

Pigliucci agrees with Nickell's conclusion that: "if it looked like a barn owl, acted like a barn owl, and hissed, then it was most likely a barn owl" (Pigliucci 2010, pg. 73). However, as I have just shown, it is rather debatable whether the details indeed fit the barn owl explanation – certainly it does not account for *every detail* as Pigliucci asserts. This is not to say that Nickell's proposal is entirely baseless – some of the details *do* fit, more or less. However, the contradictory points must then be dismissed as inaccuracies. Nickell must therefore choose to give credence to those aspects of the account that support his contention whilst simultaneously holding that the witnesses are otherwise unreliable, which is obviously a far from satisfactory methodology.

Pigliucci notes that a Ufologist may potentially argue that Nickell has not proved there was no 'monster' present, but maintains that the proper response is to consider the options in terms of the 'best explanation': to compare the alternatives and decide which you would bet on (Pigliucci 2010, pg. 74). However, this is a false dichotomy: why must we choose between just these two options? It is surely more rational to simply accept that we do not know what occurred on that hilltop than to 'bet' that it was a 'barn owl'.

More importantly, it is exceedingly unlikely that any Ufologist *would* advance such an objection, not only because it is an unworthy response, but because this event is not one that any sensible Ufologist considers as evidential. Pigliucci included it in part because it demonstrated how a careful investigator could 'solve' a very 'cold' case (Pigliucci 2010, pp. 73-74), but Nickell has similarly proposed solutions to far more important cases, any one of which would have more accurately met Pigliucci's description of his examples as 'representative' (see Nickell's arguments relating to the 'Incident at Exeter' for a more important, but equally unconvincingly 'resolved', UFO report – Nickell & McGaha 2011; Fuller 1966, pp. 11-16).

Pigliucci draws the conclusion that a major reason why many UFO reports are unsolved is because very few people have the time, resources and skills to investigate them, and so "…the 'true believer' will always be able to point to unexplained occurrences", although "…there are several mundane reasons for why explanations may be lacking in any particular case" (Pigliucci 2010, pp. 73-74). However, Pigliucci's suggestion that only Nickell and a few other 'skeptics' devote their time to investigating UFO reports (Pigliucci 2010, pp. 73-74) is another example of an unacceptable degree of ignorance on his part. Not only is it immediately evident to anyone reviewing the UFO literature that many UFO reports, including 'cold cases' (Basterfield & Dean 2017; Basterfield and Dean 2016a; Basterfield and Dean 2016b), are 'solved' by Ufologists rather than debunkers, this situation is also precisely what we should expect once the existence of sophisticated Ufologists is recognized. If only for

reasons of self-interest - because they do not want to be 'caught out' appealing to evidence that cannot withstand scrutiny – they would be highly motivated to carefully investigate UFO reports. But more significantly, they also undertake careful investigations because they are sincere in their desire to secure reliable evidence that can assist in providing a solution to the UFO problem. Certainly there can be no doubt that, over the years, it has been the members of such groups, and *not* debunkers, who have discovered the solutions for the vast majority of UFO reports (see Westrum 2000, pp. 51, 54-55; Donderi 2000, pg. 56; Jacobs 2000, pp. 2, 3; Wilson 2000, pp. 167, 359; Jacobs 1975, pp. 145-149; Paris 2012, pp. 28ff.; Appelle 2000, pg. 30; Clarke 1998, pp. 24-25; Wynn & Wiggins 2001, pp. 54, 56; Clarke & Roberts 1990, pp. 75-76, 79). Pigliucci, however implies that all Ufologists are 'true believers' with no interest in sincerely investigating the true causes underlying UFO reports (Pigliucci 2010, pp. 70, 73-74).

It is also highly inaccurate to suggest either that Ufology is sustained merely by the bare fact that cases remain unresolved, or that most cases presented as evidence remain unresolved simply because they have not been investigated. Even sensationalist Ufologists at least try to argue that evidential cases *defy* explanation in mundane terms, and routinely refer to evidence such as the 'unexplained' cases in the Blue Book files or the 'Condon Report': cases which remain unexplained despite competent investigation (Vallee 1965, pp. 78-79; Hynek 1972, pp. 26-27, 41, 239, 241; Hynek 1977, pp. 246ff., 267-268; Binder 1967, pg. 203; Salisbury 1974, pp. 100-101, 125ff.; Sturrock 2009, pp. 57-59; Clarke & Roberts 1990, pg. 17).

The third example Pigliucci discusses is generally known as the 'Campeche Video Case'. In 2004 the crew of a Mexican government reconnaissance aircraft, using an infrared camera, observed as many as eleven globular heat sources which were not, however, visible to the naked eye. They believed these 'objects' were airborne and in motion (Pigliucci 2010, pg. 74; Sheaffer 2004). A careful and detailed investigation subsequently revealed that what the aeronauts had in fact taped were the flares from a number of oil rigs off the nearby coast.

Pigliucci discusses the subsequent "…unfortunate rush of half-baked skeptical 'solutions'" by a number of scientists, "…explanations that have in common… that they were all dead wrong and that they were all advanced without much thought by someone who simply *knew* that there could not be any unidentified flying object at all." 'Explanations' of this kind are very common whenever any 'pseudoscientific' claim is widely reported in the press, and are precisely what Hynek was referring to when he wrote of scientists who reject claims without bothering to examine the evidence. According to Pigliucci, these responses "…display the downside of skepticism: it can easily turn into an arrogant position of *a priori* rejection of any new phenomenon or idea, a position that is as lacking in critical thinking as the one of the true believer, and that simply does not help either science or the public at large". He later describes them as "…disturbing evidence then even credentialed skeptics cannot necessarily be trusted to have done their homework" (Pigliucci 2010, pg. 75). I could not agree more. However, from the comments made above and below, it should also be obvious that, in my own opinion, Pigliucci is hardly qualified to lecture.

An important point that Pigliucci fails to mention is that the *correct* answer was established by the Mexican Ufologist and director of Mexican UFO investigation group Alcione.org, Alejandro Franz[77] (Sheaffer 2004). It is therefore rather ironic that Pigliucci initially explains that this case is included "…to teach a lesson to the skeptics… because it underscores that skepticism and critical thinking often require legwork on the part of the practitioner" (Pigliucci 2010, pg. 74). That the 'practitioner' who did the 'legwork' in this case, and thus practiced 'skepticism and critical thinking' more effectively than the

[77] Franz's highly detailed analysis of the evidence can be found on the website of the UFO investigation group of which he is a member, at http://www.alcione.org/FAM/REFERENCE_DATA.html (part 1); and http://www.alcione.org/FAM/FLIR_CONCLUSION.html (part 2).

'skeptics' Pigliucci is teaching the 'lesson' to, was a Ufologist, seems to have somehow escaped his attention.

Robert Sheaffer, who wrote the article in the *Skeptical Inquirer* which serves as Pigliucci's sole source for this case (Sheaffer 2004), discusses how a number of prominent, sensationalist Ufologists were dismissive of this explanation. On this basis he concludes with the assertion: "By their reaction, the "leaders" of UFOlogy have shown themselves incapable of distinguishing logical from illogical thought, and science from pseudoscience.[78] The lesson of the Mexican Infrared UFO video illustrates once again the inability of the UFO movement to perform critical thinking" (Sheaffer 2004). In thus criticizing 'the UFO movement' *in toto*, he unjustifiably focuses exclusively on those who acted irrationally, and simply ignores all of the more reasonable Ufologists who immediately accepted the finely detailed explanatory report, and the Ufologist and ufological investigation group who produced it. The same sort of conclusion could have been as easily drawn in relation to debunkers on the basis of the insufficiently critical and ignorant responses on their part discussed earlier in the article.

This is a clear example of the partisan and misrepresentative nature of much of the literature produced by the organized 'skeptical' movement. Sheaffer's conclusion is 'justified' only by his decision to focus on the reactions of such individuals in his penultimate paragraph. This paragraph provides no information of true value. However, for an author who elsewhere insists that all Ufologists are to be classed as either 'New Age' or 'Science Fiction' – descriptions he employs from the very beginning of his book, and thereafter no other (Sheaffer 2015 *passim*) – it serves one obvious purpose: drawing attention away from the fact that the case was solved through the investigative efforts and resultant scientific reporting provided by a sophisticated Ufologist, upon which Sheaffer himself relies.

Pigliucci next writes: "Ufologists do have arguments they adduce in favour of their plea for the field to be considered a real science. Yet these arguments are not only unconvincing, but fraught with logically fallacious reasoning". He then lists "…the classical arguments advanced in defense of ufology…" as described in an article by James Oberg. These are:

"*Appeals to authority*, as in 'Jimmy Carter saw a UFO…", to which Pigliucci objects because "…said authorities have no expertise in atmospheric or astronomical phenomena" (Pigliucci 2010, pp. 75-76). It is certainly true that Ufologists sometimes mention significant figures who have witnessed UFOs. However, considering that Ufology is typically dismissed as so ridiculous that no reasonable person could possibly take it seriously, and taking into account assertions to the effect that UFO witnesses are cranks, crackpots or cultists, or at the least unsophisticated and impressionable, this is in fact an entirely reasonable response, and not an improper appeal to authority at all (see Hall 2001, pg. 149; Hynek 1972, pp. 24-27; Swords & Powell 2012, pg. 108; Vallee & Vallee 1966, pg. v; Wilkins 1954, pg. 14; Wilson 1974, pp. ix, 7, 10; Clarke 1998, pg. 488).

"*Affirming the consequent*… as in… the universe is so large that there must be other civilizations out there, so there are indeed other civilizations, and some of them are visiting us" (Pigliucci 2010, pg. 76). Pigliucci argues both that the basic argument in favour of extraterrestrial life is false, and that there is no reason to believe interstellar travel is occurring either way (Pigliucci 2010, pg. 76). Obviously such an argument is relevant only to the ETH, and not Ufology overall. Further, although I have certainly come across the argument that the size of the universe renders the probability of extraterrestrial life high multiple times, it has never been advanced in isolation of independent arguments in support of the claim that aliens have visited Earth.

"*Bandwagon appeal*: since so many people believe in UFOs, there must be something to it" (Pigliucci 2010, pg. 76). Although in researching this thesis I undertook an extensive review of ufological literature, I did not once come across this argument, as such. Ufologists *occasionally* mention that a

[78] This at least implies that Franz's report was 'science'.

relative large percentage of individuals believe in UFOs, but again this should be understood as a response to the assertion that no reasonable person takes the subject seriously.

"*Conspiratorial appeal*: the government knows, but they will not tell us". Pigliucci accepts that governments "…have a poor record of levelling with their people", but counters that "…one needs *positive* evidence before scientists can seriously consider a new phenomenon", and moreover that the chances of such a conspiracy remaining secret are very low (Pigliucci 2010, pg. 76). This is certainly a common refrain among Ufologists, but not sophisticated ones, who focus rather on the Air Force misinformation campaign, which they recognize was not an attempt to hide any particularly Earth-shattering knowledge other than the latter's inability to resolve many UFO cases (Hall 1964, pg. 105; Fawcett & Greenwood 1984, pp. 209ff.; Hall 1988, pp. 165ff.; Swords & Powell 2012, pg. 469). Pigliucci's reference to governments failing to level with their people fails to acknowledge the established fact that the U.S. government failed to level with its citizens regarding UFOs specifically (Sagan 1996, pp. 82-83; Swords & Powell, 2012, pp. 84-85, 193-195, 199-200, 298, 468, 469; Jacobs 1975, pp. 89-107, 134-135, 136, 143-144). Further, Ufologists of all stripes certainly believe they *do* possess 'positive evidence', and never base their conclusions on claims of a conspiracy alone.

"*Salvational appeal*… a recurrent pattern in ufology is the idea that somehow the extraterrestrials are here to help us…" (Pigliucci 2010, pp. 76-77). Obviously this refers to claims advanced by the extreme 'contactee' or 'cult' fringe of Ufology, but not ascribed to by even most of the sensationalist Ufologists.

6.2.iv) Williams

The entry on 'Ufology' in the *Encyclopedia of Pseudo-Science*, edited by William F. Williams, is considerably better than the previous contributions, although debunkers would likely regard it as insufficiently critical. The entry begins by defining Ufology as "The study of the group of phenomena classed as unidentified flying objects (UFOs)", acknowledge the conservative claim that UFOs may be a "…significant phenomenon for study…", but also that popular culture associates UFOs with extraterrestrial visitation. Three 'parties' to the 'UFO controversy' are discussed: that "…represented by the popular press…" which accepts even absurd claims and blows them out of proportion, "serious investigators", the position of which is accurately stated, and "skeptics" (Williams 2000, pp. 359-360).

The position of the latter is represented by a quote from *Science* in 1969 by Hudson Hoagland: "The basic difficulty inherent in any investigation of phenomena such as those of UFOs is that it is impossible for science ever to prove a universal negative. There will always be cases which remain unexplained because of lack of data, lack of repeatability, false reporting, wishful thinking, deluded observers, rumors, lies and fraud. A residue of unexplained cases is not justification for continuing an investigation after overwhelming evidence has disposed of hypotheses of supernormality, such as beings from outer space… Unexplained cases are simply unexplained. They can never constitute evidence for any hypothesis". The entry then continues: "Despite the lapse of nearly 30 years [actually more than 50] that remains substantially the position of the skeptics today" (Williams 2000, pg. 359).

This quote is significant in indicating that, even in an entry that addresses the claims of sophisticated Ufologists, the 'skeptical' response provided indicates either ignorance of the arguments advanced by the former, or a failure to recognize the non-applicability of the objections raised. As has been shown, the crux of the dispute between debunkers and sophisticated Ufologists are UFO reports that the latter insist do not merely lack an explanation, but *defy* explanation, such that in order for new data to suggest a mundane cause, it would have to contradict the data already collected. That Ufologists can at least make the case that such data exists is beyond reasonable dispute, and debunkers must therefore address this case, yet it seems they have failed to apply their critical thinking skills to their own counter

arguments. Ufologists do not require debunkers to 'prove a universal negative' or base their claims on the fact that there are cases which merely 'remain unexplained'. Rather, they argue explicitly that the evidence they adduce in favour of their conclusion is *not* merely 'a residue', but rather a true 'signal' that is causally distinct from the 'noise' produced by misidentifications (Salisbury 1974, pp. 100, 125-127; Hynek 1972, pp. 217, 267, 284-285; Vallee 1965, pp. 78-79; Vallee & Vallee 1966, pp. 97ff.).

Debunkers and sophisticated Ufologists therefore agree both that most cases are caused by misidentifications, and that the fact that some cases have not been explained does not in itself constitute evidence in favour of the Ufologists' conclusions. Thus, the debunkers are continually referring to facts that would be true whether there were a genuine 'signal', as the Ufologists believe, or not, and hence base their position on an effectively irrelevant point. Significantly, the Ufologists' appeal to statistical evidence (Salisbury 1974, pp. 101ff.; Hynek 1977, pp. 243ff.; Binder 1967 *passim*; Hall 1964, pp. 143ff.; Hall 2001 *passim*; , a type of evidence that could therefore be meaningfully employed by debunkers in refutation, yet is ignored.

The statement that "…Unexplained cases… can never constitute evidence for any hypothesis" is undeniably, and grossly, mistaken. Unexplained events, as a class, are probably the single greatest impetus for scientific discovery. Obviously in the process they must become explicable, but this is an entirely trivial point. If the cases Ufologists adduce as confirming their hypothesis of a true and scientifically significant 'signal' could be shown to exhibit strong and significant patterns - particularly if the same patterns were demonstrably not repeated within the greater mass of UFO reports - then this could most certainly be reasonably employed as evidence supporting the proposition that such cases at least deserve further scrutiny, as Ufologists argue.

6.2.v) Plait

Finally, I will mention the one point raised in the chapter on UFOs in Phillip Plait's *Bad Astronomy* that has not already been discussed above (2002, pp. 203ff). Plait asserts that no amateur astronomer has ever reported to him that they have witnessed a UFO, and argues that this is because they are familiar with the skies and thus able to identify things that other witnesses cannot. Firstly, the justification for Plait's claim is hardly evidential – what makes him think that, if an amateur astronomer *did* see something they could not identify, they would report it to him? Secondly, his explanation that the claimed dearth of reports from amateur astronomers arises from their superior knowledge of the sky would be refuted if professional astronomers reported witnessing UFOs – as indeed they have, including, famously, the discoverer of Pluto, Clyde Tombaugh, among many others (Hall 1964, pp 49-55; Hall 2001, pp. 150-166). In response to Sturrock's survey of American Astronomical Society members, discussed above, sixty-five reported witnessing and/or obtaining an instrumental record of something they could not explain (Sturrock 2009, pg. 76). These oft reported facts should be well known to anyone with a working knowledge of the relevant literature, and indeed can be very easily confirmed simply by searching the internet for 'astronomers witness UFOs' – a search that also reveals the prominence of Plait's contrary claim.

The above examples are, I believe, good exemplars of debunkers' discussions and portrayals of Ufology generally. For reasons of space I have not discussed any book length treatments of the subject, but the essential features are, in my opinion, much the same. Of course, to be fair it must be recognized that debunkers are not required to focus *exclusively* on the strongest ufological arguments, and can entirely reasonably address more popular publications and claims. However, given that they insist on asserting that 'Ufology is pseudoscience', they cannot justifiably maintain that this position has been established unless they do at some point address their more sophisticated opponents. Far too many publications of the 'skeptical' movement not only fail to do this, but present a picture of Ufology from

which the most significant arguments have been excised. As a result, it is entirely clear that philosophers and other commentators who derive their knowledge of Ufology from debunkers will typically be misled as to its true nature. Although I have not the space to demonstrate as much here, the same applies also to many other ‘pseudosciences’, to the effect that the latter are mistakenly perceived as having far less in common with genuine (if bad) science than is in reality the case.

7) Implications and Recommendations

I believe that the arguments above have clearly established not only that no successful demarcation method *has* been proposed, but that the true nature of many purported 'pseudosciences', and reasonable requirements for any genuinely justified, 'traditional' use of the terms 'demarcation' and 'pseudoscience', ensure that no practically useful method *could* possibly be devised. It only remains, therefore, to determine what the implications of the above conclusion are for the 'traditional' demarcation problem.

7.1) Meaningful 'Demarcation' Possible

Crucially, to conclude that the 'demarcation of pseudoscience' is essentially an insoluble red herring, and that the relevant claims can only be addressed through standard scientific arguments with standard scientific conclusions, *is* to reject the 'traditional' form of the demarcation problem, but certainly does *not* constitute a rejection of the demarcation problem *in toto*. Laudan's rhetorical flourish in labelling demarcation a 'pseudo-problem' was, I believe, a mistake on his part. Indeed, focusing attention on the need to address the scientific/empirical virtues of unorthodox claims and state conclusions in correspondingly appropriate terms is, in fact, the *answer* to the 'Demarcation Problem'. The problem of *how* demarcation claims can be justified is thus subsumed within the general discussion of typical scientific practices. As to what should be demarcated, it is evidence that there are *many* important lines of demarcation, depending on the relevant objective. What to fund, and what to teach; what should be admissible in a court of law, and what should be accepted by politicians in developing policies and writing laws; what should be pursued by medical researchers, and what should be acted upon by medical practitioners; obviously all require that different 'bars' for inclusion be met. At the other end of the scientific spectrum, the rejection of arguments and conclusions as failing to meet the 'bar' of pursuit worthiness is, as seen, not necessarily a matter of deeming them to be 'pseudoscience', but undoubtedly a very important issue nonetheless.

Like Laudan, I am emphatically *not* arguing that *no difference* whatsoever can possibly be established between arguments that have been labelled 'pseudoscience' and genuine science, or that it would be impossible to *ever* justify using the term 'pseudoscience'. My arguments have expressly not established, nor been intended to establish, that there is no such thing as 'scientific status', or that the proper application of the term 'science' to various subjects and arguments cannot be rationally discussed. Rather, as per Laudan's argument, my contention has been that scientific status and epistemic warrant are not co-extensive, and thus establishing an argument's scientific status does not immediately justify any specific conclusion regarding its level of epistemic virtue, as demarcation would require. In short, 'science' is not always authoritative.

7.2) Demarcation as a Scientific Process

To explore the practical implications of this position, it is necessary to first disambiguate the two aspects of 'demarcation' that have often been confounded. In terms of the inclusion of arguments for consideration in making important, science based decisions, I have already shown that the 'traditional' Demarcation Problem is largely irrelevant to the deliberations of the lay persons responsible. That these individuals must typically rely on the advice of qualified scientists, is inescapable, and they should undoubtedly focus particularly on determining what the relevant established, consensus positions are. If a scientist advising such a lay audience should introduce arguments like those purported to be 'pseudoscience', it is, firstly, extremely likely they would be simply wasting everyone's time, considering

that even poorly to moderately well supported, yet undoubtedly *scientific* arguments would typically be of insufficient merit to warrant consideration in such contexts. Secondly, if such an argument should somehow be legitimately raised, it remains the case that it must be addressed in precisely the same manner as any scientific hypothesis – through scientific analysis of the relevant arguments and evidence. This *remains* the case because, in the absence of any accepted demarcation method, and because scientific analysis is undeniably the most powerful applicable tool, this was always the only justified course. That it has now been demonstrated that no relevantly useful demarcation method could *ever* be properly justified thus makes no difference to the practical situation.

In regard to debunkers and their efforts to combat irrational claims, it is certainly not correct that my position, as has been claimed of Laudan's, in any way withdraws or denies support for such endeavors. In establishing the contention that target claims must be addressed through established scientific processes, my argument rather provides approval for a method debunkers already often employ (however imperfectly), and disqualifies only those arguments that, like *ad hominem* attacks, were already manifestly improper. The major appreciable impact on debunkers' publications would therefore be primarily semantic – excision of the term 'pseudoscience' and derivative usages of 'demarcation'. This change is surely not particularly significant in terms of having any negative effect on the details of the core *arguments*, which are directed to demonstrating the lack of genuine support for insufficiently justified unorthodox claims, or the *conclusions*, which can surely be stated as effectively as previously, and arguably more clearly, given that the uncertain nature of 'pseudoscience' is no longer an issue. Indeed, an additional benefit is the elimination of any need for debunkers to begin their publications, as many do, by discussing the philosophically problematic nature of any 'demarcation' of 'pseudoscience' (Gardner 1952, pg. 7; Gardner 2000, pp. 1-2; Pigliucci 2010, pp. 1ff.; Hines 2003, pg. 13; Williams 2000 pp. ix-xi;).

My arguments do demonstrate that any assertion of ahistorical rejection, such as purportedly need never be revisited, cannot be justified. However, the conclusion that an argument or claim should be rejected as unworthy of attention, unless or until there is some significant alteration in the relevant evidence, can be justified as strongly as any other type of scientific conclusion, which is saying quite a lot. However, it is of *some* significance that further consideration of such rejected claims cannot be legitimately disqualified as improper to science.

7.3) Demarcation as Disqualification Unsuccessful

The argument can be made that demarcation in the latter sense has not achieved its purpose anyway, and indeed has quite possibly been counter-productive. Certainly, as many 'skeptics' themselves point out, claims prominently dismissed as 'pseudoscience' continue to be accepted by a troublingly high percentage of the general public. A blog by a leading figure in the organized 'skeptical' movement, Michael Shermer, discusses a 2009 Harris Poll of U.S. citizens which revealed (focusing exclusively on 'pseudoscientific', rather than religious, beliefs): 42% believe in ghosts; 40% believe in Creationism; 32% believe in UFOs; 26% believe in astrology; and 23% believe in witches (Shermer 2011). Further, in a 2001 article examining Gallup Poll results regarding similar beliefs, it was found that there had been little change over the preceding decade, and the 2001 results are also *very* similar to those of the 2009 poll (Newport & Strausberg 2001): 42% believe that houses can be haunted; 33% believe that extraterrestrials have visited the Earth; 28% believe in Astrology; and 26% believe in witches.

7.3.i) Criticisms of Organized 'Skepticism'

Members of the organized 'skeptical' movement claim it *has* had a major impact in reducing belief in irrational claims (Frazier 2009b, pg. 35). However, whatever impact it may have had has undoubtedly been seriously limited by the combative, partisan nature and low quality of many of its publications. For example, the worst of the contributions on Ufology discussed above - three chapters by Martin Gardner (2000, pp. 175-208) - were reprinted from his regular column in the most prominent 'skeptical' journal, the *Skeptical Inquirer* (Gardner 2000, pg. 2), which gives some indication of the nature of that publication. Having read much 'skeptical' and debunking literature, I can confidently say that the great majority of such publications could only have very little, if any, impact in altering the beliefs of those who accept or are drawn towards the claims 'debunked' therein, for two major reasons.

Firstly, as seen above in relation to Ufology, the 'skeptics' typically do not address the most sophisticated arguments advanced in favour of targeted claims. As has been argued of demarcation generally, it is essential for debunkers to combat those arguments most likely to be influential among essentially rational individuals. After all, anyone accepting, or inclined towards accepting, a purportedly 'pseudoscientific' claim, but who is also likely to read and potentially be influenced by debunking literature, is extremely unlikely to have been swayed by the more sensationalist and incredible unorthodox arguments. Rather, they can confidently be expected to have formed their beliefs on the basis of, and so certainly be aware of, the strongest relevant arguments. Thus, what they would discover in most 'skeptical' literature would be irrelevant to, and so incapable of having any influence upon, their 'pseudoscientific' beliefs. Indeed, it may rather support the perception that the reasoning behind their beliefs is unassailable, and hence ignored by opponents

Secondly, the tone of many debunking publications is entirely unsuited to the task of influencing 'believers'. 'Skeptics' typically approach 'pseudoscientific' arguments in a highly combative, yet dismissive, manner, with scorn, ridicule and entirely *ad hominem* arguments playing a prominent role.[79] The authors also often simply assume their audience already shares their 'skeptical' beliefs. For example, Gardner in the chapters discussed above makes no effort whatsoever to provide arguments against the claims he mentions, but rather assumes his audience will join him in regarding them as utterly ridiculous. Such attitudes ensure that individuals seeking a reasoned, rational response to 'pseudoscientific' arguments to which they may be sympathetic are disappointed and insulted, rather than educated.

Through claiming that the *Skeptical Inquirer*, for example, "…has steadfastly championed science and reason and been the leading voice for reliable scientific examination of all manner of questionable claims… (Frazier 2009a, pg. 11), 'skeptics' explicitly present themselves as the 'champions of science'. As far as I am aware, and certainly from the perspective of the lay public, this title is not challenged by the broader scientific community. Thus the inaccurate, improperly targeted and unsuitably toned publications of debunkers are perceived as being acceptable to, and representing the response of, the scientific community at large. That this is counterproductive – fostering the belief among many that science is blindly arrogant, ignorant, and dishonest in its response to contested claims – is entirely predictable. A good example of a reasonable individual, unexpectedly confronted with apparent evidence of the paranormal and seeking to determine where the truth lies, is journalist Will Storr, who wrote a book recounting his experiences. What he has to say regarding 'skeptics' is therefore highly instructive: "The problem, I found, with turning fully skeptical is that to really pull it off you've got to stitch your eyes shut, pump rubber glue down your ears and say, 'I don't care what anybody else says. They can throw whatever they like at me but it won't make any difference because I already know the truth…'. To be a hard sceptic you have to start with the belief and *then* look at the evidence. Which often, you have to twist and squash and smear to make it fit your point" (Storr 2006, pg. 222).

[79] This was evident in the debunkers' discussions of Ufology covered above. See also Sheaffer 2015; Gardner 1952; Frazier 2009a; Randi 1982

7.3.ii) Partisan Disputes

Such 'skeptics' too often proceed as though once something has been denounced as 'pseudoscience', it is entirely appropriate to employ scorn and ridicule in attacking both the supporting arguments and their proponents. However, such techniques, alongside ruling a topic to be outside the bounds of academic discussion, primarily impact upon the actions of scientists, academics and others who must similarly take care to preserve their reputations. Thus it has the principal effect of suppressing reasonable discussion, among reasonable people, particularly within the academy, yet has little if any impact on much less reasonable individuals and their less rational arguments. The former are accordingly largely eliminated, whilst the latter come to dominate the subject.

Debates entirely outside the bounds of the academy and its funding processes are completely subject to market forces on both sides, which leads to excessively partisan, increasingly polarized debates that emphasize irrational, sensational claims on the one hand and entertainingly scornful rejections on the other.[80] This is clearly not an outcome which is to anyone's benefit.

7.4 The Benefits of Academic Demarcation

On the other hand, it is very difficult to see how the far more rational discussion that would undoubtedly result from allowing scientists and other academics the freedom to discuss the relevant subjects as professionals could possibly be harmful. Those advocating ill-supported unorthodox claims can perform a number of useful functions in such a context. For example, as previously discussed, there can be no doubt that during the period in which relatively responsible civilian UFO research organizations flourished, the vast majority of UFO reports were 'explained' through their efforts rather than those of debunkers, and many of the more outrageous lies and hoaxes were exposed. The conclusions and, in particular, the concessions of the representatives of such groups are also far more likely to be accepted by individuals inclined to accept the disputed claims. Moreover, the same individuals, having demonstrated a willingness to challenge orthodox scientific positions, would have greater credibility also in responding to more socially significant challenges to scientific authority – for example, in defending the use of immunizations.[81]

Within an academic context, both sides of the relevant debates, overseen by the wider academic community, would serve to keep their opponents honest, on topic, and subject to the rules of civilized debate. Members of the lay public intending to discover answers for themselves would thus have available to them a source of genuinely informative arguments and evidence claims which may serve as a more reliable basis for their own conclusions. The scientific community at large would likewise have to hand a more defensible basis for its collective conclusions, which may then be appropriately communicated to those lay persons willing to accept the determinations of that community.

That there *are* scientists who would like to take advantage of a newfound freedom to seriously discuss previously 'demarcated' subjects has been demonstrated above, at least as regards Ufology, but there is no evident reason to expect that the same would not hold true for many other topics. Scientists are subject to the same sense of mystery and adventure that attracts the lay public to accounts of inexplicable and anomalous experiences. On some topics, there may be no sufficiently capable individuals willing and able to advance an argument suitable for publication. It would then be beneficial to modify the system whereby publication approval is decided, in consideration of the public good that may result of allowing

[80] The nature of 'skeptical' publications no doubt reflects what its authors and publishers believe the relevant section of the paying public want to read.

[81] See for example http://www.librarything.com/work/1435152/reviews/50596037

even indefensible claims to be presented in the strongest light possible, and then be soundly, and scientifically, refuted. The point here is *not* that we should be tolerant of poor scholarship, but rather that it is better to *demonstrate* that a claim enjoying wide public acceptance is seriously flawed than to simply exclude it from professional academic discussion. Medical researchers have produced some outstanding examples of how scientific debunking should be done.[82]

In short, if demarcation type arguments are worthwhile, and the philosophical 'demarcation of pseudoscience' must be abandoned, these are good reasons to return previously disqualified debates to the academic arena. Many 'pseudosciences' refer to empirical claims best refuted through scientific arguments, but the latter are inescapably subject to many potential flaws. These can only be defended against through community input and oversight, which is therefore necessary to ensure that arguments against unsustainable claims are of sufficient quality that they do not end up doing more harm than good. The public interest value of rationally debating problematic hypotheses with widespread public currency is surely sufficient to warrant both the publication of *both* sides of a dispute (otherwise there cannot *be* any debate), and the rewarding of scientists who choose to so devote their time. The extant disincentives against such activities, including the fact that scientists who so spend their time are wasting it in terms of career progression, should, therefore, be systematically and deliberately removed.

7.5 Relevant Philosophical Projects

Concluding that demarcation requires scientific argumentation does not mean that philosophers cannot continue to make important contributions. Among the relevant issues are specific considerations regarding the role of experts: for example, how expertise can be established in a context where the authority of science and its institutions are challenged, what and how scientists should communicate to a suspicious or even hostile audience, and how to best defend contested evidence. Another important problem is that of establishing what requirements should be met in order for a hypothesis to warrant inclusion within specific decision making processes. Relatedly, efforts to improve our ability to measure and compare, in a practically useful manner, the empirical support for conclusions, or other measures of scientific warrant, would be of undoubted value. In terms of a specific, practical project, it would certainly be useful to compare the histories of revolutionary claims which were found to be false (i.e. Cold Fusion and Polywater) with cases where such a claim, despite initial, justified objections, ultimately proved correct, as with Continental Drift. None of the discussions of 'pathological science' cited above make any attempt to undertake this sort of comparison, which is surely essential if we are to draw any genuinely useful conclusions from such events.

[82] See references provided above.

8) Conclusion

The ‘demarcation of pseudoscience’ has been a costly red herring which has produced a number of unfortunate consequences. Recognizing the futility of attempting to solve the ‘traditional’ Demarcation Problem, and its irrelevance to many of the objectives of demarcation, should shift the attention of philosophers and interested scientists towards more practically significant and genuinely soluble demarcation projects. Thus, rather than continuing the fruitless ‘traditional’ approach, or rejecting demarcation as a ‘pseudo-problem’, a reformulated demarcation project can employ the tools furnished by science to establish the proper attribution of degrees of scientific authority, and address unorthodox propositions in a more professional, and so informative, manner. Thereby, the most valuable objectives of the original Demarcation Problem can at last be successfully attained.

Bibliography

Anderson, Pia. (2007). 'Ancient Alien Brothers, Ancient Terrestrial Remains: Archaeology or Religion?', Tumminia, Diana G. (Ed.). *Alien Worlds: Social and Religious Dimensions of Extraterrestrial Contact*, Syracuse University Press, Syracuse.

Appelle, Stuart. (2000). 'Ufology and Academia: The UFO Phenomenon as a Scholarly Discipline', Jacobs, David M. (Ed.) '*UFOs and Abductions: Challenging the Borders of Knowledge*', University Press of Kansas, Lawrence.

Barker, Gray. (1956). *They Knew Too Much About Flying Saucers*, University Books, New York.

Basterfield, Keith & Dean, Paul. (2016a). '50 Years On... The 2nd April 1966 Balwyn, Australia Photograph – Revisited' (1), *UFOs – Documenting the Evidence*, http://ufos-documenting-the-evidence.blogspot.com/2016/03/50-years-on-2nd-april-1966-balwyn.html [Accessed 14/11/17].

Basterfield, Keith & Dean, Paul. (2016b). '50 Years On... The 2nd April 1966 Balwyn, Australia Photograph – Revisited' (2), *UFOs – Documenting the Evidence*, http://ufos-documenting-the-evidence.blogspot.com/2016/07/50-years-on_19.html [Accessed 14/11/17].

Basterfield, Keith & Dean, Paul. (2017). 'Cold case review report - 23 August 1953 - Port Moresby', *Unidentified Aerial Phenomena – Scientific Research*, http://ufos-scientificresearch.blogspot.com/2017/04/cold-case-review-report-23-august-1953.html [Accessed 14/11/17].

Bausell, Barker R. (2007). *Snake Oil Science: The Truth About Complementary and Alternative Medicine*, Oxford University Press, New York.

Beyerstein, B. (1996). 'Distinguishing science from pseudoscience' *An article prepared for the Centre for Curriculum and Professional Development Victoria Canada*.

Binder, Otto. (1967). *What We Really Know about Flying Saucers*, Fawcett Publications, Greenwich, Connecticut.

Blair, David C. (2003). 'Information retrieval and the philosophy of language', *Annual Review of Information Science and Technology*, Vol. 37, No. 1.

Boudry, Maarten. (2013). 'Loki's Wager and Laudan's Error: On Genuine and Territorial Demarcation', Pigliucci, Massimo & Boudry, Maarten. (Eds.). *Philosophy of Pseudoscience: Reconsidering the Demarcation Problem*, University of Chicago Press, Chicago.

Boudry, Maarten; Blancke, Stefaan. & Braeckman, Johan. (2010). 'How not to Attack Intelligent Design Creationism: Philosophical Misconceptions about Methodological Naturalism', *Foundations of Science*, Vol. 15, No. 3.
Boudry, Maarten; Blancke, Stefaan & Braeckman, Johan. (2012). 'Grist to the mill of anti-evolutionism: The failed strategy of ruling the supernatural out of science by philosophical fiat', *Science & Education*, Vol. 21, No. 8.

Bowen, Charles (Ed.). (1969). *The Humanoids*, Futura Publications, London.

Broughton, Richard S. (1991). *Parapsychology: The Controversial Science*, Ballantine Books, New York

Bryson, Bill. (2003). *A Short History of Nearly Everything*, Transworld Digital, 01 Edition [23 Jan. 2010].

Burke, Katie L. (2016). 'Stop Using the Word Pseudoscience', *American Scientist*, https://www.americanscientist.org/blog/from-the-staff/stop-using-the-word-pseudoscience [accessed 21/04/2017].

Carrington, Hereward & Fodor, Nandor. (1951). *Haunted People: Story of the Poltergeist Down the Centuries*, Signet Books, New York.

Chalmers, Alan F. (2013). *What is this Thing Called Science?* (4th Edt.), University of Queensland Press, St. Lucia, Queensland.

Clarke, David & Roberts, Andy. (1990). *Phantoms of the Sky: UFOs – A Modern Myth?*, Robert Hale, London.

Clarkson, Michael. (2011). *The Poltergeist Phenomenon: An In-Depth Investigation into Floating beds, Smashing Glass, and Other Unexplained Disturbances*, New Page Books, Pompton Plains, New Jersey.

Cleland, Carole E. & Brindell, Sheralee. (2013). 'Science and the Messy, Uncontrollable World of Nature', Pigliucci, Massimo & Boudry, Maarten (Eds.). *Philosophy of Pseudoscience: Reconsidering the Demarcation Problem*, University of Chicago Press, Chicago.

Coady, David. (Ed.). (2006). *Conspiracy Theories: The Philosophical Debate*, Ashgate Publishing, Aldershot.

Coleman, Loren. & Huyghe, Patrick. (2006). *The Field Guide to Bigfoot and Other Mystery Primates*, Anomalist Books, New York.

Condon, Dr. Edward U. (Scientific Director) & Gillmor, Daniel S. (Ed.). (1968). *Final Report of the Scientific Study of Unidentified Flying Objects*, Bantam Books, New York.

Daempfle, Peter A. (2013). *Good Science, Bad Science, Pseudoscience, and Just Plain Bunk*, Rowman and Littlefield Publishers, Lanham, Maryland.

Derksen, A .A. (1993). 'The Seven Sins of Pseudo-Science', *Journal for General Philosophy of Science*, Vol. 24.

Derry, Gregory N. (1999). *What Science is and How it Works*, Princeton University Press, Princeton.

DeStefano, Frank. (2007). 'Vaccines and autism: evidence does not support a causal association', *Clinical Pharmacology & Therapeutics*, Vol. 82, No. 6.

Destefano, Frank; Bhasin, Tanya Karapurkar; Thompson, William W.; Yeargin-Allsopp, Marshalyn; Boyle, Coleen. (2004). 'Age at First Measles-Mumps-Rubella Vaccination in Children With Autism and School-Matched Control Subjects: A Population-Based Study in Metropolitan Atlanta', *Paediatrics*, Vol. 113, No. 2.

Dolan, Richard M. (2002). *UFOs and the National Security State: Chronology of a Cover-Up 1941-1973* (Revised Edt.), Hampton Roads Publishing Company, Charlottesville.
Donderi, Don C. (2000). 'Science, Law, and War: Alternative Frameworks for the UFO Evidence', Jacobs, David M. (Ed.). '*UFOs and Abductions: Challenging the Borders of Knowledge*', University Press of Kansas, Lawrence.

Edmond, Garry & Mercer, David. (2002). 'Conjectures and Exhumations: Citations of History, Philosophy and Sociology of Science in US Federal Courts', *Law and Literature*, Vol. 14, No. 2.

Ellis, Geoff. (2013). 'David Attenborough: "I Believe the Abominable Snowman May be Real"', *RadioTimes*, 10/09/13. http://www.radiotimes.com/news/2013-09-10/david-attenborough-i-believe-the-abominable-snowman-may-be-real/ [Accessed 16/09/17].

Farrell, Joseph P. (2005). *The Giza Death Star Destroyed: The Ancient War for Future Science*, Adventures Unlimited Press, Kempton, Illinois.

Farrington, C. Paddy; Miller, Elizabeth; Taylor, Brent. (2001). 'MMR and autism: further evidence against a causal association', *Vaccine*, Vol. 19, No. 27.

Fawcett, Lawrence & Greenwood, Barry. (1984). *Clear Intent: The Government Coverup of the UFO Experience*, Prentice Hall, Englewood Cliffs, New Jersey.

Fishman, Yonatan I. & Boudry, Maarten. (2013). 'Does Science Presuppose Naturalism (or Anything at All)?', *Science and Education*, Vol. 22, No. 5.

Fishman, Yonatan. I. (2009). 'Can Science Test Supernatural Worldviews? *Science & Education*, Vol. 18, No. 6-7.

Forrest, Barbara. (2000). 'Methodological Naturalism and Philosophical Naturalism: Clarifying the Connection', *Philo*, Vol. 3, No. 2.

Forster, Malcolm & Sober, Elliott. (1994). 'Hard to Tell when Simpler, More Unified, or Less *Ad hoc* Theories will Provide More Accurate Predictions', *British Journal for the Philosophy of Science*, No. 45.

Frazier, Kendrick (Ed.). (2009a). *Science under Siege: Defending Science, Exposing Pseudoscience*, Prometheus Books, Amherst, New York.

Frazier, Kendrick, (2009b), 'In Defense of the Higher Values', Frazier, Kendrick (Ed.). *Science under Siege: Defending Science, Exposing Pseudoscience*, Prometheus Books, Amherst, New York.

Fuller, John G. (1966). *Incident at Exeter: The Story of Unidentified Flying Objects Over America Today*, G.P Putnam's Sons, New York.

Fuller, Steve. (1985). 'The Demarcation of Science: A Problem Whose Demise has been Greatly Exaggerated,' *Pacific Philosophical Quarterly*, Vol. 66, No. 3-4.

Furness, Hannah. (2017) 'Sir David Attenborough's Next Project: Find the Abominable Snowman, *The Telegraph*, 04/09/17, http://www.telegraph.co.uk/news/11075552/Sir-David-Attenboroughs-next-project-find-the-Abominable-Snowman.html [Accessed 16/09/17].

Garcia, Carlos E. (2006) *Popper's Theory of Science: An Apologia*, Continuum, London.

Gardner, Martin. (1952). *Fads and Fallacies in the Name of Science*, Dover Publications, New York.

Gardner, Martin. (2000). *Did Adam and Eve Have Navels? Debunking Pseudoscience*, W. W. Norton and Company, New York.

Gerber, Jeffrey S. & Offit, Paul A. (2009). 'Vaccines and Autism: A Tale of Shifting Hypotheses', *Clinical Infectious Diseases*, Vol. 48, No. 4.

Gieryn, Thomas F. (1983). 'Boundary Work and the Demarcation of Science from Non-Science: Strains and Interests in Professional Ideologies of Scientists', *American Sociological Review*, Vol. 48, No. 6.

Gillies, Donald. (1993). *Philosophy of Science in the Twentieth Century: Four Central Themes*, Blackwell, Oxford.

Godfrey-Smith, Peter. (2003). *Theory and Reality: An Introduction to the Philosophy of Science*, University of Chicago Press, Chicago.

Goldacre, Ben. (2008). *Bad Science: Quacks, Hacks, and Big Pharma Flacks*, Faber and Faber, New York.

Goldacre, Ben. (2013). *Bad Pharma: How Medicine is Broken, and How We Can Fix it*, Fourth Estate, London.

Good, Timothy. (1987). *Above Top Secret: The Worldwide UFO Cover-Up*, HarperCollins, London.

Goode, Erich. (2013). 'Paranormalism and Pseudoscience as Deviance', Pigliucci, Massimo & Boudry, Maarten (Eds.). *Philosophy of Pseudoscience: Reconsidering the Demarcation Problem*, University of Chicago Press, Chicago.

Gordin, Michael, D. (2012) *The Pseudoscience Wars*, University of Chicago Press, Chicago.

Greene, Mott T. (2015). *Alfred Wegener: Science, Exploration, and the Theory of Continental Drift*, John Hopkins University Press, Baltimore.

Gross, Barry R. (2009). 'Commentary: Philosophers at the Bar – Some Reasons for Restraint', Pennock, Robert T. & Ruse, Michael (Eds.). *But is it Science? The Philosophical Questions in the Creation/Evolution Controversy*, Prometheus Books, Amherst, New York.

Haines, Richard F. (1987). *Melbourne Episode: Case Study of a Missing Pilot*, L.D.A. Press, Las Altos, California.

Hall, Richard M. (Ed.). (1964) *The UFO Evidence*, Barnes and Noble, New York.

Hall, Richard M. (1988). *Uninvited Guests: A Documented History of UFO Sightings, Alien Encounters and Coverups*, Aurora Press, Santa Fe.

Hall, Richard M. (Ed.). (2001) *The UFO Evidence (A Thirty-Year Report)*, Vol. 2, Scarecrow Press, Lanham, Maryland.

Hall, William J. (2014). *The World's Most Haunted House: The True Story of the Bridgeport Poltergeist on Lindley Street*, New Page Books, Pompton Plains, New Jersey.

Hallowell, Michael J. & Ritson, Darren W. (2008). *The South Shields Poltergeist*, History Press, Stroud, Gloucestershire.

Hansson, Sven Ove. (2009) 'Cutting the Gordian Knot of Demarcation', *International Studies in the Philosophy of Science*, Vol. 23, No. 3.

Hansson, Sven Ove. (2013). 'Defining Pseudoscience and Science', Pigliucci, Massimo. & Boudry, Maarten. (Eds.). *Philosophy of Pseudoscience: Reconsidering the Demarcation Problem*, University of Chicago Press, Chicago.

Hansson, Sven Ove. (2017). "Science and Pseudo-Science", Zalta, Edward N. (Ed.) *The Stanford Encyclopedia of Philosophy*. https://plato.stanford.edu/archives/sum2017/entries/pseudo-science/ [Accessed 20/07/17].

Healy, Tony & Cropper Paul. (2006). *The Yowie: In Search of Australia's Bigfoot*, Anomalist Books, San Antonio

Heuvelmans, Bernard. (1958). *On the Track of Unknown Animals*, Hill and Wang, New York.

Hines, Terence. (2003). *Pseudoscience and the Paranormal*, (2nd Edt.), Prometheus Books, Amherst, New York.

Hitchcock, Cristopher. (2004). 'Introduction: What is the Philosophy of Science?', Hitchcock, Cristopher (Ed.), *Contemporary Debates in Philosophy of Science*, Blackwell Publishing, Oxford.

Huizenga, John R. (1993) *Cold Fusion: The Scientific Fiasco of the Century*, Oxford University Press, New York.

Hunt, Christopher J. (2012) 'On *Ad hoc* Hypotheses' *Philosophy of Science*, Vol. 79, No. 1.

Hynek, J. Allen. (1972) *The UFO Experience: A Scientific Enquiry*, Corgi Books, London.

Hynek, J. Allen. (1977) *The Hynek UFO Report*, Barnes and Noble Books, New York.

Jacobs, David Michael. (1975). *The UFO Controversy in America*, Indiana University Press, Bloomington.

Jacobs, David Michael. (Ed.) (2000). *UFOs and Abductions: Challenging the Borders of Knowledge*, University Press of Kansas, Lawrence.

Jordan, Stuart D. (2009). 'Global Climate Change Triggered by Global Warming', Frazier, Kendrick (Ed.). *Science Under Siege: Defending Science, Exposing Pseudoscience*, Prometheus Books, Amherst, New York.

Keel, John A. (1967) 'Introduction', Binder, Otto. *What We Really Know about Flying Saucers*, Fawcett Publications, Greenwich, Connecticut.

Keel, John A. (1970). *Why UFOs? Operation Trojan Horse*, Manor Books, New York.

Kelleher, Colm A. & Knapp, George. (2005). *Hunt for the Skinwalker: Science Confronts the Unexplained at a Remote Ranch in Utah*, Paraview, New York.

Kelly, Lynne. (2004). *The Skeptics Guide to the Paranormal*, Thunder Mouth Press, New York.

Keyhoe, Donald E. (1960). *Flying Saucers: Top Secret*, G. P. Putnam's Sons, New York.

Klass, Philip J. (1974). *UFOs Explained*, Vintage Books, New York.

Klass, Philip J. (1983). *UFOs: The Public Deceived*, Prometheus Books, Amherst, New York.

Kuhn, Thomas S. (1996). *The Structure of Scientific Revolutions* (3rd Edt.), University of Chicago Press, Chicago.

Ladyman, James. (2013). 'Towards a Demarcation of Science from Pseudoscience', Pigliucci, Massimo. & Boudry, Maarten (Eds.). *Philosophy of Pseudoscience: Reconsidering the Demarcation Problem*, University of Chicago Press, Chicago.

Lakatos, Imre. (1970). 'Falsification and the Methodology of Scientific Research Programmes', Lakatos, Imre & Musgrave, Alan (Eds.), *Criticism and the Growth of Knowledge: Proceedings of the International Colloquium in the Philosophy of Science, London, 1965, Volume 4*, Cambridge University Press, Cambridge.

Langmuir, Irving & Hall, R. N. (1989). 'Pathological Science', *Research Technology Management*, 32, 5.

Laudan, Larry. (1977). *Progress and Its Problems: Towards a Theory of Scientific Growth*, University of California Press, London.

Laudan, Larry. (1983). 'The Demise of the Demarcation Problem,' *Physics, Philosophy and Psychoanalysis*, Springer Netherlands.

Laudan, Larry. (1996a). 'The Demise of the Demarcation Problem', Laudan, Larry. *Beyond Positivism and Relativism: Theory, Method, and Evidence*, Westview Press, Boulder, Colorado.

Laudan, Larry (1996b) 'The Sins of the Fathers: Positivist Origins of Postpositivist Relativisms', Laudan, Larry. *Beyond Positivism and Relativism: Theory, Method, and Evidence*, Westview Press, Boulder, Colorado.

Laursen, Christopher. (2015). 'Poltergeist', Cardin, Matt (Ed.). *Ghosts, Spirits, and Psychics: The Paranormal from Alchemy to Zombies*, ABC – CLIO, Santa Barbara.

Lightner, Robert. (1998). *Angels, Satan, and Demons*, Word Publishing, Nashville.

Long, Greg. (1990). *Examining the Earthlight Theory: The Yakima UFO Microcosm*, J. Allen Hynek Centre for UFO Studies, Chicago.

Lorenzen, Coral & Lorenzen, Jim. (1967). *Flying Saucer Occupants*, Signet Books, New York.

Lugg, Andrew. (1987). 'Bunkum, Flim-Flam and Quackery: Pseudoscience as a Philosophical Problem', *Dialectica*, Vol. 41, No. 3.

Lugg, Andrew. (1992). 'Pseudoscience as Nonsense', *Methodology and Science*, Vol. 25, No. 2.

Madsen, Kreesten Meldgaard; Hviid, Anders; Vestergaard, Mogens; Schendel, Diana; Wohlfahrt, Jan; Thorsen, Poul; Olsen, Jørn; & Melbye, Mads. (2002). 'A Population-Based Study of Measles, Mumps, and Rubella Vaccination and Autism', *New England Journal of Medicine*, vol. 347, No. 19.

Mahner, Martin. (2013). 'Science and Pseudoscience: How to Demarcate After the (Alleged) Demise of the Demarcation Problem', Pigliucci, Massimo & Boudry, Maarten (Eds.). *Philosophy of Pseudoscience: Reconsidering the Demarcation Problem*, University of Chicago Press, Chicago.

Martin, Malachi. (1976). *Hostage to the Devil*, Harper Collins, San Francisco.

Mayo, Deborah G. & Spanos, Arias. (2010). *Error and Inference: Recent Exchanges on Experimental Reasoning, Reliability, and the Objectivity and Rationality of Science*, Cambridge University Press, New York.

Meyer, Stephen C. (2000). 'The Demarcation of Science and Religion', *The History of Science and Religion in the Western Tradition: An Encyclopedia*. Garland Publishing, http://www.discovery.org/a/3524 [Accessed 27/12/17].

Mousseau, Marie-Catherine. (2003). 'Parapsychology: Science or Pseudo-Science?' *Journal of Scientific Exploration*, Vol. 17, No. 2.

Mrożek-Budzyn, Dorota; Kiełtyka, Agnieszka; Majewska, Renata. (2010). 'Lack of Association Between Measles-Mumps-Rubella Vaccination and Autism in Children: A Case-Control Study', *The Pediatric Infectious Disease Journal*, Vol. 29, No. 5.

Mulkay, Michael & Gilbert, Nigel. (1981). 'Putting Philosophy to Work: Karl Popper's Influence on Scientific Practice', *Philosophy of the Social Sciences*, Vol. 11.

Newport, Frank & Strausberg, Maura. (2001). 'Americans' Belief in Psychic and Paranormal Phenomena Is Up Over Last Decade', *Gallup News*. http://news.gallup.com/poll/4483/americans-belief-psychic-paranormal-phenomena-over-last-decade.aspx [Accessed 20/09/17].

'New French Report on UFOs and Defense', (2006). *Strange Times: A Nexus New Times Special Edition*.

Nickell, Joe & McGaha, James. (2011). 'Exeter Incident' Solved! A Classic UFO Case, Forty-Five Years 'Cold'', *Skeptical Inquirer*, Vol. 35, No. 6.

Nickell, Joe. (2000). 'The Flatwoods UFO monster', *Skeptical Inquirer*, Vol. 24, No. 6.

Nickles, Thomas. (2013). 'The Problem of Demarcation: History and Future', Pigliucci, Massimo & Boudry, Maarten (Eds.). *Philosophy of Pseudoscience: Reconsidering the Demarcation Problem*, University of Chicago Press, Chicago.

Okasha, Samir. (2002). *Philosophy of Science: A Very Short Introduction*, Oxford University Press, Oxford.

Oppy, Graham. (2003). 'Creationism on Trial', *Sophia*, Vol. 42, No. 2, Oct.

Oreskes, Naomi. (1999). *The Rejection of Continental Drift*, Oxford University Press, Oxford.
Ouellette, Jennifer. (2012). 'Genie in a Bottle: The Case Against Cold Fusion', *Scientific American*, https://blogs.scientificamerican.com/cocktail-party-physics/genie-in-a-bottle-the-case-against-cold-fusion/ [Accessed 17/04/2016].

Overton, Judge William R. (2009). 'United States District Court Opinion: Mclean V. Arkansas Board of Education', Pennock, Robert T. & Ruse, Michael (Eds). *But is it Science? the Philosophical Question in the Creation/Evolution Controversy*, Prometheus Books, Amherst, New York.

Paris, Antonio. (2012). *Aerial Phenomena: Reviving Ufology for the 21st Century*, Aerial Phenomena, USA.

Peebles, Curtis. (1994). *Watch the Skies! A Chronicle of the Flying Saucer Myth*, Smithsonian Institution Press, Washington.

Pennock, Robert T. (1999). *Tower of Babel: The Evidence Against the New Creationism*, MIT Press, Cambridge.

Pennock, Robert T. (Ed.). (2001). *Intelligent Design Creationism and Its Critics*, MIT Press, Cambridge.

Pennock, Robert T. (2011). 'Can't Philosophers Tell the Difference Between Science and Religion? Demarcation Revisited', *Synthese*, Vol. 178, No. 2.

Pigliucci, Massimo & Boudry, Maarten (Eds.). (2013a). *Philosophy of Pseudoscience: Reconsidering the Demarcation Problem*, University of Chicago Press, Chicago.

Pigliucci, Massimo & Boudry, Maarten. (2013b). 'Why the Demarcation Problem Matters', Pigliucci, Massimo & Boudry, Maarten (Eds.). *Philosophy of Pseudoscience: Reconsidering the Demarcation Problem*, University of Chicago Press, Chicago.

Pigliucci, Massimo. (2010). *Nonsense on Stilts: How to Tell Science from Bunk*, The University of Chicago Press, Chicago.

Pigliucci, Massimo. (2013). 'The Demarcation Problem: A (Belated) Response to Laudan', Pigliucci, Massimo & Boudry, Maarten (Eds.). *Philosophy of Pseudoscience: Reconsidering the Demarcation Problem*, University of Chicago Press, Chicago.

Plait, Phillip C. (2002) *Bad Astronomy*, John Wiley and Sons, New York.

Plantinga, Alvin. (1997). 'Methodological Naturalism', *Perspectives on Science and Christian Faith*, Vol. 49, No. 3.

Playfair, Guy Lyon. (1980). *This House is Haunted: An Investigation of the Enfield Poltergeist*, Souvenir Press, London.

Plock, Karl T. (2001). *Roswell: Inconvenient Facts and the Will to Believe*, Prometheus Books, Amherst, New York.

Popper, Karl R. (1935). *The Logic of Scientific Discovery*, Routledge, London.

Popper, Karl R. (1962). *Conjectures and Refutations: The Growth of Scientific Knowledge*, Basic Books, New York.

Powell, James Lawrence. (2015). *Four Revolutions in the Earth Sciences: From Heresy to Truth*, Columbia University Press, New York.

Price, Harry. (1945). *Poltergeist Over England: Three Centuries of Mischievous Ghosts*, Country Life Ltd. London.

Randi, James. (1982). *Flim-Flam*, James Randi Educational Foundation, Falls Church

Randles, Jenny. (1990). 'Foreword', Clarke, David & Roberts, Andy. *Phantoms of the Sky: UFOs – A Modern Myth?*, Robert Hale, London.

Ritter, Stephen K. (2017). 'Cold Fusion Died 25 Years Ago, but the Research Lives On', *Chemical And Engineering News*, http://cen.acs.org/articles/94/i44/Cold-fusion-died-25-years.html, [Accessed 06/09/2017].

Rogo, D. Scott. (1974). *Apparitions, Hauntings, and Poltergeists*, G. P. Putnam's Sons, New York.

Roll, William G. (1972). *The Poltergeist*, Paraview, New York.

Rosenberg, Alex. (2005). *Philosophy of Science: A Contemporary Introduction* (2nd Edt.), Routledge, New York.

Rothbart, Daniel. (1990). 'Demarcating Genuine Science from Pseudoscience', Grim, Patrick (Ed.). *Philosophy of Science and the Occult*, (2nd Edt.), State University of New York Press, Albany, New York.

Rousseau, Denis L. (1992). 'Case Studies in Pathological Science', *Sigma Xi, The Scientific Research Society*, Vol. 80, No. 1.

Ruse, Michael. (1986). 'The Academic as Expert Witness', *Science, Technology, and Human Values*, Vol. 11, No. 2.

Ruse, Michael. (2001). 'Methodological Naturalism Under Attack', Pennock, Robert T. (Ed.). *Intelligent Design Creationism and Its Critics*, MIT Press, Cambridge.

Ruse, Michael. (2009a). 'Pro Judice', Pennock, Robert T. & Ruse, Michael (Eds.). *But is it Science? The Philosophical Questions in the Creation/Evolution Controversy*, Prometheus Books, Amherst, New York.

Ruse, Michael. (2009b). 'Witness Testimony Sheet: McLean V. Arkansas Board of Education', Pennock, Robert T. & Ruse, Michael. *But is it Science? The Philosophical Questions in the Creation/Evolution Controversy*, Prometheus Books, Amherst, New York.

Ruse, Michael. (2013). 'Evolution: From Pseudoscience to Popular Science, from Popular Science to Professional Science', Pigliucci, Massimo & Boudry, Maarten (Eds.). *Philosophy of Pseudoscience: Reconsidering the Demarcation Problem*, University of Chicago Press, Chicago.

Rutledge, Harley D. (1981) *Project Identification: The First Scientific Field Study of UFO Phenomena*, Prentice-Hall, Englewood Cliffs, New Jersey.

Sagan, Carl. (1996). *The Demon-Haunted World*, Ballantine Books, New York.

Salisbury, Frank B. (1974). *The Utah UFO Display: A Biologist's Report*, Devin-Adair Company, Greenwich, Connecticut.

Sarchie, Ralph & Cool, Lisa Collier. (1999). *Deliver Us From Evil*, St. Martin's Press, New York.

Shea, Brendan. (2017). 'Karl Popper: Philosophy of Science', *The Internet Encyclopedia of Philosophy*, http://www.iep.utm.edu/pop-sci/#H3 [Accessed 03/12/17]

Sheaffer, Robert. (2004). 'The Campeche, Mexico 'Infrared UFO' Video', *Skeptical Inquirer*, Vol. 28, No. 5.

Sheaffer, Robert. (2015). *Bad UFOs: Critical Thinking about UFO Claims*, Createspace Independent Publishing Platform.

Shermer, Michael. (2011). 'Demographics of Belief', *Skepticblog*. http://www.skepticblog.org/2011/05/31/demographics-of-belief/ [Accessed 20/09/17].

Shermer, Michael. (2013). 'Science and Pseudoscience: The Difference in Practice and the Difference It Makes', Pigliucci, Massimo & Boudry, Maarten. (Eds.). *Philosophy of Pseudoscience: Reconsidering the Demarcation Problem*, University of Chicago Press, Chicago.

Singh, Simon & Ernst, Edzard. (2008). *Trick or Treatment: The Undeniable Facts About Alternative Medicine*, W. W. Norton and Company, New York.

Smeeth, Liam; Cook, Claire; Frombonne, Eric; Heavey, Lisa; Rodrigues, Laura C.; Smith, Peter G.; Hall, Andrew J. (2004). 'MMR vaccination and pervasive developmental disorders: a case-control study', *The Lancet*, Vol. 364.

Still, Arthur & Dryden, Windy. (2004). 'The Social Psychology of 'Pseudoscience': A Brief History', *The Theory of Social Behavior*, Vol. 34, No. 3.

Storr, Will. (2006). *Will Storr vs. the Supernatural: One Man's Search for the Truth about Ghosts*, Ebury Press, London.

Sturrock, Peter A. (1999). *The UFO Enigma: A New Review of the Physical Evidence*, Warner Books, New York.

Sturrock, Peter A. (2009). *A Tale of Two Sciences: Memoirs of a Dissident Scientist*, Exoscience, Palo Alto.

Susser, Mervyn. (1986). 'The Logic of Sir Karl Popper and the Practice of Epidemiology', *Journal of Epidemiology*, Vol. 124, No. 5.
Swords, Michael & Powell, Robert. (2012). *UFOs and Government: A Historical Inquiry*, Anomalist Books, San Antonio.

Taylor, Brent; Miller, Elizabeth; Lingam, Raghu; Andrews, Nick; Simmons, Andrea; Stowe, Julia. (2002). 'Measles, mumps, and rubella vaccination and bowel problems or developmental regression in children with autism: population study', *BMJ*, Vol. 324.

Thagard, Paul. (1988). *Computational Philosophy of Science*, MIT Press, Cambridge, Massachusetts.

Thurs, Daniel P. & Numbers, Ronald L. (2013). 'Science, Pseudoscience, and Science Falsely So-Called', Pigliucci, Massimo & Boudry, Maarten (Eds.). *Philosophy of Pseudoscience: Reconsidering the Demarcation Problem*, University of Chicago Press, Chicago.

Turro, Nicholas J. (1999) 'Toward a General Theory of Pathological Science', *21stC*, Vol. 3, No. 4 ('*Strange Science*'). http://www.columbia.edu/cu/21stC/issue-3.4/turro.html [Accessed 07/09/17].

Unger, Merrill F. (1952). *Biblical Demonology*, Van Kampen Press, Wheaton, Illinois.

Vallee, Jacques & Vallee, Janine. (1966). *Challenge to Science: The UFO Enigma*, Henry Regnery Company, Chicago.

Vallee, Jacques. (1965). *Anatomy of a Phenomenon*, Ace Books, New York.

Vallee, Jacques. (1975). *The Invisible College: What a Group of Scientists Has Discovered About UFO Influences on the Human Race*, EP Dutton, New York.

Van Rillaer, Jacques. (1991). 'Strategies of Dissimulation in the Pseudosciences', *New Ideas in Psychology*, Vol. 9, No. 2.

Webster's Third New International Dictionary of the English Language, (1986). Encyclopædia Britannica Inc., Chicago.

Westrum, Ron. (2000). 'Limited Access: Six Natural Scientists and the UFO Phenomenon', Jacobs, David M. (Ed.) (2000). '*UFOs and Abductions: Challenging the Borders of Knowledge*', University Press of Kansas, Lawrence.

Wilkins, Harold T. (1954). *Flying Saucers on the Attack*, Ace Books, New York.

Williams, Dr. William F. (Ed.), (2000) *Encyclopedia of Pseudoscience*, Facts on File Inc., New York.

Wilson, Clifford. (1974). *UFOs and their Mission Impossible*, Signet Books, New York.

Wilson, Fred. (2000). *The Logic and Methodology of Science and Pseudoscience*, Canadian Scholars Press, Toronto.

Wynn, Charles M. & Wiggins, Arthur W. (2001). *Quantum Leaps in the Wrong Direction: Where Real Science Ends and Pseudoscience Begins*, Joseph Henry Press, Washington.