Reexamining the Problem of Demarcating Science and Pseudoscience

By

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Abstract

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The demarcation problem aims to articulate the boundary between science and pseudoscience. Solutions to the problem have been notably raised by the logical positivists (verificationism), Karl Popper (falsificationism), and Imre Lakatos (methodology of research programmes). Due, largely, to the conclusions drawn by Larry Laudan, in a pivotal 1981 paper which dismissed the problem of demarcation as a “pseudo-problem”, the issue was brushed aside for years. Recently, however, there has been a revival of attempts to reexamine the demarcation problem and synthesize new solutions. My aim is to survey two of the contemporary attempts and to assess these approaches over and against the broader historical trajectory of the demarcation problem. These are the efforts of Nicholas Maxwell (aim-oriented empiricism), and Paul Hoyningen-Huene (systematicity). I suggest that the main virtue of the new attempts is that they promote a self-reflexive character within the sciences. A modern demarcation criterion should be sensitive towards the dynamic character of the sciences. Using, as an example, a case study of Traditional Chinese Medicine, I also suggest that the potential for conflict between demarcation conclusions and the empirical success of a pseudoscientific discipline is problematic. I question whether it is sensible to reject, as pseudoscientific, a discipline which seems to display empirical success in cases where the rival paradigm, contemporary western medicine, is not successful. Ultimately, I argue that there are both good theoretical and good pragmatic grounds to support further investigation into a demarcation criterion and that Laudan’s dismissal of the problem was premature.

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1.1 Introducing the Problem of Demarcation

In the essay “Mysticism and Logic” Bertrand Russell says that “the highest eminence that it is possible to achieve in the world of thought” is “in such a nature [that] we see the true union of the mystic and the man of science.”[1] The distinction between the mystic and the scientist is, to say the least, profound—so profound, in fact, that a union of the two seems quite far-fetched. The vision that Russell has of the man of science is one who seeks out, as far as possible, objective facts about the world. For Russell, science comes closer to objectivity than any other human pursuit and gives us, “therefore, the closest constant and the most intimate relationship with the outer world that it is possible to achieve.”[2] Russell praises the wonder and curiosity of the mystic, but denounces mystical, *pre-scientific* conclusions about the world as a lower form of thinking which deals simply in imagination and belief. Most scientists nowadays also hold that the inquisitive activity of the mystic is unlikely to provide anything like the type of knowledge that is furnished through the methods of science.

The distinction between scientific knowledge and pseudo-scientific knowledge will be the theme of this thesis. “*Pseudo”* essentially means false or fraudulent. Thus “*pseudoscience”* literally refers to *false science.* To preempt unnecessary confusion, I will point out right now that there is a difference between pseudoscience and *non-science*. “Pseudoscience” refers to activities or disciplines which proclaim to be in the same business as science, but, in fact, are not. Non-science, on the other hand, makes no claims to be providing scientific knowledge. This thesis may, at relevant times, allude to the distinction between science and non-science, but the

[1] Russell (1929) p.6
[2] Ibid.

primary demarcation of concern will be between science and pseudoscience. This demarcation is foundational for several important questions. Take, for instance, questions like: what is science? What sorts of methods are scientific? What sort of knowledge is scientific? What disciplines are scientific? These are all questions whose answers hinge on the above distinction. These questions are important because of the overwhelming influence of scientific knowledge in both our understanding of the world and in a wide array of practical matters. To take just a few examples, we rely upon scientific knowledge: when making decisions in healthcare; for expert testimony in the court of law; as justification for environmental policies; and in scientific education itself. It is plain to most rational persons that we would not want expert testimony from someone whose scientific expertise consisted in the interpretation of fortune cookies. If there is no clear way of demarcating science from pseudoscience, however, there seems to be no justification for privileging the knowledge of the chemist over the knowledge of the fortune cookie interpreter. Although we may intuitively feel that the chemist has more credibility than the fortune cookie interpreter, there remains disagreement among scholars as to what discernible features of scientific practice these intuitions point to. Through the course of this thesis, I intend to explore the possibility of philosophically articulating these intuitions.

To ask a question about the defining features of scientific practice is also to ask a negative question, namely, what is lacking in the judgments, methodologies, and disciplines which *aren't scientific*? So, while becoming clearer about the virtues that warrant the title of science, we are, at the same time, equally engaged with articulating the vices of pseudoscience. Martin Mahner has pointed out that studying the demarcation problem is to the philosophy of science what the study of fallacies is to logic.[3] The fact of the matter is that demarcating science

[3] Mahner (2007) p.516

is intrinsically normative. This is to say that whatever doesn't 'make the cut' in the science try-outs has much less, and perhaps no, epistemic authority relative to those practices which 'make the team'. It is not the intention of this thesis to engage with the normative features of the demarcation problem, although this normativity plays a foundational role in motivating this work. It is just simply a reality of the modern world that scientific findings, methods, and hypotheses hold more credibility than their pseudoscientific or non-scientific counterparts. This credibility, however, presupposes a demarcation criterion. That an implicit normativity in the distinction between science and pseudoscience can even exist suggests that there *are certain features* of science which are can distinguish science from pseudoscience, and in such a way that they confer epistemic authority.

What exactly, this criterion is, or criteria are, has been widely debated for the past century. Endeavors to demarcate science from pseudoscience extend far before the past century, however, and can be traced all the way back to Aristotle's *Posterior Analytics*. This debate, if anything, has shown that the criterion is neither self-evident nor obvious. We might tentatively assume that there is a connection between the validity of normative judgments about the authority of scientific knowledge and the validity of the underlying demarcation criterion, whether or not that criterion is explicit. For example, one might reject the findings of an astrologer on the basis that his or her methods are pseudoscientific. To go further even, one might write off the entire discipline of astrology on this same basis. This is not an uncommon example. If it can be shown, however, that the criterion used to demarcate astrology as a pseudoscience is problematic, or even false, it would be safe to assume that the normative judgments about astrology's lack of epistemic authority are correspondingly problematic, or false. This work will attempt to avoid the normative questions by virtue of digging down to this ground and focusing, as purely as possible, upon the distinguishing features of science.

This work will unfold through four chapters. This first chapter will introduce the problem of demarcation and bring to light the method this thesis will utilize in the exploration of the problem and the subsequent elaboration of a possible solution. The method of the thesis will involve investigation into both historical and contemporary solutions to the problem. Although I will provide a brief version of this overview in this chapter, the second chapter will be devoted to a survey of three of the main theories of demarcation which arose in the early to mid-twentieth century. Originating mainly in the early twentieth century with Logical Positivists such as Rudolf Carnap and subsequently reshaped and developed by such thinkers as Popper and Lakatos, the demarcation problem received a considerable amount of intellectual focus. Each espoused a different criterion for demarcation. The positivists brought forth verificationism, which has been said to hold that statements were only meaningful insofar as the statements could be verified. The scientific enterprise, then, represented those statements which were "meaningful", and thus, verifiable. This view of verificationism, however, is an oversimplification and, one might say, a caricature of positivist thought. This point will be elaborated in the second chapter.

The second figure we will examine is Karl Popper, who is arguably the most pivotal figure in questions about demarcation in the twentieth century. Popper, motivated largely by the problem of induction, held a view commonly termed "falsificationism", which suggested that only bold, falsifiable theories held scientific weight.[4] The limitations of the human perspective prohibit us from formulating deductive proofs about the entirety of nature, since we would have

[4] Popper (1962)

to assume the uniformity of nature as a logical premise. Popper, abandoning scientific proof and verification as impossible, suggested instead that we could conclusively *disprove* theories through evidence which runs counter to the theory's hypotheses. Thus a theory's informative content, and its degree of vulnerability to falsification, became a virtue of scientific theories, whereas theories which were, in principle, unfalsifiable—and here Popper had in mind theories such as Freudian psychology—had little, if any, scientific merit.

The above view, or, *"Bare falsificationism"*, as Nicholas Maxwell has called it,[5] is the most widely cited Popperian approach to the question of demarcation. Popper did revise his views, however, and suggested a requirement that, in addition to the falsifiability criterion, scientific theories should adhere to a principle of simplicity. Simplicity here refers to the ability of the theory to fit into a more general picture that arises out of the conjunction of other, accepted scientific theories. Maxwell calls the addition of this requirement *dressed falsificationism* and sees it as Popper's tactic to overcome some of the criticisms which he met with bare falsificationism. In the second chapter we will deal with Popper's formulations in detail.

The third historical figure we will examine in our survey is Imre Lakatos. Lakatos held that counterevidence was, more often than not, insufficient for disproving scientific theories. The main reason for this is that it is never entirely clear when an observed phenomena is, in fact, a falsifying case. This is largely because theories do not function as isolated entities which can be assessed on their own accord. Theories, as Lakatos pointed out, are components of larger research programmes. If there is a problem with a given theory, such as falsifying evidence, it is easily possible that there are adjustments that need to be made to auxiliary hypotheses which are

[5] Maxwell (2005)

implicit in the experiment yielding the evidence. Therefore, the falsifying evidence could potentially be saying nothing at all about the theory in question. Rather, the counterevidence could often be pointing beyond the theory to some assumption within the larger research programme of which the theory is a part. If we are to assess whether or not research programmes are scientific then, according to Lakatos, we need to see whether these programmes are successful in their predictions of new phenomena, as opposed to being "degenerating" research programmes which devise theories that do nothing more than accommodate previously known facts.[6]

The demarcation problem is a wide problem and there are many other thinkers that would we would do well examining in order to further substantiate a broad, historical survey. In a certain respect, every philosopher or scientist who has ever drawn a conclusion about the nature of science, either in its methods, assumptions, or conclusions has weighed in on the problem of demarcation. Whether or not these individuals have explicitly stated it, by adopting the notion of 'science' and positively characterizing it, they are, at the same time, affirming that there are pseudoscientific or non-scientific disciplines which are scientifically lacking. This thesis will only afford me the time to examine the programs of Carnap, Popper, and Lakatos. As far as the other thinkers are concerned, I will make reference to them in the name of strengthening my analysis, but will beg the reader's forgiveness for not giving them their due. For instance, Thomas Kuhn is a major figure in meta-scientific analysis. In his *The Structure of Scientific Revolutions*, Kuhn laid out a characterization of science which has been immensely influential for all philosophers of science, including each of the modern day scholars concerned with demarcation that we will examine. Also of note is Paul Feyerbend whose radical vision of the

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[6] Lakatos (1978)

scientific enterprise raises serious questions for the question of demarcation.[7] I could name others, but I forbear in the interest of brevity.

Although there are flaws within the solutions provided by these thinkers, their approaches remain foundational for the demarcation problem. As is often the case, the flaws in no way discredit the immense virtues of the historical approaches. In the demarcation problem, the flawed solutions invite intellectual efforts to restructure and synthesize a new solution which partakes in the virtues of historical approaches while remaining cautious concerning their shortcomings. Thus the third chapter of this thesis will be concerned with articulating the state of the demarcation problem in the last couple of decades. As we will see in the third chapter, the contemporary approaches to demarcation demonstrate a close familiarity with all of the philosophers above. It is, therefore, essential to expand on these historical positions so as to get a sense for the ground upon which the demarcation problem stands today.

The contemporary analysis will focus mainly upon the approaches of two thinkers: Nicholas Maxwell and Paul Hoyningen-Huene. Maxwell posits an approach that he calls *aim-orineted empiricism*.[8] Maxwell takes very seriously the shortcomings of the theories of Popper, Lakatos, and Kuhn and seeks to synthesize, from the positive aspects of their work, a novel approach to characterizing science. Maxwell is one of the scholars I had in mind when I suggested that the question of demarcation is often stated *implicitly*. Maxwell's project is to formulate a positive conception of science. In his aim-oriented empiricism, Maxwell asserts that a great virtue of science is its openness to criticism, or its reflexivity. Any science, or theory of science, for Maxwell, needs to be as reflexive as possible. Reflexivity involves being

[7] Feyerbend (1975)
[8] Maxwell (2005)

philosophically self-aware in the sense that any ingrained metaphysical assumptions are explicitly stated so as to be, themselves, open to criticism. Modern physics, to take an example, operates largely within the aim of finding a unified, physical theory. In order to operate in this way, Maxwell suggests, there needs to be an undergirding, metaphysical assumption that nature is structured in a unified, knowable way. It is not inconceivable that this metaphysical assumption is false. In fact, the falsity of this assumption would entail, in many cases, different approaches and goals for scientific practice. It is thus essential that this self-reflexivity take place as deeply as possible so science does not slip into dogmatic practice.

The contribution to the demarcation question in Maxwell's view can be found by merely reversing the positive thesis. Science is largely characterized by an empirical approach which is fundamentally self-reflexive and always able to be dynamic in its assumptions. Pseudoscience, then, would be prone to dogmatic assumptions about the way nature is and would be unwavering in the face of evidence which suggests the falsity of these assumptions. As we will see in detail later on, Maxwell's fusion of earlier approaches provides a rich perspective of science and, thus, fresh insight into demarcation.

The second contemporary theory we will look at is Paul Hoyningen-Huene's. Hoyningen-Huene demarcates science on the basis of *systematicity.*[9] This theory is motivated by a significant obstacle to demarcation, namely, the sheer diversity of the sciences. What we tend to count as science ranges from the "pure" sciences (physics, chemistry) to sciences which are "less pure" (biology, ecology) to the "soft" sciences, or the human sciences (psychology, sociology, anthropology). Depending on the demarcation criterion we may or may not count the soft sciences as being *actually scientific*, but there is less controversy about labeling biology as a

[9] Hoyningen-Huene (2007)

true science.[10] The problem here is that the respective methods of physics, chemistry, and biology are vastly different, and certain methods that exhibit empirical success in one field may be largely unsuccessful in another. This raises a fundamental concern about universality in demarcation. Hoyningen-Huene suggests that this diversity is simply a fact about modern science. He is also committed to the notion that demarcation is a viable and important question at this point in history. These two positions suggest to him that demarcation needs to be addressed in spite of the potential impossibility of universality.

Hoyningen-Huene's solution is to outline a number of dimensions that the varying sciences operate within. These dimensions are all connected in the sense that they all tend toward a fundamental goal of systematicity, and do so to a greater degree than everyday reasoning. To take a couple of examples, some scientists may be in the business of increasing the sytematicity of description. This general dimension of scientific practice might include the refining of equipment for an increased accuracy in measurement. Another dimension is the systematicity of explanation. This might be achieved through a refinement of theories, either through an expansion of the theory's scope or through a more detailed and focused exclusion of aspects of competing theories.[11] Implicit in Hoyningen-Huene's theory of systematicity is the sort of self-reflexivity and flexibility that is so important for Maxwell. There is also no foreseeable end to the tendency toward systematicity. Scientific practice is always tending towards its own refinement. A result which seems to arise from perennial systematization is an increase in the diversification and disunity of the sciences since refinements in a given field's descriptions, instrumentation, and explanations tend to have the effect of segmenting the said

[10] For an interesting perspective into the approaches of social sciences see Foss (2012)

[11] Michael Friedman has drawn similar conclusions and has sought to provide an account of scientific rationality that is non-relativistic, despite the sheer diversity of the sciences. See (2001) and (2003)

field and causing it to become more specialized. Pseudosciences, on the other hand, are not dynamic in this way. Hoyningen-Huene suggests that they tend to stagnate. Pseudosciences also tend to lack "autonomous development of self-critical tests of the basic assumptions of the field." Furthermore, pseudoscience tends to be, more often than not, defensive. Rather than providing positive contributions to knowledge, or even attempting to provide positive contributions, these disciplines usually are concerned with defending their tenets in the face of other positive contributions to knowledge.

The fourth and concluding chapter of this work will focus upon one particular feature of scientific endeavors: empirical success. Using Traditional Chinese Medicine as a sort of case study, it will be asked whether or not a discipline which, according to most schools of thought, fails the demarcation test, can still attain scientific status based purely upon its empirical success. Science, in the modern world, is as much practical as it is theoretical. Science is meant to be superior to mere opinion in the sense that it is a more reliable epistemic source for articulating causal relationships. Aside from physics, however, scientific disciplines do not deal in the fundamental causes of all phenomena. Each science restricts itself to its respective domain when formulating theoretical models. Each scientific domain, beyond the formation of theoretical models, puts these models to use in practical applications. The case could be made that it is in developments ranging from the telescope, automobile, and light bulb, to power plants, particle accelerators, and pharmaceutical development, that the value and success of science is perhaps most tangible. Empirical success makes a strong impression, whether it is comes in the form of developing a new technology of overcoming an obstacle that a particular science is faced with. What Traditional Chinese Medicine is lacking, according to many, is a scientifically robust, theoretical foundation. The discipline makes for an interesting case study, however, because it has a lot of empirical success. Perhaps more interestingly, much of this empirical success appears in cases where the prevailing, scientific counterpart, contemporary western medicine, has little to no success. This success comes mainly in the treatment of chronic conditions. The medications in the western medical paradigm often come with the potential for severe side-effects and, as chronic conditions needed to be treated for an extended period of time (sometimes indefinitely) the drugs prescribed to alleviate symptoms are, correspondingly, taken for extended periods of time. As Traditional Chinese Medicine uses therapies and treatments which are more holistic and often have a much lesser degree of side-effects, they are being shown in recent studies as being efficacious and preferable to western treatments in the treatment of some chronic conditions. This is a challenging case for demarcationists because it seems to ask that the dynamic between theory and practice receive more attention. How much sense does it make to reject a discipline if it exhibits consistent empirical success, despite the view that its theoretical foundation is not robust?

The problem of demarcation, after a period of neglect spanning a few decades or so, has come back into vogue in recent years, but the question still lingers as to whether we are any closer to a solution than we were in Popper's day. The rekindling of the demarcation flame suggests that the question is a significant one. Indeed, a demarcation criterion is of great importance to the practical functioning of our modern-day society, but it seems to have a deeper significance. It is deeper because it brings us back in touch with the foundational questions of epistemology. By examining the distinction between science and its counterpart we are, at the same time, examining what makes a conclusion worthy of our rational belief and what practices or disciplines are best suited to reveal these conclusions. Through a critique of the modern demarcation approaches, I will show that, although we are a certain distance from the perfect formulation of demarcation, contrary to the views of some philosophers, progress has been and ought to be made on the question.

The main skeptic about demarcation I will consider is Larry Laudan. Laudan, in a pivotal paper written in 1983, proclaimed the question of demarcation to be, itself, a *pseudo-question.*[12] In Laudan's view, "we ought to drop terms like 'pseudo-science' and 'unscientific' from our vocabulary; they are just hollow phrases which do only emotive work for us."[13] The main impetus for Laudan's attempted disintegration of the demarcation question is a lack of agreement among scholars as to what a demarcation criterion would look like. For Laudan, when we are asking for a concrete distinction between science and non-science, we are actually asking what makes a belief well-founded. The demarcation problem conflates these two questions. Laudan holds that the latter question is interesting and worthy of pursuit while the former is both uninteresting and, according to Laudan's own historical analysis, untenable.

According to Laudan's analysis, scientific explanation can be traced back to Aristotle. Essentially, scientific knowledge is characterized as justified, demonstrable knowledge (episteme) as opposed to mere superstition or opinion (doxa). This distinction was significant for philosophers before Aristotle, but its characterization in terms of science was formalized in the *Posterior Analytics.* Scientific knowledge is characterized as theoria, or the "know-why," as opposed to praxis, or the "know-how". Rather than having the practical knowledge of how to manipulate things in the world, like that possessed by a shipbuilder or other craftsmen, scientific knowledge looks beyond the "how" to the "why". Knowing why is knowing the underlying causes and mechanisms which undergird the phenomena itself. By investigating the causal

[12] Laudan (1983)
[13] Ibid. p.125

structure, the scientist can demonstrate, with relative certainty, the “why” of the phenomena. The shipbuilder’s success does not require that he know the molecular structure of water, or other related scientific principles. This way of looking at it leads to a conception of belief as being tied to the practice of the craftsmen and knowledge being the domain of the scientist.

As both scientists and philosophers of science should realize, Aristotle’s view is, to the say the least, an ideal which is distant from the actual practice of science itself. Scientific practice often, if not always, goes the opposite way. This is to say that scientists begin in the same fashion as craftsmen. They manipulate phenomena in their experiments in order to gather data which they can then use to infer a deeper causal structure. The first principles which found the causal explanations given by Aristotle’s scientist are not readily available in contemporary science. In fact, the deeper science looks beneath the phenomena, the more murky and complex reality becomes. Scientists still put much of their efforts into discovering and articulating principles which found and unify the most basic known physical principles, but there is considerable disagreement among philosophers as to the feasibility of unification.[14] It is important here to point out that, despite Aristotle’s demarcation, science operates largely in the domain of “know-how”. As Laudan suggests, science in practice looks hardly any different from any other “know-how” activity and thus, under the original demarcation model, fails to graduate beyond mere belief.

Into the seventeenth century, when science first began to accelerate, thinkers still held onto Aristotle’s view that science offers certain knowledge of the world, but largely dispensed with the second of aspect of Aristotle’s thought, that is, that science consists in understanding

[14] See Fodor (1975) and Kuhn (1962) for arguments against reductionism and the unification of science. Positions in favor of unification can be found in Oppenheim & Putnam (1958) and Kitcher (1989).

and not know-how. In Laudan's account, thinkers such as Galileo and Newton were confident that their activity was scientific even though they were not proceeding from underlying causal explanations. "Galileo claimed to know little or nothing about the underlying causes responsible for the free fall of bodies, and in his own science of kinematics he steadfastly refused to speculate about such matters. But Galileo believed that he could still sustain his claim to be developing a 'science of motion' because the results he reached were, so he claimed, infallible and demonstrative."[15] In broadening the scientific enterprise from causal demonstration based upon primary causes to craftsmen-like experimentation, but while still retaining the assumption that science is in the business of providing true knowledge and not ungrounded belief, demarcation shifted focus towards method. The question then became about how the know-how practice of the scientist provides scientific knowledge whereas the know-how practice of the shipbuilder doesn't.

A concern with scientific methodology becomes even more significant when the first of Aristotle's scientific characterizations is put into question. Galileo and Newton, though no longer beginning from first principles, came to conclusions which were, in their minds, infallible. It was the certainty of the conclusions which made them truly scientific. David Hume, however, questioned the certainty of scientific conclusion. Hume's attacks on scientific certainty arose out of what he rightly perceived to be a logical error on the part of scientists. Science was positing conclusions that could only be certain if they were reached through a deductively sound argument. The problem is that it is literally impossible for anyone to provide a deductively sound argument for a law of nature. The reason for this is that a scientist, or group of scientists, necessarily makes their observations or conducts their experiments within a finite slice of nature.

[15] Ibid. p.114

The phenomena they observe are always particular and never universal. In order to deductively draw a conclusion which holds for all of nature, which universal laws purport to do, nature's uniformity needs to be posited as a premise which connects the particular instances to universal laws. As Hume pointed out, nature's uniformity cannot be proven and, thus, cannot function as a veridical premise in a deductive scientific argument. He most famously used this problem to demonstrate that we cannot even be certain that the sun will rise tomorrow, since the only supporting evidence we have for the phenomena is our previous observations. Since that is all we can ever have, empirical deduction is just not possible. Science, then, is a necessarily *inductive enterprise*. Some conclusions are better supported than others, but no quantity, no matter how great, of empirical observations can bring a scientist to certainty.

The rejection of both of the classic, Aristotelian aspects of science, theoria and necessity, backs the demarcation question into a corner. All that seems to be left in terms of demarcating science from non-science is methodology. But, Laudan states, there is as little consensus in what constitutes a *truly* scientific methodology as any other feature of the demarcation issue. In order to show what makes a method scientific, two points need to be demonstrated. Firstly, the method must span the entire range of disciplines which are deemed to be scientific. This requirement is called the *unity of method* requirement. If we are to distinguish science based upon its distinctive methodology, yet some of the disciplines which we take to be scientific operate on a significantly different method from other sciences, then the efforts to demarcate based upon that methodology have failed. The second point to be demonstrated is that the "epistemic credentials" of the scientific method need to be clearly established.[16] This

[16] Ibid.

requirement is obvious since the motivation behind demarcating science is so we can quarantine our justified knowledge (whether inductive or not) from belief.

The nineteenth century bore many attempts at articulating *the* scientific method. Some of these attempts were based on the “cannons of inductive reasoning”.[17] Others held that scientific methodology is unique in its ability to make successful predictions.[18] Still others maintained that the method needs to restrict its domain to observable entities. In addition to these, there arose many rules that were characteristic of a scientific method. These rules prohibited practices such as the postulation of *ad hoc hypotheses,* or *complex theories*, or *theoretical entities.* According to Laudan, these rules were laid down in a rough and ready sort of way, without any serious philosophical analysis. The result was that the rules turn out to be ambiguous. Looking back on the past century in the philosophy of science and the amount of discussion about the theoretical/observable distinction supports Laudan’s claim here and affirms that it is no clear matter.[19] To compound the problems associated with a series of ambiguous methodological rules, the diversity of approaches by various philosophers and scientists lends support to the idea that there was really no agreement about what the scientific method was. If the unity of method requirement is to be taken seriously, than this lack of agreement is not acceptable as is. Unfortunately, according Laudan, the sought-for agreement was not reached—and worse, cannot be.

Laudan then turns his attention to the prolific demarcation attempts of the twentieth century, beginning with the verificationists. The aim, according to Laudan, of the verificationist doctrine was to show that the verifiability, scientific legitimacy, and the meaningfulness of a

[17] See Herschel (1831)
[18] See Whewell (1840)
[19] For a good overview of the observable/theoretical distinction see Klee (1999)

statement were all connected. The main impetus for this doctrine was to clean our intellectual house, so to speak, and rid our thinking of approaches like speculative metaphysics which, the positivists held, could never be verified. *Prima facie*, it seems to be a reasonable approach. If our goal as intellectuals is to apprehend the truth and our aim as human beings is to live in accordance with that truth, there is something a bit irrational about committing ourselves to ideas which we cannot prove to be true. Marxist history, for instance, is an idea which had and still, albeit to a much lesser extent, continues to have a profound effect on political policy and, thus, the lives of millions of human beings. It states that human history has a direction and a telos which will inevitably be realized and it is the duty of intellectuals to see how this is the case and to bring political policy and human life into accord with this end, for otherwise revolutions will necessarily occur. History will realize itself at any cost. From a verificationist perspective, this conclusion is largely meaningless since it is largely unverifiable. There are no experiments we can conduct which would begin to verify this conclusion. The teleology of history is inferred from a certain interpretation of historical events. It is easy to imagine, however, that one can interpret history in a completely different way. One could conceivably posit that, say, capitalism or anarchism is the true end of history's trajectory. There doesn't seem to be an objective way of testing or verifying which interpretation is the correct one, however. To the verificationists then, this debate is both interpretive and speculative and is neither scientific nor meaningful. For the verificationists, we should focus our attention upon concrete, observable phenomena and only take for truth what the evidence tells us. Although it is widely accepted that verificationism will not do as a demarcation criterion, the case can be made that Laudan's hasty rejection of verificationism lacks sensitivity to the details of the approach, particularly as articulated by Carnap.

If one adopts the naïve view of verificationism, it is clear that the approach has many insurmountable problems. In a certain respect, the verificationist program brings us full circle to Hume's objections, two centuries before. Verificationism asks us to look to the observable phenomena so that it can provide the grounds for a true conclusion about the world. Verified statements about reality are taken as true premises which, through Modus Ponens, provide a sound argument for scientific conclusions.[20] Hume's objection surfaces again here in striking clarity. Verificationism seeks deductive proofs for scientific conclusions, but the problem is, once again, that we only ever observe verifying *instances*. Without a premise which generalizes these instances or a premise which asserts the uniformity of nature, any conclusion about the *way nature is* cannot be deductively warranted and can only state something as strong as *the way nature seems to be.* This is a problem for a verificationist because if the best we can say is that nature *seems* to be a certain way, it is certainly conceivable and *not contradictory* that nature could also be *a different way*. This leads to the criticism that it is only possible to *partially* verify a scientific claim, for we can only ever deal with a finite slice of the universe. In returning to the realm of induction, interpretation and speculation begin to seep back into the picture: the very things verificationists sought to expel from their theory of meaning in the first place. The above criticism, however, is recognized by Carnap, in "Testability and Meaning[21]" and, rather than exhaustive verification, it is degrees of confirmation that become important for him.

Laudan was also dissatisfied with Popper's falsificationism. Popper was well aware of the shortcomings of verificationism and was thus motivated to provide a stronger demarcation criterion. The inability of empirical theories to draw deductively sound conclusions about nature

[20] The deductive spirit of the Positivist program was taken even further in the philosophy of science by thinkers such as Carl Hempel who, within this spirit, found the basis for his deductive-nomological model of explanation.
[21] Carnap (1936)

may be a concern for verificationists, but it is not so for a falsificationist. Falsificationism is an approach which gauges the scientific merit of theories on the degree of their *refutability*. Rather than attempting to establish scientific facts with certainty, science ought to be in the business of weeding out refuted theories and holding onto those theories which hold up under rigorous testing. A sound conclusion cannot be drawn empirically using modus ponens, but one *can be drawn using modus tollens*. If a theory posits certain states of affairs which are *prohibited,* and these prohibited states of affairs do occur and are observed, the theory cannot be true. The form is as follows: *if* **a** is the case (**a** being the theory under scrutiny) *then* **b** will be observed (**b** being the specific state of affairs entailed by the theory); **not b**; *therefore,* **not a**.

Popper's approach was motivated neither by the meaningfulness or significance of scientific claims nor by the truth or acceptability of them, but by, what he saw their scientific merit to consist in.[22] As we can see above, falsificationism is not in the business of affirming theories to be true. Popper, in seeing that establishing the truth of scientific laws ought not to be the focus, suggests that through falsificationism we have rational grounds for believing a theory. According to Popper, scientific merit is demonstrated by bold and risky assertions. A theory is *more scientific in proportion to the boldness of its assertions*. This boldness is reflected in the number of circumstances that the theory prohibits. By exposing itself to a greater degree of falsifying instances a theory goes beyond a merely pseudoscientific theory.

Testability is not Popper's only condition for demarcation, since all sorts of pseudoscientific and non-scientific propositions are testable. I make several assertions every day that are falsifiable and can be tested, but virtually none of them are scientific by any stretch of the imagination. Consequently, Popper posits another pseudoscientific vice which he calls the

[22] Popper (1963)

conventionalist twist or stratagem. This is an approach used by theories which are testable, but make *ad hoc* adjustments to the theory in order to *fit the falsifying circumstances.* Marxism exemplifies the use of this strategy. Popper holds that it is conceivable that a test could be devised for Marxism. This test might involve predictions about future revolutions which could, in principle, provide grounds for falsification. In fact, Popper suggests that earlier versions of Marxist theory did provide such predictions and they were falsified.[23] That these refutations did nothing to diminish the prevalence of the theory, however, was a clear example, to Popper's mind, of *ad hoc reinterpretation* and not scientific merit. Other theories such as Adler's psychology seem to be even weaker in the sense that no falsifying tests can even be devised and, it seems, every state of affairs can be viewed as a corroborating instance of the theory. Conventional stratagems are indicators that the theory is not quite as bold as Popper would like it to be. A bolder theory would be too robust to continually shift its interpretation. Einstein's relativity is a common example of a theory that Popper found to exemplify this boldness.

One obvious issue with Popper's approach to demarcation is that any theory, no matter how ridiculous or absurd can attain the scientific stamp so long as the theory makes a claim that could, in some logically possible circumstance, be falsified. This objection might be avoided by stating that we can filter out the crank theories because the *real science* is testable to a much higher degree than its radical counterparts. It might be argued that the progress of hard sciences such as physics and chemistry is based upon extraordinary concern with vigorous testing. Laudan raises the point that this *testability criterion* is not one which can be found outside of Popper's theory of demarcation and thus there are no objective grounds for determining whether

[23] Popper (1962)

one theory is more testable than another without arguing in a circle.[24] Furthermore, even if we could conceive of an objective test which could demonstrate a ranking in testability such as, for example, between relativity and astrology, we could not make the leap from *virtue of testability* to *belief-worthiness*. Popper's insistence on separating scientific merit from truth and acceptability is a move which Laudan sees to be away from epistemological concern and towards a quibble over the semantics of terms such as "testability" and "science".

Laudan's brief history of the demarcation problem is furnished with a wealth of criticisms at every step along the way. This approach fuels the fire of Laudan's anti-demarcation in a way that is akin to a sort of pessimistic induction. This is to say, in seeing a lack of successful demarcation attempts throughout history, it is just plain unlikely that a successful approach will arise, so we may as well abandon the problem. The challenges that Laudan provides are of great concern to the contemporary thinkers engaged in the problem and they do not go unnoticed in the modern literature. The climate of the demarcation post-Laudan will, as I have mentioned, be the theme of the third chapter in this work. In addition, the third chapter will engage more deeply with Laudan's criticisms which are both more serious and numerous than mere pessimistic induction. Before that, however, a more substantive analysis of the pre-Laudan, twentieth century demarcation attempts needs to be sketched.

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[24] Laudan is suggesting here that without a substantial, objective account of the degrees of testability, Popper would need to begin with an example of a theory that is testable to a higher degree than other theories. No doubt the choice would be a scientific theory. This, however, would be to assume the very thing that ought to be proved, namely, that scientific theories are testable to a higher degree than other theories.

2

Three Historical Approaches

The ultimate purpose of this work is to suggest the need to revive the problem of demarcation. As I outlined in the introductory chapter, the problem received a fair amount of attention in the early to mid-twentieth century, but was largely abandoned after Laudan's famous dismissal of the problem. In this chapter, I Intend to explicate the two main approaches that Laudan takes issue with in what he calls the "new demarcationist tradition". These are the verificationism program of the logical positivists and falsificationism, which is the approach of Karl Popper. In addition, I will look at the thinking of Imre Lakatos, whose methodology of scientific research programmes succeeds and expands upon Popper's views while also considering the broader, historical program of science.

What I hope to convey in this chapter is that the "new demarcationist tradition" is quite a lot more substantive than what Laudan portrays in his essay. If we are to take Laudan's dismissal of the demarcation problem seriously, the shortcomings on his part to substantively articulate the new demarcationist approaches is problematic. Laudan perceives the new demarcationist approaches to be failures and suggests that persistent failures in the attempt to solve the demarcation problem are grounds to think that the demarcation problem is, itself, a pseudo-problem. In reality, however, it is not clear whether or not the new demarcationist approaches are, indeed, failures, since Laudan seems to draw more of a caricature of these views than an accurate portrayal. Laudan does offer reasons beyond the failure of new demarcationist approaches to support his dismissal and these further reasons will be the subject of the third chapter.

Laudan's misrepresentation is perhaps most evident in the case of verificationism. Laudan says the following about verificationism, "despite its many reformulations during the late 1920's and 1930's verificationism enjoyed mixed fortunes as a theory of meaning. But as a would-be demarcation between the scientific and non-scientific, it was a disaster. Not only are many statements in the sciences not open to exhaustive verification (e.g., all universal laws), but the vast majority of non-scientific and pseudoscientific systems of belief have verifiable constituents."[25] Laudan seems to have in mind an extremely strict notion of verificationism here. While it is certainly true that if exhaustive verification is a requirement for the scientific status of a statement, then all universal or generalized statements would be unscientific, it is not clear that any logical positivist actually held that exhaustive verification as a requirement.

In order to provide a more substantive account of positivist thinking, I will look at Rudolf Carnap's paper, "Testability and Meaning".[26] In order to adequately assess the virtues or lack thereof within positivist thinking for the demarcation problem it is important that we do what Laudan didn't do and provide a less superficial account of that thinking. For Carnap, a theory of knowledge needs to address two problems: "the question of meaning and the question of verification."[27] According to empiricism, Carnap suggests, the questions have essentially the same answer since we come to know what a sentence means through a process of verification or, as Carnap opts for, a process of confirmation. This implies that a sentence that is not confirmable, at least under some possible circumstances, cannot be said to have empirical

[25] Laudan p.120
[26] Carnap (1936)
[27] Ibid. p420

meaning. To convey what Carnap has in mind for his criterion of meaningfulness, this section will address the relations between four key terms. These are observability, confirmability, realizability, and testability. After providing an account of Carnap's project in "Testability and Meaning" we will see how this project fits in with the broader trajectory of the demarcation problem.

Carnap chooses to talk about confirmability over verifiability for the same reason that Laudan rejects verificationism. If the only sentences that are meaningful are verifiable, and by verifiable we mean that the sentences are open to exhaustive verification, we will find that the bulk of the statements of science become meaningless, since any generalized or universal claim is not capable of being exhaustively verified. Well-established sciences use generalized claims and universal laws all the time and thus a requirement as strict as the type of verificationism Laudan has in mind would be obviously disastrous for science. Here Carnap and Laudan agree. This point is also important for Popper's thinking and we will address it in further detail below.

General claims and universal laws cannot be exhaustively verified or, in Carnap's words, completely confirmed because these claims are inferred from a finite number of observations which engage with a miniscule portion of the universe. General claims and universal laws are, however, indispensable for the language of science. Rather than exhaustive verification, which would be impossible, Carnap suggests we use a process of "gradually increasing confirmation". In response to Hume's famous example about our failure to deductively conclude that the sun will rise again tomorrow, Carnap would respond that the laws governing celestial motions have been confirmed time and time again. In observing a relatively high degree of gradual confirmation, we make a practical decision about whether to accept or reject that the sun will rise again tomorrow. The reverse state of affairs, where the laws of celestial motion suddenly change is a possibility, but, according to the evidence available to us, has not been positively confirmed.

While verificationism in the strict sense is not feasible, Carnap holds that the confirmability, or disconfirmability, given how he defines confirmation, of sentences is indispensable and essential for science. For a sentence to be confirmable we have to know what conditions would need to be met for the sentence to be confirmed. To fully understand what Carnap has in mind when he talks about confirmability, we need to first talk about observability. Carnap defines observability in the following way:

> "A predicate 'P' of a language 'L' is called *observable* for an organism (e.g. a person) N, if, for suitable arguments, e.g. 'b', N is able under suitable circumstances to come to a decision with the help of few observations about a full sentence, say 'P(b)', i.e. to a confirmation of either 'P(b)' or '~P(b)' of such a high degree that he will either accept or reject 'P(b)'.[28]

Carnap suggests that the distinction between what counts as observable as opposed to unobservable is itself no certain matter. In drawing a sharp distinction between observable and unobservable predicates, Carnap admits that the distinction will be, to a degree, arbitrary. Our idiosyncratic position as human beings in the universe has provided us with an idiosyncratic lens through which we observe the world. Degrees of observability are continuous and what might count as observable for a different organism would be unobservable for a human being. Even between human beings, the distinction between observable and unobservable is not sharply defined. Carnap illustrates this with the example of the predicate 'red'. Although, to a person

[28] Ibid.

who possesses a normal sense of color, the predicate ‘red’ is observable,[29] to a person who is colorblind, the predicate ‘red’ is unobservable. There is always the potential for refining the observability boundary. In other words, instead of taking a color predicate, such as ‘red’, as a basic observable predicate, we might refine the boundary to take ‘bright’ as more basic, thus accommodating the colorblind person’s point of view. This refinement won’t, of course, help the blind person who relies on senses other than sight to make his observations. Thus, this boundary of observability is to be drawn on the basis of a pragmatic decision, but will be by no means definitive.

Observability is important for Carnap’s definition of confirmability. Carnap states that what it takes for a predicate to be confirmed or for a predicate to be confirmable is for the predicate’s confirmation to be reducible to a “class of observable predicates”.[30] To be confirmable, predicates themselves do not have to be observable, but just have to be reducible to observable predicates. There are many unobservable predicates that Carnap wishes to admit into his empirical language so, for him, it is necessary that we are able to reduce those higher level predicates to the basic, observable predicates.[31] For example, a predicate such as, “an electric field of such and such an amount”[32] is not observable by anyone, but rather, requires the use of instruments to reduce the unobservable predicate to basic, observable predicates, ie. the position of a needle on a gauge, etc. Since the predicate “an electric field of such and such an amount” is reducible to observable predicates, the predicate is confirmable. The sciences constantly make

[29] The predicate ‘red’ counts as observable insofar as it meets Carnap’s criteria of observability: “For a suitable argument, namely a space-time point c sufficiently near to N, say a spot on the table before N, N is able under suitable circumstances—namely, if there is sufficient light at c—to come to a decision about the full sentence “the spot c is red” after few observations—namely by looking at the table.” (T&M p.455)
[30] Carnap p.456
[31] Carnap’s definition of confirmability displays the importance of pragmatically drawing a line that defines what counts as a basic observable. Without agreed upon, basic observables, confirmability couldn’t even begin.
[32] Ibid. p.455

use of predicates which are not, for Carnap, basic observables. If sentences about black holes and event horizons, for example, are to be meaningful there needs to be some way that these concepts can be reduced to basic observations, ie. telescopic observations, etc. This process of reduction is carried out through reduction pairs or test sentences, which are tied to Carnap's notions of realizability and testability.

Just as an idea of observability was essential to understand confirmability, a notion of realizability is essential for understanding testability. In order for a predicate to be testable, the test-conditions need to be realizable. Realizability is a basic term for Carnap which he defines in the following way.

> "A predicate 'P' of a language is called 'realizable' by N, if for a suitable argument, e.g. 'b', N is able under suitable circumstances to make the full sentence 'P(b)' true, i.e. to produce the property P at the point b."[33]

For example, P(b) could mean "the space-time point b has a temperature of 100 degrees Celsius." This predicate is realizable if the given space-time point is accessible and if we can produce the temperature at that point.

If we start with a confirmable predicate it is possible to add to the gradual confirmation of that predicate by either setting up an experiment or delimiting a set of observations. If we know of a method that will result in a confirmation (or disconfirmation) of a predicate at a given space-time point, the predicate is considered to be testable. This means that we are able to make the test happen; that we know how to realize the circumstances required for a test. Through a method of testing, we will be able to determine either 'Q' or '~Q' in cases that we previously did not have

[33] Carnap p.456

knowledge of. If the test yields the result 'Q', we have an instance that adds to the gradual confirmation of the predicate. Test sentences have the logical structure of the following reduction sentences:

(R1) $Q_1 \supset (Q_2 \supset Q_3)$

(R2) $Q_4 \supset (Q_5 \supset \sim Q_3)$

Here Q_3 is the predicate which the test seeks to confirm or disconfirm (confirm the negation). Q_1 and Q_4 describe test-conditions that will yield us the desired result. Lastly, Q_2 andQ_5 describe a truth-condition for Q_3. The test-condition is a description of what situation needs to be realized in order to test Q_3 (ie. the description of an experiment, or a set of observations) while the truth-condition is a possible outcome of the test and each possible outcome will tell us something. Although, for Carnap, most reduction sentences have the form of R1 and R2, test sentences are a unique form of reduction sentences where the test-conditions are realizable.

For example, Q_3 may refer to a level of nuclear radiation in a specific location on Japan's eastern shore. Q_1 and Q_4 will state that a Geiger counter will be used in the specified areas. Q_2 will refer to a readout on the Geiger counter that is above 0 whereas Q_5 will refer to a readout of 0. If the test yields a count above 0, Q_3 will be confirmed. If the count remains 0, $\sim Q_3$ will be confirmed. In this example, the test-conditions, Q_1 and $Q_{4,}$ are realizable and the truth-conditions, Q_2 and Q_5, describe basic observations that would be sufficient for the confirmation of 'Q_3' or '$\sim Q_3$'.

If there is no practical way to realize a test-condition, Q_3 cannot be said to be testable. In addition, the truth-conditions for a method of testing, if the test is to be feasible, need to be observable or, at least, defined on the basis of observable predicates. Put another way, the test needs to produce a result that is observable or a result we can understand on the basis of other established predicates. If, for example, our aim is to determine whether or not a distant star is composed of carbon, our test-conditions would describe an experimental process which requires a powerful telescope and a spectrometer. The truth-conditions would refer to the basic, observable reading that is taken from the spectrometer.

Test sentences or reduction pairs are essential for expanding our empirical language. Since, however, science chiefly deals with gradually increasing levels of confirmation and rarely with the complete confirmation of a predicate, several reduction pairs are generally favored over a single reduction pair. A reduction pair states an experimental or observational process that tests merely one space-time point. If a generalized claim is to achieve a higher degree of confirmation, several reduction pairs (yielding positive results) need to be taken in conjunction. This means that the more space-time points that are subjected to test sentences, if they yield positive results, the greater will be the level of confirmation.

Another reason that a plurality of reduction pairs is desirable is because there are many properties which can be determined through a range of different methods. In addition to testing only one space-time point, a single reduction pair states just a single method for testing. Carnap uses the example of "the intensity of an electric current" and suggests that it can "be measured for instance by measuring the heat produced in the conductor, or the deviation of a magnetic needle, or the quantity of silver separated out of a solution, or the quantity of hydrogen separated out of water, etc."[34] Each of these methods of testing uses its own respective reduction pairs and, when taken in conjunction, allows us to increase the range of confirmation for the property.

[34] Ibid. p.445

It is not necessary for Carnap, however, that we are actually, that is, according to the present state of affairs, capable of going out to test a predicate for that predicate to be meaningful. What is necessary is that we can delimit of some possible set of circumstances under which the predicate can be confirmed. Thus for a predicate to be meaningful it only needs to be confirmable under some possible circumstances and not necessarily testable. If we weren't to allow for meaningful predicates to be confirmable under some possible circumstances it is clear that most sentences about the past or future would not be meaningful. This is because, in reference to Carnap's reduction pair and according to the present state of affairs, Q_1 in R1 and Q_4 in R2 would be unrealizable and, thus, no test condition could be realized, rendering the predicate Q_3 irreducible to observables. Those reduction sentences do not qualify as test sentences, but we still want to say that many sentences using predicates which refer to the past or the future are meaningful. Furthermore, many sentences about a present state of affairs to which we happen to not have access ought to be meaningful. Carnap uses the example of a black pencil. Through visual observation, he can conclude that his pencil is not red, but black. This conclusion does not, however, prevent the positive sentence "my pencil is red" from being confirmable. This is because we are able to "indicate the—actually non-existent, but possible—observations which would confirm the sentence."[35] The actual testing of a sentence, under real circumstances, "is irrelevant for the questions of confirmability, testability, and meaning of the sentence though decisive for the question of truth, i.e. sufficient confirmation."[36] What he means by this is that what matters for confirmability and testability is only the conceivable possibility of confirmation or testing. Sufficient confirmation or truth-value can only be achieved by the actual confirmation or testing of a sentence.

[35] Ibid
[36] Carnap p.457

Now that we have a sense of what Carnap's project looks like, let us see how it relates to his views on empirical meaningfulness. Testability is a stronger notion than confirmability and, if taken as the criterion for empirical meaningfulness, would restrict the domain of meaningful predicates too much. Carnap acknowledges that there are many predicates employed in the language of empiricism which are confirmable under some possible circumstances, but are not testable, either because we are unable to realize the test-conditions or we have no idea what a test would even look like. Testability is desirable for science because it is a platform to corroborate predictions and to increase the degree of gradual confirmation for hypotheses. Desirable as a testability requirement is, it is not, according to Carnap, a necessary requirement for the empirical meaningfulness of sentences. It is the reality of our situation as human beings that we can only observe and test a miniscule set of space-time points out of infinite possibilities. To rule out as meaningless conjectures about, as of yet, untestable space-time points or predicates whose test conditions cannot yet be realized seems impractical for science.

As we mentioned above, confirmability is also a weaker notion than complete confirmability (verifiability), but since generalized claims and universal laws are indispensable for the language of science, the weaker requirement, namely, the requirement of incomplete confirmability, is all that is necessary for empirical meaningfulness. This is because the requirement of confirmability is able to accommodate confirmable, but, as of yet, untestable predicates as well as generalized claims and universal laws. In a passage where Carnap reflects upon his project in "Testability and Meaning" he says the following:

> "As empiricists, we require the language of science to be restricted in a certain way; we require that descriptive predicates and hence synthetic sentences are not to be admitted unless they have some connection with possible observations, a connection which has to

> be characterized in a suitable way. By such a formulation, it seems to me, greater clarity will be gained both for carrying on discussion between empiricists and anti-empiricists as well as for the reflections of empiricists."[37]

Although some scientists may opt for a more restrictive requirement in order to suit their purposes (ie. A requirement of testability, or a requirement of complete confirmability), these restrictions do not have any bearing on the meaningfulness of predicates and Carnap suggests that these stronger requirements are generally unpractical for the language of science.

We are now in a better position to see how Carnap's thinking might impact the question of demarcation. Since, ultimately, the requirement of confirmability disallows only those sentences or predicates which are not confirmable (under any possible circumstances) its strength seems to lie in its ability to rule out certain claims of metaphysics. As a demarcation criterion between science and pseudoscience, however, a mere requirement of confirmability is perhaps a bit more liberal than we would like to admit. The reason for this is because it allows for virtually any theory, so long as its confirmation can is possible under some set of circumstances to be empirically meaningful. Indeed, Carnap suggests that his intention was to establish a demarcation between meaningful and unmeaningful sentences within an empiricist language and not a science/pseudoscience demarcation.

In focusing upon the empirical meaningfulness of sentences, Carnap did not outline a criterion for scientific acceptability or refutability. There is, for Carnap, no general rule that governs the decision to accept or reject a sentence.[38] Even when there is a bulk of evidence in a claim's favor, we still need to make a practical decision about whether to accept the claim or not.

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[37] Carnap "Testability and Meaning" (re-printed and revised in Feigl & Brodbeck 1953) p.84
[38] Ibid. p.426

My acceptance of the sentence, “there is a yellow coffee cup on my side-table”, seems largely determined by the evidence in front of my eyes. Even in the case of the yellow coffee cup, however, the possibility of denying the sentence remains, however small that possibility may be. Acceptance or rejection is always, for Carnap, based on a dance between the conventional component, and the non-conventional or objective component.

If we restrict our view to the scientific acceptability of a claim, we find ourselves in the thick of the science/pseudoscience demarcation problem. In practical affairs, such as courtrooms or health care, a demarcation criterion assists in, or ought to assist in our decision whether or not to accept certain claims over others. It will become clear throughout this thesis that the pragmatic function of a demarcation criterion is, in my mind, the most significant function that a demarcation criterion has. We may find that there are claims purporting to be scientific which, in fact, do not meet Carnap’s confirmability requirement. For Carnap, sentences which are not confirmable under some possible circumstances are not to be admitted into an empirical language and would have no bearing on a decision of acceptability since the sentences would be meaningless. In regard to sentences which are empirically meaningful, it is important, I feel, to elaborate further upon what features assist us in deciding upon a claim’s acceptability. Confirmability thus seems like it is a necessary condition for science, but not sufficient. Insofar as Carnap draws our thinking into what ought to be required of an empiricist language, he helps set the stage for a demarcation criterion that changes its focus from the meaningful/unmeaningful to the scientific/pseudoscientific. We will see this line of thinking leads nicely into Popper’s thought. For Popper, the scientific/pseudoscientific distinction places a much higher value on the testability of theories than Carnap’s meaningful/unmeaningful demarcation.

2.2 Karl Popper's Falsificationism

Popper's approach was motivated by many of the issues which arose out of the verification model, but still maintained a strong positivist spirit. Popper held that science had a deductive structure, but one that was quite different from the kind espoused by verificationists. It is with Popper that the issue of demarcation, as we know it today, acquired its form. The real question for him was how to demarcate scientific theories from pseudoscientific theories.

It is here that, as opposed to the meaningful/meaningless, the distinction between non-science and pseudoscience becomes important. Popper explicitly holds that science is not a category such that *all* branches of inquiry fall within it. Rather, science is a category that stands alongside other branches of inquiry such as metaphysics, ethics, theology, epistemology, etc. In this picture, the branches of inquiry which stand alongside science are not bound by the standards of scientific investigation. For Popper, this in no way renders these other branches *illegitimate* or *meaningless*. In some respects, these other branches are *more meaningful* than the investigations of science. Theories of physics or chemistry, for instance, are going to have very little relevance to ethical decision-making. One could say that it would be nonsensical to deliberate about electromagnetic charges when the aim is to make an ethical decision. The point here is that the ethical decision is not meaningless, nor is the theory of electromagnetic charge, but each fall within branches of inquiry which are relevant in their own respective contexts. Demarcation, then, for Popper, should concern itself with deciding what theories are appropriate for the category of science. Rather than demarcating non-science from science, the problem should focus solely on demarcating pseudoscience where pseudoscience refers to the theories which aspire to fall within the scientific category, but do not deserve to be within that category.

An issue within verificationism which motivated Popper's approach was the issue of partial verification. Harkening back to the problem of induction, we can say that there is no way to *absolutely* verify an empirical statement. The problem of induction notes that all we can ever experience is a finite slice of reality. From this or that finite experience we infer more general patterns in the world. The more times that we see a white swan, for example, the more confident we feel we are in making the general statement "all swans are white". The problem is that it doesn't matter how many verifying instances of a more general claim we experience. We can never experience all of nature and thus, we can never be conclusively certain that our general claim is true everywhere and at all times. The strongest claim we can validly make is that "every swan we have thus far observed has been white and this gives us strong evidence that all thus far unobserved swans will also be white." Popper's main problem with allowing partial verifications into scientific criteria is that they allow for a wealth of pseudoscientific theories to be admitted to the scientific category. As it turns out, it is notoriously easy to establish a set of conditions under which a theory can be partially verified.

> A Marxist could not look at a newspaper without finding verifying evidence of the class struggle on every page, from the leaders to the advertisements; and he would also find it, especially, in what the paper failed to say. And a psychoanalyst, whether Freudian or Adlerian, assuredly would tell you that he finds his theories daily, even hourly, verified by his clinical observations…It was precisely this fact—that they always fitted, that they were always 'verified'—which impressed their adherents. It began to dawn on me that this apparent strength was in fact a weakness, and all these 'verifications' were too cheap to count as arguments.[39]

[39] Popper (1962) p.267

This dissatisfaction with partial verifications as justification for scientific status also demonstrates Popper's narrowing of the scientific category. According to a verificationist conception of science, so long as the Freudian verified his claim, the claim was scientific. For Popper, the fact that it found these numerous partial verifications is insufficient to allow it the status of a scientific theory. The astrologer who claims that planetary alignment is causally responsible for the baby's future 'attunement' will be able to find verifying instances in every good mood and success that the human being encounters throughout his/her future. Many people find astrology to be both enlightening and meaningful to their lives. Be that as it may, when it comes to articulating facts about the world, the planetary alignment-birth theory, despite the potential for partial verification, does not seem to be a trustworthy approach.

The problem of induction was a substantial point of motivation for Popper's thought. The problem states the idea that we cannot, from a finite number of observations, verify a law which is universal. If our aim is to come to articulate universal laws then induction is not what we should be founding our science upon, "for any conclusion drawn in this way may turn out to be false. No matter how many instances of white swans we may have observed, this does not justify the conclusion that all swans are white."[40] That there is a "problem of induction" is telling. The principle of induction, itself, must be a synthetic statement. If it was analytic, like logically deductive statements, its opposite would be a contradiction. As a synthetic statement, this isn't the case. That the principle of induction might be false is not contradictory and it is possible to conceive. Hence, we have the *problem* of induction. Popper poses the question, "why such a principle should be accepted at all, and how we can justify its acceptance on rational

[40] Popper (1934) p.27

grounds?"[41] If we are to justify the principle of induction, which needs to be a universal statement, we can only do so by justifying it inductively. But how do we justify that further inductive statement? The answer is by induction, and so on. In the attempts to justify the principle of induction one gets drawn into an infinite regress. For Popper, the problems associated with induction are "insurmountable" and he outright rejects its feasibility as a foundation for science.[42]

Above, I mentioned that Popper narrowed the scope of the demarcation problem to a division between theories which were scientific and pseudoscientific. This is to say that the problem is concerned with the category of science. One key feature of the sciences which Popper professes is that the sciences are empirical. When all is said and done, all science needs to arise out of experience and needs to be subject to the scrutiny of experience. This is not in conflict with the views of verificationism and is absolutely in line with Carnap's views in "Testability and Meaning". We have seen, however, that Popper rejects induction as a principle on which to base science. Verificationism depended upon this principle in order to say anything that surpassed a mere 'partial verification'. To remain a deductivist and an empiricist, Popper has only one avenue left and that is falsification. In his view, the problem of induction prevents the verification of any universal, empirical statement. The only way to say anything *deductively valid* in empirical science is through the rejection of possible hypotheses.

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[41] Ibid. p.28

[42] Popper acknowledges some attempts to solve the problem of induction. "Kant tried to force his way out of this difficulty by taking the principle of induction to be 'a priori valid'. But I do not think that his ingenious attempt to provide an a priori justification for synthetic statements was successful."(Ibid. p.29) He also opposes the loosening of the demands on induction. "So also [regarding the insurmountable difficulties], I fear, are those inherent in the doctrine, so widely current today, that inductive inference, although not 'strictly valid', *can attain some degree of 'reliability' or of 'probability'.* According to this doctrine, inductive inferences are 'probable inferences'."(ibid) Ultimately, Popper concludes that "nothing is gained, moreover, if the principle of induction, in its turn, is taken not as 'true', but only as 'probable'. In short, like every other form of inductive logic, the logic of probable inference, or 'probability logic', leads either to an infinite regress, or to the doctrine of *apriorism*."(ibid. p.30)

By holding that science needs to be deductive and that it can only be so through falsification, Popper envisions science as *never being able to positively confirm any scientific theory*. Science can only progress negatively. The job of the scientist is to posit hypotheses to be tested against reality, not in the hopes of confirming these hypotheses, but in the hopes of the hypotheses holding up in the face of stringent tests. Resistance to falsification is the best that scientific theories can hope for. This point is significant for the contemporary demarcation theories that we will explore in the next chapter. Many modern theories characterize science as being dynamic and self-reflexive whereas pseudoscience is, more often than not, resistant to self-reflexivity. If an openness to refutation is a virtue of science, scientists ought to actively seek out objections and potential falsifying evidence, rather than the opposite.

That a theory can 'resist falsification' is only part of the story, however. A whole mess of crackpot theories can resist falsification, in virtue of the fact that they are *unfalsifiable* in principle. Popper suggests the sciences exhibit boldness, where boldness refers to: (a) "the boldness of predicting aspects of the world of appearance which so far have been overlooked but which it must possess if the conjectured reality is (more or less) right, if the explanatory hypotheses are (approximately) true;[43] and (b) in addition to (a), there is a readiness on the part of the scientist to test a conjecture against reality and to actively seek out tests and refutations. The boldness of a theory, or a theory's openness to falsification is the foundation for Popper's demarcation criterion. For a theory to be considered as scientific, there must be a conceivable way that the theory can be tested against experience such that it could be falsified. Those theories which admit of no conceivable test that could provide a falsifying instance are

[43] Popper (1974) p.122

pseudoscientific in the Popperian sense. The level of risk is, for Popper, proportional to the scientific status of a theory.[44]

In a general sense, a risky hypothesis just refers to a hypothesis which predicts an outcome that is highly specific. A hypothesis that is either overly general or vague will not furnish a scientist with much surprise if a test is unable to falsify it. If I were to predict that there will be clouds in the sky when I walk outside and, when I walk outside, I find there to be clouds in the sky, there is little gained from this hypothesis. My prediction was still empirically falsifiable, but it was by no means bold. Popper was highly impressed by Einstein's relativity theory for the boldness involved in its predictions. About one particular prediction of relativity, Popper says the following:

> With Einstein's theory, the situation was strikingly different. Take one typical instance…Einstein's gravitational theory had led to the result that light must be attracted to heavy bodies (such as the sun), precisely as material bodies were attracted. As a consequence it could be calculated that light from a distant fixed star whose apparent position was close to the sun would reach the earth from such a direction that the star would seem to be slightly shifted away from the sun…This is a thing which cannot normally be observed since such stars are rendered invisible in daytime by the sun's overwhelming brightness; but during an eclipse it is possible to take photographs of them. If the same constellation is photographed at night one can measure the distances on the two photographs and check the predicted effect.[45]

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[44] Thomas Kuhn took issue with Popper's conception of science on the grounds that Popper's theory only focused upon periods of revolution within science and neglected the 'normal science' which occupied the bulk of scientific practice. Popper did not see this to be the case and took issue, in response, with Kuhn's historical distinction. (Lakatos, Musgrave 1970)

[45] Popper (1963), pp.67-8

Einstein's prediction was risky in another sense in that it contradicted well-tested science. The risk involved in this sort of prediction is precisely the sort of thing that is in line with Popper's view of what science ought to do. Einstein's gravitational theory predicts a highly specific measurement which, when the experiment is conducted, will be measured. Einstein, in proposing his theory has, in a sense, 'stuck his neck out' to the test of Nature. "If observation shows that the predicted effect is definitely absent, then the theory is simply refuted. The theory is *incompatible with certain possible results of observation*."[46] In line with Popper's view of the aims of science, Einstein's risky prediction should not lead us to the belief that we have verified the hypothesis, but that we have a robust theory that has survived exposure to cutthroat experimentation.

The turn to falsifiability in science, as I mentioned above, has interesting implications for the ultimate goals of science. Scientific knowledge becomes perennially unprovable, conjectural, provisional, and hypothetical. The only move available which can be rightly deemed conclusive is to falsify a theory. Thus we proceed by weeding out the refuted theories from the garden of science while tending to those theories which, though potentially refutable, have not yet met refutation. In theory, the logical nature of falsification makes sense: posit a theory that can be falsified, try to falsify it, if falsified: discard. To put this simple theoretical activity into practice, however, demands ideal conditions which seem to be unrealistic.

The first problematic demand is the logical isolation of a theory. In an experiment where a hypothesis is put to the test there are always factors which constitute the background conditions for the experiment to take place. When one begins to look more closely at a scientific experiment, one begins to see that there are a lot of elements at play beyond the specific theory

[46] Ibid.

or hypothesis in question. Experiments often make use of instruments ranging in degrees of complexity. Before a given theory can even be tested there is a lot of science that needs to be done just in setting up the conditions to conduct the experiment. Suppose an astronomer posits a wonderfully bold theory about the behavior of dark matter. She then devises a test which suggests that there will be a correlation between the relative positions of a supermassive black hole and a nearby galaxy. This fictitious experiment, like virtually all astronomical experiments, requires highly complex technology. To detect the black hole, astronomers will need to make use of telescopes which are designed to detect x-ray emissions. To measure the position of the nearby galaxy, they might make use of infrared or visual light telescopes. In order to test the initial hypothesis, the measurements provided by each telescope need to be compared. If it turns out that the relative positions were not what the hypothesis predicted, is the theory falsified? It seems a bit hasty to draw the conclusion that the theory is false because there are so many other factors at play which could be erroneous. The very operation of the telescopes relies upon a wealth of optical theory, not to mention basic calibration and maintenance. The notion of a black hole, though almost universally accepted by scientists, is an entity that can only be observed indirectly. Dark matter is even more mysterious and was posited as a solution to an inconsistency between the behavior of matter in the universe and the total mass of the universe. It is clear that there are several avenues that may contribute to the unwanted result. Furthermore, when we look at the theory-ladeness of the experiment, we see that the initial hypothesis is a part of a broad network of theories which, in some sense, are interdependent. We can suppose that there could be other astronomical theories which reject the existence of dark matter. In this case, the observed behavior of astronomical entities will be interpreted through a different conceptual lens. Every observation then, although carried out in the same way, will be laden with a *different* *theoretical framework*. That observation is theory-laden makes for complications in falsificationism. In order to recognize a falsifying instance an acceptance of unproblematic background knowledge is required. In addition, the framework that is largely founded in this background knowledge will govern the manner in which the problematic instance is both observed and interpreted. This is a problem because it is unclear whether or not the observation is a refutation of the theory in question or whether or not it is some of the basic assumptions which constitute the unproblematic background knowledge that ought to be at issue.

We can take a simpler experiment to illustrate this problem. Take the claim "all swans are white". This is a basic example which is often used to demonstrate Popper's falsification. What would be required to falsify this claim? Well, a single black swan would do it. This doesn't seem to work, however. For even with a simple general hypothesis there are auxiliary assumptions at play. In this example, the general statement about swans is dependent upon a taxonomy of bird species, which is rooted in biological similarities and differences. What appears to be a black swan doesn't necessarily falsify the general claim. It might just point to an amendment which needs to be made elsewhere. For example, the criteria we use for classifying such and such a bird of the type, "swan" may prove to be too broad or too narrow. Popper acknowledges this and still holds that, although what appears to be a black swan might not be a falsification of the claim "all swans are white", it will still be informative and useful for science if the observation does lead to refinements in the taxonomy of birds.

We can draw a more general assumption which is essential to falsificationism. This is the assumption that "there is a natural, *psychological* borderline between theoretical and speculative propositions on the one hand and factual or observational (or basic) propositions on the other."[47] Any time that the senses are being relied upon for empirical data (which is all of the time), there is some assumption at play regarding what constitutes *pure observation* as opposed to *mediated observation*. A rather small portion of modern science is conducted without the use of instrumentation. Whether it be telescopes, microscopes, spectrometers, or cloud chambers there is always some theory-laden technology on which our observation depends. We can even say that naked-eye observations are theory-laden. This means that the reliability of our observational reports is *dependent* on the reliability of the background theory which mediates our very ability to make the reports. Galileo, for example, claimed that his 'observations' of mountains on the moon and spots on the sun were sufficient to refute the prior models which held that celestial bodies were pristine crystal balls. As Lakatos notes, "But his 'observations' were not 'observational' in the sense of being observed by the—unaided—senses: the reliability depended on the reliability of his telescope—and of the optical theory of the telescope—which was violently questioned by his contemporaries. It was not Galileo's—pure, untheoretical—*observations* that confronted Aristotelian *theory*, but rather Galileo's 'observations' in light of his optical theory that confronted the Aristotelians' 'observations' in light of their theory of the heavens."[48]

The view that single experiments are capable of conclusively refuting theories is problematized by the fact that science consists of several interconnected theories. The view has been named *naïve falsificationism*. Beyond pointing out that there can always be erroneous auxiliary assumptions in the background of an experiment, scientists are in the business of tweaking theories with minor adjustments, ad hoc hypotheses, etc. to better fit the data. The

[47] Lakatos (1970), p.97
[48] Lakatos p.98

reality is that theories are not so easily discarded even if, *ceteris peribus*, the theory appears to be refuted by the data. This process seems to, in fact, be an important feature of scientific practice since challenging observations and adjusting theories greatly assists the growth of scientific knowledge.[49]

Popper was not, however, a naïve falsificationist. He says in the *Logic of Scientific Discovery*, "In point of fact, no conclusive disproof of a theory can ever be produced; for it is always possible to say 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."[50] Thus Popper, himself, acknowledged the difficulties associated with falsification as a determinate principle for scientific conclusions. Ultimately, since confirmation is not to Popper's taste, falsification is the most promising path for scientific investigation despite the fact that conclusive falsifications might not be possible either. That falsifications are not conclusive is a point raised by Carnap who suggested that falsifications are nothing more than negative confirmations and, as such, can only ever gradually increase in degree. At the very least, falsification promotes a critical spirit in science which works to continually refine experimental techniques. Just because naïve falsification is not sufficient as a maxim of science does not mean that there aren't subtleties and variations within the concept of falsifiability which are sensitive to theoretical interdependence of scientific practice.

What was truly distinctive for Popper's approach to the question of demarcation was what he considered to be the critical attitude of the scientist. This marks a significant break from

[49] Kuhn (1970)
[50] Popper (1959) p.50

the approach of the logical positivists. Positivism sought to build science on a purely objective and logical foundation, and a foundation which took the subjective attitude of the scientist out of the picture. A positivist picture of science focuses on the logical relationships between theories or hypotheses and evidence and leaves the psychological component alone. We can recall that in Carnap's theory of empirical meaningfulness, he did not venture too deeply into how we come to accept or reject theories. So long as the theory was empirically meaningful, there were no general rules for choosing to accept or reject the theory. For Popper, science is, largely, a human activity among other human activities and the approaches and attitudes of the individuals engaged in that activity are of great importance. Whether it was in the field of science, philosophy, or political theory, Popper believed that human beings ought to have faith in reason.[51] This means to be committed to a progressive view of knowledge and not to succumb to stagnant dogma. Falsificationism is a natural stance to arise when considering a commitment to reason because falsificationism is the very process of exposing one's conjectures to criticism and experiment. By exposing one's conjectures in such a way, one is agreeing to the possibility of refuting one's views in the service of coming to a greater understanding of reality.

It is important to notice the spirit in which Popper lays down his views about demarcation. Popper chooses the modest term 'proposal' to describe his demarcation. He suggests that any demarcation, in his sense, "must be rough".[52] What he means by this is that the line dividing science and pseudoscience is not clear. Atomism, for example, was originally a metaphysical theory, but became a scientific theory. It is always possible that what, today, is regarded as pseudoscience will eventually become science. Thus our efforts to establish demarcation criteria are conjectural, just, as Popper says, all of our claims to scientific

[51] Popper "In Defense of Rationalism" (1945)
[52] Popper "The Problem of Demarcation" (1974)

knowledge are. Be that as it may, there are rational grounds for preferring well-tested theories with higher degrees of empirical content over untestable theories with either minimal or no empirical content. We will now turn to a prolific thinker in the philosophy of science who saw himself to be, in a certain sense, following the virtues of Karl Popper's views and extending them to a broader perspective about what science does.

2.3 Imre Lakatos and the Methodology of Scientific Research Programmes

If we were to provide some essential bullet points about what naïve falsificationism is all about, we could perhaps say the following. It maintains a logical justificationist approach, similar to the verificationists, while rejecting the possibility for *actually verifying scientific theories*. Instead it demands that we focus purely on refuting instances and weed out the theories in conflict with those instances. Here we find a logical asymmetry between falsification and verification. We conclusively disprove, but can never conclusively prove theories. The ultimate aim of science changes accordingly. The scientific enterprise now seeks to accumulate theories which, although they may not be objectively true (for the falsificationist, we could never know), have held their ground in the face of crucial experiments.[53] The object of naïve falsificationism is, in any given experiment, a theory. Finding its justification logically, naïve falsificationism is

[53] Lakatos reconstructs the following historical progression from the lens of naïve falsificationism: "According to the logic of dogmatic falsificationism, science grows by repeated overthrow of theories with the help of hard facts. For instance, according to this view, Descartes' vortex theory of gravity was refuted—and eliminated—by the fact that planets moved in ellipses rather than in Cartesian circles; Newton's theory, however, explained successfully the then available facts, both those which had been explained by Descartes' theory and those which refuted it. Therefore Newton's theory replaced Descartes' theory. Analogously, as seen by falsificationists, Newton's theory was, in turn, refuted—proved false—by the anomalous perihelion of Mercury, while Einstein's explained that too. Thus science proceeds by bold speculations, which are never proved or even made probable, but some of which are later eliminated by hard, conclusive refutations and then replaced by still bolder, new and, at least at the start, unrefuted speculations." (Lakatos p.97)

free from any sort of conventionalism. This is to say that nature is the judge and jury when it comes to refuting a theory. The scientist devises the theory, sets up the experiment, and more or less sits back while nature provides a resounding YES or NO.

Privy to the problems associated with this view, but inspired by the possibilities of falsificationism, Imre Lakatos drew a contrast between naïve falsificationism and *sophisticated falsificationism.* In his influential paper, *Methodology of Scientific Research Programmes,* Lakatos lays out an account of sophisticated falsificationism which is more sensitive to both the historical nature of science as well as actual scientific practice. The main problem facing naïve falsificationists is that they treat theories as though they are logically isolated when, in fact, they aren't. Although they claim that nature is the ultimate judge in refuting their theories, there is actually a high level of conventionalism at play. In setting up a 'crucial experiment', scientists must be able to relegate all of the experimental conditions and auxiliary hypotheses to *unproblematic background knowledge*. In order to fabricate the sort of logical isolation of a theory that naïve falsificationism demands, the scientists need to be able to, when all is said and done, assert that "*ceteris peribus*, the data shows the theory to be refuted". The establishment of the *ceteris peribus* clause is no simple matter. It requires a series of decisions on the part of the scientific community about what is actually considered to be 'unproblematic background knowledge'. As I mentioned above, it is easily conceivable that what appears to refute an 'isolated' theory is, in fact, pointing to any of the several elements constituting the background knowledge. Furthermore, the network of background theoretical commitments provides the interpretive lens for the observations. For Lakatos, these background commitments are always up for revision and, therefore, the how the data is understood is contingent and could be drastically different.

The object of demarcation for sophisticated falsificationism is not ever *a theory,* but *a series of theories*. Lakatos defines a series of theories as *a research programme*. The term 'research programme' refers to the continuity that unites the series itself. Within research programmes, Lakatos suggests that there are series of *problemshifts.* A problemshift refers to the problems and events that arise within a succession of theories. As theories are modified and refined in order to deal with anomalies which arise, the problems of focus shift. We will say more about this below. Contrary to naïve falsificationism, there is no such thing as a 'crucial experiment' for the sophisticated falsificationist. "No experiment, experimental report, observation statement or well-corroborated low-level falsifying hypothesis alone can lead to falsification. There is no falsification before the emergence of a better theory."[54] This is broadens the view of what science is up to. Theories are tangled up in such a way that it is not methodologically sound to just uproot one due to a perceived refutation. Not only is it methodologically unsound, but it is also not what has been the case historically. Anomalies abound in the history of science and have faced each and every research programme. Anomalies do not fit within a given scientific model. Anomalies cannot be accommodated within the current scientific theory. An anomaly is what ought to be, for the naïve falsificationist, a refutation. If naïve falsificationism was thoroughly adopted, it seems as if virtually no theory could survive as any anomaly would refute the theory and the theory would have to be discarded. A famous example is the anomalous behavior of the perihelion precession of the orbit of Mercury within Newtonian gravitational theory, first reported by Le Verrier in 1859. The behavior of Mercury's orbit could not be accounted for by the calculations of Newtonian gravitational model and remained an anomaly until the behavior was accounted for by Einstein's theory of relativity.

[54] Lakatos p.119

What actually happens when a research programme is faced with anomalies, according to Lakatos, is that theories are tweaked, adjusted, and held to by scientists in the face of the anomalies. This can be done in a number of different ways, either to better fit the data or to change the way the data is gathered/interpreted so that the data better fits the theory.[55] The essence of what happens when these adjustments are made is *that auxiliary clauses and hypotheses are added to the preceding theories.* Popper was suspicious of these sorts of adjustments and suggested that it is often a "conventional stratagem" that was characteristic of pseudoscientific disciplines.[56] For Lakatos, this is a reality of how science operates and cannot be avoided. Although the anomalous behavior of Mercury led to the eventual superseding of Newtonian theory by relativity, the anomalous behavior of Uranus was accounted for within the Newtonian framework by the prediction of Neptune's existence. What was, at first, a potential falsifier for Newton's theory turned out to be what seemed an instance of corroboration that also brought with it the discovery of a new planet.

These theoretical adjustments are not merely arbitrary, but they are indicative of the conventional element in science. The conventional element of science dictates that we can neither conclusively prove n*or disprove* research programmes. There will always be an element of convention when it comes to the programmes that we choose to hold on to. According to sophisticated falsificationism, what demarcates a scientific research programme from a pseudoscientific research programme is whether or not the programme is *progressive or*

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[55] Reichenbach suggests that this is the reason that Newton waited so long to publish his *Principia*. "To his disappointment he found that the observational results disagreed with his calculations. Rather than set any theory, however beautiful, before the facts, Newton put the manuscript of his theory into his drawer. Some twenty years later, after new measurements of the circumference of the Earth had been made by a French expedition, Newton saw that the figures on which he had based his test were false and that the improved figures agreed with his theoretical calculation. It was only after this test that he published his law."(Reichenbach 1951, pp.101-2)
[56] Popper (1963) pp.68-9

degenerating. Our decision to eventually abandon a research programme is rooted in how well the programme holds up in the face of anomalies. It is no surprise that anomalies will arise and challenge the theories constituting the research programme. It is the successes or failures of the auxiliary theoretical adjustments that are the guide to choosing whether or not to hold on to a programme. In order to be theoretically progressive, a research programme needs to exhibit the following characteristics. First, the auxiliary clauses which are added to theory T to form T1 need to leave T1 with *at least as much unrefuted content as* T. This holds for all subsequent theoretical adjustments. If T1 has *less unrefuted content* than T, that is evidence that the research programme is degenerating. Second, the programme needs to have some degree of *excess empirical content* over its predecessor. This means that it needs to "predict some novel, hitherto, unexpected fact."[57] If a theory is adjusted in a merely linguistic way, or a way which brings no new empirical content to the table, than the adjustment is not considered to be scientific. By gaining empirical content, T1 is said to be *empirically progressive*.[58] If T1 is both theoretically *and* empirically progressive, then the research programme is progressive. If not, the programme is degenerating. Ptolemaic astronomy, for example, was a degenerating research programme because, in positing the existence of epicycles *ad hoc* to explain the retrograde motion of the planets, the programme attempted to save itself in the face of anomalies, but was unable to predict any new facts. Newton's programme, at least Newtonian physics, on the other hand,

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[57] Lakatos p.118

[58] Lakatos demonstrates the 'new fact requirement' in the following way: "If I already know P1: 'Swan A is white', P2: 'All swans are white' represents no progress, because it may only lead to the discovery of such further similar facts as P1: 'Swan B is white'. So-called 'empirical generalizations' constitute no progress. A new fact must be improbable or even impossible in the light of previous knowledge." (Lakatos footnote 2, p.118)

demonstrated its progressive nature in the successful anticipation of Neptune's existence, the bulge of the Earth, and the return of Hailey's comet.[59]

Research programmes are composed of 'hard cores'. These cores are comprised of the basic theoretical commitments of the programme. While a research programme is still being pursued, the core is, in a sense, immune from refutation. The basic theoretical commitments act as the source of continuity for programme itself. Experiments and refutations occur only against the low-level, auxiliary hypotheses which are derived from the core. Lakatos calls this the 'protective belt'. As these auxiliary hypotheses continue to be stumped by anomalies, new hypotheses are invented and the cycle begins anew. Lakatos cites Newton's programme as paradigmatic of this. The hard core of Newton's theory consisted of his three laws of motion and his law of gravitation. Accompanying this hard core is a series of problem-solving techniques, or a *positive heuristic*. For Newton, this heuristic included differential calculus, the theory of convergence, and differential and integral equations. The protective belt of auxiliary hypotheses included the theory of atmospheric refraction, geometrical optics, etc.[60] It is at the level of the auxiliary hypotheses that anomalies arise and are to be dealt with. The hard core is thus shielded from refutation.

Lakatos makes clear that there is no immediate way of determining whether or not a research programme is progressive. It may, in fact, take a long period of time and a good deal of hindsight in order to decide that a research programme is progressive. That it may take an exceedingly long period of time to determine that a research programme is progressive leaves one wondering how anyone can actually determine whether or not to stick with a research

[59] Lakatos (1978) p.179
[60] Ibid.

programme which appears to have reached its point of *saturation*.[61] There is no easy answer to this question. One important factor in the decision is that, in the absence of a rival research programme with at least as much empirically corroborated content[62], programmes simply aren't given up. If there is a rival programme which is also progressive, this does not necessarily mean that the 'stagnant' program needs to be replaced. It might happen that, down the road, predicted facts of the stagnant program are corroborated by advances in our knowledge. This sort of occurrence would be cause for a revival of the programme. Competing research programmes, for Lakatos, is a fruitful occurrence for the progression of science. It might happen that they feed off of one another. In other words, the progressive nature of P1 may furnish P2 with anomalies to deal with, thus forcing the proponents of P2 to become creative in the attempt to 'save their programme'. Creativity is, of course, not always going to have positive results, but it is, in Lakatos' view, often a significant contributor to progressive problemshifts. Since there are no 'crucial experiments' and no guarantee that a programme's protagonists will be vindicated in their efforts anytime soon, a certain stubbornness to hold onto a programme is not a vice. Once a programme has entered into a stage of degeneration—when its theoretical problemshifts cease to be consistently increasing in content and when the programme ceases in predicting any new, empirical facts—there is little reason to hold onto it. The programme, at this stage, would cease to be scientific.

By broadening the scope of demarcation to include the historical progression of science, Lakatos makes an important advance in the problem of demarcation. This move relativizes

[61] 'Saturation' is a term that Lakatos uses to represent a research programme which no longer has any prospect of being progressive. At this point the programme is either given up for a competing programme or it becomes degenerating.
[62] Empirical corroboration is important for Lakatos. Since he, like Popper, rejects conclusive verification, he maintains that corroboration is the mark for gauging whether or not a programme is empirically progressive.

demarcation to its historical context. The argument could be made that much of what was contained within Aristotelian science was inherently pseudoscientific. This doesn't just mean that *we know now* that it was pseudoscientific, but that even at the time Aristotle was drawing conclusions he was engaged in pseudoscience. According to Lakatos, however, Aristotle was doing science in virtue of the fact that Aristotelian science was the most progressive research programme of the day. We can say that adopting ancient science today would be imprudent since we would lose a good deal of content that is contained within modern, scientific theories and, thus, Aristotelian science would be pseudoscientific in a modern context. By abandoning the dogmatism of verificationism, the findings of science, as Popper held, are never conclusive, but are always subject to revision. Progressiveness thus becomes a hallmark of science which, if ever given up, should indicate evidence of pseudoscience. In the next chapter I will explore two more recent views on demarcation in light of Laudan's dismissal of the demarcation problem. Just as scientific theories are interdependent and founded upon a wealth of background knowledge, so too is the contemporary problem of demarcation rooted in the three positions just laid out. We will see that the turn toward historical conventionalism has not been given up, but has set the stage for modern conceptions of the scientific enterprise.

3

The Contemporary Face of the Demarcation Problem

3.1 Laudan's Attempted Dismissal and Meta-philosophical Criticisms

In the previous chapter, I attempted to provide a sketch of three different approaches to the problem of demarcation that were developed in the first half, or so, of the twentieth century. This chapter will focus upon the difficulties that arise and criticisms that face any modern solutions to the problem. I will begin by examining the criticisms that have been raised by Larry Laudan in his 1980 paper which sought to outright dismiss the demarcation problem. Laudan's paper is perhaps the most significant challenge that the problem of demarcation has faced. Since it illuminates aspects of what ought to be accomplished in an adequate demarcation criterion and, thus, I suggest that in his attempts to dismiss the question, Laudan has contributed to its development. My position is twofold in regard to Laudan's critique. On the one hand, I agree with his view that there are metaphilosophical issues with the problem that need to be addressed if a successful demarcation criterion is to be reached. In fact, I feel that Laudan does not fully bring out some of the key points that ought to be addressed when considering a demarcation criterion. I argue that the difficulties facing the problem of demarcation are, in fact, greater than those Laudan proposes. On the other hand, however, I disagree with Laudan's view that the problem ought to be dismissed. Later in this chapter, I will raise two primary challenges for the problem of demarcation that, to some degree, go beyond, or even compound, the challenges raised by Laudan. These are the multifariousness of science and the historical dynamics of science. Along with raising these difficulties, I will reference two modern thinkers who, to some extent, have done some work in addressing these difficulties. The two theories that will supplement my analysis are Paul Hoyningen-Huene's systematicity[63] and Nicholas Maxwell's Aim-Oriented Empiricism.[64] I suggest that, contrary to Laudan's dismissal of the demarcation issue, these modern approaches offer progress into dealing with the challenges that face demarcation.

Laudan's criticisms are motivated by what he perceived to be a consistent failure on the part of philosophers to adequately articulate conditions that could, consistently, partition science from pseudoscience. I will suggest that another point of motivation for Laudan was Popper's insistence on the importance of the demarcation question to modern epistemology. It is Popper, after all, who receives the majority of the criticism from Laudan. Throughout his critique, Laudan includes a number of lines which one might consider to be polemical: "surprisingly (or, if one is cynically inclined, quite expectedly), the absence of a plausible demarcation criterion did not stop *fin de siècle* scientists and philosophers from haranguing against what they regarded as pseudoscientific nonsense"[65]; or "we ought to drop terms like 'pseudoscience' and 'unscientific' from our vocabulary; they are just hollow phrases which do only emotive work for us."[66] As a good, careful philosopher, however, Laudan substantiates his clear passion for the issue with clear arguments. The arguments are of two sorts. 1) Arguments aimed at the specific approaches of the "Newer demarcationist tradition." These critiques mainly cover the verificationists and falsificationism. As it was shown in the second chapter that both verificationism and falsificationism (at least in its naïve form) are untenable, I will not spend any time addressing Laudan's specific objections, though it is curious that Laudan does not include a

[63] Hoyningen-Huene (2007)
[64] Maxwell (2004)
[65] Laudan p.116
[66] Ibid. p.125

word about Kuhn or Lakatos. I suggest that this omission is tied to a general underdevelopment, on Laudan's part, of the historical nature of science. This will be addressed further below. 2) Arguments directed at meta-philosophical considerations. It is in these points that we find the meat of Laudan's argument and a both interesting and important lens to the demarcation problem.

The idea is this. Before we can decide, conclusively, that astrology is pseudoscientific while astronomy is not, there needs to be a demarcation criterion in place. Before we can start arguing about which thinker's demarcation criterion is the one to adopt, there needs to be clarity about what is expected of a demarcation criterion. In other words, in order for theorists to attempt a solution to the demarcation problem, they need to have an idea about what conditions a demarcation criterion would need to satisfy. According to Laudan, there are three questions to ask:

a) "What conditions of adequacy should a proposed demarcation criterion satisfy?
b) Is the criterion under consideration offering necessary or sufficient conditions, or both, for scientific status?
c) What actions or judgments are implied by the claim that a certain belief of activity is 'scientific' or 'unscientific'?"[67]

I will begin by explicating what Laudan meant by these three questions and, as far as I can see, his responses to these questions. Afterwards I will provide my assessment of these demands.

In regards to a): Laudan notes here that the formulation of a demarcation criterion is a different sort of business nowadays than it would have been, say, pre-19th century. The reason

[67] Ibid. p.117

for this is that explanations about what science is were largely considered to be an a priori matter in the past. It is not until science, as we know it, got off the ground and began to prosper, that the explanations about science needed to be sensitive to the methods of science. Another way of saying it is that any demarcation criterion we establish needs to take into account the disciplines that, according to widespread agreement, are considered to be scientific. Contemporary Physics, for example, is taken to be the kingpin of the physical sciences. If by some absurd bout of reasoning, a philosopher was to proclaim a solution to the demarcation problem which concluded that physics was, in fact, pseudoscientific, there would be no good reason to think that the criterion was any good. At root, it might be said that the criterion was not doing its job or, in Laudan's terms, not meeting the conditions of adequacy. "The quest for a latter-day demarcation criterion involves an attempt to render explicit those shared but largely implicit sorting mechanisms whereby most of us can agree about paradigmatic cases of the scientific and the non-scientific."[68]

It is not good enough to merely explicate the features that have been implicitly agreed upon, however. A demarcation criterion needs to go further than this if it is to be "philosophically significant." A "philosophically significant" criterion would need to state either *epistemic* or *methodological* features which are unique to the sciences such that the sciences warrant epistemic authority. Concisely put, what makes scientific belief more worthy of our attention than unscientific belief? Without a clear answer to this question, there can be no demarcation criterion, at least not one that, according to Laudan, is of any use or philosophical interest. We might say that demarcation can be viewed as having both a descriptive and a normative approach. The descriptive approach is in the business of 'delimiting the subject matter

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[68] Ibid.

of scientific knowledge'. The boundary that is constructed here separates science from non-science. While it is interesting to get clear about what the subject matter of scientific knowledge is, the major task for demarcation is to weed out pseudoscience from within the domain of scientific knowledge. This 'normative' approach is what makes the demarcation problem philosophically interesting.

As for b): Laudan holds that a successful criterion will need to be able to specify individually necessary and jointly sufficient conditions for scientific status. The reasoning is as follows. Suppose that conditions are proposed which are necessary, but not sufficient for science. We might say, for example, a condition like "one must always invoke naturalistic explanations with empirical grounds" is a necessary feature of science. But this only allows us to reject disciplines as unscientific on the basis that the disciplines do not meet the condition. Suppose that a classics professor at the University of Victoria explains all natural phenomena by reference to the passions and acts of Greek Gods. Since the explanation, "lightning struck that oak tree because Zeus was frustrated by Hera's nagging and he, in a fit of rage, threw a lightning bolt down to Victoria" invokes supernatural causes, it is decidedly unscientific. However, having a bundle of necessary, but insufficient conditions does not allow anyone to determine what disciplines are conclusively scientific. It only allows us to determine what is not unscientific. Suppose there is agreement about three necessary features that a scientific discipline needs to exhibit. Suppose these features are: 1) the discipline must invoke exclusively naturalistic explanations; 2) the discipline must collect data, either through experiments, statistics, or testing in the aim of making predictions; and 3) the discipline must attempt to disclose facts about either the natural or human world. Chemistry exhibits all of these features and, although chemistry is considered to be a science, the satisfaction of the necessary conditions is not sufficient for scientific status. Sports analysis may also exhibit all of these features, but would that make sports analysis a science? Sports analysis is not widely regarded as counting as a science even though the discipline uses naturalistic explanations, makes extensive and thorough use of statistics for the sake of making predictions, and these predictions are about historical facts. For instance, the prediction that the Canucks will win Saturday's home game against the Flames is a prediction about a specific state of affairs coming to pass in the world. Without sufficient conditions, there is no way to be certain about a discipline's scientific status. There is no way to distinguish scientific from not unscientific. At best, disciplines would be seen as provisionally more on the scientific track than others.

A similar problem arises if sufficient conditions are defined without identifying all of the individually necessary conditions. In this case, some disciplines would be identifiable as 'genuinely' scientific, but we would be in no position to say which disciplines were *not* scientific; at least, we would have no grounds for determining why certain disciplines were unscientific. Suppose that 'being physics' was sufficient for scientific status. This condition is mildly helpful insofar as it allows us to say that whatever is physics is scientific, but it helps us very little when it comes to all disciplines that aren't physics. Laudan suggests that in such a situation, disciplines which failed to meet the sufficient criteria would remain in an "epistemic twilight zone", neither scientific nor unscientific. Thus, neither sufficient nor necessary conditions, alone, will prove adequate for a demarcation criterion.

Regarding c), Laudan stresses that intellectual focus upon a demarcationist project ought to be taken with great seriousness and ought to be "especially compelling"[69] if we are to abide by its findings. Demarcation has a far greater reach than merely sorting our beliefs into categories

[69] Laudan p.120

such as “sound” and “unsound” or “cranky” and “reasonable”. Although this taxonomy seems to be the bedrock aim of a demarcation criterion, any formulation of such a criterion will have ramifications that reach far beyond a mere taxonomy. Science is an essential component of the modern world and is intertwined with educational, political, economic, and sociological aspects. Take creationist science, for instance. The scientific status of creation science is still debated and this debate has, on a number of occasions, reached the judicial system.[70] A demarcation criterion which deems creation science to be unscientific will, if it is taken seriously, have implications on school funding, educational practices, political support, as well as moral issues and an effect on the worldviews of the general public. To speak negatively about the scientific status of creation science will, in effect, amount to, at least some degree, diminishing people’s view of the rational legitimacy of the related social aspects which arise out of support for the discipline. That demarcation goes beyond a mere taxonomy of beliefs into social, educational, and political arenas suggests that it is a serious matter and should not be employed unless its own influence is given adequate consideration.

These considerations lead Laudan to an interesting conclusion about the demarcation question. Ultimately, he rejects the importance of investigating such a matter. He does, however, reject it only insofar as demarcation seeks to partition the scientific from the pseudoscientific. If we are seeking to partition well-founded beliefs from their counterpart, Laudan suggests that our endeavors may be “philosophically interesting and possibly even tractable”. To try to articulate this latter partition is, according to Laudan, not an attempt to “resurrect the science/non-science demarcation under a new guise”. I find this conclusion to be peculiar. Understandably, there is a concern about the term “science” itself. Scientific

[70] See cases: Kitzmiller vs. Dover (2005) or Mclean vs. Arkansas Board of Education (1982)

disciplines are not homogeneous in method or content. From the borders of the soft sciences (economics or sociology) to the core of the hard sciences (elementary physics) there is such a large degree of diversity that it seems as if the term "science" is spread so thin that the word is hardly of any interest. Be that as it may, a demarcation criterion can be seen as a way of getting clearer about what we mean when we say "scientific" and "pseudoscientific". The demarcationist project need not be one based on a misguided, preconceived definition of "scientific", but rather based on the goal of gaining to a greater understanding of what science is. I propose that Laudan's suggestion that the philosophically interesting question is actually the well-foundedness of our beliefs is what is largely at issue for the demarcation question. The cautionary tale that arose in c) seems to me to be an essential point *in favor* of pursuing a demarcation criterion. Articulating which of our beliefs are well-founded *is* a serious matter and one that goes well beyond our mere intellectual curiosity. It is an important and common feature of rational, forward thinking persons, philosophers and scientists alike, to think that the pursuit of truth is not an activity which is detached from the way we live our lives. On the contrary, the pursuit of truth ought to act as a guide for the way we live our lives. This notion has been professed since the rise of philosophy itself. The way in which we found beliefs about the world and the types of beliefs we have about the world are intimately tied to the economic, political, and moral spheres that make up our human lives. As Susan Haack suggests, the diverse activity of the sciences share a common approach and can be seen as embodying rationality in empirical investigation to a higher degree than other disciplines.[71] Since the role of science is so vast in our modern world, it is a profound responsibility of ours to use great care in differentiating, as far as we can, what is rational to believe from what is irrational to believe. We trust that science is in

[71] Haack (2003)

the business of getting at the facts of the matter and, because of this, offering knowledge about the world which is rational to believe.

3.2 Beyond Laudan and Two Significant Challenges to Demarcation

Although I suggest that Laudan's dismissal of the problem is not justified, I agree with him to the extent that there are significant challenges to the problem of demarcation. I will propose that the challenges are actually greater than what Laudan suggests. I will now examine two difficulties which further complicate the pursuit of a demarcation criterion. First, is the multifarious nature of the sciences. Second, is the historical embeddedness of scientific activity.

That the term 'science' refers to an array of disciplines which are highly diverse and bear little resemblance to one another is well established. Take two examples such as physics and psychology. Although qualified with terms such as 'hard' and 'soft' these disciplines are largely agreed to be scientific. There is little, if any overlap between them when it comes to what are considered to be 'essential' features of a science: content, methodology, and explanatory domain. The differences in content and explanatory domain are connected to one another, and the respective methods of each science are tailored to the specific content each attempts to explain. Physics has, as its explanatory domain, phenomena of all scales from the sub-atomic world to the intergalactic universe. Focusing on one extreme side of the scale, such as astrophysics, the methods involved in the prediction of facts and the development of explanations look very different from those used in psychology. Much of the methodology in astrophysics is theoretical and abstract, done in the form of complex, mathematical calculations. The content to be explained is often phenomena which are extremely distant from human experience and are, partially for this reason, can be considered to occupy an obscure and abstract explanatory domain. The content of psychology is much closer to the human life as its focus is the human mind (or brain). Although the explanations of psychology are still theoretical and, along that vein, abstract, the methodology of psychological investigation is more hands on. The subject matter of psychology is present-at-hand so to speak in a way that a black hole or a pulsar isn't. Direct observation and statistical studies constitute a significant portion of psychological research and the sorts of "pure math" calculations characteristic of physics are not widely present in the field. In addition, psychology (and other social sciences), deal with subject matter which is not disinterested, but is comprised of the same autonomous agents that undertake the studies, namely, human beings. Although it may be possible to find general approaches that can be abstracted out of the idiosyncratic operations of both physics and psychology, the fact remains that the specific details of the methods of each discipline are vastly different.

This presents a significant obstacle to demarcation because it is difficult to find features which are common to such seemingly disparate scientific disciplines. According to Laudan's analysis, a demarcation criterion ought to be able to specify both the necessary and sufficient conditions of scientific adequacy. Ideally, these conditions will hold across the board in all domains of scientific knowledge. When we take the diversity of the sciences into consideration, we appear to be left with the impetus to relativize our demarcation criterion to a certain extent. A pseudoscience that is professing facts that are in conflict with the facts agreed upon in, for example, evolutionary biology, would need to be evaluated in relation to evolutionary biology. This would mean that an adequate demarcation criterion would need to do two things. First, the criterion would need to satisfy Laudan's meta-philosophical requirement to articulate the necessary and sufficient conditions that apply across the board, to all sciences. A second aspect, however, would be for the demarcation criterion to remain sensitive to the inherent diversity of the sciences. The explanatory domain that the pseudoscience is engaged with would need to be matched to the corresponding scientific discipline and the features that are unique to the corresponding science would need to be taken into account. Laudan's requirement would not quite be accurate since it attempts to homogenize and thus ignore the heterogeneity of the sciences.

The problem that arises when the heterogeneity of the sciences is taken into account is that the universality of a demarcation criterion becomes unattainable. To constantly have to work with a criterion which is partially relativized to a particular science means that, rather than searching for one demarcation criterion, we are seeking a catalogue of criteria which are sensitive to the unique features of all sciences. This is significantly complicates the task of demarcation, but is seemingly necessary as the diversity of the sciences is a fact and ought to factor in to a demarcationist approach.

The second difficulty for a demarcation criterion is the historical nature of science. Both the way science is conceived and how science operates changes over time. As we have seen earlier, science was envisioned by Aristotle as being a search for the essential causes of observable phenomena. A scientist was concerned with knowing-why things (*episteme*) were the way that they were whereas it was the craftsman's business to have know-how (*doxa*).[72] To possess knowledge-why, a scientist would not need to have the practical knowledge of the craftsman and, conversely, the knowledge-how of the craftsman would be attainable without knowledge of the elemental causal relations. By around the time of Newton, Galileo, and the scientific revolution this conception of science significantly changed. Experimentation became

[72] Aristotle *Posterior Analytics*

the hallmark of the scientific method. The type of a priori contemplation that preceded the empirical method no longer carried much scientific weight. Looking back on this history, we are prone to think that Aristotelian science would not be taken very seriously in modern day laboratories. That just simply isn't how science is done nowadays. But, the historical point to be observed is that Aristotle was one of the best scientists of his own epoch. On the one hand, we look back and say confidently that Aristotle was doing, by modern standards, bad science. On the other hand, we hesitate to look back and say that Aristotle was no scientist, or that he was a pseudoscientist. This suggests that what constitutes science is historically relative. The history of science is informative about how scientific practice at any time period has been about working with what it has at its disposal—theories, mathematics, technology—in the service of moving forward. We can recall Lakatos' dissatisfaction with the Ptolemaic geocentric model of the universe. Although there was an eventual historical point where Ptolemy's model was not moving forward, or ceased to be "progressive",[73] it was the standard astronomical model for centuries and had success in making predictions. We can look back now and say that the model was false and the corresponding science was bad, but in the absence of a better, competing model, it was the best science of the day. Whether or not the findings of a given period's scientific endeavors hold up is a matter to be settled historically. I propose that what is true of all science from older historical periods is that their elements (methods, conclusions, hypotheses, etc) become informative tools in the modern scientist's repertoire. We might say that what scientific practice looks like today is a product of scientific evolution. That many of the older ways of doing science would seem rash or unscientific in a modern setting suggests that a historical lens has allowed for science to try and better itself: attempting to avoid old mistakes and to continually refine the methods of older sciences.

[73] Lakatos (1978) p.179

The difficulty that arises for demarcation in the historical relativity of science is that a demarcation criterion would be just as liable to historical reformation as the practices of science themselves. Thus the question of demarcation will always be a historically embedded question. I see two questions that are important to ask in light of this historical point: a) why should we think that our response to the question of demarcation is any better off than those that went before? And b) does it matter that our response might not be any better off?

As for a) Of course, we cannot know whether or not a modern day answer to the demarcation issue will fare any better as history passes. Perhaps it is arrogant to suggest that it will. It may be best to hold onto something analogous to the idea of pessimistic induction which suggests that since history continuously falsifies theories that were once held to be true, it is probably best to not consider new theories to be any better off. Laudan seems to lean this way in response to the demarcation problem. On the other side of the coin, thinkers such as Paul Thaagard and Edward Wilson hold that science as a whole has progressed to a point where the likeliness of drastic revolutions occurring is extremely low.[74] From this perspective, our positive conception of the scientific enterprise is as good as it has ever been and so close to ideal that drastic changes to the conception would be unlikely to arise.[75] This would bode well for a demarcation criterion since the risk of its historical contingency would be correspondingly lessened. Whether or not we think that a modern day response to the demarcation problem will fare any better than its predecessors will, I suspect, just be a matter of optimism or pessimism. Perhaps the more important question is whether or not it matters to our investigation into demarcation whether or not our historically relative findings might be doomed. To this I suggest

[74] See Thaagard (1986) and Wilson (1999)

[75] I am here crudely glossing over this issue. It is true that small revolutions occur every time that any part of science is changed. Developments in plate tectonics, dark energy, the discovery of DNA's structure, etc. are all examples of paradigm shifts in the latter half of the twentieth century.

that it is of no serious concern. We might draw an analogy to science itself. Having a greater access to historical texts than any period before us has endowed us with a historical sensibility that is unrivalled. In science, as in philosophy, we have seen theories and approaches rise and fall with the waves of time. As I mentioned above, this sensibility to the trajectory of history furnishes us with the tools to do new (and hopefully better) science or to engage in new philosophy. Even if it turns out that the next century brings an enormous transformation in our conception of science and, correspondingly, demarcation, the efforts of today will still provide an informative view into how science isn't to be done.

Another point that I wish to raise for the problem of demarcation is that of getting clear about the domain of knowledge that is referred to when the problem is raised. When speaking about knowledge in the broad, epistemological sense we are referring to knowledge in all of its senses. Scientific knowledge, although awarded a high degree of epistemic warrant, is only one type of knowledge. Immediate perception or aesthetic knowledge, for example, are other senses of knowledge which are relevant to epistemology, but which are outside of the domain of scientific knowledge. Laudan suggests that it is a more viable intellectual project to focus on the broader epistemological scope. In other words it is more fruitful to demarcate good beliefs (or justified, true beliefs) from bad beliefs without qualifying certain types of beliefs with terms like 'scientific' or 'pseudoscientific'. Laudan seems to be motivated by the view that there is no discernible 'scientific domain' or at least not one whose features can be explicitly articulated. On this point, I disagree. An important step for the problem of demarcation is to rein in focus to the scientific domain of human knowledge. This difficulty is exaggerated because of the first two difficulties that I raised. Science is a multifarious and historically dynamic discipline. Articulating unifying features which span both the inner diversity and the trajectory of history is no easy task. But, nevertheless, it is one that we have very good philosophical reasons to at least attempt.

A further complication arises when we acknowledge the different cultural approaches to unpacking the word 'science'. In the majority of the western world the term includes all of the hard sciences and sometimes the soft sciences. The inclusion of the soft or social sciences is not universally accepted. Humanities, such as history, English, or philosophy are not included as scientific under most circumstances. Looking at the German conception of science, or '*wissenschaft'*, however, provides a broader conception which includes all fields of research and, thus, the humanities are included in the conception. *Wissenschaft* is a term used for any intellectual discipline which uses systematic research. As I mentioned above, the inner diversity of the sciences is a fact which needs to be acknowledged and that a demarcation criterion would need to assess a potentially pseudoscientific discipline in relation to the existing, scientific discipline with which it is in conflict. If our conception of science broadens, as the German does, and includes the Humanities, the inherent diversity of the sciences will augment accordingly. This will further complicate efforts to establish demarcation criteria since more fields will need to factor in to the attempt.

3.3 Hoyningen-Huene's Systematicity

Paul Hoyningen-Huene is a philosopher of science who has looked a little more deeply into these difficulties. His paper "What is Science" concerns itself with both the multifariousness of science as well as the attempt to demarcate what is unique about scientific disciplines as opposed to non-scientific disciplines. Hoyningen-Huene chooses to look at science in the wider sense of *Wissenschaft* and posits that *all* scientific disciplines exhibit a *number of dimensions of systematicity* to a much greater extent than non-scientific disciplines. That science is distinguished by its focus on systematicity is not a novel idea,[76] but an attempt to elucidate a number of dimensions that a vague concept such as systematicity contains is an important step, and a helpful one for my purposes here. Hoyningrn-Huene recognizes eight different, but not entirely separate dimensions of systematicity: descriptive, explanatory, predictive, the defense of knowledge claims, epistemic connectedness, ideal of completeness, knowledge generation, and the representation of knowledge.[77] In all eight of these dimensions the sciences will focus more deeply than non-scientific disciplines. Hoyningrn-Huene does not direct his focus towards the demarcation of science from pseudoscience, but his insights into systematicity are informative for a demarcationist project, as I intend to show in the final chapter of this work.

To substantiate Hoyningen-Huene's views, I will attempt to offer a brief synopsis on each of the eight dimensions of systematicity:

1) Descriptions: these take the form of taxonomies and classifications, for example, in zoology or biology. Cosmology or geology take concern with series of historical events (or singular historical events) and the descriptions often unfold as narratives. Physics and chemistry have descriptions which are tailored more towards "generalized descriptions" where classes of events or processes are described and law-like regularities are articulated. Further, all sciences seek to provide quantifiable descriptions and the ability to do so

[76] Susan Haack pursues a similar line of thought in *Defending Science* where she describes science as using the same kind of rational thinking as in the most basic, everyday thinking, but to a much more sophisticated, or, as HH might say, systematized, degree.
[77] Hoyningen-Huene (2007) p.170

displays greater systematic prowess than a purely qualitative description. The more precise a quantitative description can be, the greater the systematization of this dimension.

2) Explanations: There are two main ways in which sciences use explanations to a more systematic degree than other disciplines and the second is a consequence of the first. The first is that scientific explanations tend to be reductive, in the sense that the causal factors are reduced to their most basic parts, in service of achieving a more concrete understanding of the whole. The consequence of this sort of reduction is that scientific explanations tend to be as exclusionary as possible, attempting to articulate not just one of many possible causes, but the true cause.

3) Prediction: What tends to distinguish systematic predictions from the types of predictions which are made in everyday thinking is the specificity of the prediction and the background knowledge which is in play for the prediction to take place. The example of Einstein's prediction of gravitational lensing is paradigmatic of a highly systematic prediction. The prediction was highly specific and novel. It predicted one logically possible state of affairs. Its systematicity was further emphasized by the all of the background knowledge, namely, the entire theory of general relativity, which was necessary for the prediction to take place. Predictions, making use of generalized descriptions form part of the theoretical and explanatory fabric of a science. Everyday predictions, if they happen to produce a novel fact, are generally considered to be lucky. Other everyday predictions tend to be sort of uninteresting when

compared with the types of predictions that are found in the sciences (ie. the sun will rise tomorrow, the Canucks will win on Saturday, etc.)

4) The Defense of Knowledge Claims: Sciences are also more systematic in their attempts to avoid erroneous assumptions or conclusions. Evidential support is essential for all empirical sciences. The more data which is gathered, the more systematic the science can be in testing, evaluating, and justifying its claims.

5) Epistemic Connectedness: This dimension is described by Hoyningen-Huene in the following way: "The basic idea of the dimension of epistemic connectedness is that scientific knowledge has more articulate connections to other pieces of knowledge than, especially, everyday knowledge which is more loosely structured….knowledge that has more (explicit) connections to other pieces of knowledge is to a higher degree part of a system of knowledge."[78] Sciences will tend towards systematization insofar as there is increased agreement between accepted facts.

6) An ideal of completeness: there is, perhaps, an obvious distinction here between science and everyday knowledge. This feature seems to be present in every scientific discipline, no matter how diverse. Chemistry seeks to understand and discover all of the elements, physics seeks to discover all of the most fundamental particles and forces in the universe, history seeks to provide the most complete account of past events it can, etc.

7) Knowledge generation: This dimension was essential for progressive research programmes in Lakatos' thinking.[79] Sciences attempt to discover and predict

[78] Hoyningen-Huene p.175
[79] Lakatos (1970)

new facts about the world, or enhance our understanding of previously known facts. The manner in which each discipline carries this task out will, again, greatly vary. Historians will scan archives, Geologists will gather and compare samples, physicists will change the parameters on an experiment, etc.

8) The representation of knowledge: Bodies of scientific knowledge have a systematized structure which begins with either general principles, axioms, etc. and moves in a direction which increases in specificity. This is directly tied to the internal epistemic connectedness of a scientific discipline. An organized representation of the body of knowledge is also functional, as it can allow for a clearer view into gaps or problems within the body of knowledge.

In explicating the eight dimensions of systematicity, Hoyningrn-Huene offers a way to conceive of unification throughout the inherent diversity of the sciences. A virtue of this approach is that it remains sensitive to the uniqueness of each discipline, that is, it does not attempt to dissolve the unique features of each science. For Hoyningrn-Huene, "the central problem of the demarcation of science and pseudoscience appears to be the inner diversity of the sciences."[80] In identifying these dimensions and in trying to convey a common thread of systematicity between them, Hoyningrn-Huene seeks to preserve the inner diversity, but suggests that sciences are related in terms of family resemblances:

> "The different branches of learning are, so it is claims, indeed all more systematic than other corresponding forms of knowledge. But the relevant concept of systematicity is split up into eight different concepts, depending on which aspect of science is in focus, and the concept co-varies further with different disciplines and sub-disciplines. Thus, the

[80] Hoyningen-Huene p.179

unity of science consists in family resemblances that hold between different branches of science, resulting in a very loose network represented by the abstract concept of sytematicity."[81]

It accomplishes this by pointing out that the way each dimension of systematicity is achieved in a respective science will be different. To be as systematic as possible in explanations, for example, is an abstract requirement that, although applicable across the board, will be hashed out in the details of each science. Physics will provide its own brand of systematic explanation which makes use of the language of physics. This means that to justify a new finding about the Higgs Boson, for instance, the explanation will need to appeal to a series of other well established facts in the field of physics. If the explanation fails to do this, or does it to an unsatisfactory extent, the scientific nature of the explanation will be suspect. In the field of history (to use Hoyningen-Huene's focus on Wissenschaft) an explanation of a given historical event will need to be sensitive to all of the other well-established bits of historical knowledge which would be relevant to the event to be explained. Although the manner and details of the explanation will differ greatly between physics and history, the unifying systematic dimension seems to be plausible, as a failure to meet it in either discipline would call a claim's legitimacy into question.

All sciences, according to Hoyningen-Huene, strive to continuously refine certain dimensions of systematicity. A scientific discipline that does the reverse, that is, strives to be less systematic in any of these dimensions would not be doing science, or at least, not be doing good science. It, of course, may turn out that some seemingly cranky research program might prove fruitful in the long run. This is the sort of historical point that Lakatos made about the

[81] Ibid. p.170-1

methods of research programmes. That the cranky research would eventually prove fruitful, however, would likely suggest that, assuming it wasn't just lucky, though it seemed cranky and unsystematic, in some sense, at the time, it actually turned out to advance some dimension of systematicity.

Perhaps one of the most significant dimensions of systematicity for the problem of demarcation is the defense of knowledge claims. There is, in a modern, philosophical conception of science a tending away from talking about absolute truths and tending towards a sort of perennial reflexivity in science. No conclusions are safe from reformation, no matter how well founded they appear to be. The history of science has furnished so many examples of theories that were held to be truly carving nature's joints. Of course, as science progressed, these theories were transformed either through falsification or systematic refinement. While Kuhn and Lakatos have both made the point that some scientists who are committed to a certain paradigm or research programme will stubbornly hold onto their research, even in the face of apparent falsifications or rival theories, too much stubbornness is often an indicator of either bad science or pseudoscience. To constantly be on the defense about one's views in the face of falsifying instances, more confirmed rival theories, or a lack of evidential support suggests that there is blatant irrationality afoot. Hoyningen-Huene makes a similar point about pseudoscience.[82]

3.4 Maxwell's Aim-Oriented Empiricism

Nicholas Maxwell, in his writings about a philosophical conception of science suggests that to be self-reflexive and self-critical is the hallmark of science. In his paper, "Aim-Oriented

[82] Ibid. p.180

Empiricism" Maxwell suggests that the findings, explanations, and conclusions of scientific explanation are not the only elements that need to be carefully scrutinized, but that these elements are couched in a hierarchy of metaphysical assumptions which also ought to be reflected upon. An example of a basic metaphysical assumption behind scientific investigation is physicalism. We can recall that Carnap defined physicalism as an approach to science that sees every term which is used in the sciences as being reducible to physical language.[83] It is perhaps odd to think that something as basic as physicalism could possibly be revised. It would seem that all scientific explanations would be dissolved should physicalism go away. It is not Maxwell's claim, however, that this is a likely event, but that a philosophically reflective science ought to be clear about all of its most basic of assumptions. That nothing should be taken for granted or blindly held on to, according to Maxwell, is the golden rule for any scientist.

Maxwell's hierarchical system is based on what he sees to be seven levels of assumptions that take place within science, beginning with the highest level of generality and descending into the specific empirical observations that scientists make. As we move from higher level assumptions to lower level assumptions, the likeliness of error increases. This is to say that the higher level, more general assumptions are less contentious and not as likely to be revised as those in the lower levels. Level 7, the highest level, is the thesis that the universe is at least partially knowable. This thesis is rather uncontentious, but is the foundational condition for science to operate at all. Since science aims to come to know facts about the universe, the inability to at least partially come to know the universe would preclude the activity of science. Level 6 states that the universe is meta-knowable, meaning we can assess our methods of gaining knowledge about the universe. A consequence of the level 6 thesis is that, supposing it is true, it

[83] Carnap (1936) p.466

is likely that there are ways in which our method of gaining knowledge could be improved. Level 5 states that the universe is comprehensible. To understand the universe completely is the ideal aim of science and this ideal aim is undertaken under the assumption of level 5. It is at level 5 that a discrepancy between Maxwell's thinking and Hoyningen-Huene's thinking arises. Where Maxwell holds that methodological reflexivity in general is essential for the activity of science, Hoyningen-Huene professes one specific methodological maxim, systematicity. An explicitly reflective science, according to Maxwell, would need to be open to the possibility that any methodological maxim, including systematicity, used for comprehending the universe could be open to revision or transformation. Level 4 states that the universe is physically comprehensible, meaning, physicalism can be taken to be true. Level 3 proposes that our current "blueprint" of physicalism is the best one. Level 2 regards the acceptance of our "blueprint's" physical theories and level 1 concerns the baseline, empirical data. It is clear that there a lot of avenues for reflection in this model and any change that is made in the upper structure of the hierarchy will have a prolific effect on assumptions below it.[84]

In noticing the perceived preference of theoretical physicists to accept and hold on to a highly unified theoretical framework, Maxwell suggests that this preference is an implicit metaphysical assumption in the way that theoretical physics operates. Of course, the preference for comprehensive theories and desire for unified theories is justified. As Hoyningen-Huene emphasized, both the development of robust theoretical frameworks and the relations between these frameworks are significant factors in systematizing science and, in doing so, expanding our knowledge. Be that as it may, as there are several possible avenues for less unified and ad hoc, but more empirically successful theories, it is curious that these avenues tend to be avoided.

[84] Maxwell p.183

Maxwell says that "this persistent preference for and acceptance of unified theories, even against empirical considerations, means that physics makes a persistent untestable (metaphysical) assumption about the universe: the universe is such that no seriously disunified, *ad hoc* theory is true."[85] It would be in the service of honest intellectual work to make each and every implicit assumption explicit and to engage critically with them. It ought to be noticed that similar implicit assumptions can be found within Hoyningen-Huene's dimensions of systematicity. Hoyningen-Huene states that one of the dimensions is the tendency to move towards completeness or unification. The point to hold onto, as far as I can see, is that whether or not unification is a true dimension of what an ideal conception of science would be (if there ever could be such a thing) is not essential. It is a strong aspect of the modern conception of science largely because there are an overwhelming number of scientific explanations and descriptions which seem to tend towards an ideal of completeness. I believe Maxwell is correct to emphasize the need for science to be as self-reflexive as possible, even when it comes to its metaphysical assumptions. This may mean that an ideal of theoretical unification may eventually lose its status as a dimension of sytematicity as Hoyningen-Huene conceives it. This feeds into the historical point that I made earlier: that both our conception of science and, correspondingly, our conception of demarcation are historically embedded investigations and, like all historically embedded intellectual investigations, cannot claim to be immune from reformations.

In this chapter, I aimed to bring to light many of the challenges and complications that the development of a demarcation criterion faces. My aim was to focus more upon the meta-methodical aspects of an investigation into a demarcation criterion without actually developing a demarcation criterion. Laudan raised some foundational considerations, but I argued that there

[85] Maxwell p.181

was more to the story. The multifariousness of science and the historical embeddedness of science are two major challenges for a demarcation criterion. Despite these challenges, I have suggested, contrary to Laudan, that the demarcationist project is viable and ought to be undertaken. Both Hoyningen-Huene and Maxwell have made progress in addressing these difficulties. Hoyningen-Huene as offered a way of envisioning similarities that exist in all sciences, no matter how diverse. Although it is not clear that a handful of family resemblances can act as an effective way to unify diverse sciences, Hoyningen-Huene's proposal is helpful in the sense that the diversity of the sciences are wholly preserved in his thinking. Maxwell's view suggests that implicit metaphysical assumptions may, at times get in the way of taking a discipline seriously. An implicit assumption about what constitutes good science, while not necessarily incorrect, ought to be made explicit. In the concluding chapter, I will turn to a concrete case to illustrate the demarcation problem and suggest that we ought to broaden our conception of the demarcation question and ask whether or not the problem itself might not have an inner dynamic that has not yet come to the surface. This is the dynamic between theoretical and practical science.

4

The Empirical Success of Traditional Chinese Medicine and the Implications for the Problem of Demarcation

In the previous chapters, I sought to sketch a representation of the current state of the demarcation problem. From the thinking of Aristotle, through Carnap, Popper, and Lakatos, to the criticisms of Laudan and into the approaches of contemporary thinkers such as Hoyningen-Huene and Maxwell, the attempt to draw a conclusive divider between science and pseudoscience has proven to be a, largely, unsuccessful task. If we heed the words of Laudan, we ought to admit that finding sufficient and necessary conditions for scientific status is unlikely and, thus far, attempts to do so exhibit more bark than bite. The word 'science' applies to a wide array of disciplines that operate under a variety of methods. This diversity alone poses a substantial obstacle for attempts to find a demarcation criterion which can apply across the board. Still, however, there is something about scientific endeavors which seems to command greater epistemic authority than pseudoscientific endeavors. Astronomers resent when their field is mistakenly lumped together with astrology. The explanations of astrology are often so vaguely formulated that the astrologer can use any outcome to justify his claim. This vagueness of explanation results in an immunity from falsifiability. We can recall that, for Popper, all scientific claims must be falsifiable. Although, as we have discussed, falsifiability is not sufficient for a demarcation criterion, it seems to be the predominant justification for rejecting astrology as a science. Taking another example, touch therapy, as a form of medical treatment, seems to be pseudoscientific for different reasons. Although it makes claims which are falsifiable, touch therapy is based upon the belief that there are energy fields around human beings that are strengthened when human contact is introduced.[86] This belief doesn't cohere with the view that modern physics has of the universe or, at least, it doesn't cohere in any sophisticated way. Touch therapy takes the energy field as a given and operates on the basis of that core belief. In this case, our intuitions about the pseudoscientific status of touch therapy are largely rooted in the discipline's lack of critical scrutiny and lack of concern with how its foundational beliefs fit into the well-established, scientific view of the universe. There are different reasons to reject the scientific legitimacy of disciplines and a sweeping criterion doesn't look like the answer.

It is tempting to ask how essential empirical success is to the scientific status of a discipline. Could there be an adequate justification for calling a pseudoscientific discipline, in the absence of competing explanations, scientific based purely on empirical success? For example, if touch therapy had great empirical success despite the lack of coherence with well-established science and no other physical explanation could account for the success, where would that place touch therapy, according to a demarcation criterion? It may be thought that there is a scientific explanation for the success, but it just hasn't been discovered or established. Touch therapy, then, would still be operating on a pseudoscientific foundation, but managed to get lucky and we ought to await the findings of legitimate scientific investigation to find out what is the true source of the empirical success. Since epistemic legitimacy is connected directly to the scientific status of a pursuit, calling touch therapy pseudoscientific is tantamount to stripping it of epistemic legitimacy. Is it possible for empirical success to trump this connection between scientific status and epistemic legitimacy? The question might be put in a different

[86] See Newbold and Roberts 2007

way. Is empirical success sufficient for epistemic legitimacy in the absence of scientifically rigorous explanations?

In this concluding chapter, I will use the example of Traditional Chinese Medicine (TCM) to address this question. Depending on whose demarcationist approach is used, TCM can fall on either side. TCM makes falsifiable claims; progresses as a discipline; uses evidence and experimentation; and is empirically successful; however, TCM also relies on a theoretical framework which is fundamentally different than the sub-atomic makeup of the physical universe that today's physics bases its causal explanations; TCM is non-reductionist in the sense that it makes little to no effort to study its pharmacological compounds on a molecular or chemical level; founds its approach on philosophical concepts which have been passed down through tradition and is unwavering in these foundations; and is unsystematic as an intellectual discipline in the sense that the transmission of knowledge is largely unstandardized and will vary greatly from teacher to teacher.[87] TCM is an interesting example to use in the demarcation debate because it has a good empirical track record while exhibiting many features which are distinct from contemporary western medicine (CWM). In the following chapter, I will bring to light some details regarding both TCM's unique features as well as the discipline's empirical success. On the unique, theoretical side, I will examine: a) the notion of the *qi* and, b) the theory of *yin* and *yang*. Both of these features derive from traditional Chinese philosophy and are essential to the practice of TCM, but don't necessarily agree with the views of CWM. On the side of TCM's empirical success, I will examine some recent research into a few of the pharmacological compounds that are utilized in TCM. In addition, I will look at a specific case where TCM appears to exhibit greater empirical success than CWM. This specific case is in the

[87] See Leung 2008

treatment of osteoarthritis. I will explore the recent, relevant research that explores the efficacy of TCM in the treatment of osteoarthritis. Beyond this case, there is evidence to suggest that TCM is effective in the treatment of other chronic conditions.

A few words need to be said about the connection between medical practice and scientific investigation. It might be contended that medicine is not, strictly speaking, a science. Certainly medicine is informed by scientific developments, but the same can also be said of molecular gastronomy or bridge building.   Are we to consider every discipline that is informed by scientific developments a science? A key difference can be pointed out between the terms "science" and "scientific". The former term refers to the disciplines which possess the features that a demarcation criterion determines to be scientific, and fundamentally aim to discover new facts about the world. The latter term can refer to any practice which exhibits the features of science, but does not have the same ultimate aim as a proper science. For example, a sound engineer in a recording studio can be said to approach his craft scientifically, while obviously not being engaged in the aim of uncovering new facts about the world. Saying that the sound engineer takes a scientific approach may mean that he has a log of frequency measurements that he refers to when mixing a drum track on a recording. Having measured a variety of different cymbals in many different mix combinations, this frequency log allows the engineer to accurately predict the mix combination that will have the clearest sound for any drummer's equipment. In this case, the scientific approach allows the sound engineer to be more proficient at engineering sound.

The case with medicine may be similar to the case of sound engineering. I would like to suggest that there is a much closer connection between medicine and science, however.   The pragmatic element of demarcation in health care can be seen if we consider what medical practices are covered by health insurance. In Canada, where we are fortunate enough to have nearly universal health care, acupuncture, a treatment unique to TCM, is not covered under health care plans. There is a demarcation criterion at work here, the determinations of which govern which practices are supported and which are not. I feel that this indicates that the demarcation question needs to extend its gaze into the practical realm of science as well. Thus it is not only acceptable to approach medicine from a demarcationist perspective, but due to the pragmatic importance of the demarcation question, this approach is desirable.

A deeper historical connection between medicine and science can also be sketched. Van Fraassen in *The Empirical Stance*[88] points out that the origins of the term "empirical" can be traced back to a school of physicians who professed that medical progress was to be made on the basis of experience and observation. The school was called the E*mpirici* rather than *Dogmotaci* or *Methodoci*.[89] Empiricism began in the spirit of openness to changing one's course if the evidence suggested it, without any worry of violating the dogma, that is, the accepted theory, of the time. For the *Empirici*, it was the collective experience of practitioners that was the basis for progress and the improvement of medicine. Thus from the beginnings of empirical thinking, the bedrock of modern science, is an embrace with medicine.

4.1 Philosophical Sketch of Traditional Chinese Medicine

TCM is largely focused on preventative therapies, diet, and lifestyle and can act as an effective measure for reducing the intervention of western medicine which is aimed more towards treating diseases and ailments once their symptoms are present.[90] While both TCM and CWM share the same foundational goal, that is, the health of the patient, there are significant

[88] Van Fraassen (2002)
[89] Ibid. p.32
[90] Leung

differences in the visions of how this goal is to be achieved. Perhaps the greatest difference between the two approaches is the non-reductionist approach of TCM.[91] While CWM is mainly concerned with localized conditions and treatment for pain at the site where the pain is present, some proponents of TCM object that western medical training does not allow the doctor to "see the whole body".[92] Modern advancements in drug treatments are virtually all at the microbiological level. While CWM agrees with TCM that "the body is a system that has built-in stability which constitutes health"[93] TCM conceives of the human body in a way which is, arguably, more systemic. The approach of TCM is top-down and assesses the individual as part of a larger process which includes the individual's environment, habits, and lifestyle. These factors which are exogenous to the body are of as much, if not more relevance for a diagnosis than internal factors. This is not meant to imply that CWM does not acknowledge a systemic vision of bodily health. Of course lifestyle, habits, and environment contribute in many diagnoses in CWM. What is characteristic of the treatments of CWM, however, is the pharmacology, which tends to treat isolated issues within the body, and often at the expense of serious side effects.

The *Qi* is a concept which is essential to TCM. Its translations vary from "vital energy" to "lifebreath". Big Leung defines the concept as:

> The binding force of life (including heaven, earth, and humans). Imbalance in life or an emptiness of *qi* reduces the body's ability to defend itself; the flow of *qi* can be blocked, and thus illness may result. Food is believed to be a source of *qi*, and Traditional Chinese Medicine is considered food. The Chinese character is made up of two parts, the top part

[91] See Balin and Battersby 2009
[92] See Renzong 1981
[93] Balin and Bettersby p.196

indicating ‘rising vapour’ and the lower part meaning ‘rice’, so that *qi* literally means ‘vapour rising from rice’.[94]

To the practitioners of TCM, the *qi* is found everywhere in the universe and is omnipresent. Diagnoses aim to detect disturbances in a patient’s *qi* and doctors recommend treatments based on what is believed to cause the disturbance. Since *qi* flows through all things, a blockage of it in the human body signifies a disharmony with the greater system of which the human is a part. This disharmony causes a susceptibility to disease. Much of TCM is concerned with precluding this disharmony and focuses on preventative medicine and therapies.

From the point of view of CWM, the *qi* is a questionable concept. There is no correlate in western science. *Qi* is not detectable, quantifiable, or measurable by the sorts of laboratory style methods of CWM. *Qi* is inseparable from the flow of the Universe and has no physical manifestation as a separate entity that can be pointed at and categorized. The *qi* can be understood in an analogous manner to the concept of motion. There is no observable, general motion that exists independently of its specific instantiations. We abstract the concept out of the movements of phenomena. In a similar way, the expectation to observe the *qi* independent of its instantiations would make no sense. As a typical diagnosis in TCM will involve the *qi*, the typical diagnosis will contain features which are not based on the sort of quantifiable evidence that is characteristic of CWM. CWM makes use of quantifiable evidence all the time in the form of various testing, the measurements of blood content, etc. TCM tends to place greater emphasis on holistic sources of evidence such as lifestyle, environment, diet, etc. CWM, of course, considers these factors as well, but these qualitative factors are often instrumental in the sense that they lead physicians to the appropriate tests in order to establish more quantifiable

[94] Leung p.209

conclusions. We can remember the work of Carnap here which sought to hold onto empirically confirmable propositions and weed out non-empirical "metaphysical" ones. According to Carnap's view, the *qi,* if it is to be a predicate that is empirically meaningful, must be confirmable (at least under some possible circumstances) in the sense that it can be reduced to, or defined in terms of, basic observable predicates. Although the *qi* is not quantifiable, it seems to be, according to TCM, confirmed on the basis of all diagnoses of the practitioner.

Balin and Battersby suggest that TCM has its own explanatory paradigm.[95] This indicates that the fundamental entities and concepts to which the explanations refer are incommensurable with western medical explanations. Whereas the western paradigm might explain the cause of a cold in terms of germs, viruses, or other microbial entities, the TCM paradigm will explain it in terms of temperature imbalance and a blockage of *qi*, where even the term 'temperature' is conceived in a fundamentally different way than the western usage. The explanatory paradigm of TCM and the belief structure which undergirds this paradigm have, however, given rise to a robust medical tradition, which, as we will soon discuss, has been and continues to be empirically successful. Before moving on to the successes of TCM, it is worthwhile to mention more about the saturation of TCM with its founding philosophies.

The philosophical concepts which are fundamental in TCM are the *yin* and *yang.* *Yin* and *yang* theory is a theory of balance, harmony, and equilibrium. The theory holds that the universe is an interconnected system in a constant state of flux and that this state of flux, or perennial change tends towards an equilibrium of opposing forces. Natural processes, according to the theory, demonstrate harmony between *yin* and *yang.* From the changing of the seasons, to the cycles of life and death in plants, to planetary motion, nature displays its mastery with

[95] Balin and Battersby p.196

inexhaustible grace. Human beings are as much a part of the universal system as any entity and the theory prescribes that human beings endeavor to cultivate a balance within themselves and their relationships. The conception of health in TCM is directly tied to this theory. A healthy person is a person whose *yin* energy and *yang* energy are in a state of balanced harmony. Thus, the ultimate goal of all therapies in TCM is to restore this balance and all endeavors of the practitioner are motivated by the theory. Originating in the *Tao te Ching*, *yin* and *yang* theory derives from Taoist philosophy which envisions the human being to be a microcosm of nature itself. All healthy relationships, for the Taoist, achieve, or at least approximate, a balance analogous to the balance and harmony which can be observed in all natural processes. The primary relationships that TCM is concerned with are the human and the environment, the human and the other human(s), and the human and his/herself.[96] Maintaining a balance in these forms of relationships is essential and a failure to do so creates a vulnerability to illness. Balance is to be kept in mind in all aspects of human activity. Temperance, or the avoidance of extreme forms of behavior is prescribed. Diets should consist of a balance of sweet, salty, sour, and spicy. Excessive emotions ought to be avoided to prevent unnecessary stress. This conception of health extends to social and cultural bonds as well. Virtuous and humanistic behavior is seen to be the backbone for a healthy and harmonious community. With this holistic conception of health in mind, a conception which is inseparable from traditional Chinese philosophy, it makes sense why a reductionist, mechanistic way—a way that is common in western medicine—of looking at the human body does not fit into the TCM framework. For some TCM practitioners, a

[96] Leung p.209

reductionist approach is backwards and the attempts to synthesize TCM with western scientific approaches will amount to TCM's dissolution.[97]

Certain symptoms in patients will refer to a deficiency or excess of either *yin* or *yang*. For example, a red complexion generally signifies an excess of *yang*, while white complexion is *yin*. Similarly, a floating pulse is indicative of *yang*, whereas a deep pulse is *yin*.[98] Just as symptoms represent a certain imbalance, the herbal concoctions that are prescribed are, so to speak, *yin-yang* recipes. Each herb, spice, or other medicinal component has its own *yin-yang* signature that will either dissipate the excess or restore the deficiency in the patient with the aim of restoring equilibrium. The fundamental theoretical background may seem to be an oversimplified view of the human body, especially when compared with the plethora of detail and categorization within human anatomy and microbiology in the western medical tradition. Looking at pharmacology, however, the subtlety and complexity of TCM shows itself. "Western herbal medicines are often standardized extracts of single herbs used for particular conditions. For example, valerian and St. John's wort are used for sleep aid and mild depression. In comparison, traditional Chinese medicines are decoctions of mixtures of up to 20 herbs that are customized for each individual patient."[99] It is the practitioner's pharmacological approaches of TCM that perhaps has generated the most interest by CWM. As the process involved in trial testing and analyzing single-herb extracts is a complex event, to do the same for TCM compounds is an enormous task and, in most cases, hardly feasible. As more herbs are introduced into a compound a complex will be developed where the herbs work together through "mutual accentuation, mutual enhancement, mutual counteraction, mutual suppression, mutual

[97] Renzong p.37
[98] Leung p.57
[99] Yuan and Lin p.191

antagonism, [or] mutual incompatibility."[100] Beyond the complexities of the compounds themselves is the individualization of the prescriptions in TCM. Although there are commonly prescribed herbal remedies, compounds are mostly specific to the individual patient and are not necessarily unstandardized. It is in the pharmacology of TCM, however, that the empirical success is most prevalent.

4.2 The Success of Pharmacology and Acupuncture in the Treatment of Osteoarthritis

Before looking at the results of studies which affirm the empirical success of TCM, a few points about the methodology of these studies need to be addressed. A charge of circularity may be raised against this thesis since the justification for TCM's scientific efficacy can be seen as couched within a perspective which assumes that there are particular methods (ie. the scientific method) that are required for justification. Thus by using an established scientific method to carry out these studies of TCM we have, from the outset, established a bias towards a particular point of view.

My response to this charge is that one must start from somewhere. Although the aim of this thesis is to show that the question "what is science" requires an ongoing dialogue which is critical and explicitly reflexive, it is important to acknowledge that this does not mean starting from scratch. There is epistemological substance to the scientific method and it seems reasonable to assume that is a viable approach to take in the study of TCM's efficacy. Naturally, the conclusions of these studies are subject to familiar criticisms. A limited sample size of patients makes for difficulty in making general claims. The bias of certain groups of patients towards a certain medical tradition will inevitably diminish the objectivity of the results,

[100] Ibid. p.192

particularly in the anecdotal evidence in favor of a certain form of treatment. I suggest the reader use his/her discretion when choosing whether or not to accept the results of the studies mentioned below. I realize the limited scope of the studies and that they are carried out with a certain methodology that this thesis attempts to get clearer about. Nevertheless, I feel that the case for TCM's empirical success is persuasive and I do not see any significant problem in using the results of these studies as support for this success.

Among the conditions that the remedies of TCM appear to be having greater degrees of success than western treatments are osteoarthritis, bronchial asthma, atopic dermatitis, and irritable bowel syndrome. As stated earlier, TCM displays most of its empirical success with chronic conditions. In addition, the herbal treatments are as much targeted towards preventative therapy, aiming to preserve health and preclude disease or illness. TCM treatment is not as successful as CWM when it comes to acute conditions or infectious diseases.[101] Drug treatments in the western paradigm are generally fast acting and predictable which means that a localized problem can be targeted and dealt with in a timely manner. A similar remark can be made about invasive surgery, which, aside from the relatively minor degree of invasiveness found in acupuncture, is absent from TCM. Chronic conditions, on the other hand, require extended treatment, sometimes throughout the patient's entire life. It is also a tendency of CWM drug treatments to come with an array of undesirable side effects. These side effects are often a great impetus for patients to seek alternative forms of medicine, especially when, in the case of chronic conditions, the patients need to take these drugs for extended periods.

There is, perhaps, an obvious problem with assessing the empirical success of preventative treatments. In order to assess the success of a preventative treatment, such as a

[101] Ibid. p.191

prescribed diet and daily ingestion of a specifically tailored TCM concoction, we would have to have knowledge about something that we can never have knowledge about. We would have to know whether or not the patient, supposing they never engaged in the prescribed treatment, would have suffered illness as a result. If patients who follow preventative TCM prescriptions do go through life with a relatively clean bill of health, it can never be clear that the preventative therapies are actually preventing anything. There are, however, strategies for attempts to assess the efficacy of preventative treatments. But instead of considering these, I will, rather, turn my attention to the treatment of chronic conditions. There is evidence to suggest that, in the treatment of certain chronic conditions, TCM can be more effective than CWM. One specific case that I will address here is the treatment of osteoarthritis.

Osteoarthritis is a chronic condition which affects the joints and causes stiffness, consistent pain, swelling, and, in some cases, dysfunction. As it stands, there is no known cure for the condition and thus osteoarthritis is, internationally, one of the most common causes of pain and disability.[102] CWM treatments seek to manage symptoms, by offering some relief of pain and by delaying the progression of the disease. The most common avenue for pain management is non-steroidal anti-inflammatory drugs (NSAIDS), however, side effects resulting from NSAID treatment such as gastrointestinal disorders prevent these drugs from being sustainable solutions.[103] Recent studies have indicated that acupuncture can be a more effective means of treating these symptoms.[104] The TCM diagnosis of osteoarthritis attributes the cause

[102] National Collaborating Centre for Chronic Conditions, Osteoarthritis: National Clinical Guideline for Care and Management in Adults, Royal College of Physicians, London, UK, 2008, http://publications.nice.org.uk/osteoarthritis-cg59

[103] See Silverstein, Faich, and Goldstein (2000)
[104] See Cao, Zhang, Gao, and Jiang (2012)

mainly to kidney deficiency, blood stagnation, and the retention of damp cold in the knee.[105] These three main causes seem to be independent of one another and patients whose condition is diagnosed as being attributable to retention of damp cold will have a condition that manifests differently than a patient whose condition is attributable to kidney deficiency. For arthritic conditions caused by retention of cold, a process called *moxibustion* is commonly used, where the herb *Artemisia argyi* is burned atop the acupuncture needles at the site of the acupoints.[106] Although the mechanisms are not clearly understood, clinical trials suggest that acupuncture is effective for the relief of some of the symptoms of osteoarthritis for certain patients. In TCM, as the specific course of treatment is generally tailored to each individual, osteoarthritis may be treated with acupuncture, cupping, moxibustion, electrical stimulation, or any combination of the four. A relevant recent study has begun to explore the effectiveness of using thermal laser acupuncture which utilizes warming lasers to replicate the effect of acupuncture needles.[107] The study found that in patients that were diagnosed with a yang deficiency, or retention of damp cold, the therapy was effective in increasing the function of the joint. In the other classes of patients, the results were minimal and inconclusive. Studies of this sort have not, thus far, been widely undertaken and, from the perspective of CWM, to legitimate TCM's empirical success, more similar studies as well as larger scale studies need to be pursued. There seems to be promise for TCM's merit in the treatment of other chronic conditions, however. For example CM medicinal compounds are being studied in the treatment of Alzheimers disease,[108] schizophrenia,[109] and depression.[110]

[105] See Wang, Wu, Zhao, Zhang, Shen, Huang, Lao (2013)
[106] Ibid.
[107] See Wang, Wu, Zhao, Zhang, Shen, Huang, Lao (2013)
[108] See Zhi-Kun, Hong-Qi, and Sheng-Di (2013)
[109] See Rathbone, Zhang, Xia, Liu, and Yang (2005)

Another virtue of the pharmacological approach of TCM is the absence of long-term side effects which are seen in substantially greater numbers with western pharmaceuticals.[111] As Yuan and Lin indicate, the empirical success of TCM's pharmacology does not, in and of itself, suggest that TCM is to be considered scientific. What they recommend is that interdisciplinary studies are to be carried out, where the rigorous analysis of western science is applied to the herbal compounds of TCM. To Yuan and Lin, the success of TCM ought to be utilized as a wealth of potential for novel, standardized western medicines. It is argued that TCM's pharmacology is lacking scientific rigor in the following areas: regulation of preparation, chemical standardization, biological assays, animal models, and clinical testing. Since the remedies are sourced from natural herbs and tend to not be subject to mechanical extraction, the chemical composition and quantities of active biological compounds are highly variable depending on the plant sample. Further, as the concoctions work by mutual interaction of the various herbs, there ought to be greater understanding at the microbiological level about these interactions. Finally, once the chemical fingerprints of the active compounds are known and their biological interactions understood, exhaustive animal and clinical trials need to be carried out.

4.3 Distinguishing Theoretical Demarcation from Demarcation in Practical Science

It is now worth asking about the value of empirical success in the context of the demarcation problem. TCM offers a competing theoretical framework in the field of medicine

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[110] See Yang, Rong, and Xi (2012)

[111] Yuan and Lin p.191

and TCM's empirical success is often present where CWM is inadequate, particularly, as I have indicated, in the treatment of some chronic conditions. Laudan suggested that a requirement for the demarcation question is to be sensitive to what, historically, has qualified and what, in contemporary society, does qualify as scientific.[112] Thus in laying down scientific/pseudoscientific judgments, demarcation needs to consider those disciplines which offer competing explanations/methodologies to the prevailing scientific disciplines.

Now that some of the relevant background is in place there are a few related questions that need to be asked here. Is TCM a pseudoscience? Furthermore, if TCM is a pseudoscience, does that make its empirical success any less valuable? An initial remark needs to be made about the distinction between theoretical science and scientific practice before we look at the scientific status, or lack thereof, of TCM. All of the demarcation criteria that I have explored throughout this work are targeted at the level of theoretical science. I have affirmed throughout this thesis, however, that the demarcation question has vast pragmatic significance. This pragmatic significance is inseparable from the demarcation problem, precisely because science has *applications* in every nook and cranny of our modern world. Scientifically informed practice is the domain where the demarcation problem displays most of its normative force. Of course, practical science is strongly tied to the founding theoretical science, but fundamental theoretical principles do not need to be correct, or even well-established for applied science or lower-level science to proceed. Thus, I propose that a demarcation criterion at the level of practical science is essential, even if it results in an internal tension between the findings of a higher-level, theoretical demarcation criterion and a lower-level practical criterion.

[112] Laudan (1983)

As it has been shown in the previous chapters, there is a lack of agreement about what the essential features of a science are. According to Carnap, scientific facts need to either refer or reduce to phenomena which are empirically observable or which could, under possible circumstances, be observed. TCM offers explanations which appear to meet Carnapian requirements. Both the *qi* and the *yin* and *yang* theories, theories which are essential explanatory foundations for the practical unfolding of medicinal practices, refer to phenomena which are not observable, but are confirmable through the theory's observable implications. Further, successful therapies provide concrete tests which substantiate TCM. Empirical success is an important consideration here because a consistent track record in certain domains, chronic conditions for example, seems to substantiate the theoretical picture of TCM.

According to a criterion of falsificationism, TCM does seem to pass the test as well. In the domain of practical therapies and prescriptions, TCM makes falsifiable claims. In many cases, these claims could be considered *risky* and would thus be afforded the approval of Popper. In the prescription of treatments that divert from treatments characteristic of CWM, TCM can perhaps be said to exhibit boldness. The empirical success of TCM is largely rooted in the discipline's ability to treat medical conditions. The therapies and prescriptions that give rise to this success can be formulated as bold, falsifiable claims which hold up under tests. If a practitioner prescribes a concoction of ten different medicinal compounds for a patient and states that if taken twice a day for one week, the patient will experience a significant reduction in acid reflux, the practitioner is making a falsifiable claim. With every successful treatment, TCM is putting falsifiable claims to the test. Beyond the falsifiability of its claims, it seems as though empirical success is cause for rational acceptance of TCM. In bringing our view down to the level of medicine in practice, empirical character is essential. If a well-established course of treatment ceases to be successful, it is rational to try a new course. CWM is an approach that is widely successful, but also, in many cases, unsuccessful. That TCM offers an alternative which has empirical results in certain cases is a substantial ground for rational acceptance.

If one were to delve deeper into the theoretical foundations of TCM and sought to apply the falsifiability requirement there, the story may go differently. Looking at the *yin yang* theory, the foundation for TCM's explanatory paradigm, it may be tempting to draw an analogy to the theories of Adler or Marx. Taoist philosophy posits the *yin yang* theory not so much as a hypothesis that is to be scrutinized, but as a description of universal balance. Every event is seen as confirming evidence for the theory. I have yet to come across literature where the author adopts a critical stance towards *yin yang* theory and considers it to be a conjecture that could be overthrown by falsifying evidence. Nevertheless, the theory does have predictive power, as is demonstrated by TCM's success at the therapeutic level. Because of the therapeutic predictions, there do seem to be grounds for refutation. The theory is, thus, less immune to falsification than Adler's was. On the theoretical level, then, it is not clear whether or not TCM meets falsifiability requirements. If the theoretical foundations are viewed as a conjectured hypothesis that could be refuted, then the predictions made through therapeutic prescriptions seem to be adequate as critical tests.

By Lakatosian standards, one might consider the theoretical foundations of TCM to be akin to the "hard core" assumptions which are part of any research programme. The dynamic between the theoretical level (which I suggested was possibly pseudoscientific for Popper) and the practical level (which, conversely, demonstrates success with the falsifiability requirement) would be an important consideration to gauge whether or not TCM progresses in a way that is fruitful enough to warrant the approval of Lakatos as a progressive, as opposed to degenerating, research programme. Medicine is a discipline which continuously faces new challenges that arise with developments in other areas of research. As the knowledge of microbiology, genetics, and biochemistry expands, so too does the encyclopedia of germs, viruses, bacteria, and diseases. With any discovery of a possible antagonist to human health there is a corresponding response from the field of medicine. TCM, although it conceives of human health in a fundamentally different way than western medicine, is still in the business of medicine and thus encounters the same illnesses and diseases as CWM. In this way, TCM is faced with the same obstacles, but offers its own brand of response. So, despite TCM's deep root in traditional philosophical views, the discipline is not stagnant, but is forced to be progressive in virtue of its being a medical discipline. Here again, the empirical success of the discipline is an important consideration because it shows that the discipline does not, in Lakatosian terms, degenerate in the face of new challenges. In fact, it offers a competing research programme which, as Yuan and Lin point out, offers a breadth of potential for interdisciplinary[113] approaches to medicine.[114] That the theoretical base of TCM may be, by a Popperian account, pseudoscientific is not of considerable consequence since the ability to effectively deal with certain challenges in a better way than the competing programme of CWM seems to be the more important consideration.

Here we see how a division of the demarcation question into both theoretical and practical spheres has the potential to result in an internal tension. Empirical success is the main point of focus in the sphere of practical science and empirical success within a discipline such as TCM suggests that the discipline ought to be considered scientific at the practical level. Although, under a Popperian interpretation, TCM may be problematic at the theoretical level,

[113] It should be noted that the interdisciplinary approach would not be in the lakatosian spirit as a commitment to one's research programme in the face of competing programmes is, For Lakatos, what science is all about.
[114] Ibid.

this should not diminish its achievements at the level of practical science. It is worth mentioning that, according to Maxwell, TCM's failure in the face of some theoretical demarcation criteria might not be so problematic since these criteria might be operating under a higher level metaphysical assumption about what science can and ought to be telling us about the universe. Maxwell discloses a seven-leveled structure in his Aim-Oriented Empiricism[115] that articulates a hierarchy of assumptions that are engrained in our conception of science.[116] An assumption at level three, namely that the current version of physicalism accepted by physical science is the "best specific current version," is an assumption which is not ultimately justifiable, but the prevailing point of view of contemporary science. The non-reductionist approach of TCM seems to be adding something to the "best specific current version" of physicalism as it tends to be explained by CWM and, thus, the manner in which TCM articulates these theoretical foundations is bound to be suspicious if the prevailing way of demarcating science from pseudoscience is also permeated with Maxwell's level three assumption. TCM does not necessarily conflict with the current version of physicalism, but the manner in which TCM can be said to be adding to physicalism seems to go beyond the domain of CWM. Maxwell's point is that the lower the level of assumption, the higher the probability of the assumption being false and science ought to be as explicit as possible about its metaphysical assumptions in order that it does not become dogmatic.[117] Level three states that contemporary physics accepts its version of physicalism as the prevailing model while level two refers to the specific physical laws which make up that prevailing model. Increasing in generality, level four suggests that the universe is, in fact,

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[115] Maxwell (2004)

[116] For greater detail about Maxwell's levels, see my ch. 3, p.74

[117] It is not clear whether TCM is explicit in the way Maxwell suggests. This can be seen in the discipline's questionable theoretical position according to a Popperian account. The reason TCM may not be acceptable by Popperian standards is precisely because the discipline's theoretical foundations are not put forth as a falsifiable conjecture.

physically comprehensible—a requirement for any successful physicalist vision. It is essential to keep in mind that these assumptions are not ultimately justifiable and require a certain act of faith. Accepting that the levels are, in fact, assumptions might mean embracing alternative theoretical models which operate on different metaphysical foundations, such as TCM. In any case, the success of TCM in some cases at the practical level is, I suggest, reason enough to award the discipline scientific status, even if, at the theoretical level, the status of TCM is questionable.

Hoyningen-Huene's conception of systematicity in science may be the conception which is best able to accommodate a distinction between theoretical and practical demarcation. Hoyningen-Huene's aim is to show that all sciences exhibit dimensions of, and aim to increase, sytematicity, but depending on the discipline, the manner in which this aim is carried out will vary. Thus empirical sciences such as biology will gather as much data in the form of evidence as is possible in order to allow for more accurate descriptions, classifications, and explanations. The more systematized the descriptions, explanations, etc. The less, it is hoped, will be the likelihood that these explanations will be subject to error. Formal sciences such as mathematics seek more robust proofs and maximal systematization can be displayed through an analytic proof. Historical sciences seek to devise narratives which capture, as accurately as possible, the event in question.[118] Hoyningen-Huene's approach is heavily focused upon methodology, and, because of this, his multi-faceted conception of systematicity need not apply just to the theoretical aspect of science, but can be useful to the assessment of applied sciences as well. Yuan and Lin make the point in their article that there is a great amount of promise for the future of medications if more substantial research, analysis, and testing is put into TCM compounds. It

[118] Hoyningen-Huene (2007)

can be imagined that they are saying that, although TCM is onto something which is efficacious, a more systematized approach to studying these compounds would do a lot of good. While TCM has its own framework for describing, explaining, predicting, etc. it is not clear that this framework strives towards an increase in systematization in the way Hoyningen-Huene suggests scientific disciplines ought to. However, TCM also does not display what, for Hoyningen-Huene is characteristic of pseudosciences: “most of the dynamics of pseudoscientific fields is defensive. It is directed against attacks by the established sciences who challenge the legitimacy of the respective field.”[119] Hoyningen-Huene’s ideas about aiming to increase systematicity is a view which seems to be motivating much of the recent research into the treatments of TCM and is having the result of integrating, as far as can be expected, the virtues of TCM into CWM.

Conclusion

By way of a conclusion, let us bring the discussion back to Laudan’s views on the demarcation problem. Recall that Laudan suggested an ideal demarcation criterion ought to accomplish three metaphilosophical tasks. A) The criterion ought to both describe the methodologies of the paradigmatic cases of science and, more importantly (if the criterion is to be philosophically interesting[120]), the criterion ought to explicate those features of scientific disciplines which give rise to epistemological warrant; B) the criterion ought to exhaustively state the necessary and jointly sufficient conditions for scientific status; and C) the criterion ought to be seriously reflexive about its own pragmatic importance in the modern world at large. Although Laudan ultimately rejects the demarcation problem as a philosophical “pseudo-

[119] Hoyningen-Huene p.180
[120] Laudan (1983)

problem", largely due to the historical failure of attempts to accomplish these metaphilosophical tasks and what Laudan sees to be the unlikeliness of future attempts to succeed, I suggested that the overwhelming pragmatic importance of the demarcation question, the foundation for Laudan's third metaphilosophical task, is impetus enough to consider the demarcation problem not as a "pseudo-problem", but a significant and philosophically interesting problem. I suggested, in agreement with Laudan, that a demarcation criterion that holds in all cases is enormously unlikely to be found. Beyond Laudan's concerns, I stated that the demarcation question needs to deal with the inherent diversity of the sciences, that is, remain sensitive to features which are unique to each scientific discipline. In addition, the demarcation question is a historically embedded question and is intimately connected with the historical movements of the sciences. For this historical reason, it seems wise to maintain that demarcation conclusions are provisional. The establishment of necessary and sufficient conditions for scientific status would seem to presuppose an apprehension of what constitutes an ideal science that is resistant to the possibility of historical development and change. The suggestion that demarcation conclusions are provisional does not diminish the importance of the conclusions, as I have argued that demarcation's pragmatic value suggests. It rather offers an ongoing debate about what constitutes scientific status.

In this chapter I have explored the idea that the empirical success of TCM invites a shift in focus within the demarcation problem from the theoretical features of a science to the practical features. In suggesting that Laudan's third metaphilosophical task for demarcationists is the soil that nourishes the growth of the demarcation project, it seems a natural step to pay more attention to the practical features of science. Although theoretical concerns are, of course, also important to the demarcation question, it is in the pragmatic sphere that the need for the demarcation project is perhaps most evident. Science education, forensics, judicial proceedings, health care, and environmental policy are just a few examples of pragmatic domains that are greatly affected by the demarcation project.

When one talks about the demarcation problem, the intention is to explore the dynamics of the concept of science. Descriptions of what prevailing scientific disciplines are up to are part of the puzzle, but they are not the whole story. This is the same point that Laudan made in regards to the responsibilities of demarcation, that is, that to be philosophically interesting the question needs to go beyond the mere description of what accepted science is up to.[121] Since scientific status is so important to the epistemological warrant that intellectual pursuits receive and, further, that these epistemologically reliable pursuits have far reaching consequences in the pragmatic sphere of the human world, demarcationist projects have an enormous responsibility to profess maximal critical reflection about the concept of science. That being said, empirical success demands our attention. It may be, that after exhaustive investigation and analysis, TCM can be explained by CWM. In other words, none of the virtues of TCM take us beyond what is already within the scope of CWM. It is, however, just as likely that there is something substantially unique about the TCM approach which would resist this sort of assimilation.

I believe that this case study shows that the very idea of empirically successful pseudoscience (at least, consistently successful as opposed to occasionally successful, as if by chance) is conceptually misguided. Consistent empirical success in a discipline which operates in a domain of practical science ought to be sufficient for a scientific title whether or not there are characteristically pseudoscientific features also at play within the discipline. I feel that a division of the demarcation question into a theoretical and an practical sphere is a solution to this

[121] Ibid.

problem. This further point can be made by a simple thought experiment. If I find myself suffering from a recurring stomach illness which has proven untreatable through western medicine, but TCM heals the condition, I will choose empirical success each and every time. The pragmatic nature of the situation, a nature which I have constantly affirmed as permeating the demarcation debate, suggests that empirical efficacy trumps the epistemological poverty of a pseudoscientific label.

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