

Complementary and Alternative Medicine: Ethics, Legality, and Use of the Best Available
Science

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Abstract

The purpose of this thesis is to provide a robust epistemological justification for Evidence Based Medicine (EBM), and thereby to demonstrate the epistemological shortcomings of Complementary and Alternative Medicine (CAM). CAM has received support from both philosophers, such as Rorty and Feyerabend, and the Sociology and Anthropology of Medicine. The thesis will thus review both the internal coherence and the application of non-realist arguments, and counter non-realism with the realist epistemology and philosophy of science that is represented by C.S. Peirce's pragmatism.

Rorty and Feyerabend and others have developed radical forms of scientific anti-realism in the latter 20th century. Subsequently, sociologists developed even more intractable forms of anti-realism, which they applied to the social study of science. This approach served to challenge the legitimacy of orthodox scientific practice (including EBM). A practical expression of this controversy was immediately identified in the question of the role of scientific authority in a democratic society. That question was immediately applied to the status of alternative medical systems and their legitimacy vis-a-vis EBM in terms of the controversy over what has come to be termed "medical pluralism": If scientific medicine has no particular authority, should other, medical systems, epistemically incompatible with EBM, be made available as well?

The thesis will suggest that non-realism is in fact a marginal position within the philosophy of science. Scientists, medical researchers and medical practitioners may thus appeal to the philosophy of science in order to justify their authority in the face of challenges from CAM. However, it will be suggested that they are frequently ill-served by a reliance on a simplistic understanding of the philosophy of Karl Popper. An alternative will be proposed in the philosophy of C. S. Peirce. His pragmatism offers to medical research and medical practice a way of understanding and justifying the scientific process, a justification of realism in the face of non-realism, and a resource for the criticism of CAM and medical pluralism, as at once epistemologically ill grounded and potentially dangerous to patients and the general public.

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List of Abbreviations

<u>TFOB</u>	<u>"The Fixation of Belief"</u>
<u>EBM</u>	<u>"Evidence Based Medicine"</u>
<u>CAM</u>	<u>"Complementary and Alternative Medicine"</u>
<u>TCM</u>	<u>"Traditional Chinese Medicine"</u>
<u>NHS</u>	<u>"National Health Service"</u>

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Introduction

This thesis is an effort to solve a specific problem: to resolve the conflict over Complementary and Alternative Medicine (CAM) and medical pluralism. There should be no doubt, that although this work fulfils every requirement for a philosophical PhD, there is another objective here as well: to bring philosophical expertise to bear on the shortcomings of medical epistemology which have given rise to the current conflict over medical pluralism -- and to provide EBM public health decision makers with the additional philosophical tools and understanding they desperately need to cope with aggressive CAM advocacy rhetorically, in the interdisciplinary environment of public health policy making. The problems with the adjudications of CAM efficacy by EBM authorities, for example, are in part meta-analytic in nature -- the use of erroneously liberal thresholds for determinations of efficacy, when working exclusively with forms of clinical evidence derived from fundamentally biased studies.

The problem with the larger question of medical pluralism itself is at least partially metaphysical. Medics concerned about CAM's claims to efficacy and attacks on EBM, find themselves struggling within the confines of current scientific rhetorical conventions, derived from a limited understanding of various philosophers and philosophical ideas, but most importantly influenced by Popper's troubled falsificationism. Would be EBM advocates, find the current "received view", Popperian scientific rhetorical conventions specifically prevent them from credibly asserting the obvious about CAM: that CAM theory is absurd and that CAM techniques do not work. These are, in some ways, both scientific and metaphysical assertions. Such metaphysics of science, are not readily accommodated by either the tacit rules of "received view" scientific discourse or the atomized nature of mainstream philosophical debates within the philosophy of science, which while worrying matters within the context of justification, like the existence of invisible entities, ignores the more instrumental, pragmatic need for warrants for action central to medicine as a discipline. The pragmatic theories of one philosopher of science however, stand out with a specific focus on scientific metaphysics and the wisdom of scientists working together, Charles Saunders Peirce.

In specific, Peirce's work 'The Fixation of Belief' offers an astonishingly concise, simple and unproblematic psychological and social psychological theory of belief, ideal in this regard as a means for medics who need a powerful and readily transportable tool for resolving the metaphysical conundrums which have stymied the interdisciplinary debate over

medical pluralism. Peirce's pragmatism offers a power tool for the understanding of a meaningful truth claim in scientific inquiry, and thus the sort of steps that must be taken to validate that truth claim. As such, it serves to legitimate EMB. Perhaps more subtly, because Peirce recognises that not all fixation of belief reaches the high standards of scientific inquiry, his schema of four methods for fixing belief help us to explain the nature of justification in CAM, and why it falls sort of the higher scientific standards of EBM, and thus will ultimately fail. Finally, Peirce's pragmatism, unlike the neo-pragmatism of Rorty, is realist in its assumptions, and justifies this realism in its own terms (for the assumption of realism is more pragmatically effective in acquiring knowledge than is non-realism). This one paper by Peirce is proposed in this thesis, not only as the epistemic engine for its claims, but as a ideal form of philosophical output -- one which can resolve a real, urgent and massive philosophical crisis in pubic health.

This thesis is an attempt at applied ethics, via epistemology. More importantly, it is an effort to put to maximum possible use the knowledge of expert philosophers toward a vexed problem within the field of medicine: medical pluralism. Non-scientific medicine, or Complementary and Alternative Medicine (CAM) as it has become known in the West, has been a continuous presence in the medical marketplace since the dawn of professional scientific medicine to the present day. The profession of scientific medicine came into existence in opposition to a medical marketplace rife with pernicious entrepreneurial exploitation, negligence and fraud. Embracing values like rationality, altruism and integrity, a profession evolved as a means of providing relief from those who would prey on the inherent vulnerability -- both emotional and epistemic -- of the sick, by leveraging the pragmatic power of a newly discovered force: science. Defining the ideal approach to medicine, medical professionalism immediately attacked the pernicious elements of the medical marketplace, negotiating with the state to protect the name "doctor" in the UK and to prohibit practicing medicine without a licence in the US, under any name.

These efforts proved inadequate to stem the tide of patients and practitioners willing to trade in non-scientific medical services, and through significant lobbying efforts of their own, these services have come to occupy an enormous share of the medical marketplace. In doing so, the marketplace has polarized dramatically along scientific lines. Reacting to scientific medicine's justified criticism and hostility, non-scientific medical modalities would eventually see a common purpose in unifying under one banner, to defend themselves and counter-attack by vilifying the very elements of professionalism that had coalesced into Evidence Based Medicine (EBM). The result is a stark division, cutting across the Western medical marketplace across the NHS and across the world. The term which is used both as a term for this epistemic conflict and as the term for the anti-realist movement in favour of maintaining multiple epistemically incompatible medical systems simultaneously is 'medical pluralism'.

There are multiple disciplines indispensable to the understanding of medical pluralism. Anthropology, Sociology, History, Clinical Medical Research and Philosophy, including medical epistemology, the philosophy of expertise, the philosophy of professionalism and medical ethics. We are certain that much work in other fields of philosophy is desperately needed in addressing this problem as well, including political philosophy, however we have chosen to limit our efforts to understanding the epistemic nature of the conflict over medical pluralism by collecting the empirically relevant data from every relevant field and outlining the real world harms associated with the mistakes we have identified in the epistemic conclusions of EBM and state medical authorities today. It is crucial that the reader keep in mind that this is not an effort to advance epistemology per se. As an effort to identify a real world problem and use philosophy to fix it, the sheer interdisciplinary of this project is unconventional by philosophical standards, even within the field of applied ethics.

However, it is in disciplinarity that this work finds its most powerful critiques. As we unpack the anthropological and sociological data relevant to our initial goal, we also discover dire problems with the epistemic positions of Anthropology and Sociology, problems which emerge in interdisciplinary public health policy settings and are pernicious enough to challenge the meaning of academic professionalism itself. We spend considerable resources simultaneously extracting the empirical data from these disciplines, and engaging their epistemology as well as their thinly veiled, reflexive advocacy for Complementary and Alternative Medicine (CAM) and against EBM.

It was in the midst of this epistemic quagmire, that the Pragmatism of C. S. Peirce came to mind as a possible solution. We begin the thesis with an introduction to Peirce, via interlocutors hostile to realism, including Rorty and Feyerabend, all in the context of the discussion of medical pluralism, which both Rorty and Feyerabend were kind enough to engage in their writings. However, in keeping with the practical ethical goal of this project, we frame the discussion with some of Philip Kitcher's ideas about what the field of expert Philosophy is *for* and the nature of the way the community of expert philosophers export their knowledge products -- their expertise -- to help with real world problems. At the end of Part I, we have outlined the elements of our epistemological position via Peirce's paper 'Fixation of Belief' and have used them to dispatch Rorty and Feyerabend's anti-realist scepticism.

In Part II we review the anthropological data on non-scientific beliefs world-wide as we engage Anthropology itself, its history and its current, endemic anti-realist epistemic commitments. We are prompted by this to digress to religion, via Don Cupitt's *Sea of Faith*, clearing up any potential conflicts between medical epistemology and human spiritual needs. From here, we engage Sociology, again extracting the relevant data while observing the curious, vertigo inducing agnosticism to scientific authority, necessitated by Sociology's mid-20th century embrace of social constructivist anti-realism. We are surprised at the radical implications of Sociology's world view, where there is no acknowledgement of epistemic authority of any kind, a by-product of the complete rejection of the pragmatic implications of expertise. However we are more surprised that, from this position of

detachment, the field of Sociology seems far more negative to EBM and far more positive in their evaluation of CAM. We end Part II with a review of the history of EBM and some of the CAM modalities, including Traditional Chinese Medicine, Homoeopathy, Chiropractic and Osteopathy. It is through the historical narrative of the development of the epistemic basis of EBM that we develop our understanding of medical epistemology, in particular the epistemic limitations of the architecture of clinical research methodology: the randomised controlled trial (RCT). From this vantage point we are able to adjudicate the clinical evidence on CAM efficacy and to critique the errors we see in the epistemic pronouncements of EBM medical authorities today on CAM efficacy. At the same time, we mine the sociological ethnographic and quantitative data for an understanding of what CAM users believe, and how those beliefs are transmitted, allowing us to better understand what the CAM's business model means in the context of the medical marketplace, the contract inherent in the meaning of medical professionalism, the implications for medical professionalism itself and finally, the implications for the wider culture of "fixing belief" in the Peircian sense.

In Part III, we take another look at the epistemic problems which have emerged from Part II. We propose specific responses to sociological anti-realism and the problem it poses for the recognition of expertise. We examine the problems with the way sociology has chosen to deploy the concept of "legitimacy" in the CAM debate. Finally, we visit the vexed EBM fixation with Popper: the naive conception of Popper as the patron saint of scientific legitimacy for EBM.

Finally, Part IV is a brief outline of the implications of our universal pragmatic medical epistemic system for the status quo. We engage in a brief discussion of the radical policy implications of our findings. A reckoning with the profound failures of professionalism which we assert, is the only way to understand what CAM is. Finally, we pass judgement on medical pluralism.

PART I: A Pragmatic Response to Medical Pluralism: Peirce's Scientific Epistemology and What it Means for Philosophy, Science, Expertise and Consensus.

We see the confusion which arises from the epistemic and ontological incommensurability of non-scientific medical systems as profoundly damaging. We have taken Peirce's scientific epistemic engine form 'The Fixation of Belief' to build our own bespoke epistemic platform for our practical project. We have combined it with some observations by Philip Kitcher to motivate a rethinking of philosophy itself at the outset. We have populated this rethinking with a small scale systematic review of philosophical literature published on Complementary and Alternative Medicine (CAM). We make the case that in order for philosophy to serve an essential purpose to our project, to adjudicate important issues of medical ontology and epistemology, philosophy must first confront the sociological elements of Peirce's scientific epistemology. As we see it, there is no other way that scientific epistemic expertise can be effectively exported from philosophy into the areas of academic,

policy and political discourse where it is currently desperately needed. After establishing this, we briefly attack Rorty and Feyerabend in an effort set up a more thorough explication of Peirce's 'Fixation of Belief'.

Why the concept of expertise and communities of experts are both indispensable links in the scientific epistemic chain and why the nature of philosophy makes consensus and thus the export of philosophical expertise in the philosophy of science, subject to considerations not generally taken seriously.

As we shall see, central to C. S. Peirce scientific epistemology, is the notion of the communal action of scientific communities. Scientific experts in expert communities behave according to professional norms, both in terms of their personal habits of mind and in terms of the ways that they interact in the course of inquiry. It is from this process that science draws inexorably toward the real. Central to this process is the notion of expert consensus, for it is upon the consensus within scientific communities that the next element of progress is achieved. Science, as Peirce sees it is indeed progressive. In the same way that science provides its participants with the access to expert consensus within a given discipline in order for them to continue the process of inquiry, so too the expert groups working on more interdisciplinary projects communicate between disciplines and to non-experts as well. Central to this thesis are certain difficulties which disciplines with different epistemic systems encounter when communicating. Philip Kitcher's work on the role of philosophy and the export of philosophical expertise, frames the way in which we intend to contribute to the problems we will shortly explore in the multidisciplinary context of wider medical epistemology.

Philip Kitcher argues that *pragmatism* was developed as a call to rescue philosophy from obscurity. He suggests that the dogged pursuit of questions inherited from earlier generations has filled journals with articles by an “...*inbred group of more-or-less-obsessed puzzle-solvers*” whose work:

...fails on three counts: there is no connection to broad issues of clear significance, there is no cooperative construction of larger insights from smaller results, and no genuine resolutions at the allegedly technical level” (Kitcher 2011a).

To our minds, this thesis is precisely an attempt to use philosophy in a more applied manner. To export philosophical expert consensus and to use it to solve a problem that Evidence Based Medicine has failed to solve on its own.

Philosophy does not export expertise grounded in professional consensus.

Kitcher warns us away from these isolated, un-stackable conundrums or to expect the “*disputes, and the articles...[to] proliferate until disenchanted lassitude terminates the enterprise*”(Kitcher 2011a). Kitcher would have us look to James’ notion that epistemology should rather concern itself with differences that make a difference.

But Kitcher went on to observe a couple of things even more relevant to pragmatism and to the project at hand. Kitcher was after all, arguing that philosophers should consider making their field more relevant (Kitcher 2011b). Philosophy, he argues, has a customer service problem. Productive scientific disciplines readily do several things that philosophy has trouble with: 1.) they are able to identify new problems confronting humanity, as they arise; 2.) they are able to propose solutions to those problems; 3.) they are able to adjudicate the relative utility and veracity of those solutions using the authority of the collective efforts of the community of experts; and 4.) they export these adjudicated solutions as a dynamic stream of pragmatically useful scientific expertise. Although we are not in a position to *prove* such a claim, the objectives of the project at hand still embrace this critique.

The question for philosophy then becomes, given the immense variety of matters covered in the sub-disciplines, and given the existence of expertise in those sub-disciplines, what is the potential for consensus among the experts in each of those sub-disciplines? Even this may not be fine grained enough: this thesis will frequently remind the reader that pragmatic epistemology is always question-specific. In other words, the way we know a given answer is true or suspect, is intimately, inescapably tied to the nature of the question. So we may be courting confusion by attempting to wring general observations about the potential for consensus among the community of experts in given philosophical subfields, from general observations about the nature of those subfields. However, if there is a sub-text to our observation about the epistemology of specific questions being inherently specific, it is that sometimes, hard questions or questions without particularly robust answers presenting themselves, may still be very important. It is thus that we will have to approach the fact that there are real obstacles to the export of philosophical consensus from our various philosophical subfields, generally. In addition, these obstacles may be more or less surmountable, depending on the nature of the sub-fields, generally. However, the reader should be forewarned that some of the philosophical questions relating to medical pluralism are some of the most important questions in the world. Furthermore, as we shall see, there are good reasons that answers to some of those philosophical questions, delivered with a sociological evaluation of consensus among the relevant philosophical experts, may have profound authority, epistemically and politically in the ongoing discourse around medical pluralism. Thus the potential for consensus in given subfields of philosophy, although it cannot be assumed, may none-the-less be extremely important. We will shortly develop some reasons why such potential for consensus may vary and why that potential may provide some hope about the possibility for consensus for some of the most vexing questions in the medical pluralism controversy. Kitcher's challenge predicates the possibility that philosophy might be able to export expertise, on the ability for philosophy to generate consensus. We think that although this might rankle some philosophers and run significantly counter to certain tendencies among philosophers generally, Kitcher's challenge makes a great deal of sense in the context of the problem

at hand. We would ask the reader to consider the notion of philosophical consensus and expertise, in the context of medical pluralism, one of the most important problems facing the world today.

Peirce's use of psychology and sociology as components of his theory of knowledge.

Kitcher's evaluation is relevant to our project for two reasons: the first of which is that Kitcher recognizes in the scientific process of knowledge production the two, principally new, elements which C. S. Peirce brought with his pragmatic epistemology to debates in the philosophy of science: the psychological and social factors indispensable to scientific knowledge production. The second reason, more germane to Kitcher's point, is that philosophy as a field generally does suffer from a lack of apparent consensus and the inability to export settled adjudications of philosophical problems as philosophical expertise. As we shall observe, the dynamics behind this phenomenon are different across various specializations in philosophy and suggest different achievements may be possible in regard to exporting philosophical consensus from different subfields. Things might be different in aesthetics than they are in medical epistemology. But first, we should briefly note Peirce's use of the psychology and the sociology of science within his epistemology. As we shall see, Popper was unable to use falsification alone to establish how science comes to better explain and model reality, and was left largely defenceless against anti-realism. Subsequently, Kuhn, Feyerabend and Rorty have been subsumed into a radical movement unwilling to accept any connection between correspondence and the methods of science, including its social and psychological mechanisms. Yet C. S. Peirce provides us with a unifying epistemology which integrates these elements, and uses both social and psychological theory to build the rest of a coherent, fallibilist, instrumentalist, depth-realist epistemic platform which meets its primary and secondary goals: the modest, pragmatic, objective to solve problems with scientific solutions that work; and an equally conservative theory of meaning which uses statistical probability to tentatively connect this pragmatic scientific process to an often disputed notion of "scientific progress" through which our understanding of the world moves inexorably toward the Truth "in the long run".

It should be noted that some other non-experimental, arguably less scientific 19th century social and psychological theories emerged at roughly the same time as Peirce's. These ideas hailed from a decidedly more Hegelian culture on the continent and included Marxism and Psychoanalysis. Both of these arrived to great fanfare, cultural impact and significant subsequent, discreditation, (in no small part due to Popper)¹. However, the psychological and sociological elements woven into the

¹ In 2009, Nature published a brief editorial noting the astonishing lack of efficacy apparent in talk psychotherapy, an issue which we will take up in some detail in exploring the notion of "placebos" and their theoretical limitations. For now, however, the following quote from that article summarizes just how unkind

scientific epistemology of C. S. Peirce are of a very different, much less ambitious and decidedly more scientific nature entirely. As we shall see, Peirce's psychological and sociological theories were confined entirely to their relevance for scientific epistemology, were influenced by Darwin and were astonishingly prescient. Indeed, as we shall see, the most recent research in the psychology of belief as well as the most empirical sociological research and theory on scientific expertise, have both converged to confirm Peirce's theoretical model.

the progression of science has proven to be for Freud's efforts to overcome mental illness by chatting with patients about the theoretical components of the patient's subconscious (Abbott 2009) [abbott2009psychology]:

"But those with a grounding in science will also be shocked by the abandon with which [Freud] elaborated his theories on the basis of essentially no empirical evidence. This is one of the main reasons why Freudian-style psychoanalysis has long since fallen out of fashion: its huge expense — treatment can stretch over years — is not balanced by evidence of efficacy."

As for Marx, we are not aware of a study which exhaustively enumerates all the testable predictions made by Marx and plots the predictive power his theories might be said to have in light of such an analysis. Likely, such an assessment of so many loose Hegelian claims is beyond the abilities of modern economics, just by virtue of the fact that no one could hope to define Marx's terms. Marx's audacious predictions were as far beyond any warrant which he could have scraped together from mere observation, as his essentially Hegelian methods were from any hope of scientific quantification. As Popper pointed out in *Conjectures and Refutations* on page 333-334 (Popper 1963) [popper1963conjectures]:

"Prophecy certainly need not be unscientific, as predictions of eclipses and other astronomical events show. But Hegelian dialectic, or its materialistic version, cannot be accepted as a sound basis for scientific forecasts. ('But all Marx's predictions have come true,' Marxists usually answer. They have not. To quote one example out of many: In Capital, immediately after the last passage quoted, Marx said that the transition from capitalism to socialism would naturally be a process incomparably less 'protracted, violent, and difficult' than the industrial revolution, and in a footnote he amplified this forecast by referring to the 'irresolute and non-resisting bourgeoisie'. Few Marxists will say nowadays that these predictions were successful.) Thus if forecasts based on dialectic are made, some will come true and some will not. In the latter case, obviously, a situation will arise which has not been foreseen. But dialectic is vague and elastic enough to interpret and to explain this unforeseen situation just as well as it interpreted and explained the situation which it predicted and which happened not to come true. Any development whatever will fit the dialectic scheme; the dialectician need never be afraid of any refutation by future experience. As mentioned before, it is not just the dialectical approach, it is rather the very idea of a sociology which is a theory of historical development--the idea that the large-scale historical forecast is the aim of scientific sociology--which is mistaken."

Peirce and Marx cannot possibly be more different in the application of their respective social theories. As Marx was hoping to hitch the credibility of his earth-shattering theories of economics to the rising authority of all things "science", purely as a means of advancing the claim that his theories were every bit as modern, groundbreaking, and true as any chemist or physicist of the day; Peirce, on the other hand, was merely using a single, extremely discrete and undeniable valid sociological claim as part of a parsimonious prescription about how to do science in the best possible way.

Does Philosophy fail to identify important problems, generate useful solutions or adjudicate among the community of experts, for export as philosophical expertise?

As motioned above, there are two reasons why Kitcher's critique of philosophy as a discipline is relevant to our project, and the second is Kitcher's observation of how philosophy fails to do four things: 1.) identify important problems; 2.) generate solutions to those problems; 3.) determine that the proposed solutions have been adjudicated by expert consensus; 4.) and finally present them to the public as such, as philosophical expertise. A multitude of reasons may be already occurring to the reader as to why much of the philosophical profession simply does not lend itself to the kind of user friendly, product oriented knowledge and technology production which characterizes so many of the sciences. Many of these reasons will be to do with the fact that philosophical inquiry often has little obviously close connection with the real – which is to say the implications of a philosophical argument are rarely capable of unambiguous testing through practice in any real world. It may, nevertheless, come as a surprise that not only is there reason to believe that scientific epistemology may necessarily share some of that scientific association with the real, but also that scientific epistemology may be on the hook -- *obligated* -- to produce and adjudicate a working technology of scientific method, in a way that other philosophical disciplines from aesthetics to ethics may not face. Finally, we will note the complexities associated with the fact that medical science has, in terms of its reaction to the challenge of medical pluralism, very specifically lost its epistemic and ontological way over the last 25 or 30 years. The challenge may be summarised in terms of a tension between the recognition of cultural diversity and its medical consequences. If non-orthodox and non-Western medicines are embraced by patients (in the West and elsewhere) due to their cultural traditions or personal choices, this has the consequence that, if non-orthodox medicine is ineffective or harmful, then its being embraced will have real and dangerous consequences to the health of those patients. Thus, at the heart of medical science's response to medical pluralism lies a philosophical problem concerning the right to assert the efficacy of Western medicine (potentially at the cost of overriding the patient's right to choose ineffective or dangerous alternative medicines). Such a problem, we are arguing, is of the sort that requires the expertise of the community of philosophers of science and medicine to resolve. This failure within medical science serves not only as a textbook Peircian example for how *not* to do science well, but also as a vivid display of just how destructive it can be when the cool headed majority of philosophers who see themselves as medical epistemologists fail to organize themselves to issue an opinion, grounded in professional consensus, while such damaging chaos ensues.

Issues relating to medical pluralism are important.

To begin with number 1, philosophical problems associated with medical pluralism are important. As Malcolm Parker, Professor of Ethics, at the School of Medicine at the University of Queensland, Australia suggested (Parker 2007):

One of the most salient features of our culture is that there is so much activity occurring in the convergence of complementary and alternative medicine (CAM) with orthodox medical practice.

We are, after all, very concerned with staying well and avoiding death. With the diversity of ontological commitments displayed by members of our species, it stands to reason that medical pluralism might generate some consternation in certain corners of the health care marketplace. Medical pluralism raises issues not only central to epistemology and other fields of philosophy, but also extremely important practical considerations: considerations which we refer to generally as “human health” or “medicine”. As a consequence, there is a vast audience of experts in other fields which includes an army of policy makers all of whom see these issues as being just as salient as Parker does above. As it turns out, there is enormous public interest focused on Medical pluralism as well. As Edzard Ernst puts it in 2009 (Ernst 2009):

It has become a hugely popular subject: hardly a day goes by that the press does not report about one aspect of CAM or another, and there are currently approximately 41 million Web sites inundating the public with information on this topic. Numerous studies have shown how unreliable--indeed, dangerously misleading--this information can be (Schmidt and Ernst 2004; Weeks, Verhoef, and Scott 2007).

As we can see, questions of efficacy, and therefore medical epistemology, play front and centre in this debate. Even more ominously, as such epistemic considerations are mapped quantifiably onto human belief behaviour on a massive scale, truly horrible realities come to light:

Complementary medicines, traditional remedies and home cures for medical ailments are used extensively world-wide, representing more than US\$60 billion sales in the global market. With serious doubts about the efficacy and safety of many treatments, the industry remains steeped in controversy. Little is known about factors affecting the prevalence of efficacious and non-efficacious self-medicative treatments. Here we develop mathematical models which reveal that the most efficacious treatments are not necessarily those most likely to spread. Indeed, purely superstitious remedies, or even maladaptive practices, spread more readily than efficacious treatments under specified circumstances. (Tanaka et al. 2009)

We would suggest that some of the considerations arising from medical pluralism constitute some of the most important philosophical problems facing humanity. Inherently so, we should hasten to add,

as it appears that these problems -- in spite of the occurrence of the Enlightenment and its attendant effects for some portion of the people on the planet -- are now arguably becoming worse:

In recent years, 60 –80% of the world's population, mainly from developing countries, depended primarily on traditional medicines, folk remedies and home cures, as well as treatment from witchdoctors and other 'supernatural practices', for their health-care needs [1]. In western societies, complementary and alternative medicine is garnering increasing interest and acceptance. At current growth rates, two-thirds of Americans are projected to be using alternative medicine by 2010 [2] (Tanaka et al. 2009).

Medical pluralism is, by any standard, a huge problem. So what exactly has philosophy done about it?

Has philosophy identified specific epistemic problems associated with medical pluralism, generated solutions and sought to adjudicate and export the solutions as professional consensus?

Ernst's research indicates that humans have some real problems with understanding medicine – and we have suggested that orthodox medicine is ineffective, due to its philosophical self-understanding, in responding to those problems. But we still need to find out what philosophy has been doing to help fix these problems. To quickly go over Kitcher's specific questions again in a slightly different way: 1.) Has philosophy identified the problems around medical pluralism which might be unsolvable without the input of professional philosophers? 2.) Have philosophers been able to propose solutions to those problems? 3.) Has philosophy been able to adjudicate the relative utility and veracity of those solutions using the authority of the collective efforts of the relevant community of experts? And 4.) have professional bodies of philosophers been able to export these adjudicated solutions as pragmatically useful scientific philosophical expertise? The most sensible way to get at this question is to take a look at the literature.

It will probably come as no great surprise to the reader that as far as we were able to determine, Philosophy as a discipline, or medical epistemology as a sub-discipline -- as an area of expertise and as a community of professional experts -- has failed to establish, much less publicise, anything resembling a unified consensus on the issue of medical pluralism. There are, however, some scholars writing about medical pluralism in philosophical journals, although in many cases the authors' academic backgrounds appear to be in disciplines other than philosophy (begging a question we will come to shortly). Some of these authors have to varying degrees, and one author absolutely has, identified the central epistemic and other philosophical problems associated with medical pluralism. At least in one case, an author has offered what we think is the only coherent solution to the problem as well, in essentially philosophical terms. So as we shall see, our PhilPapers search seems to

suggest that at least some individuals are doing all of what Kitcher would have philosophy do, except get together as a professional group and issue a joint pronouncement of professionally adjudicated expertise.

A PhilPapers search of articles with the exact words “Complementary and Alternative Medicine” (also known as CAM) produced 15 articles from journals including: the *Journal of Bioethical Inquiry* (Clark-Grill 2010, Parker 2007), *Perspectives in Biology and Medicine* (Weldon-Linne 2000, Ernst 2009), *The Journal of Law, Medicine and Ethics* (Sugarman 2003), *Theoretical Medicine and Bioethics*, among others. These articles contain the full gamut of publicly debated positions on the issue of medical pluralism, including strong advocacy for alternative medicine (Clark-Grill 2010) involving the use of strong epistemological relativism to attack scientific authority as well as the ways medicine, evidence and science are normally thought to be connected. At the other extreme, a clinical researcher makes an appearance among the interlocutors, responding to the lax epistemic standards inherent in CAM culture and CAM research, with an appeal for greater epistemic scepticism toward Complementary and Alternative Medicine (Ernst 2009). The collection of articles also features a more philosophically rigorous parsing of CAM's interaction with Evidence Based Medicine (EBM) by Malcom Parker, the medical ethicist we quoted above (Parker 2007). This author parses the relationship between EBM and medical pluralism in one of the ways we find to be most productive: ontologically. In Parker's case, probably because he is writing for a medical audience, he avoids the term "ontology" by re-naming "naturalism" thus: "...the totally integrated evidence-based model (TI)..." where he describes CAM or Traditional Medicine they are simply treated as sources of potentially useful medical substances and practices and subjected to the same epistemic and ontological parameters as EBM (Parker 2007). He is then free to discuss all remaining medical systems, recognizing that they depend on other ontological commitments, which for many CAM advocates involve to varying degrees the abandonment of EBM epistemic standards for the determination of efficacy. This ontological demarcation is primary for this thesis as well, because, as we shall see, naturalism is a fundamental component of the best possible epistemic basis for medical decision making. Parker recognizes the dire consequences of radical ontological pluralism as it has expressed itself in the medical marketplace, and rejects the intentions of some, who would attempt to integrate ontologically and epistemically different medical models with EBM. He calls these efforts collectively the "multicultural-pluralistic model (MP)" (Parker 2007). He goes on to reject such efforts "...on the bases [sic] of conceptual and clinical coherence, ethical justification and regulatory capability" (Parker 2007). We think this is a very wise determination, and observe that he is clearly doing some extremely valuable philosophy: both having identified an important philosophical question (Should EBM abandon its ontological and epistemic commitments in an effort to adopt medical pluralism?) and having offered a reasoned answer (No.) supported by reasons drawn from epistemology, law and medical ethics. So at least one person, and likely several, have identified the problem as we see it, in more or less philosophical terms. The next question in this case, relevant to

the question at hand, is: are these people philosophers, or more specifically, medical epistemologists? Parker is clearly a doctor first, so polymathism may be very helpful if not vital to medical epistemology. All three of the articles just mentioned were written by doctors working in medical schools (although like Parker, Clark-Grill appears to have steered her career at one point specifically in a philosophical direction, as she did a PhD entitled *"Reclaiming the Full Story of Human Health: The Ethical Significance of Complementary and Alternative Medicines"* at the Bioethics Centre at the University of Otago in New Zealand). Ernst, Parker and Clark-Grill account for five articles out of the 15 generated by our "complementary and alternative medicine" "exact phrase" search in PhilPapers, and the remaining 11 papers include a sociologist (Derkatch 2008), a lawyer (Weldon-Linne 2000) and David J. Hufford (Hufford 2003) whose University of Pennsylvania web page suggests he is involved with "Folklore and Folklife", but whose article suggested he was teaching medical students at Penn State about CAM after extensive research. The question of *who is an expert and why* is relevant to Peirce's scientific epistemology, and an issue we will be exploring in much greater depth. For now, without going into more detail about the nature of the papers or authors which turned up in our PhilPapers search, we would like to assure the reader that these works do not converge on a philosophical consensus with regard to medical pluralism any more than they represent any apparent criteria for "expert in the field of medical epistemology" which might be any more specific than "published in a journal which was then listed on PhilPapers."

To summarize, we learned the following from our PhilPapers database search in regard to Kitcher's first two points, it seems, if perhaps irrelevantly, that yes, some individual academics are identifying and suggesting answers for important questions relating to the epistemic problems presented by medical pluralism. It also appears that if there is a mechanism with which medical epistemologists determine anything as a group that it is unknown to the current author and not apparent in any of the articles turned up in our search in PhilPapers.

Medicine is confused: is this philosophy's fault?

Clark-Grill, whose papers advocating strongly for medical pluralism served as the most radical contributions to the pro-medical pluralism case in our PhilPapers sample, feels confident that the discussion in the medical community over medical pluralism is unusually fervent and has resulted in medical pluralism becoming integral to health care (Clark-Grill 2007):

Few issues have been debated as fervently in medicine as the status of complementary and alternative medicines (CAM), particularly since it has become evident that the use of these therapies is steadily increasing and that, despite their marginal place, they have become an integral part of the public's regular health care [1].

Perhaps Kitcher is right and responsibility for this epistemic confusion rests inevitably with a profound failure on the part of philosophy and that this is due to the nature of philosophy itself. However, as we shall see, "medical science" has had a difficult time generating consensus on issues of efficacy regarding alternative medicine as well, something for which their epistemic and even ontological traditions had indemnified them against for decades. Unfortunately something changed in the 80's as the old epistemic and ontological walls of medicine were breached and now CAM advocates are sometimes awarded research grants, do clinical research and publish in a variety of journals. The core problem, lies in the blurring of the (philosophical) understanding of the nature of rigorous research that has accompanied the promotion of CAM. As we shall see, the results of CAM studies are directly related to the level of bias they allow, so that the "demonstrating" of profound effects resulting from CAM treatments occurs only in wildly biased studies (but never with robust studies). In addition, CAM advocates have gained membership in medical schools (Clark-Grill for example), and persist in their philosophical efforts to erode the status of randomized clinical trials as the gold standard of clinical research. In both cases, the case of the philosophers and in the case of the medics, the failure lies in medical epistemology (or perhaps more precisely, in the degree to which a respect for cultural pluralism and patient choice has, we will argue misguidedly, overridden a respect for scientific research methodology that medical epistemology, at its most rigorous, articulates). We would argue that to the extent that medical epistemology is a sub-field of expertise within philosophy, then philosophers are at least partly responsible for the abysmal state of confusion and chaos in the debate over medical pluralism -- and it is precisely their failure to work together as a self identified community of experts and to issue statements generated by consensus that lies at the heart of this failure.

The relative political authority of Philosophical claims varies by sub-discipline and scientific epistemology occupies the most authoritative end of the spectrum.

Martha Nussbaum has argued that although moral expertise does exist, the authority of such expert judgement does not rise to the point at which fellow citizens might be required to defer to the views it might suggest (Nussbaum 2002), specifically in the case she discusses in her article, as expert moral philosophers selected as expert witnesses to testify on core constitutional questions. As we will frequently make clear in the course of our argument, the epistemic nature of any given question varies with the details of the question. Yet it may be that Nussbaum is right and that there are literally no moral questions at all regarding which moral philosophers should be granted a politically dominant form of expert moral authority. To her this conclusion is reached through a discussion of Rawls' political philosophy. She does not dispute the existence of something called "philosophical expertise" or even that philosophers play a valuable public role (Nussbaum 2002). An important distinction she

does make is that expert scientific testimony regarding *the facts* relevant to moral questions is in a different category. She uses the example of cloning as a topic about which certain scientific information may be crucially necessary to help people understand the concept: for example, that identical twins are precisely natural clones of one another. Such factual, scientific input, she argues, carries a higher level of political authority than any expert input provided to advise decision makers on normative decisions. So scientists can and should demand that lay people defer to their authority when they assert that twins are clones. However, the requirement that lay people should adopt what scientists suggest they should in terms of some policy toward cloning does not rise to the same level as the previous case. Scientists command authority about facts within their field of expertise, but moral reasoning, even when delivered by a moral expert, cannot be called upon in the same way as forensic or scientific evidence -- in court. Peirce would suggest that the consensus of the expert community is an indispensable link in the epistemic chain which constitutes that authority. In the case of biologists, there is zero confusion or controversy as to the status of twins as clones. For Nussbaum, there are reasons for why anything a moral philosopher might have to say about cloning policy should not carry more than a measured level of authority. But Kitcher would also observe that we can also expect that philosophy as a discipline will not be issuing a consensus adjudicated answer to any questions about cloning at all, even though, both Peirce and Nussbaum agree that science does carry a special authority and relevant experts and their input is crucial to knowledge production and decision making.

The reasons for this may have something to do with another element of Peirce's scientific epistemology. Peirce's loose notion of correspondence has our contact with reality occurring through both general and precise interactions with the world: we interact with the world generally through the identification of problems which stimulate doubt, and through falsification in the very specific context of experimentation. At first glance, one might be tempted to observe that philosophy is special as a discipline, because (notwithstanding elements of experimental philosophy) philosophy has no mechanism by which it makes contact with the world. Its problems are not physical and it does not (with the noted exception) conduct experiments on material stuff. However, the philosophy of science, in particular with respect to historical analysis of scientific processes and their outcomes, pirates the data and experiments of other disciplines, thereby arguably putting it in contact with the real. For this reason it might be useful to think of the wider discipline of philosophy in terms of a continuum, upon which sub-disciplines vary in their correspondence to contact with a material reality. At the most correspondence-heavy end of this continuum sits the philosophy of science with its deep ties to experiment and the analysis of the connection between the real world and the process of knowledge production. Recognizing this locates the various arguments we may make about medical pluralism on the same continuum, and this perhaps provides us with some idea as to how seriously the wider community might be obliged to take our claims.

The authority of medical epistemological claims is dependent upon the ability to establish the extent to which said claims represent the consensus among medical epistemologists.

We have seen that the present authors are unaware of any easy way to determine definitively what the professional consensus might be on any given questions about medical pluralism. In our experience, by far the most salient demarcation cutting through the philosophical and sociological literature on medical pluralism is the relativist/non-realist/post-modern vs. realist divide. This epistemic canyon, divides its respective tribes so profoundly that the middle ground is rarely contested. It is this feature which gave rise to the science wars and the continuing alienation between huge swaths of disciplines relevant to the subject at hand and other huge swaths of equally relevant areas of expertise. It is not an understatement to suggest that the primary achievement of this project, ancillary to its primary goal, will be the development of the pragmatic framework which will outline how the indispensable contributions of medical anthropology, ethno-science and sociology must be understood, in order to be coherently integrated with medicine and analytic philosophy. Just getting these disciplines to make sense together will be an achievement given the total incommensurability of their epistemic commitments. We will engage Rorty and Feyerabend shortly, since they are the representatives of this kind of thought within analytic philosophy itself. For now, the reader should keep in mind that the tendency for authors to end up in one or the other of these tribes is correlated 100% with their position on Medical Pluralism -- and the sometimes explicit and sometimes tacit understanding of this fact is never far from the motivations of the participants in the medical pluralism debate.

Literature searches do not present a uniform perspective and there are no organized bodies of medical epistemologists issuing decrees (other than statements made by medical organizations like Cochrane, WHO, NICE, etc. who, as we will demonstrate, are variously epistemically flawed). Still, for our purposes -- to determine the best possible system of medical epistemology and adjudicate medical pluralism from that perspective for the wider medical, legal and public health policy audience -- we recognise that merely philosophically demonstrating this through argumentation may not be enough. We will argue that Peirce's observation about the essential role expert consensus plays in the adjudication of knowledge, and therefore knowledge production, is widely recognized intuitively both inside and outside of philosophical writing. Furthermore, because many of our considerations sit at the scientific epistemology end of the spectrum of philosophical deliberations, the determinations we make about medical epistemology will be both affected by the added political authority that Nussbaum suggested and an added Peircian scientific epistemological requirement that our assertions be adjudicated by expert consensus. It may sound like we have already dispatched any hope of gaining the latter -- the agreement of *some relevant quorum of other* philosophical experts. However, the motivation to both determine and leverage expert consensus is both intuitive and of philosophical necessity, both within exclusively philosophical discourse and in discourse between philosophy and

other disciplines or when philosophy engages in political discourse in the public sphere. If it has not occurred to the reader already, Peircian philosophy already sounds right to a scientist. One is reminded of the idea that scientists often have an orientation to philosophers of science as birds do to ornithologists. Peircian epistemology may have an important advantage in its more natural appeal to scientists themselves. Scientists, according to Peirce, have *developed* pragmatism organically. This kind of respect for so many organic elements of the scientific method would not be lost on scientists. The political authority of scientific epistemologists among scientists themselves would be more likely enhanced by pragmatism. Lastly, the ever increasing tendency for both scientific and philosophical specialization should also be considered both in the context of philosophical discourse by itself and in the export of philosophical expertise. Let us begin by exploring the way these intuitions and epistemic requirements affect philosophy and philosophical discourse by itself.

Consensus as access to knowledge: the Peircian requirement for expert consensus and what it means for scientific epistemology.

While pragmatism offers certain advantages in terms of the export of philosophical expertise from philosophy to medical science, philosophy itself is different from science in terms of the places from which authority can be derived from within philosophy itself. A sense of what might be true from the field of philosophical ideas can be derived from a number of places, including the fundamental quality of the argument's reasoning. Although not often discussed as essential elements of any analysis of a philosophical argument, there are other, historical and sociological factors which come into play. In some cases, the observation that "most philosophers agree that X" pops up either historically, or in an effort to bring a reader perhaps unfamiliar with the state of play within a particular sub-discipline up to speed on the current state of affairs. What is characteristic of philosophy, is that such observations are never considered to be *philosophically* important. Thus, arguments from authority may be instinctively, but wrongly, considered by many philosophers to be both irrelevant and fallacious *all of the time*. It may be that it is this attitude which drives the sociological differences between scientists and philosophers, and which prevents the mechanisms designed by scientists to reveal consensus from developing in philosophy. Agreement is thus not easily established and expert consensus is elusive, given the possibility of competing and divergent authorities. Still considerations about *what* other philosophers think and *when* is never far from our epistemic considerations. May we remind the reader of the inherent connections between epistemology and history (Feest & Sturm 2011, Kitcher 2011a, Sturm 2011). It should be immediately understood how important history is to epistemology. Medical epistemology for example could not be adequately understood without considering the historical trajectory of some of the epistemic notions employed by Galen, including naturalism, empiricism, and experimentation. Indeed, an adequate

understanding of the difference between scientific medicine and its alternatives would be impossible otherwise. But there is still another reason why it is relevant and even necessary for philosophers to understand how well received an idea was or is among the community of experts. Such sociological evaluations help us to understand the level of appeal of philosophical ideas to the community of professional philosophers over time. As we already observed, this may sound like a decidedly un-philosophical concern, but that is wrong. We recognize that philosophy has historically wedded itself to the explicitly a-social nature of philosophical ideas and perhaps thought of itself as a timeless, objective forum for the examination of said ideas – *purely on their own merit*. Certainly Descartes' reliance on geometry and even the cogito seem to point toward solitary hand wringing as the sole contributor to intellectual rigour. Not so says Hume, epistemic determinations from geometry to business are improved by a little bit of sociological input as well. We would merely observe that Hume's observation extends to philosophy as well². Even the traditional emphasis on the *a priori*, has allowed philosophers to ignore sociological observations of consensus, relying on a sort of substitute for consensus -- an *a priori* consensus by logical consistency. Peirce was aware of this as well and as we shall argue, he could see the potential for fashion to eclipse any real understanding of expert consensus. Thus, the evolution of ideas in a historic context is not just about understanding nuances affecting *what* ideas *mean* philosophically. It is also important to understand the *currency* that ideas had and have with philosophers as the community of experts are perceived as having reacted and to reacting to those ideas. Perhaps it would be useful to illustrate this point with a quote about pragmatism itself from Thayer's *Meaning and Action* (Thayer 1968, p. 1) :

Pragmatism was the most influential philosophy in America during the first quarter of the twentieth century. Viewed against other idealisms and intellectual currents that have

² Hume dispatches Descartes inflation of our capacity for epistemology in a vacuum thus (Hume n.d.) [hume2003treatise] (1.4,1):

"In all demonstrative sciences the rules are certain and infallible; but when we apply them, our fallible said uncertain faculties are very apt to depart from them, and fall into error. We must, therefore, in every reasoning form a new judgment, as a check or controul on our first judgment or belief; and must enlarge our view to comprehend a kind of history of all the instances, wherein our understanding has deceived us, compared with those, wherein its testimony was just and true. Our reason must be considered as a kind of cause, of which truth is the natural effect; but such-a-one as by the irruption of other causes, and by the inconstancy of our mental powers, may frequently be prevented. By this means all knowledge degenerates into probability; and this probability is greater or less, according to our experience of the veracity or deceitfulness of our understanding, and according to the simplicity or intricacy of the question.

There is no Algebraist nor Mathematician so expert in his science, as to place entire confidence in any truth immediately upon his discovery of it, or regard it as any thing, but a mere probability. Every time he runs over his proofs, his confidence encreases; but still more by the approbation of his friends; and is raised to its utmost perfection by the universal assent and applauses of the, learned world. Now it is evident, that this gradual encrease of assurance is nothing but the addition of new probabilities, and is derived from the constant union of causes and effects, according to past experience and observation.

In accompts of any length or importance, Merchants seldom trust to the, infallible certainty of numbers for their security; but by the artificial structure of the accompts, produce a probability beyond what is derived from the skill and experience of the accomptant. For that is plainly of itself some degree of probability; though uncertain and variable, according to the degrees of his experience and length of the accompt."

characterized American life, it stands out as a movement that not only had an impact upon academic philosophy but profoundly influenced students of the law, education, political and social theory, religion, and the arts.

We are arguing that philosophers say this kind of thing all the time, but the closer they are to scientific epistemology the more they *have* to say things like this -- things which serve to sociologically evaluate the consensus apparent in the expert community's reaction to an idea. The rigour of the philosophical arguments expressed matter too, of course, but expert consensus in philosophy requires more than this, if we are to identify well reasoned yet otherwise eccentric or outlier positions. Thinkers such as Kuhn, Rorty, Feyerabend are certainly important, even essential, in their role as challengers to epistemic complacency. But like Descartes' mental gymnastics, they are not the *only* kind of philosophy one needs if one is attempting to provide pragmatic epistemic assistance to a confused and frightened world. These thinkers are very *unusual* in that they are attracted to (Kuhn, less dispositionally so than Rorty and Feyerabend) a form of epistemic anarchy most of us just do not find credible. Rorty and Feyerabend habitually reject the real. After a while, they both start to seem more and more like they are in the same boat as Descartes -- clever, mostly well reasoned, revolutionary, ground-breaking -- and wrong. Sociological considerations are essential, both because, as Nussbaum would suggest, scientific epistemology carries more political authority, but also because of the Peircian requirements for consensus as a fundamental component of the epistemic chain in matters scientific. We are for now only talking about such evaluations *within* philosophy itself. However, the reader should keep in mind the latter part of Thayer's claim above about the wider influence of pragmatism as we move on to consider the following section, about how the need for consensus evaluations among professional philosophers becomes even more important when ideas are exported outside of philosophy and the intuitive desire to employ such arguments is even greater. Before we move on to consider the export of philosophical expertise, we need to visit the notion of specialization within the field of philosophy itself and what this means for evaluations of consensus from within the communities of philosophical experts in those subfields.

The specialization argument for the necessity of evaluations of consensus in medical epistemology.

As the sheer volume of scientific knowledge exponentially explodes, scientists and policy analysts must rely increasingly on trust and expert consensus as an indispensable link in their access to pragmatic scientific knowledge. Peirce explicitly made reference to this sort of role for the community of experts as an indispensable link in the pragmatic epistemic chain. Similarly, as philosophy's subdivisions proliferate, the same thing is happening in philosophy. Specialization makes philosophers increasingly isolated from one another in subfields. If philosophical expertise means

anything, if one is not an expert in a given subfield, if one does not have time to become an expert in that sub-field, if one wants to know something involving the expertise within that subfield, the obvious path to take is to determine what most of the experts in that sub-field think about the question at hand. The other possibility is that philosophy may only be “done” or utilized by experts within their field, and that if one wants to “do” or use some specialized argument of philosophical knowledge for philosophy, or anything else, then the only way is to become sufficiently well versed in the sub-disciplines to objectively make up one’s own mind. Perhaps, some philosophers might simply insist on this rather hard line. Still, as we consider the vast interdisciplinary scope of this project, and consider the essential inclusion of sub-disciplines ranging from the philosophy of expertise, to the philosophy of belief and medical ethics -- we are unable to muster the hubris required to assume we could not benefit from an understanding of the professional consensus among the members of the expert communities in those sub-fields. We argue that as we plunge into the research required to tie all those subfields together in a coherent effort to forge a pragmatic understanding of, and reaction to medical pluralism, as an exportable product of philosophical expertise, we will be relying not just on our personal abilities to adjudicate the ideas we encounter, but on every available accurate evaluation of what “most (expert) philosophers think” in each respective sub-field. Even without the special responsibilities of science that Nussbaum and Peirce would have us apply to scientific epistemology, the interpretation of other highly specialized subfields in philosophy will *always* be better understood by *any* participating philosopher regardless of her familiarity with that particular field, if sociological data about the consensus which exists around any particular idea is available.

To summarize, within the field of philosophy generally and even more so within the sub-specializations of both scientific and medical epistemology, there are important reasons to consider sociological evaluations of expert consensus. Closely related to historical epistemology and intuitively, even tacitly present in most broadly focused forms or historical forms of philosophical writing, we agree with Kitcher in asserting that sociological evaluations of consensus among expert philosophers are a necessary, if poorly understood, requirement of doing philosophy well. In the case of scientific and therefore medical epistemology, Nussbaum would remind us that scientific evaluations carry more political heft when exported outside philosophy into public, political environments and Peirce would remind us that the most pragmatically useful scientific knowledge is derived necessarily from expert consensus. However, we maintain that increasing sub-specialization within philosophy itself makes wide ranging practical ethical projects which touch on a number of subfields even more ideally situated to benefit from sociological evaluations of expert consensus. As we sum up our consideration of the need for consensus evaluations of expert philosophical communities, we rest our case as to why we are committed to the effort to leverage expert consensus in the context of this project whenever possible, and as we shall see, in particular, as we seek to export this project, as philosophical expertise in an effort to have the most significant possible pragmatic and political impact on the issues of medical pluralism. It is into the public arena we shall now take these

same issues, to explore how much more essential evaluations of professional consensus are in non-philosophical discourse environments.

To non-philosopher consumers of philosophical knowledge, sociological evaluations indicating the relative acceptability of an idea among the field of relevant experts, is the only means by which legitimate philosophical expertise can be differentiated from bad philosophical ideas, which have been deployed in the public sphere, solely in the service of pernicious political activism.

When delivering some kind of philosophical knowledge product into a larger debate about something, in addition to communicating philosophy's expert work product of just ideas *by themselves*, we must also communicate some sense of the relative *acceptability* of those ideas among experts in the relevant subfield. As we have seen, we have reason for scientific knowledge and associated philosophical ideas to have added political authority. We also have reason to acknowledge that scientific knowledge and associated philosophical ideas are most pragmatically valuable, and even, if very tentatively, slightly more progressed toward the inevitable "long run" correspondence with the real. We acknowledge the inability of even experts in multiple fields to know everything or to achieve some sort of unlimited polymath status with regard to expert knowledge of subfields both inside and outside of philosophy. All of these ideas affect our ultimate goal, to export a broad, interdisciplinary, practical ethical project into the medical, the larger academic and the public policy spheres. The public sphere however and even academic disciplines like medicine and sociology (we will later prove) harbour additional philosophical and sociological obstacles to the export of philosophical expertise into these areas. These obstacles are again, yet another, and perhaps the single most destructive result of the failures outlined by Kitcher: if philosophy fails to export philosophical ideas with some sociological evaluation of the relative expert consensus from which their authority arises, there is nothing to prevent radical and therefore radically pernicious ideas from being repeatedly exported from philosophy into debates in other disciplines, to debates in the public policy sphere and to debates involving the public at large -- *as current philosophical expertise*.

As we have seen, in order to complete the philosophical process and get knowledge into the non-philosophical hands of those who would use it, we must do some sociological work as well and include it as part of the package. As we have demonstrated, there is significant evidence and reason to believe that just demonstrating that an idea is wrong is simply not the final step in the creation of philosophical expertise ready for export: we must deliver that analysis with some understanding that it represents what the relevant field of experts think is true. As an example of this kind of scientific epistemological communication, to decidedly non-philosophical audiences, we would present the following -- with the added bonus of being rather dead-on specific to the topic at hand: on the 28th of

September, 2013, the *New York Times* online opinion pages carried a fawning article on Traditional Chinese Medicine entitled *The Enigma of Chinese Medicine*. It was written by a philosopher whose by-line reads as follows:

Stephen T. Asma is a professor of philosophy at Columbia College Chicago, and the author, most recently, of “Against Fairness.” He will be in Beijing on a Fulbright grant in 2014.

The article cites Karl Popper, Larry Laudan, Thomas Kuhn, Paul Feyerabend and Aristotle, and includes links for most of them to the relevant pages of the Stanford Encyclopaedia of Philosophy.

The article ends with this conclusion:

Ultimately, one can be skeptical of both qi and a sacrosanct scientific method, but still be a devotee of fallible pragmatic truth. In the end, most of us are gamblers about health treatments. We play as many options as we can; a little acupuncture, a little ibuprofen, a little turtle’s blood. Throw enough cards (or remedies), and eventually some odds will go your way. Is that superstition or wisdom?

The Rortian abuse of the word “pragmatism” as well as the calling to arms of other radical agents of epistemic anarchy, are front and centre in the public debate over medical pluralism, and are even right there in an opinion piece in the NYTs. On a public policy level, as we shall see, this kind of confusion is as disastrous as it is philosophically unfounded. For now, however, we would call the reader’s attention to the fact that this author is clearly proffering these ideas as philosophical expertise to the public discussion on medical pluralism. In the October 10, 2013 rebuttal, printed in the same publication and written by Massimo Pigliucci and Maarten Boudry, and during the process of philosophically refuting Asma’s arguments one at a time, the authors also found themselves resorting to sociological references of consensus regarding the expert community of philosophers, no less than three times:

- 1.) “Philosophers of science have long recognized that there is nothing wrong with positing unobservable entities per se,…”
- 2.) “Philosophers nowadays recognize that there is no sharp line dividing sense from nonsense, and moreover that doctrines starting out in one camp may over time evolve into the other.”
- 3.) “The verdict by philosopher Larry Laudan, echoed by Asma, that the demarcation problem is dead and buried, is not shared by most contemporary philosophers who have studied the subject.”

If the first two examples are not explicit enough there should be no doubt about the meaning of example number 3. This is precisely the kind of appeal to the community of experts we will see Peirce making as we develop his epistemology of science. Pigliucci and Boudry seem to intuit as Peirce did that both in Nussbaum’s necessarily public and democratic forums as well as in matters of science,

consensus among the relevant experts *matters*. As we shall see, the authorities currently weighing in on medical pluralism, from the American National Institute of Health (NIH) to the UK's Department of Health, from the National Institute for Health and Care Excellence (NICE) to The Cochrane Collaboration and the World Health Organization (WHO), these authorities are all arguably in need of philosophical assistance in regard to medical pluralism, because, their approaches to it are fundamentally confused and potentially harmful to patients. To a large degree, these organizations are suffering from a debate occurring external to philosophy, where philosophical ideas are used by advocates from other disciplines to make epistemic arguments in support of their particular brand of medical pluralism -- but the ideas are never contextualized by their acceptability to the relevant philosophical experts.

As an example, Feyerabend, as a self-avowed epistemic anarchist, in the conclusion of his introductory chapter in *Against Method* (Feyerabend 1975, pp. 18-19) peddles his epistemic confusion as a brave and rebellious alternative to “lower instincts” like “objectivity” or “truth”:

To those who look at the rich material provided by history, and who are not intent on impoverishing it in order to please their lower instincts, their craving for intellectual security in the form of clarity, precision, 'objectivity', 'truth', it will become clear that there is only one principle that can be defended under all circumstances and in all stages of human development. It is the principle: **anything goes** (Feyerabend's emphasis).

As he does in the Asma article mentioned above, Feyerabend looms large in the debate over medical pluralism in medicine, in sociology, and in public health policy. As a consumer of alternative medicine³ and as an ardent supporter of it as well, Feyerabend applauded Mao's brutal suppression of Western Scientific Medicine in Communist China as a necessary means of overcoming what Feyerabend called “scientific chauvinism”(Feyerabend 1975, p. 48). As we shall see, Feyerabend's radical contribution to the medical pluralism debate is simply that science has no special epistemic status over any other domain of knowledge. This idea is frequently exported as philosophical expertise, trafficked as such in non-philosophical disciplines and areas of discourse very frequently, and much of the time Feyerabend is credited for it. For our *philosophical* purposes, an argument grounded in Peirce's pragmatism readily dispatches such a bizarre assertion by observing, equally simply, Peirce's principle hypothesis of reality from *The Fixation of Belief*: “*There are Real things, whose characters are entirely independent of our opinions*” and that these real things affect every man equally and are the object of rational scientific inquiry. However effectively the development of such an argument – which is to say to draw upon Peirce's classical pragmatism to expose the

³ The Stanford Encyclopedia of Philosophy entry on Feyerabend details his personal conversion to alternative medicine in the section entitled “2. Feyerabend's Life and Work: A Critical Appraisal” and in the subsection entitled “2.13 Against Method (1970–75)”. The web page was retrieved on September 19th, 2014 from the following URL:
<http://plato.stanford.edu/entries/feyerabend/#2.13>

incoherence of *Against Method* – and however quickly we may dispatch Feyerabend philosophically to the satisfaction of Pigliucci and Boudry’s community of ‘most contemporary philosopher’, we are still left with our *other* philosophical problem, the other problem facing Massimo Pigliucci and Maarten Boudry: when Feyerabend’s idea is exported and trafficked as philosophical expertise, both by sympathetic philosophers and by people in other disciplines, his idea does not come with a disclaimer like: “most scientific epistemologists find this silly”. We would argue, that to the extent that scientific epistemology exists as a discipline, it would be either negligence or fraud for an expert epistemologist to float this particular Feyerabendian idea without such a contextualizing sociological disclaimer. To do so, would be to commit sophistry -- to use a false and pernicious philosophical idea for rhetorical gain. We will, in due course, investigate the intersection between the burgeoning sociological field of the study of Scientific Expertise and the philosophical subfield of the philosophy of expertise. For now, it will suffice to observe that there are well established norms of behaviour which prohibit precisely what Asma did with his sociological context-free inclusion of Feyerabend's epistemic anarchy in his article for the NYTs:

...the concept of expertise typically implies proficiency in a domain of practice so that the testimony of an acknowledged expert will have priority over that of a lay witness in matters where proficiency in the practice might inform perception or judgment. (Selinger et al. 2011)

This, however, carries with it the following caveat from the same work:

Endorsing the combination of virtue and discourse ethics leads one to conclude that there is at least one instance where activists who dissent from the core group of scientific experts are morally obligated to frame their case to the public with a robust set of qualifiers that effectively undermines the persuasiveness of their case.

Without some idea of what *the philosophical community of expert philosophers* thinks about Feyerabend's non-sense, non-philosophers are left to do their own philosophy: to listen to the arguments between Asma, Massimo Pigliucci and Maarten Boudry, and make their decisions based on their best evaluations of relative philosophical merit. If philosophy is an area of expertise, then we can assume non-philosophers will do a pragmatically worse job at epistemically evaluating medical pluralism if left to their own devices with Asma's underhanded assertions, than they would if assisted by competent, ethical professionals like Pigliucci and Boudry. If philosophical expertise exists, non-philosophers simply are not in a position to do philosophy as well as experts. Expecting them to do so is not only wrong, but it is precisely this which has caused the current and deeply confused status quo in the debate, policy and public understanding of medical pluralism.

There may be one more reason why philosophers are uniquely important in such borderline metaphysical disputes. We would ask the reader to recall the previously mentioned literature on the epidemiology of pernicious health ideas:

We show that the treatments that spread are not necessarily those that are most efficacious at curing the ailment, and explain how 'superstitious treatments' with little efficacy and even maladaptive practices can spread under broad conditions. (Tanaka et al. 2009)

We will eventually go into some detail about why exactly people are uniquely bad at evaluations of medical efficacy. For now it should suffice to remind the reader of the above mentioned disease dynamics which govern the spread of pernicious health ideas, and to bring to the reader's attention that we will be investigating the specific neurological reasons human beings are biased in favour of closely held health beliefs -- regardless of how pernicious those beliefs may actually be. The enormous and incontrovertible psychological literature documenting motivated reasoning is over 20 years old (Kunda 1990). However inconvenient such research may be for certain oversimplified assumptions about human rationality which may still commonly inhabit philosophy as a discipline, the evidence is undeniable that human beings are, in fact, predictably irrational. Peirce not only brought this to our attention in 'The Fixation of Belief', he specifically designed scientific modes of thought to carry both falsificationist and social consensus elements together in his pragmatic, scientific epistemology to overcome this inherent human tendency. As things have turned out, Peirce has been overwhelmingly validated by current psychological research. Perhaps more surprisingly, Peirce was heavily influenced by Darwin and speculated about the evolutionary pressure which must underlie such irrationality, anticipating the same underlying structure which would later motivate the above quoted epidemiologists of pernicious health beliefs when they suggested their work be characterized "... as contributing to the developing field of Darwinian medicine" (Peirce 1955). Human beings are prone by evolutionarily tendencies to readily absorb information which agrees with closely held beliefs but to automatically reject (or at least reject with substantial prejudice) ideas which appear to conflict with their currently, very closely held, beliefs. We will again observe when we take on relativist and non-realist epistemologies, the disturbing point we brought up before: that people who support medical pluralism are 100% non-realists. Given the emerging facts about motivated reasoning, we do not feel we have a long way to go to assert that non-philosophers who are already committed to medical pluralism, ontological pluralism, and relativism, would simply not be motivated to agonize over Feyerabend's baroque argumentation beyond learning the basics of it for rhetorical use. They would not be motivated to grind away on the structure of the arguments and make evaluations like professional philosophers, rather, to people outside the discipline of philosophy, Feyerabend is just evidence -- evidence that they are right -- which is the only kind of evidence they are looking for. To the extent that philosophers and scientists are theoretically trained to resist this tendency a little more effectively than non-philosophers and non-scientists, or at least are more capable of evaluating philosophical considerations on their merits, Peirce, Kitcher and the current author all would assert that philosophy, as a profession, owes the people of the world some assessment of Feyerabend's attack on rational epistemology specifically and

medical pluralism generally. To the extent that scientist and philosophers are human and thus prone to motivated reasoning like everyone else, we submit that Peirce's reliance on expert consensus cannot be ignored -- even amongst professional philosophers. In fact, we submit Paul Feyerabend and his personal commitment to Traditional Chinese medicine as evidence to that effect. It goes without saying however, that if the community of philosophers and their commitment to careful reason and minute disquisitions, as well as the razor sharp court of philosophical peer review, cannot rule out the possibility that a flagrant penchant for anti-authoritarianism and self-avowed epistemic anarchy might eventually emerge from philosophy as a serious philosophical theory, then other disciplines and the court of public opinion are even more in need of serious philosophical and epistemic help. Lest Feyerabend's atrocious epistemic ideas become available for export as scientific epistemic expertise, for anyone who might see them as useful for their political agenda, Kitcher's ideas must be taken seriously. In the meantime, it is our contention that medical epistemology should organise a response to medical pluralism, and export some sense of the level of disciplinary consensus among medical epistemologists regarding the ontological and epistemic issues at stake. As we mentioned above, the resulting clarity is missing from the debate, and this missing clarity is directly responsible for massive harms which we will demonstrate in due course.

To summarize thus far, we are arguing that Peirce's scientific epistemology builds the psychological tendencies of human beings (the findings of contemporary experimental psychology) into itself, while Kuhn simply saw human psychology as an obstacle to both commensurability and paradigmatic change. Peirce coherently builds sociological components into his epistemic chain, they allow us to acknowledge the insights from Kuhn as historian of science, but effectively healing the damage done by the excesses of Feyerabend's epistemic anarchy and even providing a way to overcome "social constructivism" in the sociology of science by building a social warrant for "scientific expertise" which maps directly onto the current research and theories of knowledge and expertise coming to light in the sociology of science and expertise. Secondly, philosophy does not seem to export practical solutions to problems adjudicated by expert consensus in the same way science does. However, to the extent that it is possible to do so (and it is more possible to do so the closer the discipline gets to scientific epistemology), it should do so. Destructive philosophical ideas, repeatedly injected into the medical pluralism discourse outside philosophy, are often (particularly when presented by medical pluralism advocates), wrongly presented to non-philosophical audiences *as philosophical expertise*. This is deeply problematic and can and should be addressed in the context of this project by collecting any evidence that may prove useful in evaluating the philosophical community's position on these issues and by asking medical epistemologists, as a field of expertise, to organize themselves over this issue in order to make collective statements designed to undo the damage done by refusing, up to this point, to take responsibility for the proper dispensation of its expertise. To the extent that we want the philosophy of science to assume its rightful place in the

process of solving humanity's problems, we must be willing to make some *sociological* evaluations and proclamations about philosophical ideas in addition to philosophical ones.

This very cursory discussion sets the stage for our first in-depth look at the epistemic engine we will build as the core of the thesis. This next section outlines the basis for the bespoke epistemology we will develop and apply to specific questions of medical efficacy. *Pragmatism* will be explored and evaluated as the broader epistemic foundation for our very specific evaluations and prescriptions about how we might come to know if medical practices work or not and what we should do with this knowledge. Along the way, whenever possible, we will include anecdotal references to sociological facts about the popularity of ideas among philosophers at different points in time in an effort to address the above problem and to assist any audience members outside the discipline with relevant sociological context.

Pragmatism: the best epistemic engine for a bespoke epistemology designed to dispel medical pluralism.

Peirce is the philosophical engine behind James' aspirations, as laid out by Peirce in 'Questions Concerning Certain Faculties Claimed for Man' (1868); 'Some Consequences of Four Implications' (1868); 'The Fixation of Belief' (1877) and 'How to Make Our Ideas Clear' (1878)). Based on these four articles, Kitcher argues that Peirce is making the case for the abandonment of the "...static picture of human knowledge... justified in terms of evidence available to the individual, ...". Instead argues Kitcher, Peirce reasons that we should embrace a more (Kitcher 2011a):

...dynamic picture...that sees us as dependent on one another and on those who have preceded us and that asks not for the justification of belief but for the justification of change of belief.

We should be working to " ... identify the points at which genuine doubt arises...[and working]...to replace what is problematic with something better" (Kitcher 2011a). Knowledge is best produced by a dynamic and, in historical terms, a communal process. Knowledge advances toward success in the long run, through changes in belief justified by experimental falsification and logic, as determined by the community of experts in the relevant field.

These ideas converge with four central themes of this thesis: 1.) that medicine, and thus medical epistemology, is a fundamentally outcome oriented enterprise and for this reason, is best served by *pragmatism* as an epistemic basis; 2.) that *pragmatism's* conception of the continuing scientific project as a process of confronting and resolving problems, problems that arise from new circumstances including new falsifications, and that this progression and its consequences for

correspondence in the long run is what Rorty and Feyerabend fatally ignore in their flawed efforts to unmake scientific authority, 3.) that the continuing expansion of scientific knowledge and the centrality of the community of researchers and their methods to Peirce's epistemology, makes the philosophy of expertise an unavoidable requirement for an understanding of, and a prescription for, the means by which our correspondence, as pragmatic consumers of knowledge, is derived through expert consensus; and 4.) that medical science is failing to resolve obvious philosophical problems related to medical pluralism, and that philosophy, keeping number 3 above in mind, must address, propose solutions to and adjudicate the proposed solutions to these problems as a community of professional philosophical experts, exporting the solutions with the necessary attendant evaluations of expert philosophical consensus. Just as problems more easily addressed by the sciences become apparent as humanity lives in and reacts to the world (the need to move or communicate rapidly - communications and transportation networks and infrastructure), so too philosophical problems emerge from our (and thus science's) interaction with the parameters of the real. Science simply does not possess the expertise required to solve these philosophical problems unaided -- to understand its own procedures and problems. Therefore, philosophy and science must remain intimately and inextricably linked: with science explicating practical solutions to some problems rooted in matters of fact and philosophy explicating practical solutions to problems arising in the practice of science itself, as well as other domains as required, if with varying levels of authority.

Having engaged 3, and in doing so, seen some excellent evidence for 4, we now turn to 2 with an eye to 1. Keeping 1 in mind, allowing that although curiosity and explanation exist as motivations for other epistemic questions, this project will involve the building of a very specific pragmatic form of epistemic system, solely as a means to material, medical ends. Our understanding of expert consensus will be expanded in a subsequent section, engaging with sociological and philosophical literature in the philosophy of expertise, because articulating the concept of "expertise" is central both to understanding what is entailed in the derivation of medical practice as an expertise, and also to the way we will argue that we should understand *the authority* of philosophical expertise in political contexts like world public health policy. It is important to keep expert consensus in mind in *this* section, for the development of *our specific* epistemology for two other reasons. The first is because of what "expertise" means for our role here, what we are trying to do and how we should go about it. We are seeking to export philosophical expertise to other experts in the medical field. As per number 4, we will seek to identify and clarify epistemic problems facing scientific medicine which have recently arisen from developing circumstances. That means getting very specific about what these questions are, including very brief acknowledgement of how they have arisen and why they are important to medicine right now. We will seek to map these problems onto mainstream philosophical debates, and export a snapshot of the current state of play in expert philosophical consensus on the relevant subjects, specifically as prescriptions for better ways for medical professionals to do medicine. In Kitcher's words, our pragmatic "*...task is to identify the points at which genuine doubt*

arises, to scratch where it really itches, and to replace what is problematic with something better” (Kitcher 2011a). Secondly, we must keep expertise in mind for the building of a pragmatic medical epistemology because expertise is essential to the scientific process and knowledge production. As we shall see, pragmatic epistemologies gain specific access to correspondence through falsification events, while a more general or diffuse correspondence is associated with the world in which we live. It is from this more diffuse form of correspondence that our doubts and problems arise. One of the principle epistemic questions arising from medical pluralism is whether a given medical procedure works or not. The list of possible medical procedures is, however, infinite. It is very inefficient to try and test every possible one, thus giving rise to a strong motivation to adjudicate whole medical systems. Medicine for decades earlier in the 20th century relied on an ontological short cut in this regard. Until it was overcome, by scientific medical culture's often crude over-reliance on a poorly understood Popperianism, cashed out in the 80's as an overturning of the standing ontological rejection of medical pluralism. When such options are removed, this is where the relative rarity of falsification events can seem like the scattering of holes in a target from a distant shotgun, the relative lack of them leaving us with an even greater role for adjudication by scientific or philosophical expert consensus. This, we will argue, should begin with a re-assertion of authority for such ontological considerations. It is essential to later sections of this project to establish *for us* that the boundaries of scientific medicine are established by expert consensus and that the efforts of some to degrade the epistemic warrants for science, and thus to undermine efforts at demarcation, fail in the specific cases of our highly prescribed questions about medical efficacy that are central to this thesis.

Rorty, Feyerabend and Traditional Chinese Medicine

Peirce begins ‘Fixation of Belief’ with a nod to the link between history and epistemology by identifying a few of the essential elements of science in their historical context, as they came into existence. He builds a case for the notion that “...each chief step in science has been a lesson in logic.” Observation, logic, statistics, probability and finally, predictions and explanations “...in which questions of fact and questions of logic are curiously interlaced.” Here, Peirce is seeking the isolating of a core epistemic warrant: a simultaneous description of a process we *should* use to get knowledge and the justification of that knowledge as epistemically special.

As if he had already read the "motivated reasoning" literature we previously mentioned briefly, which psychologists would develop 100 years after Peirce's writing, Peirce opens with the sardonic observation that everybody figures their powers of reasoning are “proficient”. It should be noted, we find ourselves facing a pluralistic health care marketplace involving questionable ontological elements as diverse as “Chi”, non-specific, transferable, vitalistic “healing energy”, “subluxations”, the “memory” of water, witchcraft and the interaction between sin and an omnipotent

and merciful God. As we shall see, the constellation of pernicious health beliefs is more than vast enough and a generator of more than enough misery to justify our agreement with Peirce that the internal processes and evaluations of the personal beliefs of human beings are often profoundly mistaken and prone to systematic error. When Peirce suggested that our personal confidence in the accuracy of the things we believe is limited to our own personal “ratiocination” and does not necessarily “...extend to that of other men...”, he was making a basic claim preliminary to the epistemology he was about to prosecute: that people *can be wrong* about things *and* that other people can *know* both *that* those people are wrong and *why* those people are wrong. A substantial portion of this thesis will later be dedicated to tracing the philosophical history of immensely popular and influential ideas which are so epistemically sceptical and hostile to scientific authority that they would roundly reject all of the grounds upon which one might mount the above claim. There is a strong dispositional tendency on the part of some writers, or maybe even a universal human desire, to avoid telling other people they are wrong. The erroneous confusion of this urge with “tolerance” is a prime mover for sociological, anthropological and postmodern relativism as will be discussed in detail in a later chapter. In the discussion at hand, we will engage two philosophers: Richard Rorty and Paul Feyerabend, who are not above suggesting that someone else is wrong, but who also seek to attack scientific authority with extreme scepticism and level it with any other form of knowledge.

Dispositionally, Rorty and Feyerabend appear to be motivated by – or at least playing to -- the same anti-authoritarian objective, driven by the same rabidly democratic impulses, indulged to the point of anarchy. The irony here is that Peirce was engaged in a nuanced, psycho-social rendering of scientific epistemic practice very much as a progressive response to the blind acceptance of authority (and thus as part of the same Enlightenment project as not only Descartes but also Hume), but also to the remarkably unquestioning acceptance of absurd, unsustainable scepticism by philosophy itself as a major “problem” at that time. The fact that Rorty and Feyerabend have rendered this threadbare “scandal” of epistemic scepticism for their own proto-post-modern ends, is a perversion of anti-authoritarianism. Since they represent the extreme, and within the community of philosophers of science, unpopular and largely discredited continuum of analytic scepticism of scientific authority, we will engage Rorty and Feyerabend to demonstrate two things: that their arguments against method and correspondence are non-credible over-reactions to legitimate issues, and that there are deeply pernicious real world consequences to their lines of reasoning when applied to the epistemic problems developing around medical pluralism.

It is perhaps almost too much to ask that both Richard Rorty and Paul Feyerabend would aid us in our project to the point of weighing in specifically on Medical Pluralism, but they have. In the process, they have provided us with profound (even empirical) insight into the crippling effects of their one-sided epistemic beliefs on their personal and professional judgement. They both used acupuncture specifically as an example of an effective, non-scientific medical practice with ontological and epistemic origins incompatible with scientific knowledge. To both of them, the fact

that acupuncture works⁴ is evidence that science has the world wrong and is incapable of adjudicating the material claims of other knowledge systems. We will engage the specifics of their epistemic arguments in due course, but before we do that we will evaluate a few of the real world consequences of their radical epistemic programme. We will begin by briefly setting the historical stage for the times in which they were writing and then let Feyerabend and Rorty display in their own words, precisely how damaging their epistemology is when deployed in the real world of medicine.

At the start of the 1970's the clinical efficacy of acupuncture was entirely unknown. The research the Chinese had done was not credible and it was still very obscure in the West (Ernst 2008). During Nixon's trip to China, James Reston, a reporter from the *New York Times* who accompanied the Nixon delegation, underwent an emergency appendectomy, received acupuncture and returned to write very favourably about it. On Monday July 26, 1971, he published an article about his experience with acupuncture in the *New York Times*. This may have been the single greatest triumph of Chinese propaganda in the history of Western culture. However, the resulting flurry of interest and research was soon bogged down in the methodological problems inherent to blinding such dramatic interventions. As we shall see, the methodological limitations of clinical research make it impossible to completely blind Acupuncture trials, so the only "evidence" ever generated by clinical research is fundamentally biased to some extent, creating a shadow which unavoidably obscures the line between effects which are "real" and those caused only by flawed research methodology. In the popular culture, the optimism about acupuncture was outrunning any evidence for its efficacy. We will later document the devastating effects the rapid growth of CAM culture and its specifically anti-professional nature has had on the integrity of the medical marketplace, consumer trust in Evidence Based Medicine and to medical professionalism itself. In the 70's that process was just getting underway. *Against Method* and *Philosophy and the Mirror of Nature* were written during a time in which there was almost none of the large volumes of research available now, but the same basic epistemic conditions for the claims apply both then and now: claims that acupuncture is dramatically

⁴ We will provide the reader with a thorough analysis of acupuncture research in due course. For now we would ask the reader to consider five observations which resulted from that exploration: 1.) acupuncture as conceived, is a panacea and the high street shops in the UK as well as TCM practitioners all over the world are still advertising acupuncture as a cure for dozens of medical conditions -- still clearly reflecting this static, oblivious pedigree; 2.) the only thing scientific medical authorities suggest acupuncture works for is back pain and for this with the most profound marginality, the reader needs to keep in mind the astonishing downgrade this represents in the overall "acupuncture works" adjudication 3.) We are confident in asserting that acupuncture does not "work" for back pain either in the way it was intended to; 4.) Acupuncture is ontologically absurd and its original rejection by the Western medical establishment should have been upheld, as the judgements of the rest of TCM as hopelessly mired in superstition. TCM should never have been seen as anything other than potential ethno-medical and medical anthropological objects of research; 5.) If the American and British medical Associations had stuck with their original ontological rejection of TCM, taking the rest of the English speaking world along with them, and prosecuting TCM practitioners as "conducting medicine without a license", the loss of medical utility represented by the marginal efficacy for treating intractable back pain would have been outweighed by savings in the billions in the West alone, for utterly useless treatments still saturating the medical marketplace.

effective rely on data which does not rise significantly beyond the level of anecdote⁵. Thus was the historical epistemic status of evaluations of Traditional Chinese Medicine (TCM) at the time Rorty and Feyerabend were writing.

Feyerabend, in an early edition of *Against Method* (AM), does a credible job of broadly characterizing the position of the cooler heads on TCM at the time. Feyerabend cites John Eccles (“one of the ‘Popperian Knights’”) who had predicted the downfall of TCM based on the absurdity of its premises⁶. Across significant time and space, Feyerabend describes the position of the nascent Chinese system of scientific medicine prior to the communist revolution which (very appropriately) demarcated itself from the religious and magical elements in the old medical system that it wished to leave behind. It is from this full understanding of the convergence between diverse sources of scientifically rational perspectives on TCM across time and culture, that Feyerabend mounts his wholesale rejection of Peirce’s universal principles of rationality -- in favour of the coercive authority of the state. Displaying a pathological antipathy for what he calls “...the scientific chauvinism of the time...” Feyerabend declares that the Communist Party’s coercive, ideological suppression of emerging Chinese scientific medicine was perfectly justified. In a spectacularly ironic turn of argumentation, Feyerabend tells us that the “*special privileges*” granted to science had to be crushed by state authority in order to retain the “*Pluralism of theories and metaphysical views [which are] not only important for methodology, [but] also an essential part of a humanitarian outlook.*” (Feyerabend 1975, p. 38). To Feyerabend, Peirce’s ideas about science are so dangerous they need to be crushed by the state. Perhaps it was irrelevant to Feyerabend if acupuncture actually worked or not. But given the extent to which it can matter to a man who is so explicit in his rejection of universal principles of rationality, it appears that he did.

Rorty does not go into nearly the same level of historical detail. In *Philosophy and the Mirror of Nature* (p. 351), Rorty churlishly gloats over how “hopeless” science is in explaining acupuncture. It is safe to assume that the only thing Rorty could reasonably be referring to is the *efficacy* of acupuncture. He was, as we have addressed briefly in a footnote, completely wrong in his assumption that acupuncture “works”. If Rorty indeed seriously looked at the state of play in the evidence around acupuncture, he did a poor job, but as we shall see, this may not have mattered to him. He too has written off universal rules of rationality as a warrant for the epistemic superiority of science. As we shall see, the classical pragmatism of Peirce, precisely because it offers a subtle account of scientific method that allows for the adjudication of the epistemological worth of different scientific

⁵ We will consider the hierarchy of evidence in due course. For now the reader is asked to understand that anecdote is the most highly epistemically suspect form of scientific medical evidence, being tragically used to justify bloodletting as a therapy in the face of emerging clinical evidence for some time. Randomized controlled trials, or RCTs and the resulting amalgamation of them in meta-analysis are far superior epistemic tools.

⁶ Feyerabend does not cite any publication, so his knowledge of Eccles's position was in all likelihood the result of personal exchange. It would be instructive to see more philosophical reactions to TCM among Western philosophers at that time, as per our projects concerns with philosophical consensus and expertise.

methodologies or paradigms, and thus allows us to separate Feyerabend's too rapid conflation of scientific method and political culture, and Rorty's too hasty demand for a new scientific paradigm. Peirce further allows us to demonstrate how both Rorty and Feyerabend were wrong about the efficacy of acupuncture, wrong about the importance of absurd ontologies, wrong about science's ability to explain and predict the efficacy of TCM, and wrong about the epistemic status of science.⁷

The Fixation of Belief (TFOB)

Having framed a debate around the universality of rational principles and their consequences for medical pluralism with a little bit of history and an introduction to our interlocutors Richard Rorty and Paul Feyerabend, we may see how Peirce chooses similarly to set up his taxonomy of belief in the introductory sections of TFOB. Peirce begins by observing that "... the full possession of our power of drawing inferences..." is less of "...a natural gift..." and more of "...a long and difficult

⁷ In pages 36-37 of *Against Method* (Feyerabend 1975) [feyerabend1975against] Feyerabend makes the following claim:

"Everywhere science is enriched by unscientific methods and unscientific results, while procedures which have often been regarded as essential parts of science are quietly suspended or circumvented."

The process is not restricted to the early history of modern science. It is not merely a consequence of the primitive state of the sciences of the 16th and 17th centuries. Even today science can and does profit from an admixture of unscientific ingredients. An example which was discussed above, in Chapter 4, is the revival of traditional medicine in Communist China. When the Communists in the fifties forced hospitals and medical schools to teach the ideas and the methods contained in the Yellow Emperor's Textbook of Internal Medicine and to use them in the treatment of patients, many Western experts (among them Eccles, one of the 'Popperian Knights') were aghast and predicted the downfall of Chinese medicine. What happened was the exact opposite. Acupuncture, moxibustion, pulse diagnosis have led to new insights, new methods of treatment, new problems both for the Western and for the Chinese physician."

This is a vivid display of credulity. To the extent that dispositional factors guide our philosophy or vice versa, Feyerabend displays a clear predisposition for not only believing that TCM works, but for the positive evaluation of a system of medicine which he knows, judging from his apparent familiarity with the *Yellow Emperor's Textbook of Internal Medicine*, brings with it ontological commitments so at odds with both naturalism and rationality that they are wholly incompatible with Western Science: "Chi", "yin", "yang", "meridians" or metaphorical diagnostic systems with names like "wind", "cold", "fire", "dampness", "dryness" and "heat". To take such notions as the epistemic equal to medical science is a radical step and has attracted at least one author who stands on Feyerabend's shoulders in this regard in building a "...defence of the cognitive value of alternative medicine" (Kidd 2013). For Rorty, on page 351 of *Philosophy and the Mirror of Nature*, acupuncture's efficacy is something which of course can be assumed and serve as an offhand example of sciences "hopelessness":

"But contemporary science (which already seems so hopeless for explaining acupuncture, the migration of butterflies, and so on) may soon come to seem as badly off as Aristotle's hylomorphism."

Rorty's credulity, makes it difficult to avoid the perception that it is precisely Rorty's dismissal of science which destroys his bullshit detector – and causes him to assume the efficacy of something so miraculous with so little thought (Rorty et al. 1980).

art.” He suggests such a historical account of the genesis of significant innovations in scientific reasoning and technique would make a great book – and he was right. He anticipated a great deal of philosophical work which, beginning in the '60's would turn the history of science into a major contributor to the philosophy of science, a field which in Philip Kitcher's words "...suggests that the history of inquiry can serve as a laboratory or a field-station for the exploration and testing of proposals about the advancement of knowledge"(Kitcher 2011a). Although to the casual reader in the 21st century, it might seem natural for Peirce to begin this essay with a quick historical account of some of the developments in scientific logic, Peirce is actually building a philosophical case in anticipation of a field which will ultimately come to be known as the history of epistemology (more on this in a moment). Peirce notes Bacon's contribution to science in his emphasis on experimentation but observes that Bacon woefully oversimplified the scientific process and was painfully over-optimistic about what might be accomplished without further developments. Peirce allows Kepler's contribution to the aggressive, experimental pursuit of the mathematics which described planetary movement *exactly*. Peirce notes with no little respect Kepler's dogged presentation of hypothesis after hypothesis which allowed him to solve his problem. At the same time Peirce observes that more recent logical habits of mind and mathematics would have rendered Kepler's answer immediately. Peirce does substantive historical epistemology here (similar to modern history of science) and at the same time he does something much more philosophical: the history of epistemology (epistemic evaluations characteristic of philosophy) (Sturm 2011). Peirce makes the reason history is essential to epistemology very clear:

... every work of science great enough to be well remembered for a few generations affords some exemplification of the defective state of the art of reasoning of the time when it was written; and each chief step in science has been a lesson in logic.

From Lavoisier to Darwin, from experimentation and hypothesis to the application of probability and statistics to chemistry and biology respectively, Peirce paints a picture of a progressive science based on the progressive discoveries of the universal principles of logic – a science which is disposed to get things right in the long run.

Peirce then proceeds to anticipate the fields of evolutionary psychology and ethnoscience, as well as recent developments in psychological research on the irrational elements of human perception and decision making (see "motivated reasoning" above).

Logicity in regard to practical matters (if this be understood, not in the old sense, but as consisting in a wise union of security with fruitfulness of reasoning) is the most useful quality an animal can possess, and might, therefore, result from the action of natural selection; but outside of these it is probably of more advantage to the animal to have his mind filled with pleasing and encouraging visions, independently of their truth; and thus, upon unpractical subjects, natural selection might occasion a fallacious tendency of thought.

These elements are all central to the controversy over medical pluralism. As we shall see, perceptions of efficacy on a human (rather than statistical) level are distorted by the very “...*pleasing and encouraging visions...*” which Peirce describes and which are almost certainly examples of when “...*natural selection might occasion a fallacious tendency of thought.*”⁸ Peirce was astonishingly prescient.

Peirce reduces the movement from given premises to “...one inference rather than another...” to a habit of mind: “constitutional or acquired,” “good or otherwise”. These inference determining habits Peirce calls “guiding principles”. Peirce observes that we could write a book and write down the most important of these for reasoning, but most people would ignore them in favour of the “thoroughly beaten path” they daily take in the service of routine. Peirce again is getting at the need for scientific method when involved in explorations of new frontiers of inquiry beyond the “thoroughly beaten path”. It is here, Peirce asserts, that the guiding principles of reasoning become indispensably useful.

Peirce then proceeds to link belief and action. We act or move to obtain a desired condition when we believe we know the relevant facts. We ask questions when we doubt, and other than the act of asking questions, doubt does not motivate action. On the contrary suggests Peirce, we do not like the relative discomfort and paralysis of doubt and struggle to free ourselves from it. Here Peirce anticipates the contemporary research on attitude change and motivated reasoning, which clearly shows that we are loath to abandon closely held beliefs, even when we are confronted with evidence devastating to our position. Peirce puts it like this: “On the contrary, we cling tenaciously, not merely to believing, but to believing just what we do believe.” As Peirce moves toward his taxonomy proper, he terms the struggle to achieve belief, motivated by the irritation of doubt, “*inquiry*”.

Peirce characterizes changing beliefs as solely motivated by a breakdown in our surety that the way we are going about doing things is going to get us what we want. It might be useful to consider Alexander Bain's notion of a belief as “that upon which a man is prepared to act.” Bain attended a “Metaphysical Club” with James and Peirce, and Peirce was influenced by Bain's idea. The practical nature of Peirce's approach, to tie epistemology itself so profoundly to relatively empirical assertions about human psychology and behaviour cannot be overstated: doubt matters because when in doubt, we do not *know* how to act, or how to go on. Doubt happens when it becomes apparent that our actions are likely to no longer result in the fulfilment of our desires. At that point, belief is replaced by the doubting process, the doubting process begins, action stops and the inquiry process takes over until another belief is achieved. The sole purpose of inquiry is to get ourselves back to a stable, comfortable state where we know what to expect from our actions, should we choose to undertake them.

⁸ Of course, this explains Rorty and Feyerabend's credulity. As we will shortly be at pains to demonstrate, human beings are too prone to perceptual distortions and limitations to evaluate medical efficacy very well all by themselves. This is precisely why medical science built clinical medical epistemology and methodology.

At this point Peirce introduces fallibilism and brings pragmatism another step forward: he observes that of course, opinion is not the same as absolute truth. That is to say that whatever belief we might be said to hold at any given moment must (with the exception of the temporally unavailable consensus of experts in the "long run") be seen as necessarily impossible to square with any certainty as the absolute truth. The process of inquiry brings us to a belief, the quality of that belief being entirely dependent upon the quality of the inquiry process. The resulting belief is by no means *necessarily* a true belief, in the correspondence sense of true (and thus does not necessarily map the world as it is independently of the human observer). Peirce may have little confidence that we have access to perfect correspondence *yet*, but he is unmistakably a realist in that he believes the real world is out there for us to discover. Our beliefs are, however, just what we happened to come up with given a particular mode of inquiry. Crucially, the real world exerts an influence upon the formation of belief through an unavoidable process of fallibilism. Eventually, any false belief will be found out as such, because actions based upon it will fail. More sophisticated methods of inquiry will incorporate this mechanism, consciously, into their procedures. That is it for inquiry and belief. Although the real world is out there and we react to it, and slowly, through a much more programmatic process of falsification, our communities of experts will inexorably converge on a true understanding of it, any *short cuts* to absolute truth are abandoned as “...*nothing out of the sphere of our knowledge can be our object, for nothing which does not affect the mind can be the motive for mental effort.*” Peirce argues that this simplicity, specifically the claim that “...opinion is the sole end of inquiry...” sweeps away flawed conceptions of proof and he begins to innumerate these “...vague and erroneous conceptions of proof.”

The first notion of proof Peirce takes aim at is Cartesian scepticism: methodically doubting everything. He asserts that it is not possible because we are too practical for that. We cannot just will ourselves to doubt every “... proposition [we put] into the interrogative form...” Without “living doubt” there is no real inquiry. In the same way that Hume could not envision actually conducting one's day to day existence in the paralysing fog of a sincere Cartesian state of doubt, Peirce recognizes that Descartes was describing a thought experiment, totally inapplicable to the actual psychological state of any real human being. Human beings, notes Peirce, are motivated -- always -- by practical necessity. Next Peirce takes a shot at foundationalism more broadly and suggests that first principles and first sensations are irrelevant. If an inquiry has resolved to our complete satisfaction following a “*demonstration*”, and the propositions are “perfectly free from all actual doubt” then that is all that is required: “If the premisses are not in fact doubted at all, they cannot be more satisfactory than they are.” Peirce seems to anticipate Kitcher's indictment of contemporary philosophical hand wringing when he concludes with the observation that:

Some people seem to love to argue a point after all the world is fully convinced of it. But no further advance can be made. When doubt ceases, mental action on the subject comes to an end; and, if it did go on, it would be without a purpose.

Inquiry is a practical effort to get what we want. It results in beliefs which may or may not be true.

The method of tenacity

As Peirce embarks on the last section of 'The Fixation of Belief', he dedicates a substantial section to his nascent theory of "motivated reasoning". The section is worth quoting at length because Peirce directly touches on two ideas we have addressed: 1.) the notion that some consumers of information (in our case, perhaps a bit too optimistically, non-philosophers), might be vulnerable to some arguments due to their lack of philosophical expertise; and 2.) the ideas more central to motivated reasoning, including cognitive dissonance and biased cognitive processing -- Ziva's seminal 1990 psychological paper on the subject that we previously mentioned, which to date according to Google Scholar, has been cited over 2800 times (Kunda 1990).

If the settlement of opinion is the sole object of inquiry, and if belief is of the nature of a habit, why should we not attain the desired end, by taking as answer to a question any we may fancy, and constantly reiterating it to ourselves, dwelling on all which may conduce to that belief, and learning to turn with contempt and hatred from anything that might disturb it? This simple and direct method is really pursued by many men. I remember once being entreated not to read a certain newspaper lest it might change my opinion upon free-trade. "Lest I might be entrapped by its fallacies and misstatements," was the form of expression. "You are not," my friend said, "a special student of political economy. You might, therefore, easily be deceived by fallacious arguments upon the subject. You might, then, if you read this paper, be led to believe in protection. But you admit that free-trade is the true doctrine; and you do not wish to believe what is not true." I have often known this system to be deliberately adopted. Still oftener, the instinctive dislike of an undecided state of mind, exaggerated into a vague dread of doubt, makes men cling spasmodically to the views they already take. The man feels that, if he only holds to his belief without wavering, it will be entirely satisfactory.

This is Peirce's first method of fixing belief: this is the method of tenacity. It might at first appear as a shortcoming that Peirce seems to describe this as a global trait, as though he means that there are some people like this, or some societies like this, particularly as he moves on to other methods of fixing belief, like the next one he describes, the method of authority. However this is not the case, rather Peirce is recognizing a universal human tendency, one which is present in all of us. A tendency to cling to closely held beliefs and to fight internally against rational evidence which might serve to contradict them. But Peirce is quick to mention that this process does not take place in a vacuum.

This conception, that another man's thought or sentiment may be equivalent to one's own, is a distinctly new step, and a highly important one. It arises from an impulse too strong in man to be suppressed, without danger of destroying the human species. Unless we make ourselves hermits, we shall necessarily influence each other's opinions; so that the problem becomes how to fix belief, not in the individual merely, but in the community.

We will, when we examine the arguments for medical pluralism greater detail, recognize distinctly, this kind of unshakable ideological commitment. The culture of CAM in the United States and the UK in many ways reflects this kind of commitment, understandably, as in many cases, businesses, careers, livelihoods and identities are on the line. Sliding from one side of the medical pluralism belief system to the other can have terrifying consequences for a homeopath or a practitioner of Reiki: they could in one act of changing an ontological belief go from the possession of a long and cherished history as a helpful medical practitioner, to someone who has just made the transition from negligence to fraud. Upton Sinclair, the American socialist, was wielding a tacit understanding of cognitive dissonance when he quipped: “It is difficult to get a man to understand something, when his salary depends on his not understanding it” (Sinclair 1935) (Sinclair 1935). As the motivated reasoning research got underway 20 years ago, researchers wondered why cognitive dissonance, a well understood phenomenon had not been previously applied to cognitive processes:

Interestingly, this view of motivation as cognitively mediated has always been integral to the understanding of dissonance reduction phenomena, at least in theory. In the 1968 source-book on consistency theories, McGuire expressed regret that the dissonance ideas had not been used to shed light on cognitive processes (Kunda 1990).

This is what Peirce was getting at in the previously cited paragraph. Our beliefs, no matter how well defended, are ultimately subject to influence by other people. Humans are social animals and ultimately, beliefs are fixed within a community. Cognitive dissonance serves to motivate people to avoid circumstances in which they might experience input which could challenge their beliefs. This is not always possible, however, particularly since, as we shall see, complementary and alternative medicine overwhelming fails to actually generate therapeutic effects, other than those which might result from deception. As a result, patients with serious medical problems often (but unfortunately not always), wisely, will see orthodox, scientific medical practitioners when they are affected by a serious medical problem. The patients are at least aware that their doctor may not share their medical pluralistic beliefs. Although, potentially, embarrassment might play a role in what they would choose to share or not share with their doctor, research suggests that those who use complementary and alternative medicine are frequently reluctant to tell their orthodox medical general practitioner about it (Ventola 2010) (Ventola 2010). We would argue that the case of medical pluralism is very much a matter of conflicting 'belief communities', whose interests are personal, social, economic and ideological.

The method of authority

Let the will of the state act, then, instead of that of the individual.

Peirce then introduces the second method of fixing belief: the method of authority. It is probably important to note that the American Civil War had just drawn to a close, leaving an indelible, collective state of shock following killing on a scale the citizens of that country had not previously thought possible. Perhaps it is for this reason that Peirce felt it necessary to discussing the process and to his mind the ultimate failure associated with running collective belief systems through the power of the state. Perhaps this is worth mentioning simply because of the fear that such tyranny can illicit in most of us. It is however notable, as well, that the advocates for medical pluralism, and, as we shall see, authors of relativist anthropological and sociological studies of Traditional Medicine, commonly see Western Medicine as Feyerabend did: as a form of chauvinism, authoritarianism or imperialism. On the other hand, what actually occurred in China, during the cultural revolution, in regard to the enforcement of Mao's choice of Traditional Chinese Medicine as the official medical model of Communist China (which necessarily involved the destruction of the embryonic Western Medical establishment there -- see foot note number 6), is still reverberating today. The Chinese, driven by both entrepreneurial spirit and national pride, have been aggressively marketing Traditional Chinese Medicine worldwide for decades and continue to do so.

The a-priori method or "fashion."

Let the action of natural preferences be unimpeded, then, and under their influence let men, conversing together and regarding matters in different lights, gradually develop beliefs in harmony with natural causes.

Peirce observes that everyone is at one point or another free to make up their minds about some things, free of either deep personal commitments or a government mandate which might necessitate some or another belief upon the pain of some great penalty. People, Peirce suggests, when they are left to make up their minds about things unrestricted by anything at all, are essentially engaged in a matter of taste. If neither previous personal preference, some kind of obligation from an external authority or some other intrusion by reality exist to compel one belief or another, our choice is nothing more than fashion. He ties this immediately to metaphysics suggesting that

...accordingly metaphysicians have never come to any fixed agreement, but the pendulum has swung backward and forward between a more material and a more spiritual philosophy, from the earliest times to the latest.

Thus Peirce weighs in on Kitcher's criticism of philosophy, and as we have observed, perhaps referred indirectly, to several of our main themes, including: 1.) the likelihood that it is science, just as Peirce is about to suggest in his final method of fixing belief, which provides "...*some external permanency...*", which allows us to escape the confusion and inevitable disagreement of fashion and taste; and thus 2.) the possibility that the epistemology of science carries both a special capacity for and an epistemic requirement for sociological evaluations of expert consensus.

The method of scientific investigation

From his discussion of the inadequacies of the preceding three methods of fixing belief, all of which might work in the short run but, all importantly, not in the long run, Peirce concludes:

...therefore, it is necessary that a method should be found by which our beliefs may be determined by nothing human, but by some external permanency -- by something upon which our thinking has no effect.

The scientific method, to Peirce involves both psychological and sociological components, but they reduce to a simple notion: to use logic and reason to observe the real, and order ones beliefs and interactions with others so as to remain consistently truthful to these ideals and to the other members other members of the community in the pursuit of inquiry.

It is remarkable that Peirce specifically addresses the idea that he is not talking about "*mystics*" who claim their "*private inspiration from on high*" because this so closely matches the quasi-religious aspects of many medical pluralism modalities. In fact, most of the CAM modalities in the West, all "energy" medicines, homeopathy, and foreign Traditional Medicine imports like TCM and Ayurveda rely on revealed knowledge of the supernatural as their main selling point. Even naturopathy and chiropractic, relying respectively on the idea that "natural" things are healthy and that "subluxations" are responsible for all illness, are just claims to esoteric knowledge, which practitioners claim to have learned by attending special schools. The modalities encouraged by medical pluralism, by virtue of the isolation of their justifications from the real, in the form of falsification, or rational, expert adjudication by a community of scientific experts, is simply not the scientific method of investigation. Rather:

...that is only a form of the method of tenacity, in which the conception of truth as something public is not yet developed. Our external permanency would not be external, in our sense, if it was restricted in its influence to one individual.

Thus Chiropractic, conceived of in 1895 by a magnetic healer, who theorized that what he imagined might be "God's presence in man", in the form of vitalistic forces he called "innate intelligence", could be manipulated to cure *all disease* (Martin 1993) was conceived of by one person. After specifically

considering using his idea to start a religion, he happened upon the idea of starting a school instead and Chiropractic was born. It should be noted that Chiropractic shows no evidence of contact with the real in the form of any changes to its basic premise today and as such illustrates the psychological points made above in relation to the method of tenacity. Even with the "regulation" of a statutory regulatory board under the department of Health in the UK (the general Chiropractic Council GCC), the only serious contact with the real has been the reduction in what it can now publicly claim to treat, by virtue of changing European Trading Standards Law and the threat of enforcement by the Advertising Standards Authority. Incredibly, what was only a decade ago, a relatively unrestrained embrace of its heritage as a panacea, advertising itself through the statutory regulatory board website as capable of treating all known disease, has been reduced to its current state, as reflected by the absence from the GCC's website today, of a single specific medical condition which they feel they can list as treatable :

Chiropractors provide care for patients of all ages, who present with a range of acute and chronic conditions. As well as advice about self-help, exercise, diet and lifestyle, chiropractors often provide support for pain management, sports injuries and active rehabilitation.

In contrast to such tenacity and mysticism, the scientific method of fixing belief must involve a more direct connection with the real, one which, *"...must be such that the ultimate conclusion of every man shall be the same."* As we shall see in some detail, this has not been the case with Chiropractic. Although the study of Chiropractic is dogged by the same methodological problems which render double blinding impossible in a variety of touch, poke and talk therapies, which necessarily involve "sham" procedures designed for control conditions to blind subjects to their assignment to the experimental or control group, the evidence for Chiropractic's efficacy is 100% consistent with the fact that it was made up, out of thin air, by a single person, in the 19th century and has no basis what-so-ever in reality.

Peirce lays out the epistemic basis for his method:

...Reals affect our senses according to regular laws, and, though our sensations are as different as are our relations to the objects, yet, by taking advantage of the laws of perception, we can ascertain by reasoning how things really and truly are; and any man, if he have sufficient experience and he reason enough about it, will be led to the one True conclusion.

Thus replication is a component of the scientific method. But it is at this point that Peirce engages the challenge: *"...how I know that there are any Reals."* Peirce's arguments become considerably more subtle here:

If this hypothesis is the sole support of my method of inquiry, my method of inquiry must not be used to support my hypothesis. The reply is this: 1. If investigation cannot be regarded as proving that there are Real things, it at least does not lead to a contrary conclusion; but the

method and the conception on which it is based remain ever in harmony. No doubts of the method, therefore, necessarily arise from its practice, as is the case with all the others.

This is the tentative fallibilism which we find so appealing about Peirce's epistemology. Peirce is unconcerned about the kind of aspirations which would plague positivism and even Popper's near anti-realism. Peirce is only concerned with making sure the actions his epistemology motivates make sense. He deftly eschews any stronger realist claims at this point. Peirce then proceeds to leverage psychology and the human tendency toward pragmatic action in the way which might have prompted Popper to apparently comment that he wished that somebody had told him about Peirce: Peirce fills in the gaping holes around Popper's falsification with a loose association with the real, an association implying just enough contact to motivate human discomfort:

The feeling which gives rise to any method of fixing belief is a dissatisfaction at two repugnant propositions. But here already is a vague concession that there is some one thing which a proposition should represent. Nobody, therefore, can really doubt that there are Reals, for, if he did, doubt would not be a source of dissatisfaction. The hypothesis, therefore, is one which every mind admits. So that the social impulse does not cause men to doubt it.

What else could it be? Something has changed and caused us to lose confidence in the belief we once held in the ready to act upon when the need arose. But now, *something* has caused us to experience doubt. As Peirce quite humorously puts it toward the end of his essay:

It is the nature of the process to adopt whatever belief we are inclined to, and there are certain flatteries to the vanity of man which we all believe by nature, until we are awakened from our pleasing dream by rough facts.

It is the "rough facts" which are the real. The school of hard knocks. Experiences uncomfortable enough to induce uncertainty. Non-realists, postmodernists, "social constructivists" and relativists would be forced to argue that the *something* which has motivated our loss of faith is very specifically *not* the real. We disagree. There is no other likely candidate. Peirce escapes this delicate demarcation of the real with two final points:

3. Everybody uses the scientific method about a great many things, and only ceases to use it when he does not know how to apply it. 4. Experience of the method has not led us to doubt it, but, on the contrary, scientific investigation has had the most wonderful triumphs in the way of settling opinion. These afford the explanation of my not doubting the method or the hypothesis which it supposes; and not having any doubt, nor believing that anybody else whom I could influence has, it would be the merest babble for me to say more about it.

Providing that our only concern is to connect ourselves with a reasonable course of action under the circumstances, we are finished with the core engine of our bespoke epistemology for this project. By avoiding doubts about our epistemic objectives and our means to achieve them, we have escaped the

scepticism of Descartes, Hume, Locke, Rorty and Feyerabend -- and done so in a way which allows us the utmost simplicity in justifying a universal public health policy -- this is the best way to prevent and treat illness. As Peirce points out, the scientific method of fixing belief is the only one of the four which demarcates right and wrong: the method of tenacity is impossible to get wrong, as we only need to think what we feel comfortable thinking; the method of authority is impossible to get wrong as it is the state which bears responsibility; the a priori method is impossible to get wrong because whatever seems to be the right thing to do based on the input from others, cannot be significantly contradicted or confirmed by anything or anyone...and so boils down to a matter of taste. Peirce's discussion of Hegel is instructive here:

So with the a priori method. The very essence of it is to think as one is inclined to think. All metaphysicians will be sure to do that, however they may be inclined to judge each other to be perversely wrong. The Hegelian system recognizes every natural tendency of thought as logical, although it be certain to be abolished by counter-tendencies. Hegel thinks there is a regular system in the succession of these tendencies, in consequence of which, after drifting one way and the other for a long time, opinion will at last go right. And it is true that metaphysicians do get the right ideas at last; Hegel's system of Nature represents tolerably the science of his day; and one may be sure that whatever scientific investigation shall have put out of doubt will presently receive a priori demonstration on the part of the metaphysicians.

It is the explicit license to integrate scientific metaphysics into scientific practice which will come to be very important in our arguments with EBM epistemology. However, we are confident that philosophical consensus over these matters will not be difficult to achieve. We agree with Peirce when he says that *"metaphysicians do get the right ideas at last."*

Peirce simply makes the case at this point that it is quite possible to do science *wrong*. Which interestingly is what a substantial portion of this project is committed to demonstrating, which is to say that bad science has actually occurred in scientific medicine's attempts thus far to evaluate the efficacy of CAM modalities specifically and medical pluralism generally. As is so often the case with CAM researchers, their ideological commitments doom them to an endless cycle of bad research designs, bad research data, bad research conclusions, bad research funding and more bad research designs to continue the cycle indefinitely. At every stage, they do their best to publicize their "progress" which in reality actually demonstrates the same thing over and over again: biased studies "demonstrate" effects, and robust studies do not (Ernst & Pittler 2000). As Peirce put it:

The test of whether I am truly following the method is not an immediate appeal to my feelings and purposes, but, on the contrary, itself involves the application of the method. Hence it is that bad reasoning as well as good reasoning is possible; and this fact is the foundation of the practical side of logic.

Science has to be dispassionate and pay attention to the method. Over and over again, that is the lesson we learn from CAM research. As we shall see, the last three decades since scientific medicine

abandoned its ontological commitments in the face of public pressure applied at least in part to bad philosophy, have been spent mired in flawed research practices funded and run largely by people whose methods were dictated by their "feelings and purposes" rather than a commitment to a logically rigorous method. As Peirce prepares to close his essay he reflects on precisely the failures associated with bad CAM research and bad the philosophy which continues to muddy the epistemic clarity required to see it as such:

The force of habit will sometimes cause a man to hold on to old beliefs, after he is in a condition to see that they have no sound basis. But reflection upon the state of the case will overcome these habits, and he ought to allow reflection its full weight. People sometimes shrink from doing this, having an idea that beliefs are wholesome which they cannot help feeling rest on nothing.

It will be necessary to confront the psycho-social and political forces which have marshalled in defence of medical pluralism. Because, behind the strange anarchy of medical anthropology's relativism for example, lurks the raw fear associated with telling a whole culture that their ontological and epistemic commitments are killing them. It was female genital mutilation which finally tipped the balance after four decades of unilateral cultural relativism in medical anthropology. It will be argued in subsequent chapters that it is the philosophical confusion of "tolerance" with epistemic anarchy which has led us, at least partially to the current philosophical paralysis gripping sociology, anthropology, critical and cultural theory and probably philosophers like Rorty and Feyerabend as well. But now those pathological ideas have combined with Chinese lobbying to produce the World Health Organization's current, incoherent position on Traditional Medicine⁹. As Peirce so precisely advises us, pragmatism, the pursuit of important outcomes, must be fearlessly embraced.

The person who confesses that there is such a thing as truth, which is distinguished from falsehood simply by this, that if acted on it should, on full consideration, carry us to the point we aim at and not astray, and then, though convinced of this, dares not know the truth and seeks to avoid it, is in a sorry state of mind indeed.

Perhaps Peirce has left the realm of the philosophical with this his closing lines from 'Fixation of Belief'. We would argue that for our purposes, the martial metaphors are apt and as such we are saving them for the closing of this section. We have, after all, embarked on an ambitious project: We have

⁹World Health Organization, Traditional Medicine, Fact sheet N°134, December 2008

WHO and its Member States cooperate to promote the use of traditional medicine for health care. The collaboration aims to:

- support and integrate traditional medicine into national health systems in combination with national policy and regulation for products, practices and providers to ensure safety and quality;
- ensure the use of safe, effective and quality products and practices, based on available evidence;
- acknowledge traditional medicine as part of primary health care, to increase access to care and preserve knowledge and resources; and
- ensure patient safety by upgrading the skills and knowledge of traditional medicine providers.

combined Peirce and Kitcher in an effort to establish Peirce's epistemology as the epistemic engine at the core of our project, and to use Kitcher as an opening to apply the philosophy and sociology of expertise to get our product out of philosophy and into the world public health policy where its destiny ultimately rests. We will move from here to connect the basics of medical epistemology firmly to clinical efficacy. We will be working to build a warrant for action, by showing how evidence based medicine at its best exemplifies Peircean pragmatism, thus allowing us to take the clinical efficacy data that it yields and then to assemble the greatest possible authority for the prosecution of a full scale practical ethical project. In the course of this we will work through substantial critiques on the basic epistemic commitments of sociology and anthropology, and significant currents in philosophy, as we attack the arguments in favour of medical pluralism. Finally, we will engage the philosophy of expertise and professionalism with the latest sociological research in the sociology of scientific expertise and professionalism and the philosophy of belief in the effort to establish the responsibility for the application of the solutions we propose as widely as we possibly can. To this, we close this section with Peirce's final words from 'The Fixation of Belief', because he understood as we do, and as Kitcher does, that philosophy does not belong alone in an ivory tower, but rather engaged in a struggle to make the world better¹⁰.

...he knows that he was right in making that choice. And having made it, he will work and fight for her, and will not complain that there are blows to take, hoping that there may be as many and as hard to give, and will strive to be the worthy knight and champion of her from the blaze of whose splendors he draws his inspiration and his courage.

¹⁰ Our close focus on Peirce's single article "The Fixation of Belief" with only brief supplementation from his other writing, is itself an interdisciplinary strategy. As we have already stressed, this work is not an effort to advance expert Philosophy's understanding of Peirce's scholarship, rather we have chosen to use "The Fixation of Belief" as a powerful philosophical tool, to work around the epistemic pitfalls in which the interdisciplinary, yet necessarily realist debates around medical pluralism are currently mired. We have kept our discussion on Peirce brief, pointed, easily exportable and readily applicable as philosophical expertise. Of course, there is a great deal of scholarship on Peirce's other works. What follows is a list of additional sources, covering the wider significance of Peirce's diverse body of work (Apel 1995; Hausman 1993; Rosenthal et al. 1999; Murphey 1993; Haack 1995; Aboulafia 2002; Debrock and Hulswit 1994; Ketner 1995; Chiasson 2001; Putnam 1995; Hookway 2003).

PART II: Anthropology, Religion, Sociology, History and Medical Beliefs

Medical Anthropology

The Questionable Epistemology of Medical Anthropology: What public health policy experts from non-social science disciplines must understand before consulting the medical anthropological data regarding medical beliefs and Medical Pluralism.

The first objective of this section of the thesis is to leverage the expertise of medical anthropology, sociology and medical geography, to ascertain generally both how and how well the people on this planet understand their health and the extent to which the nature of the beliefs in question might be harmful. As straightforward as this might seem, this task is profoundly complicated by the epistemic habits of mind shared by the academic disciplines of anthropology and sociology today. Anthropology and Sociology generally pursue an epistemology incompatible with the rest of the scientific community as a whole (and not coincidentally, the pragmatic realism of our proposed epistemology of medicine). What surprised us, is that the epistemic rift between Medical Anthropology and the naturalistic sciences is *so* wide, *so* uniform, and *so* vexingly hidden from view for anyone not familiar with medical anthropology or the social sciences in general. We will argue that this fundamental epistemic incompatibility is the single most obfuscating factor in the interdisciplinary discourse on medical pluralism.

Medical Anthropology, shortly after its inception, discovered that the medical beliefs held by indigenous people were ubiquitously degraded by suspect ontological systems (Rivers 1924, Evans-Pritchard 1929). This discovery occurred roughly as Western medicine turned the final corner onto its current path, by rejecting vitalistic theories to embrace a uniform physicalism. As both fields progressed through the 20th century, the epistemic inequality inherent in traditional medical beliefs continued to grow exponentially with the increasing pragmatic potency of Western medicine. At the same time, the rejection of colonialism and empire, and the trauma of two World Wars created a perfect storm of anti-authoritarianism among Western academics, particularly in the social sciences. Medical anthropologists found themselves in possession of a profound discovery about the essential superiority of Western medical systems, at a time when all of academia was primed to search out and destroy every remaining vestige of chauvinism. Understandably, Medical Anthropology's observations of epistemic inequality between medical systems became a prime target. Unfortunately, as decades of additional research would finally reveal, although a few indigenous medical systems have generated limited means of prophylaxis, diagnosis and treatment, the jarring pragmatic epistemic inequality recognized by early anthropologist still holds.

Strangely, this observation -- that on the level of pragmatic power, early medical anthropologists were exactly right about traditional medical systems -- is completely lost on medical

anthropology today as we shall see. The anti-realism employed by medical anthropology effectively eliminates the category of "mistaken" for all but the tiny corner of barely allied research that is ethnobotany. By abandoning the very idea of epistemic inequality, modern medical anthropology has made it impossible for the vast bulk of its professional community to even address the idea of the efficacy of traditional medical techniques.

We will argue that the extreme epistemic posture of medical anthropology today is not the result of reasoned, interdisciplinary philosophical development (Peirce's fourth, scientific means of fixing belief). Rather, the field's adoption of radical anti-realism was an ad hoc change, made primarily in response to overwhelming social forces, both internal and external, which, to use a Peircian term, made it much more *fashionable* to attack the epistemic authority of Western medicine than to acknowledge the pragmatic realist implications of the profound epistemic inequality discovered by early researchers. To put it another way, as people all over the world languish at the mercy of benighted medical systems, Medical Anthropology, the field which discovered this is the case, has since chosen to uniformly ignore the issue. Instead, it is exporting a form of anti-realism -- one which has been rejected by the community of expert philosophers -- into interdisciplinary discourse, as advocates for traditional medical systems, in defence of medical pluralism.

Although we chose Peirce's *Fixation of Belief* as the best epistemic engine for a universal pragmatic medical epistemology, what we had in mind was the lower, more instrumentalist bar that pragmatic potency provides as a threshold for actionable truth. Frankly, it is something of a surprise that Peirce's epistemic model would prove as powerful as it has in unpacking the habits of mind associated with medical anthropology. As we attempted to interrogate the medical anthropological literature for the answer to our question: *how and how well do people in the world think about their health*, we discovered to our amazement, a field specifically opposed to the idea of comparing the validity of one culture's medical belief to another and similarly resistant to the obvious research objective of identifying particularly pernicious medical beliefs as part of a larger effort of world public health education. Finally, we accepted that we could not properly contextualize the relevant professional work products of medical anthropology in this section of the thesis without accounting for the associated epistemic peculiarities. To do this, we realized we would have to consider medical anthropology's epistemology as epistemology, but also historically and ultimately culturally and to some extent psychologically. The level of digression required is substantial but illuminating in that it allows us to connect the epistemic issues at the centre of this thesis with the defining events of the run up to and into the 20th century and the fracture points in the wider landscape of the theory of knowledge: in particular the distinction between naturalistic knowledge and hermeneutics, and the way social constructivism and allied anti-realist movements can be seen as attempts to collapse the relative epistemic authority of the former into the assumed incommensurability of the latter.

In the following section, we will briefly discuss the epistemic posture of modern medical anthropology, vis-a-vis the disciplinary consensus of Anglophone philosophy on the subject of anti-

realism (we will save the minutia of the epistemic discussion about anti-realism for Part III). We will then consider the historical roots of Medical Anthropology in the realist discipline of Public Health and the profound discoveries of epistemic inequality in indigenous medical beliefs made by early medical anthropologists. We will then argue that those early discoveries of epistemic inequality were so profound that the twisting of medical anthropology's epistemic posture into its currently contorted shape can largely be attributed to the fear that those early discoveries were simultaneously too critical of other cultures and too hubristic to actually be true. We argue that this situation was in fact the driver which necessitated the production of the epistemic rationale required to reject those discoveries as merely chauvinistic, rather than the extremely important medical realities they actually are. We will then consider the cultural and psychological factors which made this necessary, exploring drivers like anti-imperialism, anti-colonialism, anti-chauvinism and the aftermath of the World Wars. We will then turn to psycho-social tensions inherent between ethnography, advocacy and epistemic inequality. Finally, we will consider religion and the close relationship of medical anthropology to religious studies and the ways in which religious anti-realism drives medical anthropology's refusal to accommodate biomedical realism.

The Current Epistemic Posture of Medical Anthropology

Anthony Carter characterized anti-realist thinking in medical anthropology in the introduction to the relativist work *Medicine, Rationality and Experience* by Byron J. Good in 1994: '*...our conception of scientific knowledge as an objective mirror of nature has come to be less convincing*' (Good 1994, p. xi). Particularly in light of the allusion to Rorty, this is a strange position indeed for a field which grew out of the naturalistic epistemic motivations of public health. At the same time, Good himself proposes that this shift was not only motivated by factors within Medical Anthropology itself, but rather, naturally occurred in intellectual unison with all the academic disciplines associated with epistemology, including philosophy. On page 3, Good observes that although Medical Anthropology is a leader in anti-realist issues, the medical anthropological embrace of the most radical implications of full strength anti-realism is fully supported by an interdisciplinary consensus among philosophers, sociologists of science and science historians.

Our convictions about the truth claims of medical science rest uneasily with or recognition of our own historicity and our desires to respect the competing knowledge claims of the members of other societies or status groups. Indeed the confrontation between the natural sciences and historicism -- the view that all knowledge is unavoidably relative to historical context -- has been the central issue of philosophy, the sociology of knowledge and historical studies of science for much of this century. Within anthropology today, I would argue that medical anthropology is the primary site in which these issues are being addressed and investigated.

There is little reason to fear that Good is off the mark in characterizing medical anthropological orthodoxy, including the rationale behind Medical Anthropology's epistemic position. To the extent that ethno-botany constitutes a biomedical realist sub-disciplines, publishing within medical anthropology, its epistemic effects have yet to be felt on the field as a whole. However, as philosophers, we are considerably more certain that Good is absolutely wrong about the strength or uniformity of anti-realist consensus among philosophers. Good's above concerns about "historicity", "historicism" and "historical context" boil down to questions of incommensurability, the resolution of which through pragmatism rests at the centre of our claims to universality for our pragmatic medical model. As Good explains in greater detail a few pages later (p.22), it is realism which he claims is no longer tenable among the relevant experts engaged in epistemology, and it is the universal abandonment of realism, by all relevant disciplines and participants which constitutes what Good later refers to as "...this philosophical climate." In this new age of ambivalence, Good explains "the empiricist paradigm", "positivist approaches to epistemology" and the "empiricist theory of language" have all "...come under sustained criticism..." by relevant experts, including, of course, philosophers (Good 1994) :

Whichever authors one invokes -- Thomas Kuhn, Michel Foucault, Paul Feyerabend, Hilary Putnam, Richard Rorty, or a generation that grew up with these figures -- older theories of the relationship between language and empirical reality now seem dated. Rationality and relativism no longer neatly divide the field. Increasingly, social scientists and philosophers have joined in investigating how language activities and social practices actively contribute to the construction of scientific knowledge

It would appear that Good is unaware that Putnam is a realist, or, as we considered in the previous section, that Rorty and Feyerabend are radicals and outliers, whose ideas were never embraced as a philosophical consensus at any point, much less as an enduring transition. It is in response to such profound mischaracterizations of philosophical consensus that the failure of philosophers to speak as a unified profession is most sorely felt¹¹. Instead, it can be remarkably difficult to find even individual philosophers making observations about professional consensus. We are, however, not the only philosophers willing to go out on the limb of anecdotal sociological claims to assert that anti-realism is not the epistemic posture of most analytic philosophers today. Paul Boghossian, in the introduction to *Fear of Knowledge* sees professional philosophical consensus the way we do:

But for all their distinguished intellectual pedigree and for all the attention they have received in recent times, it remains fair to say that such anti-objectivist conceptions of truth and rationality

¹¹ This is precisely the problem we enlisted Kitcher to describe in Part I: to the extent that philosophers are a group in possession of expertise, they should be responsive to such damaging erroneous claims about the nature of philosophical consensus.

are not generally accepted within the mainstream if philosophy departments within the English speaking world (Boghossian 2006, p.8.)

It should be noted that the epistemic crux of this thesis, the universal nature of the weak pragmatist realism we use to drive our pragmatic medical epistemology, is far less contentious than realism as a whole. Regardless of the volume of ink spilled on the topic of anti-realism, Rorty and Feyerabend are extremely lonely as advocates for completely scrapping the authority of Western medical epistemology. To the best of our knowledge, philosophers let Rorty and Feyerabend's specific attacks on scientific medical epistemology, via their defence of Traditional Chinese Medicine specifically (see Part I), pass without addressing them seriously -- and certainly never holding them up as a warrant for the abandonment of scientific medical epistemology. It is false to suggest that there is a single sub-discipline of expert philosophers who support medical anthropology in its willingness to throw scientific medical authority away, in the way that they have.

The transparent falsehood of claiming that philosophy as a community of experts is somehow supportive of medical anthropology's tortured epistemology is not Good's most spurious claim. Neither is his effort to characterize Medical Anthropology's choice to embrace anti-realism, as the convergence of advancements in the field and ambient epistemic conditions developing in other disciplines. At the end of the first Good quote above, he not only locates Medical Anthropology's anti-realist proclivities at the centre of a broad and uncontested agreement among the constellation of relevant disciplines, he goes on to suggest: '...that medical anthropology is the primary site in which these issues are being addressed and investigated.' When writing about subjects ostensibly outside of one's discipline, it is one thing to claim that experts *agree* with you. It is another to claim that the people in your field *are* the leading experts. Especially, when the issues you are writing about are in the domain of another, completely different discipline, which is much older than your own and has a much longer history of writing about the issues in question. The notion that medical anthropologists might be the leading experts on epistemology is questionable at best. However, the strange collective willingness of an entire discipline of professional academics to over-reach the boundaries of their area of expertise is not nearly as interesting by itself as it is in the way it begs the question: if medical anthropology did not reinvent, advance or (as we will argue) even *understand* the basics of epistemology, then what was it that propelled them to occupy their current position at the end of the anti-realist plank?

The history of Medical Anthropology: the profound discovery of universal indigenous medical epistemic inequality

By the end of World War II, naturalist, realist, social scientists studying traditional folk medical beliefs, had learned something profoundly worrying as well as indisputably true: by the naturalistic, scientific lights of the social scientists of that time, every person whose ontological beliefs regarding health hailed from anywhere other than a Western scientific world-view, including from within the still largely dominant non-scientific folk cultures within the West, were struggling with fundamentally mistaken interpretations of the forces affecting our health. 'It is an inevitable conclusion from Azande descriptions of witchcraft that it is not an objective reality.' As it turns out, Evans-Pritchard's description of the Azande just as well describes the medical beliefs of everyone in the world, except for a few Westerners who cling to a strictly naturalistic understanding of our health. From a naturalist perspective, most people in the world are still labouring under epistemic inequalities derived from mistaken ontological systems so profoundly crippled by error, that we are just as profoundly and viscerally shocked by observing them as Evans Pritchard was after observing the Azande. As Bruce Kapferer put it in an introduction to an issue of *Social Analysis* in 2002: "Magic, sorcery and witchcraft are at the epistemological centre of anthropology (Kapferer 2002)." The discovery of the ubiquity with which the rest of the world is uniformly mired in the occult forced medical anthropology to recognize itself *primarily* as the study of un-reason (Kapferer 2002) .

Anthropology established itself as the science of unreason, initially at least. This constituted its principal object. Indeed, unreason, or apparent unreason, defines the Other, the conventional region of anthropological enquiry.

The discovery in the last century, that indigenous health systems, ubiquitously burdened with mistaken ontological notions, and that these magical and religious methods, estranged to various degrees from the impact of empiricism or even rationality, were doomed to a static, pragmatic inferiority in comparison to the naturalism and bio-science of Western medicine, was arguably the single most consequential discovery ever made by social science to the field of public health. The gravity of this discovery would ironically be a principle factor in its subsequent marginalization. It is our contention that the centrality of health concerns in the lives of human beings, along with the inherent psycho-social dissonance associated with epistemic inequalities makes Medical Anthropology's discovery, the most fundamental inter-cultural epistemic difficulty faced by humans on our planet. Medical pluralism is precisely the continuing failure to unify medical epistemology under naturalism and bio-science world-wide. As we have seen, major public health authorities from the World Health Organization to international development banks, endorse and fund traditional medical programs all over the world. We have proposed a pragmatist medical epistemic model primarily as the vehicle by which we believe we can best overcome the resulting epistemic confusion

in world public health policy discourse. For now, we are attempting to interrogate the medical Anthropological literature for an answer to a specific question: how, and how well do people, world-wide, think about their health -- specifically with an aim to identifying pernicious health beliefs. In order to answer that question, we have to understand if modern medical anthropology has made any significant discoveries in this regard, beyond the profound discoveries of medical anthropology's realist years before World War II. In order to understand that, we will need to understand how modern Medical Anthropology reacted to the great discoveries of its past.

Current Medical Anthropology and the reaction to the original discovery

It is our contention that current Medical Anthropology has learned little which might hollow out the staggering importance of their initial discovery to public health. The ubiquity of indigenous medical beliefs in their estrangement from the basic epistemic elements of modern medicine renders them helpless in the face of the vagaries of unaided human perception. Much as the last century has seen the consolidation of biomedicine under concepts like clinical research design and meta-analysis, primarily because these are powerful tools for seeing beyond the blinders inherent to human perception, so too has medical anthropology learned little to dispute that indigenous medical systems will continue to suffer pragmatic impotence in the absence of such tools. Even as medical anthropologists are beginning to uncover what could be nascent empirical elements present in the static methods of some traditional medical systems, these discoveries do not significantly diminish the gap in epistemic equity between medical science and traditional medical systems. We should keep in mind that Peirce would concur, in that traditional medical systems can be said to be the result of his third method of fixing belief. Whatever the mechanisms with which traditional medical systems developed their beliefs, they were not the consensus of a community of scientists, working in a naturalistic environment with the benefit of scientific values and norms of behaviour.

Still, due diligence, any hope of understanding the current state of the medical pluralism debate, and even remaining within the boundaries of our Peircian epistemic model all necessitate unpacking the current scientific consensus among Medical Anthropologists in regard to our question about how and how well people on this planet understand their health. The interrogation of the medical anthropological literature should thus begin with an effort to identify the research which comes the closest to answering our question.

The methodological obstacles to parsing the biomedical efficacy of CAM or Traditional medical practices block the definitive evaluation of efficacy for any treatment which cannot be adequately placebo controlled. That is not to say that there are not other ways to answer general questions about the efficacy of folk medicine, but we will spend a good deal of time parsing the limitations of clinical medical research methodology in this regard later in this section. For now,

Medical Anthropologists are right to observe that there are significant methodological challenges associated with determining the clinical efficacy of folk medicines, particularly in places where people are unfamiliar with Western medicine, where the presence of anyone doing Western medicine is very unusual. What we think it is important to point out however, is that the same discussion of the methodological obstacles to determining the efficacy of traditional medicine will not mean the same thing to realists and anti-realists. Strong social constructivists collapse all of reality into heuristics. To committed anti-realists, the methodological obstacles to the clinical evaluations of folk medicine are merely academic -- in the sense that they have no real bearing on the practical realities of people living and dying. By this we mean that even if we could manage to overcome these obstacles, in the minds of committed anti-realists, we would still not be tying into the real world. In other words, any study which did manage to overcome such obstacles, and to successfully interrogate the efficacy of some traditional medical technique, complete with double blinding and placebo controls, would still be an artefact of bio-medical science, and as such, part of an empirical program, which even if operated by anthropologists, would remain: '...deeply problematic...' from the standpoint of a Medical Anthropologist like Good (Page 24). We have been unable to ascertain if there is some kind of firm consensus within the field of medical anthropology as to the 'nature of authority' they are willing to '...grant to biological and medical knowledge...', and we frankly suspect that there is no such consensus beyond its general rejection. Rather, we have come to the conclusion that Medical Anthropology has been compromised by the failure to resolve this question coherently and the associated limits of their anti-realism. As a result, the field awaits researchers willing to import realism into medical anthropology like Anna Waldstein and Cameron Adams have in the 2006 paper we will shortly discuss: 'The Interface Between Medical Anthropology and Medical Ethnobiology'. Such impulses, if tolerated in the support of the thesis that indigenous medicine is actually more sophisticated than researchers in the previous century anticipated, could be turned just as effectively to the quantification of pernicious health beliefs as public health problems in need of a solution. However, as of yet, it is an open question as to whether medical anthropology will overcome their self-imposed prohibition, their ambivalence about what Good called the 'empiricist' program (p.24). It may be that the obvious benefits of a productive research program delineating the prevalence of pernicious ideas of this kind will ultimately be attributed to another field. Medical geography, for example, a field in which the mathematics of network science have already been applied to the transmission of beliefs as a component of cultural transmission, is arguably well positioned and may be the first discipline to contribute to world public health in this way.

Kleinman and Gale suggested, in 1982, that to the best of their knowledge, their work between 1975 and 1982, designed to study folk medical practices in Tiawan, was the first effort to ever attempt to consider the efficacy of Traditional Medicine. A close reading of the study however, suggests that the researchers, while aware of "...symptoms or underlying biological processes..." on an anecdotal level, did not appear to attempt to quantify objective clinical medical outcomes as measures

intended to establish the relative efficacy of EBM vs. folk medicine. The researchers instead betrayed the usual medical anthropological bias toward hermeneutics and chose self-reported patient 'satisfaction' as their only measure of efficacy. Tragically, the researchers in this case could not bring themselves to embrace the biomedical ethos to the point in which they could consider the careful measurement of concrete medical outcomes, like living or dying, or the presence or absence of knowledge of forms of disease, or any other measures more clearly interpretable as objective medical tests. More importantly, they make anecdotal comments suggesting they were aware of worsening symptoms or the presence of chronic conditions, making it even more evident that they could have potentially collected such information, but they chose not to, apparently not prioritizing the collection of such evidence as an important aspect of their work. Kleinman and Gale's passage quoted below conforms to what we believe are an incoherent anthropological convention: clinical efficacy is spoken about as if it is an important concept, but never methodologically addressed in good faith as a real, naturalistic phenomena; the background anti-realist beliefs now accepted as Medical Anthropological orthodoxy relegates matters of clinical medical reality to a kind of epistemic no-man's land, where such facts are occasionally spoken about on the level of anecdote, but rarely accounted for as anything more objective than forms of patient heuristics fully accountable for by questions of patient satisfaction.

In the passage quoted below, Kleinman and Gale consider the idea of a realist investigation of clinical efficacy in what can only be interpreted as realist terms, only to then claim that in endeavouring to design such a study, intended to compare the relative efficacy of EBM and folk medicine in terms of 'outcomes', various problems including difficulties establishing placebo controls and accounting for effects associated with patients getting better on their own, actually make such an effort impossible (p. 1) :

In 1975, Kleinman and Sung (1976) conducted a pilot study of 19 patients treated by two Taiwanese sacred folk healers (tdng-ki, whom we will refer to from here on as shamans). Seventeen of these patients evaluated their health problems as improved and the shamans' treatment effective. All seemed to assess outcome more in terms of the psychosocial aspects of sickness (e.g., marital, family, work, and personal problems), which we shall refer to as illness problems, than in terms of symptoms or underlying biological processes, which we shall refer to as disease problems (Kleinman 1982). In four of these patients, symptoms either didn't improve or actually worsened, yet they still assessed treatment as effective. The only patients who perceived treatment outcome to be ineffective were two patients suffering severe acute medical problems (cf. Kleinman 1980).

Based on these findings, we hypothesized that folk healing may be principally effective in the treatment of illness problems, while biomedical practice may be especially effective in the treatment of disease problems. We assumed that patient evaluations would take into account both dimensions of sicknesses, but, except for episodes of acute severe symptoms, that remedying illness problems would be uppermost in patients' evaluation of treatment outcome. This we posited would be the opposite for biomedical practitioners who would assess outcome almost solely in terms of biologically based disease processes. Shamans, we thought, would apply evaluative notions closer to those employed by patients.

In 1977 we set out to examine a few of these ideas in the context of an empirical study conducted in Taiwan comparing the outcome of patients treated by shamans with roughly comparable patients treated by biomedical physicians. To the best of our knowledge, this is the first study of its kind. Serious limitations are placed on any outcome study of indigenous healing owing to a number of problems: patients in folk healing settings frequently will not accept biomedical assessments; if such assessments are extensively applied the healing situation is radically altered and is no longer a "folk" one; patients often cross over between folk and biomedical practitioners for the same problems; it is usually infeasible to control for placebo effects and natural course of sickness.

For these and other reasons it is not realistic to attempt to attribute efficacy to a particular component of folk healing or even to folk healing per se. And in the study we conducted, we made no attempt to say who or what was effective.

Although it is not impossible to imagine a situation in which patients in 'folk healing settings' will not accept some kind of 'biomedical assessment', the researchers do not get any more specific than that. We can imagine measures as simple as vital signs or merely checking to find out if the patient has died or not, as a potentially sufficient clinical measure, to at least get beyond reported levels of patient 'satisfaction'. As medical anthropologists are fond of suggesting, Tradition medical models must be successful, at least symbolically. Disease, on the other hand, actually kills people, and we are more than justified in hypothesizing that sometimes people can be helped by biomedicine but not by folk medicine or that certain traditional or otherwise non-scientific medical beliefs can be pernicious enough to have a measurably negative impact on health communities.

The problems of expectancy effects and the presence of foreign researchers are certainly real, but we refuse to accept that they make such forms of research impossible. Without speculating further as to why such important anthropological data are so hard to come by, there can be no doubt that there are enormous ontological warrants to assume that folk medicine is not pragmatically potent in fighting disease in the same way that biomedicine is. Kleinman and Gale speculate about this very fact in the above quotation, utilizing their semantically impoverished distinction between 'illness' and 'disease'. Kleinman and Gale appear readily willing to grant that in cases where life-threatening disease has been identified, and the patient's doom effectively sealed without lifesaving biomedical treatment, that the patients will still die even if treated by folk medical practitioners. It is crucial to observe however, that they are not suggesting that this is actually, in reality, the case. On the contrary, to them this is speculation on the level of a hypothesis too methodologically difficult to test (p. 1). We believe this is not only untrue, but an effort to conform to the unspoken conventions of medical anthropology which have lined up to oppose biomedical realism in a way insurmountable by conforming leaders in the field.

We agree with medical anthropologists about the profoundly different nature of indigenous medical beliefs, in comparison to evidence based medicine. We depart from medical anthropology in our willingness to conclude that the severity of the epistemic gap dooms indigenous medical traditions to a profound relative pragmatic impotence in terms of clinical efficacy. The reader might be tempted to argue that people have been living this way for 1000's of years, so they can't be doing everything

wrong. Of course, they are not doing *everything* wrong. Medical Anthropologists are well aware of traditional medical experts who are in possession of real technical expertise in useful areas including bone setting and midwifery. In addition, a few useful drugs have been identified through ethno-botanical means and some researchers have begun a research program piecing together indigenous use of observation, physiology and plants as a means of teasing out the signs of empirical strategies among indigenous people.

The extreme realist end of medical anthropology, really more of a tiny, isolated island of realism in a sea of anti-realism, is the field of ethno-botany. What we would argue is that the article from which the following quote is taken is the single most earnest effort to import realism into medical anthropology to date: '*The Interface Between Medical Anthropology and Medical Ethnobiology*', the 2006 meta-analysis by Anna Waldstein and Cameron Adams in the *Journal of the Royal Anthropological Institute*.

The ethno- symptomology encodes particular health concerns and provides for particular and consistent responses to them. Adams (2004) draws on both these lines of inquiry in his study of highland Maya ethnophysiology and shows that Maya understanding of how the body functions underlies popular beliefs about health and illness. Moreover, research on medical ethnobotany in Chiapas (E.A. Berlin & Berlin 1996; E.A. Berlin et al. 1995; E.A. Berlin, Berlin, Tortoriello, Meckes & Villareal 1995; Brett 1994; Casagrande 2002; Stepp 2002; Stepp & Moerman 2001) further confirms the empirical character of highland Maya medicine. This literature shows that plant selection does not exhibit random patterns of distribution and that medicinal plants do display bioactivity for their claimed usage in clinical trials. Given these two bodies of research, highland Maya medicine is a system based on observation and experimentation that is theoretically driven, a far cry from the world of cosmology and witchcraft as described through the 1960s.

Ethno-botany is the only context in all of medical anthropology in which the language can be so unequivocally realist: '...medicinal plants do display bioactivity for their claimed usage in clinical trials'. Ethno-botany is also the edge of medical anthropology which has the most contact with realist medical researchers, some of whom work for pharmaceutical companies with material interests in identifying plants containing useful new drugs to bring to market. It should be clear from the above passage that indigenous ontological commitments do not preclude success in the development of effective medical techniques. In fact, infectious disease transmission may also be a source of empirically rooted coping strategies among some indigenous cultures (Green 1999). What should also remain clear however, is that such mistaken ontological systems can conflict, discourage, or preclude the development or adoption of better medical ways to do things. There can be no doubt that whatever the pragmatic potency of any given indigenous medical system, such systems are at much greater risk for failing over the long term at prophylaxis, diagnosis and treatment in any given instance than Evidence Based Medicine. Furthermore, traditional medical systems are by definition relatively static, therefore, as Evidence Based Medicine continues to advance, this imbalance will persist and worsen. All of this precisely reflects Peirce's *Fixation of Belief*. To get back to the

argument at hand, these passages have demonstrated several things to keep in mind, including: 1.) The discovery of the profound epistemic inequality fundamentally inherent in indigenous medical beliefs was, is and continues to be a profound discovery which has massive implications for world-wide public health; 2.) although researchers in some respects may be closing in on evidence of some level of efficacy of some traditional modalities, research of this kind is rare and inconclusive, while research into the *inefficacy* of traditional medicine is *non-existent*; and finally 3.) these findings are all consistent with what we might expect given the relative pragmatic power of different modes of fixing belief in the Peircian sense.

There is one more thing to take from the Waldstein and Adams quote above and that is the tone of the final line and the way it sums up the emotional reaction to the great discovery of early modern medical anthropology: 'Maya medicine is a system based on observation and experimentation that is theoretically driven, a far cry from the world of cosmology and witchcraft as described through the 1960s.' As pragmatists interested in worldwide public health, we would respectfully disagree. Mayan medicine, regardless of the impressive correspondence of its empirical components, is still labouring under a static, largely ontologically confused epistemic condition which is profoundly less pragmatically potent than scientific medicine. If we were to choose to adopt Mayan medicine, or Traditional Chinese medicine, instead of Evidence Based Medicine, the pragmatic consequences would be dire. Still, and with no small degree of irony, when modern medical anthropology does finally manage to employ a creeping measure of realism to make a point, the point is to bash the early researchers who discovered ubiquitous medical epistemic inequality, by pointing out that things might in fact be more complex. Again, we think this reflexive response begs the question: Why is medical anthropology so committed to anti-chauvinism that they seem to hold the obvious value, indeed the moral imperative, associated with the discovery of world-wide epistemic medical inequality, as verboten?

The origin of the philosophical fashion of Medical Anthropology: anti-imperialism, anti-colonialism, anti-chauvinism and the aftermath of the World Wars.

The discovery of ubiquitous medical epistemic inequality, by the lights of a medical pragmatist, has essentially endured, largely unchallenged as we have seen, by any substantive evidence of the efficacy of indigenous medical systems, generated by medical Anthropologists. We now must turn to the narrative of how Medical Anthropology itself reacted to its role as the bearer of such disquieting tidings. As we suggested before, there is no evidence that medical anthropologists adopted anti-realism at the behest of the consensus of philosophical experts. Rather, it must be that Medical Anthropology adopted an anti-realist stance for reasons other than what the bulk of professional philosophers had to say at the time. This failure to consult the community of experts is

what Peirce used to characterize the third mode of belief: fashion. We argue that one of the forces which drove medical anthropology to adopt ant-realism is the reaction to the discomfort associated with epistemic inequality itself, and the fear of being left in the position -- after WWII, after the winding down of colonialism and the rapid transformation of empire -- of being the defenders of the claim that Western medical epistemology is profoundly pragmatically *better* than that of any other medical system in the world.

As it would turn out, bad timing would subject the clear-cut findings of medical anthropology -- the fruits of the combination of a Western scientific medicine coming of age, and a similarly maturing field of medical anthropology -- to the manic scepticism of the age of ambivalence, ushered in by the close of WWII and the rollback of colonialism. Anti-colonialism and anti-chauvinistic sentiment would find easy targets in the writers of early medical anthropology. Spurred by the profound guilt which early anthropologists had taken on in acting as the witnesses to the terrible crimes of colonialism, anthropology was primed to condemn those whose work might smack of chauvinism. It is a terrible irony that the great medical anthropological discovery of our age was not only true, but also looked precisely like a chauvinistic fantasy or a warrant for a colonial power grab. To anyone willing to dismiss the epistemic authority of medical science, early medical anthropologists were clearly chauvinists. The idea that everyone in the world, who was not a member of the same brand new and relatively rare Western Enlightenment culture as the medical anthropologists themselves, was suffering with pernicious medical beliefs, seems like just too easy a target to pass up. The resulting avalanche of attacks on the older medical anthropological writers sets the stage for current orthodoxy in medical anthropology. Today, medical pluralism debates are frequently brushed with anti-colonial, anti-imperial rhetoric and burnished with pseudo-expert philosophical arguments involving incommensurability. In reality, medical anthropological arguments of incommensurability in the medical pluralism debate are a straight-across articulation of the same old anti-chauvinistic impulse: to reject western scientific thinking as an authority. Anti-authoritarian medical anthropologists can take refuge in the idea that medical epistemic systems of other cultures are just as valid and worthy of authority as any other, because they cannot be compared. More a regurgitation of decades of social science orthodoxy, as we have seen, such arguments are anti-chauvinism dressed up by writers like Byron Good with a counterfeit "approved of by expert philosophers" sticker.

By the 1950's, Realist medical anthropologists had discovered a great deal about the ubiquity and inherent pragmatic impotence of the magic and spirituality playing a central and potentially pernicious role in traditional medical systems. The point we have made here is that the discipline wide epistemic transition that followed this discovery was not motivated not by the consensus of expert philosophers, but rather at least in part by the internal and external anti-authoritarian tensions affecting medical anthropology at that time. Below we will discuss additional, non-philosophical drivers for the same transition.

The discomfort of knowing when other people are wrong: the psycho-social tensions inherent between ethnography, advocacy and epistemic inequality.

It was the startling rejection of medical scientific authority which first caught our attention as a incoherent element of the medical pluralism debate. However, we were outright surprised to find that leaders in medical anthropological epistemology¹² have squarely rounded in an attack on the very idea of a "pernicious health belief". After all, as pragmatists, beliefs and their relative pragmatic potency are the basis of our medical epistemology. The ability to parse the relative "perniciousness" of medical ideas is at the centre of our project. As it turns out, we not only have to prepare the reader for the peculiar bias that anti-realist epistemology imparts to the medical anthropological data about the health beliefs of humans on our planet -- we have to prepare the reader for medical anthropology's discipline-wide refusal to even *acknowledge* that a single one of those ideas may be harmful. Instead of the leading experts in the world on medical beliefs assisting in the project to discern the health belief communities in the world most in dire need of education, we are told by leading medical Anthropologists that our desire to identify such information and use it in the service of public health is naive, misguided, and part of a "deeply problematic" empiricist impulse that has no place in medical anthropology (Good 1994).

Again, we are faced with the question of why Medical Anthropology as a discipline would be motivated to steer its collective path around the obvious problem of medical epistemic inequality in the effort to stay clear of criticizing other people's beliefs. We believe the answer lies in the *experience* of ethnography itself. Medical anthropologists are facing an increasingly globalised world populated by research subjects who are increasingly aware of what social scientists write, why they write and where they get published. The "double hermeneutic" as it will be referred to by Sociologists later in this section, means that if researchers constitute a threat in the minds the research subjects in question they may also be vulnerable to attack by allied Traditional Medical practitioners and, in the case of more professionalized forms of Traditional medicine (take for example, Traditional Chinese Medicine) of the advocacy groups and PR firms hired to protect the economic interest of the non-

¹² Byron J. Good's *Medicine, Rationality and Experience: an anthropological perspective*, was published in 1994 and as of this writing has been cited by 2124 other articles and books according to Google Scholar. Good appears frequently in this thesis in the form of quotes from this book because this thoughtful effort to articulate the epistemic boundaries of medical anthropology is so rich, not only with historical and philosophical context, but with the kinds of raw visceral and proto-ethical motivations he reveals as the emotional core of medical anthropology as a discipline. We struggled to understand the motivational antecedents of anthropologies epistemic peculiarities. Good's honest and passionate account was not just the key to our understanding, but included in the first 20 pages of his book, an astonishingly narrow, focused and well developed attack on the very plausibility of a core concept of this thesis: the idea of the pernicious health belief. We see Good as an anthropological expert, doing philosophy poorly. However, we are deeply indebted to him for providing such an authoritative, yet emotionally rich and detailed account, of precisely what those philosophical mistakes are and why they are so widely (and tragically) embraced.

scientific medical establishment in question¹³. Both the research subjects and the medical anthropologists themselves, see medical anthropological work as part of an important political and economic *struggle*.

This leaves medical anthropologists with a choice: a choice between sympathy or contempt; a choice between mirroring their subject's epistemic and ontological commitments or calling them into question. The reader might consider for a moment the pressure bearing on social scientists who find themselves working in the midst of a struggle between medical systems where the stakes involve the abandonment or even (in the case of Western Complementary and Alternative medicine), criminalization of traditional medical networks by policy elites. Political neutrality and academic neutrality/naturalistic objectivity in such circumstances, part ways. There no doubt are instances in which an objective, naturalist account would certainly entrain the alienation, if not lasting hatred, of the subjects of study immediately following publication. As Medical Anthropology has become more politically committed to the subjects of its study, it has been forced to simultaneously re-write its epistemology to allow itself to do so -- coherently -- or at least coherently to medical anthropologists. The retreat from naturalism into hermeneutics is the result.

Realism has been scrapped precisely in the effort to better serve what anthropologists see as a kind of academic mission, what Good described on page 25 of *Medicine Rationality and Experience* as: "...our attachment to those with whom we have carried out research and our dedication to represent their interests and point of view in our writings,..." Indeed, it has been argued that some ethnographic work on medical beliefs is not even possible unless the anthropologists themselves also come to believe the same things as their subjects of study.¹⁴ We recognize how difficult it must be for

¹³ It is not a stretch to consider chiropractic in the UK right now: a 19th century, pre-germ theory holdover which, in the course of taking some life-threatening blows from the likes of the leading clinical researcher in the UK on its efficacy, chose to sue the researchers under British liable law, over the presentation of stark evidence of its uselessness as a medical modality. Non-scientific medicine is a massive part of the medical marketplace worldwide and in the West -- Roy Porter suggest that by the early 80's in the UK there were more alternative therapists than doctors (FitzPatrick 2010). The sheer monetary and political power of such an enormous segment of the marketplace does not just lay down when threatened. Like any institution, when the livelihood of its participants is threatened, then all allies and enemies are quickly identified and either enlisted, exploited or attacked respectively. As we shall see, there is no shortage of social scientists or professionals from other relevant disciplines and professions willing to leverage biased and philosophically flawed social science work products to advance the agenda of medical pluralism.

¹⁴ In Jeanne Favret-Saada's early relativist work, *Deadly words: Witchcraft in the Bocage* (Favret-Saada 1980) , she specifically makes the point that the French country people she encountered were savvy enough to refuse to offer up evidence of their occult beliefs lest they "...be taken for idiots in the way that public opinion would have them be..." It was not until she herself began to suspect that that her illness and accidents might be the product of witchcraft that she was able to truly get the story straight as an ethnographer. We are certainly willing to accept that minus substantial deceit, this is probably correct. If the target population knows that Western ethnographers think their beliefs are false, they will likely not interact with them in the same way they might with an ethnographer with whom they were convinced was willing to seriously consider their beliefs as real. We understand this as an indisputably thorny methodological conundrum for anthropology. As medical epistemologists, we also see this as "drinking the cool aid". While such methodological extremes may yield results unobtainable otherwise, this is not a reason to abandon or even question the epistemic superiority of Western medical epistemology.

medical anthropologists when the issue of epistemic inequality becomes impossible to ignore with a long term subject of study -- a person, who has a reasonable expectation of honesty and may well have become a friend. This difficulty must be not only the fundamental methodological conundrum of medical anthropology, but also the fundamental motivation behind the advocacy for medical pluralism emerging from anthropology and residing within public health policy circles.

It is surprising the candour with which leading medical anthropologists will speak of the impulse to advocate for their subjects of study. Kleinman is remarkably cavalier about this when explaining his approach to the (potential) study of medical efficacy:

Much of what I have to say about efficacy -- that it is complex, differentially constructed, even contested in experience, and needs to be examined simultaneously on several levels -- I can still stand behind. The question of efficacy hinges on the analysis of cultural processes that continue to be a tantalizing reminder of medical anthropology might contribute. Yet the study of a few detailed cases produced a conclusion about the performative basis of efficacy -- namely, that cultural healing always must succeed, at least in symbolic terms -- that a study I conducted more systematically with a larger sample from 1977 to 1979 contradicted. It is disconcerting that the latter study continues to be one of only a few that systematically assesses the efficacy of indigenous healing (see Kleinman and Gale 1982); it is as if medical anthropologists find it difficult to take on this particularly cherished convention. I too have occasionally misrecognized these findings in settings where alternative healing required defence.

This passage illustrates a great deal about what it is like to read through modern medical anthropological literature. Kleinman's observation, that there is no medical anthropological research into the clinical efficacy of traditional medicine still holds true (the studies he is referring to were far from good faith efforts at clinical research). Which is why readers from other disciplines who are constantly confronted with medical anthropological literature suggesting that this or that traditional medical treatment "works" for this or that medical condition are so confused: to an anthropologist, '...cultural healing always must succeed, at least in symbolic terms.' The interdisciplinary reader is not aware of this, or of the fact that the vast bulk of medical anthropological writing about efficacy is in symbolic terms. Kleinman's last line though is the amazing disclosure here: sometimes even for leading medical anthropologists, advocacy trumps specificity. We would argue that it also drives their epistemology as well as their research programs. Byron Good suggests in *Medicine, Rationality and Experience* that advocacy inhabits the driving ethos of medical anthropology '...because our primary goal has been to make understandable other societies in a non-judgemental way...'. Good makes this point on page 20 in the middle of his discussion of why the whole concept of pernicious medical belief is too problematic to be the focus of a research agenda. The notion that it might be perniciously judgemental to identify medical beliefs which are the source of great misery, seems short-sighted.

The unfortunate fact that most of the people in the world are burdened with epistemically and ontologically flawed medical belief systems is not just a profound discovery, it is the first of two components which we believe constitute the most fundamental inter-cultural epistemic difficulty

faced by humans on our planet. The second component has to do with the consequences, material, political and ethical, imagined or real, of communicating to other people that they are profoundly, cosmologically, ontologically and pragmatically mistaken about what things happen in the world to make people sick or healthy. Social scientists are, as we all are, acutely aware of the social costs associated with sharing one's negative evaluations of the beliefs of another person *with* that person. Well meaning anthropologists responding to these impulses frequently defend indigenous belief systems, by evoking exactly how much indigenous people dislike the more unvarnished evaluations characteristic of anthropologists writing in the early 20th century. As one anthropologist and theologian put it in a handbook of world religions: "Tribal people do not like to be called heathen, savage, primitive or superstitious and words such as animistic, pre-literate, traditional or ethnic are not accurate (The Worlds Religions, 1984)¹⁵." Unfortunately, when it comes to concrete social development milestones -- say for example, a culture's potential to benefit from germ theory and epidemiology during an outbreak of disease -- words like "primitive", "superstitious", "animistic", and "pre-literate" carry some unavoidable pragmatic consequences.

Exacerbated by the very fact that indigenous people are often adversely medically effected by their own benighted epistemic conditions, anthropologists find themselves violently torn between the obvious medical harm associated with the culture under study and their desire both as good people and good anthropologists to avoid at all costs the negative evaluation of any culture by another culture's values¹⁶. Cultural relativism is not just a bedrock methodological *tool* for anthropologists, it is their answer to what we suggested earlier is the most fundamental inter-cultural epistemic difficulty faced by humans on this planet: the problem of cultural epistemic inequality. Anthropologists have collectively chosen to deal with the terribly important epistemic shortcomings -- the undeniably pernicious nature of some medical beliefs often apparent in the subjects of their study -- by eliminating the category of "mistaken" from their discourse and furthermore, questioning the use of cross-cultural judgements when applied by other disciplines in debated about medical pluralism.

We will leave a consideration of the tenability of such choices for the meaning of academic professionalism for Part III. Our intention here has been principally to bring to the reader's attention the following points about the state of the data with which medical anthropology might be said to respond to our question about how and how well people in the world think about their health: 1.) the stunningly gloomy discovery of profound pragmatic medical epistemic inequity, by the early realist researchers, has yet to be significantly challenged with robust research or in any other way; 2.) successful or even serious efforts to evaluate the efficacy of folk or traditional medical systems are

¹⁵ We were unable to determine the author of this source.

¹⁶ Daniel Gordon's brilliant opening salvo (Gordon 1991) in this discussion observed that it is precisely the horribly destructive and dangerous nature of the phenomenon of female genital mutilation which demonstrates that in such an instance, anthropology can no longer walk a line along which it can be "...both non-judgmentally descriptive and experiential, on the one hand, and a practical adjunct to medicine and public health, on the other."

non-existent in medical anthropological literature and are widely considered to be outside the boundaries of good practice by that discipline; and 3.) the frequent assertions in the medical anthropological literature that traditional or folk medicines "work" are either not serious or are not literal and are meant to be understood in a symbolic context where traditional medicine is uniformly successful. In short, although it took us a great deal of research to understand it, we now understand what medical anthropology has to say about our question: that most of the people in the world are dangerously benighted in terms of medical knowledge. Of course, current anthropologists would prefer to problematize rather than to answer our question. Although we believe that medical anthropology, after addressing its epistemic problems, has a great deal more to offer the very pragmatic, realist debate about medical pluralism, most of the philosophical work required to understand their current contribution to the medical pluralism debate, has involved unwinding their confusing and erroneous epistemic posture and recognizing their enthusiastic advocacy for Traditional, Folk and Complementary and Alternative Medicine as the principle driver of their epistemology¹⁷.

¹⁷ Perhaps the most difficult challenge in the effort to do high-impact interdisciplinary work, is the need to accurately capture the expert consensus of the fields involved. In the case of the field of Medical Anthropology, we have discovered a startling state of affairs: that there appear to be literally no published writers, no published field studies and no publishing research programs or sub-fields which do work on pernicious health beliefs derived from Non-Western or CAM medical systems. It is important to note that we have explained and demonstrated how and why this set of circumstances has come to pass: 1.) that Medical Anthropology's orthodoxy is characterized by an ideological preference best understood as advocacy for medical pluralism (see Kleinman's very specific admission to this effect on page 60 above (Kleinman 1997, p. 10)); 2.) that the philosophical reasons behind this ideological position are badly flawed; 3.) That as such, we see this state of affairs as a profound professional failure, while leaders in the field of Medical Anthropology would disagree. We believe we have identified the leading medical anthropological writers on this issue and that we have substantively addressed their work, thus arriving at our conclusion. We understand that this is a radical claim and that some kind of substantial, bibliographic evidentiary structure must be in place to support it. The precise nature of this structure is a methodological matter. We have taken a number of steps to ensure that while we cannot rule out that there may be realist medical anthropologists attacking this orthodoxy in publications *somewhere*, we have, with due diligence, searched the appropriate databases, encyclopaedias and textbooks, and have not found a single one -- with the possible exception, as we have already noted, of Daniel Gordon's paper, which suggests that the phenomena of female genital mutilation presents a fundamental challenge to Medical Anthropology's cultural relativist orthodoxy (Gordon 1991). Finally, our methodology constitutes a robust effort to resolve a fundamental obstacle to meaningful interdisciplinary work: to characterize the professional consensus, the epistemology and when necessary, the pernicious ideological bias of a discipline. This goal is fundamental to this work. In every discipline we have addressed, including philosophy, we have actively sought to include quotations of leaders in relevant fields of expertise characterizing relevant consensus in their field (as we have pointed out, Peirce would agree that such consensus statements are fundamental to the functioning of science). As such, in the case of Medical Anthropology, we quoted Kleinman at length on page 60 above, claiming that his work is the only work on the efficacy of Traditional medicine ever attempted (and as we noted, it was not a serious effort). To the best of our knowledge, no other research of that kind has been carried out in the field of Medical Anthropology to

The Final Analysis: what Medical Anthropology has told us in answer to our question

We acknowledge that Western scientific naturalism and pragmatism are not shared by traditional medical models, so in almost every case, traditional medical systems address extra-medical objectives, ranging from the magical to the spiritual to the aesthetic. This explains the current anthropological fixation with symbolic meaning. We believe that the extent to which a medical practice "works" in one of these non-pragmatic contexts is less contested ground, because the objectives are less pragmatically quantifiable or relevant and strictly speaking they are not *mutually exclusive* with Western medical pragmatic naturalism. In other words, the patient's religion is not necessarily in conflict with evaluations of the practical utility of any particular scientific medical intervention -- although it could be.

The reader should rest assured that we are unperturbed by the straightforward reporting of indigenous beliefs in regard to such religious or aesthetic aspects of traditional medicine. We are still concerned, however, that the anti-realist commitments of medical anthropologists compel them to mix the pragmatically pernicious medical beliefs of their subjects, right in with everything else -- presenting it all on a level epistemic playing field. Another way to consider how harmful such an oversight might be in the interdisciplinary context of public health policy would be to recognise that if medical anthropologists cannot eventually bring themselves to apply any criteria which will allow them to discern obviously harmful medical beliefs when they observe them, then another, otherwise identical field, will have to spring up to do this essential job. We believe that anti-realism has in fact left a gaping hole in the traditional role of medical anthropology. As we will argue in Part IV of this thesis: realist medical anthropological research is absolutely essential. In fact, we will further propose that to the extent that this hole in the practice of medical anthropology persists, medical philosophy should step in to develop the epistemic basis for such a program and the warrants for its funding, and if necessary, launch such research independently of existing anthropological norms to form a new field of study, or rather to restart the one abandoned by contemporary medical anthropology -- and to restart it on our proposed universal Pragmatist epistemic foundations.

In sum, it should be clear that medical anthropology answered our questions about how people on this planet think about health generally, early on in the 20th century. Medical anthropology, then as a field, balked at the prospect of adjudicating the pragmatic medical potency of the ubiquitously erroneous medical ideas they had discovered. Not so much as a reaction to the very real

difficulties of running adequate clinical trials to determine if thousands of magic rites, traditional medical modalities or herbal remedies actually help people or harm people, but rather, because it was easier to attack a field of study bearing the bad tidings of the uniform failure of traditional medical systems, than it was to stand behind it. The current literature in medical anthropology is not going to provide us with an honest assessment of the presence of pernicious medical beliefs. Instead it leaves us with a hole -- a hole we must fill as best we can with induction based on ontological considerations, the contributions of other disciplines and what is left of current medical anthropological work after it has been stripped by its practitioners of naturalistic medical evaluations.

Religion and medical pluralism

Religious anti-realism

In one sense, religious pluralism and the philosophy of religion might seem beyond the outer limits of digression for a discussion of the present state of the evidence adjudicating CAM and Traditional medicine. As we have seen, an understanding of what expert medical anthropologists publish about traditional medicine is not possible without an understanding of a host of cultural impulses, philosophies and ideologies which make up the now largely tacit, but no less radical epistemic philosophies of Medical Anthropology as a culture. In the same way, if perhaps even more broadly, social science anti-realism cannot be adequately understood without a familiarity with the history of the study of comparative religion, the backdrop of the evolving religious issues of the last two centuries in Western culture.

As we considered the role of cultural issues ranging from colonialism to WWII in Medical Anthropological epistemology, because it helped us to better understand the underlying motivations that perhaps drive their otherwise puzzling discourse around CAM and Traditional Medicine, there is something in Western religious philosophical discourse which we need to understand, both in order to see it echo through sociological epistemology, as well as to perceive the same forces at work both in the sociological defence of CAM and Traditional Medicine -- in the fundamental human need for spiritual meaning which drives some parts of CAM and Traditional medical culture.

We will suggest that sociology's reflexively democratic approach to human knowledge systems has more in common with religious apology and transcendental metaphysics than contemporary scientific epistemology. We will observe that the culture of sociology both sees itself and expresses itself in professional work products as a defender of beleaguered epistemic systems like religious faith, which find themselves increasingly under threat from the dominant forces of modern cultural life. The advocate-nature of sociological culture bears more than an accidental resemblance to Wittgenstein's indignant attempt to defend human spirituality from Frazer's effort to reduce it merely

to science done poorly. We have come to understand that sociology has in fact mistaken Wittgensteinian metaphysics for epistemology to the court, thereby applying philosophical argumentation, undeniably devised and deployed as religious anti-realism, as scientific anti-realism by mistake. Although we will reserve the final case for how we believe Wittgenstein has been misunderstood by sociology for Part III, at this point, we would ask the reader to consider that sociological epistemology can be more richly understood in the context of Western religious demythologization and the struggle to develop religious anti-realism as the replacement for irreconcilable conflicts between competing religious ideologies. It is within this context that positivism's failed effort to simultaneously evict both spirituality and metaphysics from the realm of legitimate human knowledge can be seen to underlie sociology's antagonistic posture to scientific authority. It is our claim that the things that sociology has to say about CAM and TM are far better understood in light of the above .

Sociology misunderstands Wittgenstein as a warrant for anti-realism

Don Cupitt's book, *Sea of Faith*, places Wittgenstein's reaction to Frazer in the context of the centuries long struggle to retain intellectual respectability for traditional Christian religiosity during a rapid secularization of Western intellectual discourse. Cupitt's larger narrative, spanning multiple thinkers, paints a compelling picture in which Wittgenstein's attacks on Fraser, are best understood in the context of the ongoing defence of religiosity as a respectable impulse. It is our contention that there is a similar goal discernible in the heart of the debate about medical pluralism. In many ways, this discovery is a relief to us, in that for much of our research it appeared that social science anti-realists, for no reason in particular, had simply misunderstood Wittgenstein in spontaneously coming to the determination that the barriers at the edges of "forms of life" were such impenetrable sources of incommensurability that they could serve as a plausible foundation for full-strength constructivism. For some time during our research, which included extensive contact and direct conversations with leading experts in anti-realist science and technology studies, the explanation for anti-realist sociological epistemology, appeared to boil down to evoking Wittgenstein's concepts of "forms of life", "family resemblance" and "language games" as a philosophical basis for claims about the nature of reality more radical than anything this side of Berkeley. We found this explanation outright mysterious for some time. Consider the following passage by Harry Collins¹⁸:

¹⁸ This passage is an excerpt from a working paper on Elective Modernism retrieved from Harry Collin's Cardiff University web site. We were able to retrieve it from the following URL on August 16, 2014: <http://www.cf.ac.uk/socsi/contactsandpeople/harrycollins/expertise-project/draftpapers/index.html#modernism>

How can we both believe the arguments that show that science is politics yet maintain our faith in science as an idea?

The answer is the idea of the social. All those arguments have logical force but social life, as the sociology of scientific knowledge has shown, does not run on logical arguments. Society is made up of forms-of-life where rules do not contain the rules of their own application and where there is no clear definition even of something as simple as the idea of a 'game' – as Wittgenstein pointed out (Harry Collins, 2011, pg. 2).

We disagree completely with the content of the above quote. As we shall see in Part III, Collins is unable to rescue his "...faith in science as an idea..." from his own anti-realism. Sociology, particularly science and technology studies, tends to reduce all conflicts involving scientific authorities to politics, suggesting in its most radical forms that science is politics by other means. as Bruno Latour suggested on page 229 of *The Pasteurization of France* (Latour 1993).

This is why the sociology of science is so congenitally weak. Auguste Comte, the father of scientism and sociology, has invented a fancy system of double-entry bookkeeping. Science is not politics. It is politics by other means. But people object that "science does not reduce to power." Precisely. It does not reduce to power. It offers other means. But it will be objected again that "by their nature, these means cannot be foreseen." Precisely. If they were foreseeable, they would already have been used by an opposing power. What could be better than a fresh form of power that no one knows how to use? Call up the reserves! Homage to Shapin and Schaffer (1985).

This passage displays a central element of contradictory sociological dogma that we will spend more time on in Part III. Latour artfully couches the contradiction in the claim that science is somehow simultaneously both politics and not politics -- by virtue of being both *something* and *not something* simultaneously, by being something "by other means" (Latour 1993). To put the problem in more understandable terms: the fundamental contradiction about science within sociology has to do with what they make of science's Peircian pragmatic power. If it is unquestionable dogmatic orthodoxy in sociology that science is without any special epistemic authority, then science's obvious pragmatic power is very difficult to account for indeed. Thus, Collins' claim above, suggesting that science as a subset of "...social life...does not run on logical arguments..." seeks to undermine scientific authority along the same line of reasoning. By suggesting that since logic is out of the question for all social life including science, Collins implies that science's commitment to logical reasoning as a value cannot serve as a warrant for scientific authority either. Peirce, of course, would disagree. What we would point out is that medical epistemology in particular is a very pragmatic game and functions with undeniably profound pragmatically material success as such -- levels of success which simply cannot be accounted for without reference to logic, reasoning and the scientific application of a range of technologies.

Similarly, Collins' reference to Wittgenstein's notion of "forms of life" is a further attempt to justify scepticism of scientific authority. We reject this as well. For a concept that Wittgenstein only used five times in *Investigations*, and according to the Stanford Encyclopaedia of Philosophy "...has given rise to interpretative quandaries and subsequent contradictory readings...", "forms of life" is

simply not a big enough gun as a concept to justify the global indictment of scientific authority in the face of its pragmatic potency. More importantly, there is significant reason to believe that Wittgenstein himself would never have used "forms of life" as means of impugning the universality of medical epistemology. Reserving the rest of this argument for Part III, we would like the reader to keep in mind for now, that the philosophical potential of "forms of life" simply does not justify its use at the centre of social science's radical epistemic program. The way Collins uses Wittgenstein's other ideas are equally mysterious. In the following passage, when Collins refers to "Wave Two", or the second wave of science studies, he is speaking of what is arguably the high water mark of the anti-realism movement in the sociology of science, a discipline which although still committed to anti-realism, has since acknowledged that there must be a balance between democracy and scientific authority. In the example below, "Wave Two" should be interpreted as extreme scientific anti-realism, in which science is stripped of its authority¹⁹.

Wave Two shows us that demarcation is not a matter of logic, it is a matter of culture, or 'form-of-life.' Wave Two has shown us that demarcation criteria, if they are going to be found, are always going to be fuzzy edged – they are going to be social rather than logical.

The central insight is Wittgenstein's discussion of family resemblance. In the light of Wave Two we can ask: 'Why should there be logically inviolable demarcation criteria for science when, as Wittgenstein shows, there is no logically inviolable definition of "game"?' As soon as one allows that the process of demarcation is fuzzy – that it is a matter of family resemblance, ill-defined 'ways of going on', and socially understood rules rather than logical universals – one sees that scientific values can be demarcated in spite of the many exceptions found in the practice of science.

Again, we are using Peirce precisely to avoid concerns like the demarcation problem. What medical science uses to make pragmatic decisions, and what we might choose to ignore as claims derived from modes of belief which are not derived from scientific medical consensus *is* what Peirce would call *science*. To the extent that one might be tempted to collapse scientific demarcation to all of the values, behaviours, social conventions and beliefs characteristic of Peircian scientific communities, then *loosely*, in a more "received-view" of-science kind of common sense way, one might consider this demarcation. Our point here is that if one strips away the philosophy of science obsessions with the problem of induction, explanatory power and the reality of unobservable objects, etc., the limited

¹⁹ Collins uses the term "Wave 1" to refer common sense scientific realism. This is how he sees "Wave 2" in comparison: "*Wave 1 science seemed to stand well above the cultural plain offering a viewpoint from which advice could be given with authority in respect of every other political and cultural endeavour. Under Wave 2 the mountain of science had been eroded away and the cultural plain had been levelled; there was no longer anything special about science.*"

The passage is selected from the 2011 Collins paper entitled *The Third Wave of Science Studies: Developments and Politics*. We retrieved this from his Cardiff University Web site. The resulting document says that this particular paper was "forthcoming. in *The Japan Journal for Science, Technology & Society*, Vol. 20, 00, 000-000 [simultaneously published in Japanese, SHISO, 00, 00, 000-000]" We were not able to find the online publisher. The URL we retrieved the paper from August 10, 2014 is:
www.cf.ac.uk/socsi/contactsandpeople/harrycollins/.../japan-paper.doc

claims of our strictly pragmatic, fallibilist medical epistemic position largely side-step what Collins is referring to when he talks about the "logically inviolable demarcation criteria". More broadly, quibbling within the philosophy of science about demarcation has long since descended into the atomized esotaria which is the modern philosophy of science, and as Kitcher pointed out, appears ever more remote from utility in solving real world problems like answering to medical pluralism. At the same time, no philosophers *are* suggesting that the problems of demarcation *matter* in the evaluation of the obvious pragmatic utility of disciplines like medical technology. What we hope to show by quoting Collins again is that Wittgenstein's philosophy is not used here to make specific epistemic claims, but rather as a kind of talisman, to grant Collins what passes for philosophical legitimacy in the culture of sociology. More to the point, Wittgenstein's elevation to the status of patron saint of the social studies of science amounts to little in the context of pragmatic medical epistemology. Although we will revisit this in more philosophical detail in Part III, we wish to suggest that what sociologists have lifted from Wittgenstein is better understood as a kind of poetic metaphysical anti-realism, which may be primarily a religious anti-realism that in any case was never intended as scientific, or even global epistemology, to be applied by sociologists in support of scientific anti-realism.

Peirce, Wittgenstein and Cupitt: consensus against Sociology's position on the relationship between Religion, Science and anti-realism

Cupitt characterizes a massive commitment of scholarship over the last few hundred years as taking a quietly genteel stand against a wave of atheism which, as Peirce would have predicted, was progressively marginalizing divinely revealed truth as a source of meaningful ideas. We argue that Cupitt, Wittgenstein and Peirce all have similar conceptions of religious experience and its relationship to scientific epistemology: 1.) religious experience bears significant importance on the level of phenomenology, which cannot be dismissed, as Frazer might have, as merely bad science; 2.) religious ideas may themselves be subject to certain pragmatic considerations, like their effects on ethics, our ability to get along with one another or existential succour, but the material judgements of religious ideas are best handled with an anti-realism very much distinct from the realism with which all three thinkers would have us understand subjects of scientific consideration

It is our contention that from Descartes to Wittgenstein, anti-realist argumentation until the middle of the 20th century is better viewed through the lens of religious philosophy than what we find today in social science epistemology in the form of radical notions of scientific anti-realism. Put another way, we are confident that the philosophers often cited by social science anti-realists would be appalled if they were aware of the scientific success of modern medicine and the extent to which their century/s old ideas are used to indict the scientific authority responsible for such powerfully useful technologies. We will demonstrate this in due course in Part III, for now, we wish for the

reader to consider the deep need that humans have to protect spirituality from the onslaught of modernity and the way this is served by sociology's muddled effort to account for and to ultimately collapse science's formidable pragmatic utility into a matter of politics alone -- if, incoherently, "by other means". We should be keeping this impulse in mind, not only as we briefly address Wittgenstein's reaction to Frazer, but also as we seek to understand sociology's reaction to medical pluralism and Evidence Based Medicine.

Nothing in our argument requires that we prove that Wittgenstein's essential motivation, with regard to these matters, was to protect the intellectual viability of religiosity. Rather, we would merely like to observe that this is not only a significant outcome of his writing after the *Tractatus*, but also that this impulse is shared precisely with the advocates of medical pluralism. This helps explain why, as we shall see, the very soul of sociological epistemology seems to spring reflexively to the *philosophical* aid of medical pluralism. The violent and vociferous reaction against perceived scientism that Wittgenstein can be seen as taking part in, is not an important driver in the contemporary philosophy of science. In fact, the larger contemporary legacy of the anti-scientism project -- to defend the epistemic underdog from perceived scientism in all cases -- is not born of contemporary scientific or philosophical inquiry at all. As such, it is on increasingly shaky ground. Sociology is, to the extent that it has committed itself to this steadily worsening position, at a tacit level at this point, finds itself wrongly motivated to knock down scientific authority, even when that scientific authority is quite obviously correct, free of any reasonable charge of scientism and carrying massive pragmatic implications for human health. This unusual position, born of the reduction of all scientific authority to the political, is both antithetical to common sense and expert philosophical consensus, and yet asserted with all the attendant privilege of academic authority. The resulting cudgel is irresistibly valuable to both CAM advocates and sociologists in the medical pluralism debates: to CAM advocates because they rightly see sociology as their most authoritative, academically potent political defenders -- and sociologists, who see themselves as the virtuous, academic experts, coming to the political defence of those who would otherwise be left to suffer at the hands of close-minded authoritarian, scientistic bullies. For both CAM advocates and sociologists, radical anti-realism serves as the basis to paint medical authority's well grounded concerns about CAM's efficacy as nothing more than a callous, scientistic overreach. However, the underlying motivation at work is rooted in the centuries old backlash against the emergence of scientific and secular authority -- the same kind of human impulses which drove the fashions of religious philosophical discourse in the early and mid 20th century -- and which have evolved into the postmodern fashions of the 21st.

Our use of the word "fashion" here is not merely pejorative. Peirce leaves us no choice other than to see the excesses of sociological epistemology for what they are: the product of the third mode of fixing belief. Cupitt's tour through centuries of religious philosophy showcases a notable conformity of events to what a pragmatist might have predicted using Peirce's ideas, demonstrating

again and again that as science and technology have gained pragmatic control of the world, religious ideas entrained by inferior modes of belief are necessarily abandoned. The epistemology of postmodern sociology can be productively understood as a continuation of this same misguided intellectual resistance. At the same time, Cupitt's pathos flags up for us what is perhaps the single greatest opportunity for us to get our thesis wrong in the end: by failing, like Frazer, to account for and meaningfully accommodate the deep psychological need for religious succour that drives medical pluralism, at least in the West.

Sociology and the reflexive application of anti-realism as a reaction to epistemic inequality

To organize these interlocking ideas, in this section, we leverage the structure of Cupitt's *Sea of Faith*, the relevant parts of which we will essentially summarise while pointing out: 1.) the similarities between the conflicting religious and philosophical desires and motivations of Christian philosophers for the last several hundred years and the conflict over medical pluralism -- the yearnings of those who see CAM and Traditional medicine as a source of spiritual succour missing from EBM; and 2.) the perfect conformity of Cupitt's narrative of Enlightenment religious discourse among leading philosophers and what one might predict from Peirce's *The Fixation of Belief*. In this context we will look at Wittgenstein's attack on the religious anthropology of Frazer. Thus allowing us to contextualize Wittgenstein in terms of religious anti-realism and make a case for his writing in this regard as making considerably more sense as religious apology than as a compelling argument for full strength anti-realism, which might be extended all the way to medical epistemology. As a general way of making sense of this, we will contrast the plausibility of religious anti-realism, which Cupitt very compellingly advocates with what boils down to sociologically pragmatic reasons -- ie. everyone getting along without murdering each other - versus the wildly less plausible notion of the full strength scientific anti-realism we are attacking here, specifically as applied to scientific domains like medicine, which we will argue is profoundly mistaken. In short: religious anti-realism is a great idea, scientific anti-realism is a terrible idea. We will save the philosophical minutiae for Part III, *here* we are seeking merely to understand what we need to about sociological epistemology and culture, prior to exploring what the rest of social science has to say about CAM and Traditional medicine as a social phenomena: what it is, and why people use it.

The connection between religion, medicine and magic is ubiquitous as we have seen. All human societies maintain medical knowledge, whatever form it takes. The conflicts inherent in CAM and Traditional medicine's interaction with EBM are painful, in many respects because the pragmatic superiority of beliefs derived from scientific thinking are so profoundly undeniable. As we noted in Part I, there are significant psychological harms associated with exposure to overwhelming evidence

that one's traditional worldview is clearly mistaken. Cupitt rightly observes that the seductive power of science and its "effective action" is disruptive to "...traditional world views and values" (pg. 5). He illustrates the Peircian inexorability of Western medicine in particular, in terms of the way it penetrates into other cultures by virtue of sheer pragmatic efficacy, simultaneously illustrating the way that people on the losing end of such technological conflict are suffering, and as such, can be seen as clients to a sociology willing to defend their beliefs:

...people everywhere are today experiencing the disruptive effect of modern knowledge upon traditional religious world-views and values. Science is now vast in scope and very powerful. Recession or not, the engine of new research and technical innovation keeps turning unstopably. Claiming to be value-neutral and independent of local political or religious beliefs, it penetrates all cultures with equal ease. It inevitably has a strong secularizing influence, for it opens up such wide possibilities of non-religious knowledge and effective action. Yet how can you keep it out? However stable and beautiful your traditional culture and values may be, the fact remains that science-based Western medicine (to quote one very impressive example) visibly works. Suppose you were a tribesman and see a medical team arrived in your area and cure the blind in hundreds, at a few dollars at a time. You are bound to be impressed -- and your gods are bound to shiver in their shrines. Your traditional beliefs and values no longer seem quite so all-inclusive and immutable and perfect as they used to be. Suffering which you have traditionally regarded as sent by the gods in punishment for ritual offenses now turns out to be caused by things like parasites and bacteria and to be curable quickly and cheaply, without any reference to religion. What you make of that? (pg. 6).

There can be little doubt that at least in medical epistemology, there are Peircian winners and losers. The pain, the disruption, the potential for the loss of identity, livelihood and even the loss of whole traditions of traditional medical practice is keenly felt by the losers. We argue that it is precisely a reaction to this kind of loss which has driven sociology to such epistemic extremes. Sociological epistemology has been developed over time into a tool with a wide capacity for the defence of those who find themselves on the losing end of scientific enquiry, until today, when we find a sociology which reflexively steps forward to defend such losers, as we shall see, without regard for the Peircian consequences.

Sociologists have long been aware of the growing tension between scientific knowledge of the natural world and the human struggle with both ethics and meaning. While rapid advances in naturalistic knowledge and antipathy for dogmatic nonsense had prompted many thinkers by the late 19th century to participate in the wider positivist project (to relentlessly de-prioritize what they saw only as "metaphysics" and interpretations of meaning), there is a limit to the areas of human knowledge and concern which are to be either radically revealed and clarified by scientific knowledge or productively discarded as unimportant. While positivism can be roughly seen as the project to force as many areas of human concern into one of these two groups as possible, Peirce would have attacked Positivism for similar reasons that he attacked Cartesianism, because it is clear that some matters of great importance must be retained which can neither be rendered theoretically infallible by the efforts of scientists (induction) nor productively ignored. Wilhem Dilthey sought to retain the gains of

naturalism (hence the natural sciences or *Naturwissenschaft*) while retaining an understanding as a method to the human sciences (*Geisteswissenschaft*) which accommodated human experience while simultaneously allowing methodological rigour²⁰. We shall be returning to the distinction between *Naturwissenschaft* and *Geisteswissenschaft* later in the thesis. For now, it is crucial to understand that profoundly anti-positivist sentiments rest at the centre of sociology's contributions to the medical pluralism debate. Sociology's now traditional role as the heroic defenders of the victims of positivism and scientism has, in the case of medical pluralism, caused them to venture much further than Dilthey would have thought reasonable -- far too far in the direction of prioritizing human experience over either naturalistic knowledge or even Dilthey's even-handed understanding of the human science's need for methodological rigour. The sociologist proponents of CAM and Traditional medicine have over-prioritized narratives derived from Peirce's a-priori mode of fixing belief, not only as a methodological preference, but politically. In over-reacting to scientism, they have proposed an equally narrow and misguided inversion -- thus rendering a justification for medical pluralism which ignores science completely and treats both disease and public health policy as a purely hermeneutic problem.

The advance of Naturalism and Science, the retreat of Religion and the effect of this dynamic on Medical beliefs

To return to our discussion of Cupitt, and the Percian dynamics at the heart of Western de-mythologization, Cupitt argues forcefully that pragmatic power drives the evolution of beliefs. He is remarkably Peircian in his evaluation of the forces which have come to effect the transformation of religious thought in the West.

As the body of available science knowledge and technology grows ever larger and more powerful, science progressively takes over from religion, in one area of life after another, the task of explaining what is happening and prescribing what is to be done. Even in fields whose precise scientific status is disputed, such as economics, social science and psychology, it is still the case that secular, utilitarian and 'engineering' or instrumental ways of thinking have taken over. The older super naturalist types of explanation have progressively diminished ineffectiveness and looking back we can now see that since about the time of Darwin they have played no part in any major branch of knowledge. Inevitably, religious belief and practice have been left looking like a hobby, a minority world-view and an opinion basically unimportant leisure-time pursuit on the part of those who have a psychological need for 'roots' in an ethnic past or cultural tradition (pg. 9).

²⁰ We consulted the Stanford Encyclopedia of Philosophy web site's page entitled Wilhelm Dilthey First published Wed Jan 16, 2008; substantive revision Thu Mar 22, 2012. It was retrieved from the following URL on August 17, 2014:
<http://plato.stanford.edu/entries/dilthey/>

The tension between potential modes of inquiry implied in the word "explaining" is precisely what Dilthey was getting at. Further, Cupitt's observation is so rich in the themes relevant to the thesis that we best enumerate them: 1.) Just as Peirce would observe, different means of fixing belief generate different results -- with scientific methods generating beliefs which are both subject to rapid progress in development and profoundly superior pragmatic power; 2.) our social development is driven by our inability to ignore these obvious differences in pragmatic power; and 3.) that, at least in the West, religious belief and practice has been profoundly marginalized in favour of scientific modes of action. Cupitt will later argue in *Sea of Faith*, just as we will later in the thesis, that this process of marginalization of traditional beliefs in the face of the pragmatic efficacy of scientific beliefs is a ubiquitous human phenomenon. The fact that science in its most potent form began in the West does not diminish its power to drive social development and cultural change among human beings worldwide. Cupitt goes on from the passage above to immediately over-generalize in terms of the extent to which supernatural beliefs have been abandoned in regard to day-to-day decision-making:

Doctrine, supernatural belief, maybe tenaciously maintained, but it becomes sick apologized, privatized, a matter of one's secret identity and no longer effectively connected with the way one behaves in one's real social and economic life (pg. 9)

What makes this passage so interesting, is that it is so obviously wrong when we take into account trends in the medical marketplace. CAM and traditional medicine take up nearly one half of the medical marketplace in the West alone. The consumer attraction for CAM and Traditional medicine in the West is not just a breach in the wall of pragmatic consumer knowledge (although we believe this is a very important factor), it is simultaneously the expression of the desire, even the *need* for a supernatural component to medicine. Bruce Charlton has suggested that CAM in the West is best understood as a new age spiritual healing practice which is completely disconnected from science, medically inert and reduces entirely to: "...the subjective meaning to the individual -- the self evaluated effect it has on a person's sense of well being (Ernst, et. al 2008, p. 72). Although Cupitt may have neglected to consider new age spirituality in the above consideration of supernatural belief, CAM makes it clear that at least in a medical sense, such beliefs are front and centre in the way some of us live our "...real social and economic..." lives.

As Cupitt continues to describe the transition from supernatural beliefs to naturalism in the West, he touches on another concept important to the thesis: Naturalism (p. 13).

Scientific and critical ways of thinking do in the long run move towards naturalism, the doctrine that the only way we can satisfactorily explain any phenomena in nature is to connected up in a regular way with other phenomena and thereby show it to be just one instance of a recurrent pattern.

Again, as we shall see, naturalism is one of the pillars of scientific medical thinking. Its adoption, to various degrees, has been instrumental in medicine's pragmatic success. Cupitt's identification of the progressive adoption of naturalism as a key component of contemporary social development echoes Peirce's idea that over time, scientific modes of belief get things progressively right, developing more and more predictive and pragmatic power. As Peirce would observe as the most important justification for his hypothesis of reality: science works -- and as such when applied to medicine, in the same way Cupitt applies naturalism to social development, science is successful enough to warrant its adoption to the exclusion of all competing modes of fixing belief.

Cupitt sums up the section by noting that realism has been abandoned in the contemporary interpretation of Christian mythology. He draws the distinction between those who abandon supernatural realism in the past and became non-religious and those who today can choose to be conscious believers: "...people who know that religion is just human but have come to see that it is no less vital to us for that (p. 18)." This is an idea which we have to hold more closely to the thesis than one might expect. We were struck while reading Cupitt that his articulation of the irrepressible desire for spiritually Christian modes of thought, even in the harsh light of the complete rejection of the superstitious elements of church life and the Christian myth, mirrors exactly the motivations underlying the rebellion against EBM in the West. Cupitt, therefore, lends support not only to understanding the inevitability of the transition from religious to pragmatic medicine, like Auguste Comte, Max Weber and indeed James George Frazer would have argued in their respective socio-evolutionary models of religious change, but also the inevitability of the turn back to CAM and Traditional medicine as an attempt to re-claim the spiritual aspects of healing no longer available after EBM began to expunge superstition from the medical landscape.

Cupitt seems to weave these two strands together as his justification for religious anti-realism.

As the change from the old world-view to the new takes place, people feel less and less need to refer to the religious realm. It no longer seems so urgently necessary to secure the favour of spirit-beings behind the scenes. We no longer instinctively look for supernatural admonitions in the small reverses fortunate that happen to us every day. Those people who do think they are receiving little messages all the time seem to be superstitious or even mad. The explanatory utility of religious ideas thus fades away. Myth begins to get a bad name and to be equated with untruth (p. 32).

Over and over again, Cupitt juxtaposes the inevitable roll back of superstition he describes in the quotation above, with the inability of some people to live without meaning, value, moral purpose, or what we will later discuss, what medical anthropologists will often describe as the "why me" component of medical understanding.

How frail the human is, how wretched most people's lives, and how threatened our happiness is by evil within us and about us! It is still the task of religion to generate in order of meanings and values for us to live by, and order which can give moral weight and purpose to individual

and social life. Human society will always need such an order as a gyroscope that keeps the moral life properly poised between commitment and nonattachment, pursuant of the active social virtues and inner disinterestedness and freedom (p. 33).

William James would have agreed. As he argued in "The Will to Believe", human beings are compelled to believe in God and are justified in doing so:

To take a trivial illustration: just as a man who in a company of gentlemen made no advances, asked a warrant for every concession, and believed no one's word without proof, would cut himself off by such churlishness from all the social rewards that a more trusting spirit would earn,--so here, one who should shut himself up in snarling logicity and try to make the gods extort his recognition willy-nilly, or not get it at all, might cut himself off forever from his only opportunity of making the gods' acquaintance. This feeling, forced on us we know not whence, that by obstinately believing that there are gods (although not to do so would be so easy both for our logic and our life) we are doing the universe the deepest service we can, seems part of the living essence of the religious hypothesis (James 1912).

As sociological experts will tell us in the section immediately following this one, there are many in our society who believe that a more emotional or spiritual connection is essential to healing, and this is a serious subject of study at this time in EBM. The field of narrative medicine can be consulted with psychology, to determine an appropriate means of addressing this concern. Certainly the continuing tradition of medical chaplaincy could also be expanded to address more of these needs.

As we will address in Part IV of this thesis, we believe a necessary side to significant action to reduce the abuse of these desires by the negligent and fraudulent elements of CAM, is to take seriously the human desire for emotional and even spiritual succour, and however reductionist its study may become, to generate a means to heal those whose ailments may be essentially psychological in nature. In other words, although it will be frequently argued by sociologists in the following section that CAM and Traditional medicine have arisen to satisfy some needs that they assert are not adequately addressed by EBM, we firmly believe that EBM's professionalism is a unique and crucial bulwark against exploitation, and that this makes it a far better choice as a source of all possible forms of medical succour than CAM. Part of the process of removing superstition and bad science from the medical marketplace, is to acknowledge, study, understand and address the deficits in EBM that CAM could be said to have evolved to fulfil. As we shall see, sociologists will argue that many CAM users can be characterized as the "worried well" who seek a connection with their CAM therapists which is not possible in the time pressured and mechanistic world of EBM. The problem, of course, is not the need itself, but rather the potential failure of the need to be satisfactorily accommodated by EBM. The principle argument for medical pluralism is patient choice. However, a pluralistic medical system is fundamentally incoherent, and most importantly, lacks the common ground upon which serious threats like negligence and fraud can be clearly delineated. The

ontological incompatibility of CAM and EBM leaves them locked in the very same stalemate that Cupitt describes for contemporary Christendom:

The believers say, 'The faith works, so the theology must be true.' The skeptics retort, 'The theology can't be literally true, so how can the religion work?'. And the familiar arguments between believers and skeptics begin, arguments which everyone feels have now become inconclusive and profitless (pg. 34).

As the sociologists will tell us in the upcoming section, the above passage perfectly describes the stalemate between CAM and EBM. The operators of the two cultures are divided by their willingness to embrace naturalism or science. Cupitt again ties this immediately to the paradox of the need for meaning. It similarly strikes us that patients who subscribe to CAM's spiritual warrants, are in need of the same kind of consolation that a realist atheism finds itself so unable to provide in the context of traditional Christian church life: answers to the "why me" questions: why is my child handicapped; why do I have cancer? We understand these impulses as fundamentally central to healing, yet in the current medical marketplace, CAM exploits the interface between spiritualism and EBM.

CAM Holism can be read as an effort to purposely obscure the pragmatic poverty of what CAM brings to the table, intentionally aping medical language and symbols like lab coats, and using the word "Doctor" to refer to themselves in an effort to confuse patients and convince them that CAM provides both medical efficacy *and* spiritual counselling. This is not just an unethical yet lucrative business model, providing such a spiritual component is what EBM must ultimately do as well. Unfortunately, CAM clearly exploits both ignorance and the fundamental human impulse Cupitt is describing as it lacks the scientific means to identify its own negligence and fraud. CAM and EBM represent two, mutually exclusive means of integrating the psychological needs of patients with effective medical techniques -- mutually exclusive means of providing a synthesis between secular and spiritual.

Cupitt's proposal for the integration of these two mutually exclusive cultures mirrors our own. Cupitt's very personal motivation nicely illustrates why we believe that medical magic is harmful to the public health marketplace. Cupitt worked in the UK as a religious official in a small church. He was confronted with the following crisis:

At the time I did not fully work out the implications of what I was saying. I could not stomach the idea that calamities like a baby's deformity or a man's cancer were directly caused by God. If I sincerely believed that about some evil that had happened to me, I believe I might go mad. A malignant God might send any number of further agonies at any moment and I could do nothing at all to ward them off. I clearly felt the scientific view of things to be a blessed release from that kind of terrorism. The world is a continuous natural process, and we should not think that natural events bear little occult messages to us. That is nowadays a kind of insanity. We ought to love natural science for its power to free us from superstition and fear, and science is a jealous consort who rightly demands complete fidelity. You must be consistent. You cannot hold the scientific attitude part time.

In the same way that Cupitt refuses to acknowledge the superstitious fear of a "malignant God" we seek to deny the commercial viability of those who would *sell* protection from bad energy or those who would suggest that cancer can be cured by positive energy, if only the patient's desires are sufficiently pure of heart. Similarly, we see such tortured misunderstanding as a form of insanity, contracted through the transmission of pernicious medical beliefs, which can be immunized against by a more uniform embrace of naturalism by medical leaders, educators and policy makers initially, and ultimately by society as a whole. We should not be confused in recognition of the fact that spiritual needs can be met within the context of an EBM medical marketplace. In doing so, EBM can be constrained to provide hermeneutic succour in a way which is superior to CAM -- a way which is patient centred and without the ulterior motivations and actions of CAM practitioners who are not sufficiently research based or regulated and are free to base the meanings they pass on to patients more on their own financial or personal needs than the patients wellbeing. What we will be arguing, is the medical marketplace outside of EBM is insufficiently restrained both scientifically and hermeneutically. While we will seek means to administer to patients spiritual needs through EBM, we will similarly seek to restrict the sale of CAM's cocktail of spiritual services and fake medical remedies on the open market, where they are harmful by virtue of their lack of respect for scientific values and what expert scientific communities have to tell them about their own negligence and fraud. Like us, Cupitt does not see the uniform embrace of science as the end of religion. Rather, the embrace of a more Peircian pragmatic approach which acknowledges that scientific explanations are better than notions of spirit agency, leaves room for spirituality. As Cupitt puts it: "Religion is a way of responding to life, shaping life, giving ultimate meaning and value to life (pg. 36)."

In a process that sociologists will shortly term "medicalization," Cupitt sees people beginning to take the problems they used to take to priests, to doctors instead. The difference being that Freud and the psychologists created a tool to help people which:

... freed the patient from the domination of disabling allusions, it delivered her only into a clear recognition and acceptance of the necessities of the human condition. In no sense that it promised any emancipation from those necessities (pg.76).

This is not the same as a religious experience. From within this emergent psychological tradition, Jung suggested that secular society could understand its own sanity better by accepting that humans have deep-seated religious impulses we ignore at our own psychological peril (Crowley 1999). The result of ignoring this, is what Cupitt describes as people becoming "spiritually starved". As we shall soon see, this mirrors exactly the motivation of what many observers have come to characterize as the worried well" who make up much of CAM's clientele in the West. Cupitt suggests:

By contrast religion often sets its golden age in the past and tends to regard any change as being for the worse. The goals of secular and progressive science-based social development are regarded by the religious as unsatisfying, unspiritual and shallow (pg. 88).

This is a precise characterization of the appeal of Traditional Chinese Medicine, which as we shall see, derives its main cache of plausibility in the West from the spurious claim that it is 3000 years old. The idea that medical technology must be effective because it is old, is a fallacy frequently evoked by those who would sell Traditional medicine in the West -- Ayurveda, Maputche and American Indian traditional medicine are all advertised in this way. Furthermore, the CAM advocacy charge that EBM is "unsatisfying, unspiritual and shallow" is the flip side to the appeal of Naturopathy, the broad appeal of the fallacy that "natural" things are better as medicine and finally that a "holistic" approach to medicine, which "considers the patient as a whole." As we shall see, personal relationships with patients are impossible to control for in a clinical trial, so definitive clinical trials are not available as a tool to investigate the efficacy of any procedure which is inextricable from a therapist's ongoing personal relationship with the patient. As it turns out, this vulnerability of research design has been a tremendous boon for CAM advocates. CAM therapies are now piling on to claim that their particular modality is inextricable from a personal relationship between patient and therapist, and therefore is not vulnerable to the conclusive debunking of double blind clinical trials. To conclude his observations on the tension between the need for religion and the simultaneous need for scientific understanding, Cupitt again echoes Peirce:

Because religious knowledge claims completeness, personality and eternal validity it becomes involved with cosmology. Religion commonly furnishes the community with a general characterization of the cosmos and of their own history. It offers an account of human origins and destiny, and provides interpretations and remedies for disease, famine and other contingencies of life. But these are just the territories that modern science has particularly made its own. Its power has proved so great that in every society nowadays the intellectual authority of scientific medicine, agriculture and astronomy is acknowledged as soon as they are understood. In these areas, religion must simply withdraw (pg. 88).

Cupitt's use of medicine as an example of both a necessarily and traditionally religious domain and one of the domains most dominated by the inexorable intellectual authority of science, underscores both that medicine is a ubiquitous pragmatic concern of all human beings, but also that Peirce's notion that the pragmatic power of scientific belief is what makes it ultimately more useful and more successful, is playing out universally as well.

The inexorable progress of medical science still does not in itself extinguish either human spiritual needs or the entrepreneurial spirit which invariably seeks to exploit them. It is, after all, only the presence of more effective medical alternatives to Traditional Medicine which makes the continued marketing and sale of inferior Traditional Medical techniques "exploitation" anyway. As

Cupitt points out, Traditional Medicine like all traditional modes of thought, is fundamentally resistant to the kind of pruning which contact with scientific modes of thinking inevitably requires.

The old pre-modern sciences -- astrology, alchemy, herbal medicine, bestiaries, weather-lore -- no doubt did contain some particles of truth, but it was useless as long as it remains so mixed up with errors and superstitions. It was in Descartes's time that people began to grasp the vital point that to make a system of knowledge stronger you must ruthlessly prune out the errors in it. Traditional thought hates pruning and subtracting. It always adds, and so it ends with an untidy mess (pg. 132).

It is in this mess which we see the ethical problems with medical pluralism. As Peirce would observe, science prunes away false beliefs. CAM, on the other hand, is unconstrained by anything but fashion in the a-priori method of belief. Both CAM and Traditional Medicine exist in the marketplace as a result of the market's failure to grasp Peirce's warrant for the scientific mode of fixing belief.

Unfortunately, CAM is big business and the sum of its practitioners, clients, researchers, university professors, lawyers and PR apparatus constitute a huge culture. As Cupitt brings to our attention, CAM, like any dogmatic spiritual system, exhibits a tendency to segregate itself.

There is an obvious link between a religion's claim to complete and exclusive truth and its tendency to segregate by sealing up its adherents in a closed world. The two features of religion work to confirm each other. Vivid religious faith often tends to shut one up in a subculture of like-minded people. Within that world the truth of the faith seems obvious and unquestionable, something so much taken for granted that it is rarely mentioned. A tacit consent of this kind creates a strong and distinctive atmosphere that works to exclude sceptical outsiders and their uncomfortable questions. And the more we are able to assume that our faith is the truth and our world the world the less we shall be aware of any world outside our own world (pg. 166).

Thus, as we shall soon see, sociology will describe the insular world of CAM.

It is, however, upon this point that Cupitt rests his initial assertion that since conflicting worldviews are doomed to incommensurability and conflict, matters of religion and spirituality must be consigned to a different epistemic status: that of anti-realism. Now that the West has come to agree that conflicts regarding the supernatural, the revealed and the spiritual rest at an impasse, the most reasonable course of action is to declare all spiritual ideas on a level playing field where they can be left to their remaining utilities to vie for human attention -- "as diverse constituents in a single social world." The basis for the notion of religious tolerance, Cupitt argues that spiritual matters are fundamentally different to material ones: where material matters are commensurable and of the kind of mutual pragmatic concern we must see our way to authorizing universally, spiritual matters can and should be relegated to anti-realism, and epistemic condition where no particular authority arises for any particular position.

Our conclusions regarding anti-realism and Wittgenstein

Cupitt frames the evolution of religious anti-realism in the discourse of relevant thinkers including Wittgenstein:

Here Schopenhauer adumbrates an idea that was greatly to influence Tolstoy and Wittgenstein: the essence of religion is not doctrine but holiness itself, something wonderful and very rare, possibly only on the basis of a kind of knowledge that cannot be put into words. However much its intellectual superstructure may have become discredited, there remains at the heart of religion something supremely important but in the affable. It is 'the mystical' (pg.177).

This movement toward the development of the modern conception of the mystical as a kind of fact- and even logic-free ground upon which the spiritual can be expressed, free of the usual confines of realism and its attendant restraints, is crucial to understanding the point at which arguments for religious anti-realism depart from epistemology writ large.

Wittgenstein plays prominently in sociological epistemology, as we have seen. It is our contention that Wittgenstein's post-*Tractatus* writing, particularly when he comes the closest to addressing medical pluralism in his critique of Frazer's *Golden Bough*, is directed toward the same anti-realist terrain of epistemology as Cupitt's religious anti-realism:

What is clear is that by 1933 Wittgenstein had already rejected most of the basic principles of his early philosophy and was developing something so novel, strange and hard to understand that he himself thought he was inventing a new subject. It may be that he was changing the whole course of Western thought; but if so the change was not from one system to another, but rather from system to method. It is a change that needs to be learned is still studied in process in a large number of particular cases. It was a change from which one needs an apprenticeship that is what makes it so hard to understand. My attempt to give summary description of it as if it were a metaphysical shift would have incurred his wrath. Silly account given must negate itself.

Roughly, then, Wittgenstein renounced the idea that the chief function of language is to copy or trace the structure pre-existing independent reality out there. Language does not gain its meaning by referring and copying in that way. Instead, language has to be seen as embedded in and interwoven with human practices. It is highly rule-governed, and it gets its meaning from the various jobs does in furthering language-games and forms of life (pg. 227).

It cannot be overstated how certain it is that Wittgenstein would not have allowed his ideas to be imported as warrants for the full strength anti-realism present in much sociology of science today (see Harry Collins above). Cupitt's use of the term "rejected" to characterize the way in which Wittgenstein's later works bears on the *Tractatus*, makes perfect sense in the context of religious anti-realism, but not in terms of medical epistemology. This is the core of our argument about the way Wittgenstein is misinterpreted by sociological epistemology: Wittgenstein's later work does not inform the cut and dried, pragmatic material terrain of scientific epistemology. Here, Wittgenstein is addressing language and the way it functions outside of science. This is where we part ways with Cupitt, who claims on page 230 that Wittgenstein intended to extend his linguistic doctrines to

"...natural science..." as well, suggesting of Wittgenstein that: "If he is a non-realist about religion, he is a non-realist about everything else." It is here we would remind the reader of Cupitt's impression that Wittgenstein's writing was "...so novel, strange and hard to understand..." that Cupitt was certain that Wittgenstein would have reacted negatively to Cupitt's summary. We are sure of the same thing, but for different reasons than Cupitt. We are quite certain that Wittgenstein was not a global anti-realist. In the interest of tying our argument as closely as possible to medical epistemology, we will quote Wittgenstein from his reactions to Frazer's *Golden Bough*.

Wittgenstein was aware that the pragmatic nature of medicine places it in a firmly realist epistemic landscape.

We should distinguish between magical operations and those operations which rest on a false, over-simplified notion of things and processes. For instance, if someone says that the illness is moving from one part of the body to another, or if he takes measures to draw off the illness as though it were a liquid or a temperature. He is then using a false picture, a picture that does not fit. (Rhees, page 5).

Wittgenstein's use of his "picture" language from the *Tractatus* serves to anchor his argument in a specifically realist mode. There can be no doubt that Wittgenstein is unwilling to relinquish realist aspirations when dealing with medical epistemology. His argument with Frazer, in this case, is in the religious realm, or at least in Frazer's too ready willingness to reduce primitive worldviews to bad science:

What narrowness of spiritual life we find in Frazer! And as a result: how impossible of him to understand a different way of life from the English one of his time! (p. 6)

Wittgenstein, while disgusted with Frazer's crude armchair anthropology, is nevertheless very Peircian in his observation of the static nature of magic.

Simple though it may sound, we can express the difference between science and magic if we say that in science there is progress, but not in magic. There is nothing in magic to show the direction of any development. (p. 13)

Finally, W. suggests that animals conduct "...what we might call animal activities...", they feed themselves and do other things, but only people carry out ritual actions, which according to W., "...bear a peculiar character and might be called ritualistic (page 7)." There can be no doubt that Wittgenstein maintains in his own mind precisely the same anti-realist boundary across the landscape of human knowledge that Cupitt describes as the result of Western demythologization. The line that demarcates the anti-realism we must apply to religious and spiritual thought and the scientific realism we must apply to the practical world of medical epistemology: a world of the practical where we depend on science to manipulate the forces in our lives to the best of its ability. Wittgenstein, like

Dilthey, understood the difference between naturalistic and hermeneutic knowledge -- and the different levels of scepticism and epistemic methods ideally characteristic of each.

It is crucial that as we engage the sociological literature on CAM that we understand the following: 1.) CAM is to Sociology what Traditional medicine is to Anthropology; 2.) That Sociology shares many of the same anti-realist habits of mind that Anthropology does, but also entertains specific philosophical misconceptions born of mistaken interpretations of expert philosophical consensus regarding medical epistemology; 3.) the misinterpretation of Wittgensteinian anti-realist metaphysics as global anti-realism sits near the centre of this mistaken position; and finally 4.) the antagonistic impulses which drive demythologization and the human drive for spiritual connection are not only present in the tension between EBM and CAM but in the way Sociology reflexively seeks to defend CAM's epistemic status through anti-realist attacks on the authority of EBM. Although we must depend on sociological experts for the ethnography of CAM, we must interpret their findings in light of their inability to accept the basic Peircian warrants to privilege scientific information, central to our epistemology of medicine.

Sociology of Medical Pluralism

Professionalism: Foundations and Conclusions

We begin our look into medical pluralism through sociology with a brief digression into the sociology and philosophy of professionalism and expertise. The notions of professionalism and expertise -- including both medical and academic professionalism and expertise -- are concepts crucial to the thesis in multiple ways. As the thesis has developed, we have discovered that issues of expertise and professional conduct both bring the ethical to bear on the epistemic, as well as the epistemic to bear upon moral necessity. Of course, we will later address the notion of *medical professionalism*, the constellation of obligations that expert doctors have toward the sick in some detail, both in terms of its fundamental altruism and in terms of the way such obligations interact with epistemic considerations. However in the context of our look at the sociology of medical pluralism and professionalism, we will also consider the wider notion of *academic professionalism*, a concept we will use both to map Peircian epistemic stipulations about the behaviour of expert scientific communities onto sociological conceptions of professionalism, as well as a means to critique the recent sociological output on medical pluralism.

The two-way connection between epistemology and ethics lends a subtlety and intimacy of interconnection to both our critique of sociology's treatment of medical pluralism, and what we will end up concluding about medical pluralism ourselves. Consider the following two ways professionalism underlies both the foundation and the conclusion of our work here. First, according to Peirce, the academic professionalism of expert scientific communities is a necessary condition for the

scientific mode of fixing belief, and thus the progression toward the Peircian conception of the real. We will shortly flesh out Peirce's ideas in this regard in more detail to highlight just how closely his requirements for scientific behaviour map onto sociological and more contemporary philosophical conceptions of professionalism. However, in a wider sense, scientific modes of fixing belief outside of the originating scientific community also rely on the academic professionalism of wider academic communities, for the interdisciplinary interpretation, evaluation and dissemination of pragmatically powerful scientific beliefs. In other words, the values and norms of behaviour that constitute the professional expectations of working scientific experts, are ethical considerations which bear directly on the production of pragmatic knowledge, as a necessary condition. While at the same time, new pragmatically powerful beliefs, within an isolated group of experts, no matter how pragmatically valuable the knowledge in question might potentially be, is useless unless that knowledge can penetrate the belief systems of other participating academics, policy makers and public health actors along the chain of healthcare decision-making. In the case of medical pluralism in the West, when the best available metaphysics, evidence and reasoning are ignored, allowing pragmatically valuable beliefs to be mistakenly mired in conflict with pragmatically impotent beliefs, either within or downstream of the originating expert scientific communities, this constitutes a failure of norms of academic professionalism. Thus ethical norms drive the development of pragmatically powerful beliefs, while precisely these new, pragmatically powerful forms of knowledge, to the extent that they hold great promise for alleviating human suffering, impose great ethical responsibility upon all the actors along the epistemic chain -- right up to the direct provision of medical care. Some failures of professionalism can thus be understood as the mistaken use of inferior modes of fixing belief, which constitute a pernicious disruption in the vital utilization of pragmatic knowledge.

Central to our critique of Sociology's handling of medical pluralism is our claim that Sociology has, to a much greater extent than Anthropology, abandoned academic professionalism along with realism. Just as we will further flesh out Peirce's ideas on scientific professionalism and epistemology, we will reiterate Peirce's characterization of what, in the modern academic policy setting, tends to take the place of scientific modes of fixing belief, when such a breach of professionalism occurs: "fashion". We henceforth adopt the word "fashion" as Peirce's pejorative short-hand for a-priori thinking for the rest of this analysis, referring interchangeably to beliefs derived from a-priori modes of thought as matters of "fashion" -- or what is "fashionable" -- in much the same way that the word "political" is often used in common parlance to distinguish forms of decision-making which ignore reality, in order to conform more closely to some ideological objective or to the desires of some politically powerful group. Indeed, this is another good characterization of how mistaken policy regarding medical pluralism has developed in the West. It is in this way that we will conclude that sociology's uncritical embrace of CAM, along with its continued conformity to anti-realist scepticism toward the scientific output of other disciplines, including medicine, constitutes decisions made more on the level of conformity to fashion, rather than decisions made on the level of

a robust, interdisciplinary effort to entrain the relevant philosophical and scientific experts to answer the questions raised by medical pluralism. It is from within this critique of the sociological data and analysis of medical pluralism that our own, much more philosophically and sociologically robust conclusion about the nature of CAM in the West will emerge: that CAM is precisely and entirely a series of artefacts of the broad failure of various forms of professionalism.

Peirce and Science as Social Principle: Beliefs, Expertise, Professionalism and the Real.

1.) Science and the Social Principle; 2.) Science and Individual Beliefs; 3.) Expertise; 4.) Professionalism.

1.) Science and the Social Principle

Peirce's notion of the real is tied intrinsically to the psychology -- the cognitive operations and resulting beliefs -- of scientists themselves. Furthermore, the use of the laws of logic, as part of the architecture of the real, along with direct observation, experimental methodology and a host of values and social behaviours which allow scientists to communicate their individual contributions to the expert community, all of these things are necessary to allow the community of scientists to progress inexorably toward the real as a community, in the form of their collective command of increasingly pragmatically potent beliefs. In short, *professional conduct by and between experts* is not only essentially what science is, but *professional conduct by and between experts* is also humanity's most pragmatically potent means of access to the pragmatic control of the real. As Peirce puts it, from page 248 of the *Philosophical Writings of Peirce* (Peirce 2009).

The real, then, is that which, sooner or later, information and reasoning would finally result in, and which is therefore independent of the vagaries of me and you. Thus, the very origin of the conception of reality shows that this conception essentially involves the notion of a COMMUNITY, without definite limits, and capable of a definite increase of knowledge. And so those two series of cognitions--the real and the unreal--consist of those which, at a time sufficiently future, the community will always continue to re-affirm; and of those which, under the same conditions will ever after be denied. Now, a proposition whose falsity can never be discovered, and the error of which therefore is absolutely incognizable, contains, upon our principle, absolutely no error. Consequently, that which is thought in these cognitions is the real, as it really is. There is nothing, then, to prevent our knowing outward things as they really are, and it is most likely that we do thus know them in numberless cases, although we can never be absolutely certain of doing so in any special case.

Pragmatic access to the real is limited by the short-comings of individual perceptual proclivities, but these "vagaries of me and you" can be overcome by the communal consideration of scientific claims. "Information and reasoning" will eventually get the community to the real, "as it really is". As we shall see, evaluations of clinical efficacy, specifically, clinical research design and statistical meta-analysis, are nothing other than tools which allow groups of scientists working together to overcome

the otherwise insurmountable perceptual limitations inherent to solitary actors or non-scientific groups. This Pragmatist, Peircian understanding of the real, along with both the work products and the over-all consensus of clinical medical experts, is precisely what is overlooked or actively rejected by the anti-realist sociologists we are about to engage.

Peirce's emphasis on "community" in the above paragraph suggests that ethics is intrinsic to scientific practice. As we mentioned in the opening paragraphs to this section, pragmatic power is *important* for human flourishing. It takes good scientific ethics to produce valuable knowledge, and that knowledge, by virtue of its pragmatic value, belongs to everyone and carries a mandate that it be shared. Peirce realizes, with the same kind of boundary dissolving insight with which he concludes that metaphysics is continuous with science, that ethics is continuous with the pursuit of human knowledge -- that logic itself, is rooted in the social principle (Peirce 2009, p. 162)

They must not stop at our own fate, but must embrace the whole community. This community, again, must not be limited, but must extend to all races of beings with whom we can come into immediate or mediate intellectual relation. It must reach, however vaguely, beyond this geological epoch, beyond all bounds. He who would not sacrifice his own soul to save the whole world, is, as it seems to me, illogical in all his inferences, collectively. Logic is rooted in the social principle.

This is a rather powerful argument for the universality of our medical epistemology, an argument which calls into question the ethics of those who would consign medical knowledge to the purgatory of scientific anti-realism. We will soon encounter sociologists who will claim, without evidence, that Evidence Based Medicine acts in ways which can be understood only as "self-interest" and that will reflexively question EBM's ethical motivation. Our response to these arguments will be in reference to Peirce's argument here: that logic, science and pragmatically potent medical epistemology -- evidence based medicine itself -- is the universally useful product of good social practice. Good social practice is essential to knowledge production in the context of scientific practice and there are ethical constraints on actors downstream of the production of that knowledge to share the pragmatic benefits with everyone.

To Peirce, to us, and to the well developed, explicitly stated and utterly uncontested ethics of modern biomedicine, pragmatic matters are essentially engaged in the altruistic pursuit of the social impulse (Peirce 2009, p. 163):

It may seem strange that I should put forward three sentiments, namely, interest in an indefinite community, recognition of the possibility of this interest being made supreme, and hope in the unlimited continuance of intellectual activity, as indispensable requirements of logic. Yet, when we consider that logic depends on a mere struggle to escape doubt, which, as it terminates in action, must be in emotion, and that, furthermore, the only cause of our planting ourselves on reason is that other methods of escaping doubt fail on account of the social impulse, why should we wonder to find social sentiment presupposed in reasoning?

To Peirce, the process of science is essentially pro-social. To us, medical epistemology in particular, in that it is focused very closely on specific human problems, is *even more* pro-social than science generally. We set out here to outline Peirce's vision of scientific practice, to make the important point, as we shortly will, that notions of "expertise", "professionalism" and even the psychology of scientific learning map precisely onto Peirce's conception of scientific practice. At the same time, we have also accumulated some ammunition for use when we engage sociological analysis, like Cant and Sharma's *A New Medical Pluralism?* (1999) which rails against EBM's "conflation" of "scientificity (sic) and professional altruism" (p.94), the imagined contradiction between "reductionist" science and "the ideal of the caring practitioner"(p. 101), or EBM's supposed "shift" from the cognitive authority of scientific knowledge to the moral authority of professionals concerned about consumer wellbeing and standards of competence among CAM practitioners. We have always questioned the notion, popular among social scientists and critical theorists that science is somehow evil or pernicious. We see this particular habit of mind as a conflation of the pursuit of knowledge and the use of knowledge. However, Peirce profoundly undermines this kind of anti-science assumption, by observing that the very practice of science and logic are by their nature fundamentally pro-social. Of course Evidence Based Medicine is altruistic. To argue otherwise is to misunderstand both medicine and science.

2.) Science and Individual Beliefs

Having established the connection between science, logic and the social principle, we can immediately see that the values and expectations we have come to associate both with expertise and professionalism, already appear to fit precisely into notions of scientific expertise and professionalism. There is however one last idea to highlight in Peirce's writing before we look at expertise and professionalism more closely: the psychology of science. As the burgeoning field of neuroscience continues to unpack the way human beings think from the bottom up, it is not surprising that neuroscience is being fruitfully applied to questions about science education. Neuro-imaging now validates phenomenological and psychological constructs within specific "*domain-general cognitive processes*" as the fundamental components of scientific thinking. As such, neuroscience seeks to provide us with an account of "*The Development and Application of Scientific Reasoning*", an account, which maps precisely onto Peirce's account of the same thing.

Scientific reasoning is by definition a broad term, and encompasses the mental activities that are involved when people attempt to make systematic and empirical- based discoveries about the world. The goal of the scientific reasoning process, as highlighted by Zimmerman (2000), is to extend our world knowledge thus allowing us to gain a more detailed and conceptually richer understanding of the domain of inquiry. Throughout this scientific reasoning process, people make use of several domain-general cognitive processes that are employed across different situations to facilitate the discovery process. It has been argued that these domain-general

cognitive processes, such as causal reasoning, deductive reasoning, analogical reasoning, hypothesis testing, and problem solving, are the same cognitive tools that humans use in everyday nonscientific contexts (see Dunhar & Pugelsang 2005, for broad coverage of these and other domain-general cognitive tools).

These excerpts, taken from page 237 of the textbook *Educational Neuroscience*, from under the chapter heading "What is Scientific Reasoning?" seem straightforward enough, both in terms of how scientific reasoning maps on to what Peirce has to say about scientific practice and how closely this conforms to contemporary accounts of scientific professionalism and expertise (Mareschal et al. 2013) (Mareschal et al. 2013).

However, for both Peirce and the neuroscientists, the most important element of scientific thinking is worth highlighting on its own: the ability, the readiness, even the *desire*, to identify the circumstances in which one is justified in changing one's beliefs. As the authors noted above, many of the same rational abilities used by scientists "are the same cognitive tools that humans use in everyday nonscientific contexts." Peirce recognizes this as well, but he highlights very specifically an additional proclivity of the scientist, a step uncharacteristic of more natural modes of thought: the ready ability to *change* beliefs.

Now it is quite impossible for a practical man to comprehend what science is about unless he becomes as a little child and is born again. Scientific men are made out of youths who during the plastic period of life are set to study science for a number of years. Most of these develop into mere teachers; only a minority imbibe the spirit of science. The practical man has a definite job which he sets himself to accomplish. For that purpose he has to adopt some consistent plan which must be based upon a theory, and to that theory he must be wedded before the work begins. Even if his practical problem is no more serious than playing a game of whist, when there are only three rounds of a hand to be played, he must go upon the supposition that the cards lie so that he can win the odd trick. If he is a judge presiding over the hearing of a cause, that cause must be decided somehow, no matter how defective the evidence may be; and consequently he is constrained to lay down a rule for the burden of proof. But the idea of science is to pile the ground before the foot of the outworks of truth with the carcasses of this generation, and perhaps of others to come after it, until some future generation, by treading on them, can storm the citadel (p. 311 (Peirce 2009) (Peirce 2009)).

Peirce continues following the above selection to draw the distinction between the everyday commitment of belief and the scientific attitude, concluding finally that:

But a scientific man, who has any such immovable beliefs to which he regards himself as religiously bound to be loyal, cannot at the same time desire to have his beliefs altered. In other words he cannot wish to learn the truth.

This emphasis on the importance of embracing fallibility, skates well clear of the unworkable scepticism of either anti-realism or even Popper's gossamer thin realism, a point we will address in Part III. What we see here is Peirce identifying the dangers of motivated reasoning, well in advance of the broad consensus on this subject which would eventually emerge from the psychological

research. The neuroscientists we consulted above about the nature of scientific thinking identify this idea as a central concept (p. 238):

In addition, once all of the data have been collected, one needs to carefully examine the data (often performing statistical analyses), which then leads to the evaluation and coordination of these new data with pre-existing theory. This latter stage is often complicated by the presence of contradictory data (i.e., data inconsistent with theory). Here, the reasoner may need to revise and update existing theories or models in order to accommodate the new data (Dunbar & Fugelsang 2005; Fugelsang, Stein, Green, & Dunbar, 2004; Koslowski, 1996), or develop an entirely new theory altogether. These processes rely on a synergist interplay between domain-general reasoning skills (such as those alluded to above) and domain-specific content knowledge.

The necessity of maintaining the ability to "develop an entirely new theory altogether" was not lost on Kuhn as an essentially scientific process. What Peirce and the Neuroscientists are getting at here, is that this ability is essential for all scientific actors, even outside of scientific revolutions. The very process of progressing through a scientific education, involves the constant re-evaluation of previously held beliefs.

3.) Expertise

Harry Collins, the sociologist and expert on scientific expertise, whose work we have discussed already to some extent, is fond of observing in his seminars that the Third Wave of Science studies is an effort to account for the fact that we do not elect our experts with a lottery, from the pool of all adults. Collins' work, however, is still radically, and we believe, mistakenly, restrained by the social constructivism of the Second Wave of science studies he contributed to so substantially. Collins characterizes the philosophical basis of the Second, social constructivist Wave of Science Studies, specifically as an anti-realist approach which *sidelines* the world -- the universe, the real -- as the source of scientific knowledge (Collins 1981) (Collins 1981).

One school, however, inspired in particular by Wittgenstein and more lately by the phenomenologists and ethno-methodologists, embraces an explicit relativism in which the natural world has a small or non-existent role in the construction of scientific knowledge. Relativist or not, the new philosophy leaves room for historical and sociological analysis of the processes which lead to the acceptance, or otherwise, of new scientific knowledge.

We have already engaged this reference to Wittgenstein in an earlier section, and we will again in Part III, for now the reader should be aware that we disagree with this claim regarding Wittgenstein. However, the second sentence above refers to the methodological utility of social constructivism for the social study of science: if the relative "rightness" that any particular scientific theory may later be found to possess is written out of "historical and sociological analysis of the processes which lead to the acceptance, or otherwise, of new scientific knowledge", then the troubling bias toward actors in the

history of science who appear vindicated by subsequent research, can be minimized, revealing an arguably more objective perspective on the process of scientific discovery. As we shall argue in more detail in Part III, this is a very reasonable methodological approach to sociological work. However, when sociologists attempt, as Collins does, to export this idea as a global epistemic world view, it fails, like any other form of full-strength anti-realism, to pass the kind of pragmatic plausibility test Hume suggested for any extreme form of skepticism. Peirce specifically rejected such skepticism on similar but more pragmatically specific grounds (Peirce 1955) (Peirce 1955)(p. 228):

We cannot begin with complete doubt. We must begin with all the prejudices which we actually have when we enter upon the study of philosophy. These prejudices are not to be dispelled by a maxim, for they are things which it does not occur to us can be questioned. Hence this initial scepticism will be a mere self- deception, and not real doubt ; and no one who follows the Cartesian method will ever be satisfied until he has formally recovered all those beliefs which in form he has given up. It is, therefore, as useless a preliminary as going to the North Pole would be in order to get to Constantinople by coming down regularly upon a meridian. A person may, it is true, in the course of his studies, find reason to doubt what he began by believing; but in that case he doubts because he has a positive reason for it, and not on account of the Cartesian maxim. Let us not pretend to doubt in philosophy what we do not doubt in our hearts.

Peirce has no patience for the kind of skepticism which requires anti-realists to claim what we think he rightly identifies as "what we do not doubt in our hearts." More to the point however, the anti-realist position taken up by Collins and the Second Wave of Science Studies, destroys completely any plausible distinction between experts and non-experts. For Peirce, such a perspective ignores the basis of his realism: the power of logic and scientific behavior to produce pragmatically powerful knowledge.

The 2006 book, *The Philosophy of Expertise* (Selinger 2006) (Selinger 2006) includes a Collins and Evans paper which proposes that sociologists accept the problems inherent in the Second Wave of Science Studies are indeed unsustainable and must be addressed. They term the work on this problem the Third Wave of Science Studies. The introduction to *The Philosophy of Expertise*, written by editor Evan Selinger, characterizes Collins', and indeed sociology's, problem with anti-realism and expertise (p. 11).

Harry Collins and Robert Evans's "...argue that while their field has demystified expert authority, it has not yet addressed adequately the issue of expertise itself, therein making social studies of science research less socially and politically relevant than it could be. The authors know full well the importance of the issue, writing that "one of the most important contributions of sociology of scientific knowledge has been to make it much harder to make the claim: 'Trust scientists because they have special access to truth: Our question is: If it no longer is clear that scientists and technologists have special access to the truth, why should their advice be specially valued?' 'This, we think, is the pressing intellectual problem of the age.'" Collins and Evans note that the price that science and technology studies (STS) practitioners have paid for deconstructing expert authority is that their analyses fail to treat expertise as real and as being more substantive than the judgment of history and the play of competing social attributions. "By emphasizing the ways in which scientific knowledge is like other forms of knowledge,

sociologists have become uncertain about how to speak about what makes it different; in the same way they have become unable to distinguish between experts and non-experts.

Attacking Collins' proposed solution to this difficulty is the subject of Part III. For now, the reader should be advised that as near as we can tell, Collins' has no coherent solution. Collins appears to be wedded to the anti-realism at the heart of the Second Wave of Science Studies. He is unwilling to accept that full strength social constructivism, as a global epistemic position, cannot be reconciled with scientific authority.

Interestingly, *The Philosophy of Expertise*, beyond acknowledging Collins' terminal epistemic difficulties, also gathers together work of a few expert *philosophers* to weigh in on a subject that the editors suggest in the introduction has thus far garnished little focused philosophical attention -- particularly given the obvious centrality of the notion of expertise to our society. As our philosophical judgment of Collin's work in this regard will confirm in Part III, his presence in the only philosophical book we know of to focus specifically on the subject of the philosophy of expertise, is itself evidence of the continued life left in the conflict over anti-realism. We have already engaged the notion of expertise, when we referenced Philip Kitcher and his assertions about philosophical expertise and the potential for the community of philosophers to, like other communities of experts, issue public statements when necessary, to resolve conflicts which bear on philosophical analysis. It is with this in mind that we observe that in regard to anti-realism, it would not be at all controversial in either community, for us to observe that professional philosophical epistemologists are generally realists and that professional sociologists are generally anti-realists. The profound differences and ongoing conflict between these two professional communities begs the obvious question: who is right -- who are the real epistemic experts? As we shall see, anti-realist sociologists indeed find themselves in a very difficult position viv-a-vis anti-realism and expertise. While realist philosophers, who believe that the world, like logic itself, does exist independent of human action, have an important case to make as the appropriate experts here -- the case that that expert epistemologists who acknowledge the pragmatic utility of certain forms of knowledge like medicine, should claim the mantel of authority -- particularly in regard to areas of knowledge, like medicine, upon the successful satisfaction of which our pragmatic desires so profoundly depend, *because* they constitute the expert community whose work is specifically dedicated to epistemology. From a Pragmatist perspective, it may be reasonable to argue that sociologists have failed to consult the appropriate experts on epistemology -- or even that they are arguing outside their area of expertise.

Among the general observations, made in view of the work contributed to *The Philosophy of Expertise*, the editors observe something which we will be shortly devoting significant attention to as we engage the reflexive indictments of EBM in the context of the debate over medical pluralism. Specifically, the sociologists Cant and Sharma (Cant & Sharma 2004) and their claim that to privilege expert biomedical knowledge in the formation of public health policy is a fundamentally anti-

democratic provision of "insider" status to EBM epistemic authorities. This is a very unsettling claim, leveled as it is in a medical context, yet as Collins suggests in the above passage, sociology has apparently accepted the claim and pose the question which makes the ultimate destination of their anti-realism so problematic: "If it no longer is clear that scientists and technologists have special access to the truth, why should their advice be specially valued?" The editors of *The Philosophy of Expertise* observe that the question of the appropriate role for expert authority in a democracy is vexing.

The deepest and most controversial issue surrounding experts and expertise, however, concerns the nature and exercise of political rationality itself. The fundamental tension can be posed most simply as follows: Democracy depends not only on an educated citizenry, but also on educated decision making in the myriad judgments that have to be made in the day-to-day operations of government. The United States and other countries attempt to accomplish this in a practical way by incorporating experts into governmental operations through their participation in various agencies regulatory and review panels, committees, and advisory capacities. Yet the authority so conferred on experts seems to collide with the democratic and antielitist urge to accord equality to all opinions: it also risks elitism, ideology, and partisanship sneaking in under the guise of value-neutral expertise. Indeed, charges that this has in fact happened in how the United States has handled its science policy have become more direct and specific than ever before. Can this fundamental (elision ever be resolved? If it cannot be resolved, how can it be best approached? The answers to these questions are central to the theory and practice of democracy.

Some of this confusion here may derive from the assumption that all participants in a democracy are rational, an unfortunately problematic default position which is not supported by sociological or psychological research. In any case, the idea that medical pluralism is a matter of democracy, of consumer choice and of the basic right to the healthcare of one's choice is central to sociological critiques of EBM. Although we plan to attack such arguments by sociologists philosophically and we plan to make our own specific proposals for the how medical experts and advocates should be treated differentially as a matter of public health policy, in Part III and IV respectively, it is worth the digression prior to our return to Peirce's ideas about expertise and our impending engagement with the sociological work on medical pluralism, to ask the reader to keep in mind that: 1.) there is little but still *some* attention being paid specifically to the issue of expertise by philosophers; 2.) that in the sense that Philip Kitcher might suggest, that philosophy and expert philosophers are likely *the* essential experts to weigh in on such things; and finally, 3.) that the problems inherent in sociology's effort to export anti-realist ideas as a global epistemology, certainly in the context of medical epistemology, demand the immediate deployment of significant philosophical expertise in the real world in regard to these issues -- with the medical pluralism debate between sociology and EBM we are about to engage, providing a case in point.

At the conclusion of their introduction, the editor of *The Philosophy of Expertise* summarize the general implications of their contributors' work as follows:

However, this volume will contribute to the construction of that framework because, by virtue of its range of critical approaches and philosophical perspective it points out certain features that will be indispensable to that framework. The essays in the first section show that expertise is not a simple property or relation, but rather a multilayered set of interactions between individuals and institutions whose complexity will have to be respected in any adequate account. The essays in the second section reveal the need for a phenomenology that can detail how expertise is acquired and maintained and that is hermeneutically sensitive—that is, that takes into account the cultural and situational embeddedness of experts. The essays in the third section highlight the need to be aware of how experts and those whom they address are socially and culturally positioned with respect to each other; this in turn will help clarify the conditions under which outsiders might question or challenge experts with respect to their judgments.

Essentially, expertise involves "interactions between individuals and institutions". Expertise can best be understood in the context of specific expert communities, and the connections both between experts and between experts and non-experts are essential to the understanding of both the development and the utilization of expertise. At this point, prior to getting more specific about these issues in Parts III and IV, we believe it can generally be seen that the harmony with which the above synthesis of current philosophical thinking on expertise maps onto Peirce's characterization of scientific practice. As we progress through the thesis, this alignment should become increasingly clear as another powerful warrant for accepting Peirce's Pragmatist epistemology as valid for our purposes. Peirce sees our access to the real as an outgrowth of precisely the same ideal communal actions implied by the passage above, essentially social conventions which make the transmission of knowledge adequate to achieve "complete information" throughout the larger community.

Finally, as what anything really is, is what it may finally come to be known to be in the ideal state of complete information, so that reality depends on the ultimate decision of the community; so thought is what it is, only by virtue of its addressing a future thought which is in its value as thought identical with it, though more developed. In this way, the existence of thought now, depends on what is to be hereafter; so that it has only a potential existence, dependent on the future thought of the community (p. 250).

The whole process, from the identification of experts, by other public health policy experts, who were, in turn, appointed and tasked by government officials who are elected by the public -- all of this, is what Peirce is referring to here by "the community".

Generally speaking, the philosophy of expertise, does not appear to diverge substantially from Peirce and his description of scientific practice. Peirce however, also understood that the conventions and values required to best produce and utilize knowledge do not stop at the boundaries of expert communities. Some of the same sorts of behaviours required for the communal functioning of experts communities to produce knowledge are also required for them to share it. Thus "professionalism", a concept related to expertise, informs the behaviour and values that many groups of experts use to communicate knowledge to each other and the public -- to all actors -- all along the chain of implementation of medical knowledge.

4.) Professionalism

There is no doubt that in Peirce's mind, science, and the progression toward the real is facilitated by certain beliefs held by participants in the scientific communities. The values, beliefs and behaviours of what we have subsequently come to call "expertise" and "professionalism" generously overlap. However, there are additional considerations related to the commercial operation of certain "professions" including medicine which carry ethical prescriptions for how experts should interact with the public directly as clients, or as patients, as the case may be. An exploration of the philosophy of professionalism as well as both pre-and post anti-realist sociology of professionalism will prove useful to us for four reasons: 1.) an examination of the philosophy of professionalism will again demonstrate that the contemporary philosophical understanding of what "professionalism" means for scientists is generally in line with Peirce's vision of what science is and how it works; 2.) we will again see that older, realist sociological conceptions of professionalism are in line with both Peirce and contemporary philosophy, while post-modern sociology, after the turn toward anti-realism, commits itself to notions regarding professionalism which are incompatible with both Peirce and contemporary philosophy; 3.) we will also observe that in the case of medicine, the wider conceptual components of professionalism necessarily include "expertise", but they also include the motivation, associated values and behaviours directed to *help the public* rather than harm the public and to protect the public from harms originating from outside the profession -- from those intending to *exploit* the public trust in the profession, and those who may *mistakenly* believe that they are *entitled* to the benefits of claiming to be a member of that profession (when in reality, they are not) -- thus motivating the kind of "exclusionary market regulation" characteristic of "professions" like medicine and law; and finally 4.) we can propose that when all of these ideas are taken into consideration, CAM in the West, which has become both a huge player in the medical marketplace and such a vexed question for medical authorities, can be understood as --- or essentially reduced to -- an artefact of various failures of professionalism.

Generally, professionalism implies expertise first and foremost. By the very nature of expertise, those of us without it are often compelled to seek to obtain the services of those who have it, in furtherance of whatever pragmatic goal we may have in mind. In some cases, as in the hiring of a portrait painter, or a plasterer, it may become immediately clear as to whether the person we are considering for the job is a professional or not just by looking at her work. A little further along the continuum we have in mind, if we hire an engineering and construction company to install a bridge, they may show us some beautiful bridges they have built, but it may be a matter of asking about the track record of their bridges when confronted by strong winds or floods, to see if the bridges were actually built to a professional standard, and even then, we would need another expert to tell us if the bridge was overbuilt, or inefficiently built, or involved some other failure of professionalism which would fail to manifest itself to a non-expert. There are some professions which are affected by the fact

that it is often extremely difficult for the consumer to assess the *professional quality* of a potential professional actor, before, during or even after the provision of the service. As Andrew Edgar put it in his 2011 paper “Professional values, aesthetic values, and the ends of trade” (Edgar 2011):

...the historical struggle to distinguish professions from trades turns about a theorisation of the ends served by professions, in contrast to those served by trades. While the ends or purposes of trades are self-evident, and the success or failure of the trade- worker may be assessed, with some accuracy, by a lay observer, the ends of the professions are opaque, and the success or failure of a professional may not be self-evident.

Edgar is drawing the distinction between trades and professions. Given that the practical goals of medicine are essentially concrete (I'm sick, make me well), the issue of *what the medical consumer knows* or perhaps *the epistemic status of the patient's understanding* is absolutely central to this thesis. Just as we are inclined to question the convention of assuming the rationality of political actors, and just as we will characterize clinical research design methodology as a means of overcoming the inherent perceptual limitations and bias of observers, we recognise that in many if not most cases, it is simply not possible for consumers to evaluate the efficacy of an individual medical treatment when it is provided by a doctor, and much less possible for the average lay person to evaluate the professional status of the doctor in the larger sense. The patient finds herself at the mercy of two qualities: expertise and altruism. It is through this inevitable vulnerability of the patient that professionalism generally, and medical professionalism specifically, came to acquire not only the value of competence -- of expertise -- but necessarily the value of altruism as well. As we have observed, Peirce sees logic and the practice of science as essentially pro-social activities. The vulnerability of a medical patient and the professional obligation of the doctor, both in terms of expertise and altruism, are entirely consistent with both Peirce's epistemology and our pragmatic medical epistemic project.

Interestingly, mid 20th century sociologists shared our concern for the epistemic vulnerabilities of patients and similarly saw medical professionalism as an extension of this very concern. Talcott Parsons, on page 300 of his 1951 book, *The Social System*, describes the motivation for altruistic medical professionalism, as an outgrowth of precisely this epistemic vulnerability (Parsons 2013):

...the combination of helplessness, lack of technical competence, and emotional disturbance make him a peculiarly vulnerable object for exploitation. It may be said that the exploitation of the helpless sick is “unthinkable.” That happens to be a very strong sentiment in our society, but for the sociologist the existence of this sentiment or that of other mechanisms for the prevention of exploitation must not be taken for granted. There is in fact a very real problem of how in such a situation, the very possible exploitation is at least minimized. The other general point is the related one that the situation of the patient is such as to make a high level of rationality of judgment peculiarly difficult, he is therefore open to, and peculiarly liable to, a whole series of irr- and non-rational beliefs and practices. The world over the rational approach to health through applied science is, as we have noted, the exception rather than the rule, and in our

society there is, even today, a very large volume of “superstition” and other non- or irrational beliefs and practices in the health field.

This passage illustrates a number of things: 1.) it reiterates what we observed previously, that non-science, or even irrationality is the default setting for the way human beings understand their health -- that desperate medical patients cannot be relied on to be rational and that irrational medical beliefs are widespread; 2.) as a result, medical professionalism in particular is uniquely compelled to protect patients from exploitation; 3.) Parsons, at this point in history, is not reflectively suspicious of the motives of scientific medicine (and he is likely representative of his field as a whole, during that time in this regard. Sociology mid-century, was in perfect alignment with realism, and the ethical implications of Peirce and our universal medical epistemic model).

Parsons is arguably the most persuasive voice appearing in the thesis in regard to the dim view of the epistemic status of medical patient beliefs and their resulting vulnerability. We will refer back to precisely these observations by Parson's when we encounter laissez-faire arguments in the context of medical consumer regulation as well as when we consider the meaning of patient's rights and medical choice arguments. We would ask the reader to keep Parson's ideas in mind, as we will frequently encounter arguments which leverage the assumed rationality of patients when considering their agency.

Parsons, in *The Social System*, lists out-right the virtues which are ultimately entrained by the notion of medical professionalism arising out of the vulnerability of the patient (p. 305): "...achievement, universalism, functional specificity, affective neutrality and collectivity-orientation." There can be no doubt that this is rooted in the understanding of the ultimately pragmatic utility of medicine -- to make sick people well. Parsons is speaking of scientific "achievement" here, and he understands, like Peirce that the outcome for the patient, hangs on the pragmatic power of medical knowledge. Parsons here specifically lists another of the core themes of the thesis and a goal for the medical epistemic system we propose: universality. As we have seen, anthropological critiques of EBM in culturally different contexts are often painted with notions like "epistemic imperialism" or "medical colonialism". We reject these ideas for the same reason Parsons and Peirce do: there is only one kind of pragmatic knowledge: the kind that works.

As we have seen, Parson's notion of "collectivity orientation" is a major theme in Peirce's epistemology as well, and it could be argued that for both Peirce and Parsons, it is the very pragmatic gravity of the needs of the desperate and vulnerable patient, which infuse the entire biomedical process from the point of discovery to hands-on implementation with a connection between ethical and epistemic goals which are too intimate -- too interlocked -- to separate. It is tempting to begin to draw the contrast between EBM and CAM at this point, in particular in regard to the more entrepreneurial nature of CAM's role in the medical marketplace. We will however, save this analysis for later in this section when we look at the sociological accounts of CAM directly. We would

however, ask the reader to keep in mind the intimate connection between ethics generally, professional ethics, medical ethics and the pragmatic commitment to the utilization of true beliefs, and the implications this has for the sale of CAM. We need to make one last point in regard to Parsons and his "collectivity orientation." The sociology literature on medical pluralism is marked, as we have mentioned, by a reflexive, assumption that EBM is driven by a capitalist "self-interest", which, we think questionably, CAM is painted as a reaction against. It should be noted that Parsons was at pains to point out that he saw medicine as very distinctly *not capitalist* in its motivations in this regard, that medicine very particularly "deviates" from the pattern of capitalist motivation, by virtue of its altruism (p. 311).

This brings us to the last pattern element, collectivity-orientation. It is this which is distinctive of professional roles within the upper reaches of our occupational system, especially in the contrast with business. Indeed one of the author's principal motivations in embarking on a study of the medical profession lay in the desire to understand a high-level occupational role which deviated from that of the businessman who, according to certain theorists, represented the one strategically crucial type of such role in modern capitalistic" society.

We agree with Parsons and believe that EBM is currently essentially altruistic in ways that make it different from business. Of course, the corrupting influences of markets and money are frequently visible, stretching across and through the medical universe in various ways, from the effects of Big Pharma on the quality of clinical trials and drug advertising through to the rapidly changing standards of plastic surgery. Still, we believe that altruism can be sustained as a professional medical value, or virtue as the case may be, and that a fundamental characteristic of EBM's professionalism is that it encourages this altruism. This is something we plan to retain not only as a characteristic of our proposed universal medical epistemic model, but also as one of its most powerful warrants.

By 1970, the sociologist Eliot Freidson in his introduction to *Profession of Medicine: A Study of the Sociology of Applied Knowledge* (Freidson 1988) suggests that one of the problems he will address in his book, is the "sociology of knowledge" (p. xvi), citing Peter L. Berger and Thomas Luckmann's 1966 book, *The Social Construction of Reality* (Berger & Luckmann 1991) (Berger & Luckmann 1991). Freidson, however, retains an essentially realist attitude, laid out early in his book (p. 12).

I believe that the empirically demonstrable outcome of medical work is important to its development as a consulting profession. Analytically, this is a question of the cultural acceptability of the practices of a special occupational group to a receiving public. While we need not believe that men are thoroughly rational pragmatists, uninfluenced by wishful thinking or by a priori assumptions about the nature of the commonsensical world, or, in the case of medicine, immune to the influence of faith and hope on bodily processes, there is nonetheless a massive collection of evidence that rational and pragmatic material advantage plays an important even if not exclusive role in public acceptance.

Even though Freidson is still certain that the pragmatic nature of medical knowledge cannot be reduced entirely to a social construction in 1970, sociology as a discipline was about to become much more sceptical of this position.

By the turn of the century, some sociological authors writing about medical pluralism would propose theories which appear to relinquish all of the epistemic authority which the pragmatic power of biomedical medical knowledge might have been thought to hold universally, across disciplines. By 1999, Cant and Sharma, the two sociologists writing about medical pluralism we have previously mentioned, would pose the following, apparently global, question at the end of their first chapter on page 20:

Does increased usage of alternative medicines represent a destabilization of biomedicine and science as forms of knowledge? If so, should we regard the resurgence of non-biomedical forms of healing as an instance of postmodern rejection of universalism, a fragmentation of the authority of the medical meta-narrative?

Here, it is proposed that the universalism at the heart of medical professionalism may be in the process of being rejected, that scientific knowledge as the basis of expertise and professionalism may no longer be valued, and that the rest of the medical meta-narrative, presumably including ethics and altruism is somehow "fragmenting". Cant and Sharma appear to be at pains to avoid giving the impression that they have an opinion on these matters. However, they certainly did not preface their work in the way Freidson did in 1970. Cant and Sharma appear to wish to convey that whatever is going on in the world of medical pluralism and EBM, they are not personally wedded to beliefs regarding the pragmatic power of EBM scientific medical epistemology.

Philosophers are not restrained, as Cant and Sharma appear to want us to believe, to observation alone. Philosophy, in its discourse about medical professionalism, remains much closer to what EBM itself, in the form of its leading organizations, tells us about its own understanding of its own professionalism (an articulation which we will shortly explore). For now, with regard to the philosophy of medical professionalism, it would not be an exaggeration to suggest that medical philosophy, at least in terms of its writing on medical professionalism, is *continuous* with EBM itself. We believe that both EBM and medical philosophy would embrace the notion of philosophy as under labourer for, in this case, the medical sciences. That in the Peircian sense, doctors and philosophers see themselves as part of the same civic project to constantly improve medicine, to take seriously its legitimate criticisms and to move forward with the larger project to collectively take care of ourselves. Issues like the ideal expression of professional authority in a democracy, the sustainability of altruism, professional autonomy, and the reaction to a perceived reduction in medical professional legitimacy termed "new professionalism" -- which boils down to efforts either on the part of the doctor or the medical establishment to help medical professionals become "better" (Edgar, 2014). In any case, all of this argumentation takes place within an essentially realist framework. As the

previously mentioned philosopher Andrew Edgar summarized the latest issues relating to the philosophy of medical professionalism and the "new professionalism" in his book chapter "Professionalism in Health Care:"²¹

In summary, new professionalism has responded to the crisis in public trust by rethinking codes of conduct, and thus the image of what 'good doctoring' is, integrating such codes rigorously into education and validation, but more radically questioning the traditional value of autonomy. Professionalism thereby ceases to be a mere ethic, and comes to embrace the acceptance of regulation and co-operation with other professional disciplines; professional altruism is transformed into the acceptance that the patient lies at the centre of health care provision, and has a fundamental right to consistent, high quality care.

The philosophy of professionalism is firmly consistent with the Peircian notion of science as a pragmatic enterprise. Philosophers clearly have no issue with the pragmatic basis of medical authority and recognise medical expertise, along with a general sense of the desire to help, as the legitimate basis of medical professionalism. Sociology, on the other hand, has become much more diverse in this regard. While some, realist authors like Freidson continued to cleave to realism after three decades in regard to issues of professionalism (Freidson 2001), others, like Cant and Sharma, are willing to abandon -- or at least refuse to accept -- the assumption that the pragmatic utility of scientific knowledge is essential to good medical practice.

The Sociology of Medical Pluralism: the data

Introduction

Sociology, being the study of what people think and do collectively, must be an essential part of any serious effort to understand medical pluralism. However, as we have seen, the embrace of anti-realism by many sociologists has exploded the foundations upon which the rest of the academic world thinks about medicine. The basis of professionalism, both expertise and altruism, are "problematized" to the point that sociological writers, inventing a new perspective from which to claim scholarly objectivity, propose that the scientific authority and altruism which have been seen as the heart of medical professionalism for the last 150 years are no longer *real*. We grant that altruism, like all matters of ethics, is hardly uncontested, although we would argue that as a value, or a virtue, it is holding up well enough for our purposes here in medical ethics and epistemology. As for sociological writers engaging medical pluralism, their affected air of enlightened objectivity and a stated, if dubious, committed refusal to "judge", is clearly not as prudently agnostic as sociological writers would like us to believe (Cant & Sharma 2004, p.54).

²¹ Edgar, A (2014) 'Professionalism in Health Care' Draft Manuscript.

...as sociologists, we must not judge the status of alternative medicine against this archetypal example of a profession [EBM]. Historically the sociology of medicine has tended to fall into this trap and view alternative groups in some way as inadequate or limited.

In their book, Cant and Sharma planned to explore CAM and EBM without acknowledging the pragmatic epistemic warrant for expertise or professionalism. When compared to the perspective taken by philosophers, which is largely continuous with biomedical realism and just as continuous with, quite literally, the rest of the fields weighing in on public health policy, this kind of anti-realist position is very radical indeed. It bears keeping in mind that of the two assumptions underlying the abandonment of professionalism, even if ethical scepticism is worth considering, the epistemic scepticism implied here is extremely radical. A large part of Part III will involve the philosophical examination of full-strength social constructivism: the verdict is that medicine is too universally pragmatically important for medical decision-makers to embrace such a high level of epistemic scepticism in regard to any pragmatically powerful medical belief. On one level, this finding puts sociology in precisely the position acknowledged by Collins, who, to paraphrase, suggests that sociology has problems exporting claims about what people should do in the real world.

This however, begs the question: how do we read the sociology on medical pluralism? Are they saying anything about the pragmatically real world or not? These questions are important to keep in mind as we read through their work. If we leverage Peirce and a philosophical interpretation of professionalism, a rather dim view of Cant and Sharma's work on the sociology of medical pluralism begins to emerge: advocacy. As Peirce was at pains to point out, science is the only bulwark against a-priori beliefs. Once scientific authority is removed from the equation, beliefs are no longer restrained by the world to the same extent. This allows for the embrace of beliefs which were previously not credible, and are dangerously pragmatically mistaken, including beliefs which are very popular with the public. In the CAM debate, academic "experts" including, legal, ethical, sociological and medical, who are willing to defend CAM, particularly in regard to CAM's disputed efficacy, are rewarded with everything from book sales to notoriety to participation in state public health policy working group discussions to grant funding²². By abandoning professionalism, as a concept, the only way left to "judge" the "legitimacy" of CAM, is by popularity. This brings us back to the shorthand term Peirce proposed for a-priori thinking: "fashion". We devote some time to pointing out that Cant and Sharma,

²² Sarah Cant's Principle Lecturer staff profile page on the Canterbury Christ Church University website, retrieved on September 10, 2014 from here: <http://www.canterbury.ac.uk/social-and-applied-sciences/psychology-politics-and-sociology/Staff/Profile.aspx?staff=82d9e5491432006f> contains the following text under the section "External Activities":

"My research in complementary and alternative medicine has resulted in conference presentations at many national and international conferences. During the House of Lords review of CAM, I contributed to working group discussions and I have acted as an Advisor to the Open University on a Wellcome Foundation research project and for their undergraduate degree."

the sociologists writing on medical pluralism that we will be looking at primarily²³, appear to frequently cite *popularity* itself as a form of epistemic warrant. Furthermore, Cant and Sharma, on the same page as the above quotation, propose a solution to the question we posed above: how do we read the sociology on medical pluralism (p. 54-55)?

...sociology does feed its findings and ideas back into the environment it studies -- what Giddens (1990) referred to as the "double hermeneutic", the potential of academic studies to not only describe social processes but in some part constitute them. Already our work has been used by one complementary group as evidence that they are involved in scientific research (see BMA 1993). Therefore, to describe alternative medical groups as in some way marginal, or to use other such value laden concepts, in turn defines the terrain of the health care market and can, as Coulter (1991) argues, serve to actually reinforce medical hegemony.

We believe there may be an important reason why Cant and Sharma accomplish all four of the following in this short passage: 1.) dismiss professionalism, 2.) suggest that to write about social movements is also to participate in or to "constitute" them; 3.) acknowledge that CAM advocates see Cant and Sharma's work as credible, academic "evidence", ironically, that CAM has embraced scientific principles; and finally 4.) suggest that to maintain current standards of medical professionalism amounts to the reinforcement of a very pernicious sounding "medical hegemony." It could be argued in this case that anti-realism provides the license for academics to serve as advocates for bad medical ideas, which, while legitimately opposed by medical authorities, happen to be popular -- "fashionable" -- with groups of advocates and the public.

As we suggested in our introduction, such matters could be interpreted as a significant breach of academic professionalism. Unlike Anthropology, which has the benefit of a dearth of research on Traditional medicine to rely on, when abstaining from judgement on the efficacy of Traditional medicine, as we shall see, this is not the case with sociology. There is a great deal of clinical evidence on the efficacy of Western CAM treatments. We will shortly be looking through the abundant evidence regarding CAM's efficacy. We will conclude, in short, that CAM is quite certainly clinically inert. Even the overly cautious, Popperian refusal of some medical authorities to completely rule out efficacy in the case of treatments which, by their nature, cannot be subjected to double blinded clinical trials, is as we shall be at pains to point out, mistaken. The reader should keep in mind that not a single alternative medical technique has proven to be dramatically effective beyond placebo. The closer the design of clinical trials on CAM techniques approach perfect blinding, the more the measured effects approach zero. Perfect studies always demonstrate zero effects. The marginal effects present in un-blinded clinical trials, on treatments like Chiropractic's efficacy for back pain, are fully accountable, solely, as placebo and expectancy effects. CAM does not work. All leading EBM

²³ We cite Cant and Sharma frequently in this section of the thesis although not exclusively. We first encountered their work as the main citation in the entry on medical pluralism in an encyclopaedia of medical sociology called *Key Concepts in Medical Sociology* (Gabe2004,p. 204). We feel Cant and Sharma's work is characteristic of the dominant mode of sociological thought on the subject of medical pluralism.

authorities, clinical researchers and meta-analysts are in perfect agreement about CAM's crisis of efficacy in regard to the vast bulk of alternative treatments, with niggling doubt entertained for only a tiny fraction of treatments still left in the shadows of methodological limitations.

Is it reasonable to ask if there is a point at which evidence presented by experts for a pragmatic claim becomes so undeniable, even if that evidence is not adequately understood by the public, that even anti-realist sociologists are under a professional obligation to take the implications seriously? For that matter, as Parsons observed, medical professionalism came into existence due to the unique vulnerability of medical patients and the public to the threat of their own irrationality. To abandon completely the possibility that CAM is partially, or even wholly, some combination of market motivated negligence and fraud certainly seems like a heavy, if not unjustifiable risk -- especially for a field as historically concerned with exploitation as sociology. The power of different medical institutions is a common theme in medical sociological analysis. By what means are we to distinguish the legitimate from the illegitimate exercise of power if medical evidence, as a means of adjudicating legitimacy, is denied any legitimacy?

It is worth considering the extent to which the research on CAM's efficacy has evolved since Cant and Sharma published. In their defence, as we shall see, it is safe to say that literally *all* of the credible research outcomes involving CAM in the last decade have been negative, and there has been a lot of it. Perhaps we are unfairly judging Cant and Sharma's position in hindsight here. Alternatively, perhaps sociologists have made a grave mistake and will soon be seeking to correct it. As one of the text books we consulted, *Key Concepts in Medical Sociology*, suggested about the medical pluralism literature just five years after Cant and Sharma published their book on medical pluralism (Gabe 2004) (Gabe 2004)(p. 206):

There has been a tendency to adopt a non-reflective and non-critical stance towards alternative and indigenous healing systems and more attention must be given to the efficacy, evaluation and role of alternative medicine in the overall provision of health care.

It could also be, as Kitcher might suggest, that there is a demarcation at play here between philosophical and sociological expertise. Perhaps medical epistemology -- the interpretation of the constellation of current clinical research, analysis of the pronouncements of medical authorities and work beyond the awkward and mistaken limitations of conventional scientific discourse in regard to inference and metaphysics -- is *more* philosophical than sociological work. It is possible that there is under-labouring to be done here: that philosophers may be the appropriate experts in this case to suggest a range of potential epistemic solutions to this confusion, thereby removing an obstacle to the synergistic work between disciplines, which is the larger pragmatic goal of public health.

For now, the reader should be aware that as we work through sociology's exploration of medical pluralism, we will of course seek to understand what we can about CAM and EBM, practitioners and patients, what they believe and what they do. However, we will also consider what

writers like Cant and Sharma and the rest of the sociological experts we cite are saying about these things, what their input means, and how what they have to say might ideally be reconciled to the realism and the professional expectations of the rest of the constellation of expert disciplines relevant to public health policy. We will be keeping our potential under labourer role in mind, while simultaneously noting ideas and observations which we will later bring to bear in a proposal for resolving some of the problems posed by medical sociological anti-realism in an interdisciplinary context.

How does Sociology understand medical pluralism?

In the textbook, *Key Concepts in Medical Sociology* (Gabe 2004) (Gabe 2004), there is a chapter on Medical Pluralism (p. 183). The authors define medical pluralism: "Medical pluralism refers to the co-existence in a society of differing medical traditions, grounded in different principles or based on different world-views." The authors credit anthropologists with the first work on medical pluralism and keenly aware of the agnostic epistemic attitude adopted by anthropology, the authors begin the chapter, firmly rooting themselves in democratic values:

Pluralism is particularly associated with 'empirical democratic theory', which was popular from the 1950s to the 1970s, but has become a key concept for social scientists who wish to account for the existence of multiple selves, identities, sub-cultures and social relations (McLennan, 1995: 6).

There is no epistemic judgement implied between medical systems anywhere in the chapter, until near the end, when as we mentioned previously, the authors anomalously warn of "...the tendency to adopt a non-reflective and non-critical stance towards alternative and indigenous healing systems more attention must be given to the efficacy,..." (p.187). The authors do not appear to acknowledge the extremely vexing questions where democracy and efficacy collide.

The authors of *Key Concepts in Medical Sociology* acknowledge that CAM practices like Traditional Chinese Medicine (TCM) and chiropractic have become more of an issue in the medical marketplace recently, however, we believe they underplay CAM's surge in popularity. Another author perhaps better characterized the deep roots of and recent increase in popularity of "folk medicine" and CAM in the West, as merely a resurgence after a brief period of "eclipse" by scientific medicine (Bakx 1991). Cant and Sharma suggested that the current time period's movement to embrace non-scientific medicine should be termed "the new medical pluralism" (thus the title of their book), in order to distinguish it from the uniform embrace of non-scientific medicine from antiquity through the period of the slow development of EBM.

The authors of *Key Concepts in Medical Sociology* tell us that the principle strength in the sociological medical literature regarding medical pluralism is:

...a wealth of literature that documents the socio- demographic characteristics of users of alternative medicine, the attractiveness of alternative medicine to users, the professionalizing project of alternative medicines and the ways in which different healing systems collaborate (see Cant and Sharma, 1999).

As we mentioned, this is the textbook entry where we discovered Cant and Sharma's work. It will be largely to them and their sociology of "alternative medicine" or "complementary and alternative medicine" (CAM) as it is also known, that we now turn.

How does Sociology understand CAM?

Cant and Sharma define their terms at the outset of their book *A New Medical Pluralism* (p. 5):

In this book we use the term "alternative medicine" to refer to forms of healing that depend on knowledge bases distinct from that of biomedicine and which, as such, do not share the special legitimation that the state has conferred upon biomedicine. Yet, non-biomedical modes of healing found in western countries are highly diverse and it can be argued that they should no longer be treated as a single category (Wardwell 1994: 1062). Certainly, it is more proper to write of alternative medicines rather than alternative medicine.

Cant and Sharma go on to draw distinctions between Western "folk" medicines and older non-scientific medical notions like homeopathy and Western medical herbalism, and newer, 19th century non-scientific systems like chiropractic and osteopathy. They suggest that there is also a distinction between Traditional Medical ideas transplanted into the West, by Westerners, like acupuncture, and Traditional Medical systems brought into the West by immigrant groups, like Traditional Chinese Medicine, Ayurveda and other forms of "spiritual healing".

Cant and Sharma observe that "alternative medicine" as a category is complicated by various factors including; 1.) the term came into existence after biomedicine became dominant and only then as a term including many medicines which "never really went away", so understanding the scale of the current resurgence is not entirely clear (p. 6); 2.) some doctors use alternative medical techniques, which, to Cant and Sharma, calls into question what exactly lies "inside and "outside" of biomedicine; 3.) the term "alternative medicine" implies "subordination" to biomedicine (p. 8); 4.) it is a "...fluctuating and heterogeneous category, incapable of being sharply and universally demarcated from other forms of healing,..."; 5.) It is a negative and formal category "...that which is not medicine..." (p.9). Finally Cant and Sharma assume that most people understand what they mean by

"alternative medicine" and they go on to suggest that by embracing this term they embrace none of the judgements that readers might be attempted to assume are connected with it: either in "degree of self sufficiency" or relationship to biomedicine in the present or the future.

In review, and in the interest of understanding the philosophically relevant claims applied to Cant and Sharma's definition, it should be noted that by our pragmatic realist lights, a demarcation between scientific and non-scientific medicine is not encumbered by nearly the same number of difficulties of more global attempts to demarcate science, nor is our ability to demarcate between scientific and non-scientific medicine affected by any of Cant and Sharma's litany of concerns. We will later argue confidently that the pragmatic, universal, naturalistic and professional elements of medical science are enough to clearly cleave CAM from biomedicine, with no controversy whatsoever among EBM experts in terms of well over 99%, if not all of what is now commonly referred to as CAM. The problems Cant and Sharma imagine in regard to distinguishing between CAM and EBM are more closely connected to their commitment to *avoid* epistemic judgement, than they are to an actual lack of basis for such judgements.

Cant and Sharma suggest that in general terms, CAM therapies are marked by radically "...different ideas about disease causation" (p. 58). Cant and Sharma seem to downplay the issue of the ontological commitments of CAM therapies, for example: chiropractic's "subluxations" or TCM's "qi". Importantly, Cant and Sharma suggest that CAM has evolved somewhat since the '70s, suggesting that when specific CAM modalities first began or resurged, they were practiced by "charismatic" non-medical practitioners. They describe "a grassroots, sporadic and disorganized movement" (p. 78.). Cant and Sharma suggest that chiropractic, osteopathy, acupuncture and homeopathy all went through "charismatic phases" (p. 56): "proponents were treated as leaders and had extraordinary qualities which inspired enthusiasm in their followers." Teaching the techniques was an "unstructured" "cottage industry" (p. 78), occurring in teachers front rooms, involving no written curriculum but catering to "...dedicated followers who came from all walks of life and did not require specific credentials beyond enthusiasm." In some cases there is a heavy emphasis on spirituality and in many cases EBM was demonized as harmful. Many, if not most CAM modalities were introduced initially as panaceas which had unlocked the mechanism of all known disease (chiropractic, homeopathy, osteopathy and TCM are all members of this subset). Cant and Sharma devote considerable time to the discussing the way these panacea movements and their claims to efficacy are "moderated" to fit into the medical marketplace more gracefully. We will show that Cant and Sharma were overly optimistic in their analysis, at least in terms of their assessment of chiropractic, which they suggested, wrongly, had abandoned its most radical claims of efficacy.

Cant and Sharma suggest that one of the principle dynamics in play between CAM and EBM is reflected in CAM's transition from panaceas marketed by non-medical charismatic leaders, to large movements, necessarily characterized by a host of traits copied from EBM: "professional" organizations, the accreditation of training institutions, statutory regulatory status, etc. Individual

practitioners are sometimes quite resistant to efforts to professionalize their modality. We see this interplay between anti-professionalism and professionalism at play in the expression of CAM modalities as the key to understanding CAM. The dynamic plays itself out like this: 1.) CAM emerges from a profoundly anti-professional, non-scientific place, in the form charismatic leaders who seize upon mistaken ideas in isolation, rather than in the context of a scientific community and its associated expertise; 2.) These ideas *become* CAM, because by their suspect or clearly mistaken nature, such ideas are rejected by scientific communities; 3.) these mistaken ideas are then passed through social networks and are taken on by the credulous -- people who are engaged in everyday forms of the a-priori mode of fixing belief, and by a failure of professionalism, some of the people who take these ideas on, include medical professionals, government actors and academics; 3.) as the movement grows in size, and the modality becomes popular, or "fashionable" in the Peircian sense, a structure of organizations begins to arise, seeking to promote the modality as a "profession", taking dues, selling degree programs to students, negotiating with the state, while simultaneously protecting the suspect ontological commitments and questionable beliefs from attack. There is some evidence that over the last decade this process reached a kind of status in terms of CAM's uptake of consumers. A 2012 study billing itself as "...the most comprehensive and systematic review to date of surveys reporting the prevalence of CAM use by the general public...", generated the following finding(Harris et al. 2012) (Harris et al. 2012):

UK surveys also suggested that CAM use has remained fairly constant since 1998 (40) with about one in four adults using CAM and one in eight consulting a CAM practitioner during 2005 (42).

As such, CAM can be understood to be an enormous but relatively stable part of the UK healthcare marketplace. In the UK, this stability has been significantly disrupted by the work of anti-CAM activists, causing watershed events in the UK regarding CAM, which we will explore in detail. The narrative of these events highlights in the tension between professionalism and the pseudo-professionalism characteristic of CAM in the UK, and will provide source of understanding of medical pluralism.

How does Sociology understand Holism, Narrative Medicine and the issue of time spent with patients in regard to CAM?

Holism is a concept which has become nearly synonymous with CAM. Cant and Sharma suggest that the term "holistic healing" in the United States, is sometimes used to refer to CAM (p.8). Holism involves "...taking into account the whole person and context of the patient." Cant and Sharma observe that not only is holism something that EBM aspires to, but also that some CAM modalities

are "more holistic than others" although most alternatives therapies apply this term to themselves, it is not always understood in the same way.

We believe holism can best be understood as an element of narrative medicine, a term that Cant and Sharma do not appear to use, but one which they essentially address in their discussion of the "why me" question which is often an important matter to patients facing ominous medical circumstances. Cant and Sharma suggest that there is a great deal of research indicating that when a patient with an ominous diagnosis asks the question "why *they* have become ill...that biomedical explanations about the causes of illness are never sufficient", thus necessitating the use of a broad range of "socially and culturally available categories...from personal and family stories", and generally whatever the patient can use to reinterpret their lives in the wake of extremely bad, or at least life altering news (p. 23). Cant and Sharma then pose a question: "is it pertinent to ask if alternative medicine draws more explicitly upon such repertoires than biomedicine?" As we shall see, EMB takes narrative medicine very seriously. For our part now, we would like to observe that neither Cant and Sharma, nor any other sociological source that we are aware of has demonstrated that CAM therapists have some sort of edge regarding narrative medicine. Consistent with our pragmatic realist medical epistemology, narrative medicine is a matter of study: a matter of determining what kinds of beliefs and meanings are helpful to patients and what kind are harmful; what kinds of grieving or adjustment activities or therapies facilitate these beliefs; and finally what kind of therapeutic or professional relationship is required to accomplish these things and what kind of time does that take. These are not mysterious phenomena. EBM is already working to understand them. We are not convinced CAM is in a better position to develop or provide care based on a professional, expert understanding of narrative medicine than EBM. We wonder what the basis for such a claim might be.

To break down patient/practitioner interaction a little bit, Cant and Sharma talk about four relevant dynamics: 1.) the length of the appointment (how long the practitioner spends with the patient); 2.) the personal relationship between patient and practitioner in terms of friendship; 3.) the personal relationship between patient and practitioner in terms of perceived status or power; 4.) and the presence of religious or spiritual dimensions to the relationship. More time with doctors rather than less is, not surprisingly, valued by most patients, and is an element of the research on private healthcare in the UK, although it is the speed of access to a consultant, rather than the length of time with her, which is the most desired (p 26). It is also interesting to note that some chiropractors make more money than doctors by reducing the time they spend with patients to 11 minutes, even less time than doctors tend to spend with patients (p. 75). Still Cant and Sharma claim that: "All studies have revealed that consumers are drawn to consultations where they have more opportunity to discuss their problem in depth."

The issues of power and relationship are relevant as well and form part of a complex picture of why people visit CAM therapists. Although Cant and Sharma cite evidence suggesting that some homeopathy patients are motivated by "positive results", this serves to tell us more about the failure of

human perception, as we shall see, Homeopathy is one of the least problematic forms of CAM to study and as such has been abundantly proven to be clinically inert. In summary, Cant and Sharma note that patient satisfaction may not be wedded completely to matters of clinical efficacy but rather to the extent that "consumers feel that their experiences are taken seriously and given due attention and time" (p. 39). We would merely observe that EBM is capable of this up to a point -- the point at which irrationality or unlikely ontological commitments are required to take patient experiences "seriously".

Finally, central to the practitioner/patient relationship in Cant and Sharma's minds is the issue of power and epistemic inequality. They suggest that as the bedside medicine of the 18th and 19th centuries was replaced by hospital medicine, patients were no longer working with epistemic equals: the bedside doctors who were pedalling specific, straightforward and commonly understood medical services. Then as medicine evolved to laboratory medicine, it became "more scientific", as "patient's views and explanations of their disease, and care became distinctly doctor centred". Epistemically, this seems to imply a growing gap between what patients and doctors know about medicine, which is the definition of expertise and fits perfectly with a pragmatic scientific narrative. Cant and Sharma then pose another question (p. 23):

To what extent can the increased popularity of alternative medicine be seen to be a challenge to this balance of power, especially as alternative practitioners claim to put the patient centre stage (Johannessen, 1996)?

The "balance of power" here seems clear enough. If the "balance of power" is a matter of epistemic inequality, then the better the expert doctor is, assuming the knowledge of patients stays the same, the greater the epistemic gap and apparently, the greater the imbalance of "power". Strictly speaking, most of us want the best doctor we can get, thus exposing ourselves to the greatest possible imbalance in this sense. It is not clear why this is inherently bad to Cant and Sharma. It seems to follow that every time we find a better doctor, the worse off we are in terms of Cant and Sharma's "balance of power". In this sense, Cant and Sharma appear to be suggesting that what may be attracting consumers to CAM, is that CAM practitioners do not know as much as EBM doctors. Even if we are missing something more subtle in terms of "power relation" here, it is particularly unclear as to what it means that CAM therapists "put the patient centre stage". We would assume that the patient's location on the stage somehow relates to what patients know, what doctors know and what CAM practitioners know in terms of pragmatic medical knowledge. It seems likely that Cant and Sharma are after something else here, perhaps the social authority, the social power imbalance between doctor and patient may allow for some doctors in combination with some patients to cause patients to feel "powerless". Still, the reporting of this "powerlessness" in the sociological data by CAM users may well be connected with erroneous CAM beliefs. Although Cant and Sharma are unwilling to consider the status of the "empowerment" offered homoeopaths might be, we are not: a person who believes

a homeopathy treatment will help them with their illness is mistaken. The moral status of actors providing this form of false hope is not addressed by Cant and Sharma. We will address this in Part IV.

Questions about the difference in the nature of the epistemic competences of practitioners and patients are at the heart of the discussion above, and we will dedicate more analysis to this important topic. Interestingly, as we shall see, Cant and Sharma also cite sociological research bearing a contradictory empirical claim to the one above: that currently, medical patients have *more* medical knowledge now, are *more* knowledgeable about their bodies than people used to be, and that this has eroded respect for EBM doctors and is driving patients to CAM practitioners. The research data itself appears to be in some ways contradictory. We should acknowledge that there are some legitimate "public understanding of science" gains which have been made in the area of undesirable side effects of medicine. However we would also observe that this kind of self-criticism is characteristic of EBM, and not CAM, which shows no similar impulse. In any case, the issue of what all the actors relevant to medical pluralism *know* about medicine -- their medical beliefs -- is central to the questions Cant and Sharma are putting forward, and equally central to this thesis. Unlike Cant and Sharma however, we are clear on the fact that medical beliefs exist in relation to medical reality in ways which allow us to make important judgements about the nature of those specific medical beliefs in terms of their impact on specific medical goals: are they pragmatically powerful; are they mistaken; are they pernicious? Cant and Sharma have vowed, as we have noted previously, "*as sociologists, we must not judge the status of alternative medicine against.*" EBM. This does not stop them from asking question as to the relative level of medical knowledgeability of doctors and patients and the way that this relates to power, but it does make it harder to understand exactly what Cant and Sharma would propose is anchoring such judgements or what it means we should try to do about it.

More about Power, Anti-science Sentiment, Postmodernism and Marxism

Cant and Sharma suggest that "Where sociologists are concerned, characteristics of medical hegemony have emerged from at least three strands of research" (p. 11). The first strand is historical work, which acknowledges the pragmatic power of scientific medicine, and "describes the rise of a medical profession privileged by the state." They suggest that Foucault's work in this area permits the consideration of medical power in a broader context as "biopower" including things as diverse as state surveillance and "an emanation of governmentality." The second strand is "micro-studies" of communication within clinics where doctors "control talk and interaction" in ways in which "'the voice of medicine'...marginalizes...the 'voice of the lifeworld'." In the third strand addresses the expanding set of aspects of life which are now "medicalized": "...infertility, alcoholism, mental disturbance, infant care,...school refusal...general unhappiness..." all of which are now areas in which

we may find assistance in the form of medical experts as opposed to priests or kin. Cant and Sharma are emphatic, declaring:

The biomedical power which social scientists have wished to critique is no illusion. Historically speaking it has grown from (on the one hand) biomedicine's political alliance with the state and (on the other hand) its espousal of scientific method as the basis for its authoritative claims to knowledge and expertise.

This power extends as well to "marginalizing other forms of healing" (p. 12) even to the extent that in some countries, biomedical professional organizations have rules that doctors "should not cooperate with biomedical healers." We should note that of this litany of concerns, we are in full agreement in two out of three: we agree both that medical science is epistemically hegemonic, and has been identified as such by the state and to a certain extent recognized as the principle arbiter of medical knowledge, and that the ability of medical experts to help people continues to expand into areas of life where people were previously left to fend for themselves using other available resources. We are not quite as certain that EBM is necessarily antithetical to "the lifeworld", as we have mentioned in terms of narrative medicine, there is room for EBM to consider pragmatically what the "lifeworld" might be for most people as well as EBM's current effect on it and finally, to use this data to consider ways to improve how a patient's "lifeworld" might fair in future interactions with EBM.

Cant and Sharma however, are clearly uncomfortable with medical "dominance", and their anxiety prompts one of the principle goals of their book (p. 14):

What we intend to do in this study is to give due importance to the crucial role of biomedical dominance in determining the position of alternative medicines in the present century without seeing them entirely through a biomedical lens. Even sociologists who have confined their study to biomedicine are now asking questions about the demedicalization of society, a deprofessionalization or even proletarianization of doctors. Medical power itself is no longer regarded by sociologists as constant and unproblematic and some have focused on the various challenges to biomedicine that stem from the greater "consumer" awareness (even litigiousness) of patients, widespread anxieties about side effects of drugs, the presence of alternative healing practices, and many other factors (Elston, 1991 Gabe et al. 1994a). Taking medical power seriously, without taking it for granted means that we will avoid the assumption that biomedicine and alternative medicines are involved in some kind of zero sum power contest; we can see both of them as embedded in a wider field of power relations.

If by "demedicalization of society" Cant and Sharma mean the normalization of illness, and if by "deprofessionalization" they mean the process of doctors losing their status, or some aspect of their autonomy, and if by "proletarianization" they mean the process of doctors shifting from sole proprietor private practice to jobs in larger medical organizations or businesses where they are paid wages as labourers, we can readily agree that dynamics are afoot. We do find it harder to understand how it is that any of this might inform the change in the way sociologists see "medical power" as no longer "unproblematic".

The claim that patients (consumers) might have a greater awareness about some aspects of medicine, contradicts an earlier claim, and suggests that there could be some complicity associated with what CAM users know about medicine. The extent to which, or even the way in which things like consumer knowledge, anxiety about drug side effects or "the presence of alternative healing practices" constitute "challenges" to biomedicine, could stand to be unpacked. At face value, Cant and Sharma appear to be referring to two common beliefs about the way attitudes toward medical science may have changed over the last two decades: 1.) there is a growing anti-science sentiment among medical consumers; and 2.) as a result, there is a market for non-scientific or even anti-biomedical medicine. We believe these are real social phenomenon, we suspect, and we will shortly look into them. Regarding the vague use of words like "power" or "challenge" without making a clear point: we are again left wondering who is being treated unfairly and what Cant and Sharma would have us do about it.

Anti-science and anti-biomedical sentiment takes significant pride of place in Cant and Sharma's book. Disturbingly, it is not always clear whether they are reporting on it or advocating it. Their writing appears to embrace a postmodern trope about science's broken promises (p. 25):

Where the expansion of scientific knowledge promised progress and predictability, it can be suggested that instead there have been unforeseen and unwelcome consequences associated with the expansion of knowledge (Giddens 1990), notably risks, and in the case of medicine concerns, about side effects associated with many biomedical interventions.

We are at a loss to identify the point at which "science" promised anything. We find this belief, that "science" has somehow failed to come through on a contract, to be nearly as absurd as it is ubiquitous in the sociology of science and medicine. Cant and Sharma quote another author to drive the point further:

.biomedicine is in danger of losing both its actual and ideological hegemony: firstly, it has culturally distanced itself from the consumers of its services; secondly it has failed to match its propaganda policies with real breakthroughs; thirdly patients have become further alienated by negative physical and psychological experiences at the hands of the biomedical practitioners themselves. (Bakx 1991:33)

The use of the words "ideological hegemony" in the quote above still do not allow us to understand if Cant and Sharma actually believe that medical "propaganda policies" have failed or not in their minds. It is not always clear as to whether these authors are reporting on the beliefs of others or weighing in with their own beliefs, but towards the end of their book, they do appear to finally make what appear to be unmitigated global epistemic claims (p. 192):

The failure of medicine like other scientific discourse to offer predictability, certainty, and progress and the risks associated with the expansion of scientific knowledge have meant that (if

it ever existed) unquestioning faith in the modernist project has been eroded. In turn the ability of biomedicine to assert cognitive authority can no longer be assumed. However, the decline of the trust in biomedicine has not been accompanied by a consequential lack of trust in all practitioners who claim some expertise in health matters. we have seen that (paradoxically) rejection of some parts of biomedicine by the lay public can be associated with a "leap" into faith with alternative medicine.

It is worth considering what Peirce might make of such a statement. We would suggest that Peirce would even reject "unquestioning faith" in the project of science and the workings of scientific communities *as he described them*, in favour of vigorous debate regarding his work. In sociological terms, the relative "unquestioning-ness" of the public's "faith" in science is an interesting but empirical question which, unfortunately, has little epistemic value. The notion that Cant and Sharma here are passing a global epistemic judgement on science, in asserting that medicine, like science has "failed", is absurd on its face. The charge that biomedicine can no longer assert "cognitive" authority, is apparently, again, a psychological or social psychological question regarding of the status of conflicting beliefs held by the person in question. Whatever Cant and Sharma mean to say here, one of the more pernicious aspects of sociological writing as we see it, is that interdisciplinary authors would have every reason to simply read paragraphs like this as a global epistemic claim. We are concerned that, indeed, there may be significant political incentives, in some contexts, for doing so.

More charitably, we can assume that the reality of which the authors speak is a social reality - the Zeitgeist, or some similar amalgamation of public beliefs. Even if we interpret this passage in this mode, whether we are speaking of a global indictment of science and medicine on the grounds of some imagined "failure" or associated beliefs which might cause a "leap" into faith with alternative medicine, these ideas are largely unconnected with the scientific mode of fixing belief, they are the product of the a-priori mode of fixing belief. As such they constitute an pernicious erosion of the authority of more scientific modes of judgement. As we continue working through the sociology of CAM, we suggest that there is certainly a class of things we can refer to as "pernicious medical beliefs", but more importantly, there is something more subtle and much harder to fit into the way medical pluralism is normally debated, and that sits at the intersection between the regulation of the medical marketplace and the public understanding of science: the culture of the way we fix belief -- the way we set policy and how it may affect the way people in our society make decisions

What is clear, is that Cant and Sharma are certain that there are lots of people who have very negative views of "science" and who do not trust biomedicine. We agree with this and like Cant and Sharma, we see the quantification of the prevalence of these beliefs as an important goal both generally and in terms of this thesis. We part ways with Cant and Sharma in two respects however: 1.) although we see the fear and mistrust of biomedicine as a serious problem for both narrative medical experts as well as for the public understanding of science, we find the use of words like "propaganda" an unsettlingly pejorative expression, of the anti-realist belief, that the communications of medicine are purely political statements, wholly disconnected from either ethics or pragmatic reality (we see

rhetoric like this as a particularly pernicious expression of social constructivism when such statements are exported into the wider academic world, where readers might not understand the postmodern disclaimers which apply); 2.) we are not tempted to leverage the prevalence of negative ideas about science and biomedicine among the general public as some kind of warrant for a global adjudication of medicine's "ideological hegemony". Frankly, we are not sure if Cant and Sharma are applying a disclaimer here or not. They may not be as detached from such judgements as they have previously attempted to lead us to believe. We understand that for sociologists, in the absence of "the real", there is only "the social" and that in the way sociologists use language, the things that large groups of people believe *are reality* -- for those people. What we wonder about is the extent to which this makes popularity into an epistemic warrant. We understand that Cant and Sharma could use their blanket social constructivist disclaimer to reject our charge that they may be equating popularity with what is true. Still, we will dedicate some time to pointing out where they appear to use popularity as an epistemic warrant -- if for no other reason, to point out how desperately misleading this is for any reader unaware that Cant and Sharma, as social constructivist anti-realists, are wholly undedicated to any meaning whatsoever, beyond what they can show that large numbers of people believe.

The word "Postmodernism" comes up numerous times in Cant and Sharma's book. They pose the question: is CAM a postmodern rejection of the universal (p. 20)? Adopting the talisman of Wittgensteinian language they ask (p. 78):

if we adopt a postmodern interpretation that sees a plurality of language games, narratives and social practices, having equal validity, are we now seeing a more even playing field in health care?

Similarly, (p. 80):

If we accept that the postmodern diversification of knowledge is occurring, it could be argued that we are seeing the revival and growth of multiple ways of knowing about health and illness and how to deal with sickness.

The second quotation effectively asks: if people have rejected the universalism of modernism, then are people thinking about their health in the same way? The first question is harder to understand, but presumably means that epistemic anarchy is epistemic democracy is epistemic equality. Our pragmatic medical epistemology is profoundly "modern" in this sense. In terms of the use of pragmatically powerful medical knowledge, the benefits are universal, inherently progressive and undeniably valuable. From this perspective, there is nothing to be gained from discarding modernism other than a return to pragmatic impotence. As to Cant and Sharma's commitment to the postmodern thesis, it appears that they fail to answer any of the question they pose.

Marxism, or at least its language, also plays a problematic role in Cant and Sharma's work.

They observe that (p. 18-19):

it has often been suggested that biomedicine is of its nature particularly suited to serve the needs of capitalism, it is a "core institution of capitalist society and...a system that reinforced domination at the micro-social level" (Singer and Baer 1995; 62). If this is so, do alternative medicines represent some kind of counter culture or *résistance*?

Like the charges of "self interest" that Cant and Sharma level at EBM, the above quote appears to brandish Marxist theory like a talisman, without any attempt to hook the notion into reality. Like the previous discussion of "power", even at the "micro-level", "domination" in this context would appear to reduce to the psychological or social-psychological experience of talking to a doctor about one's illness. If a doctor is trained to anticipate the patient's feelings of helplessness, and is also trained to help the patients feel empowered, or even trained to evaluate them in regard to these matters in some way and refers them to someone who can fix the problem, perhaps the patient's sense of agency can be restored. The mistake here is to assume that there is some other way, some ideal medical system being articulated here, with different rules altogether, which EBM can be measured against and found wanting. If it turns out that patients are mistaken about the malevolent nature of EBM, then that is a problem, but it in no way suggests that "science" or rationality should be sidelined in favour of something better. There is nothing better. Barring the issue of epistemic inequality between doctor and patient, and the problem of bad news, EBM's alignment to capitalism, as an intrinsic quality, is hard to understand. Medicine, as practiced in the UK, serves the interests of capitalism no more than it serves the interest of the public, or rather, the proletariat. Unless Cant and Sharma can demonstrate how either the expertise or the professional conduct which characterizes EBM functions to oppress workers, or somehow otherwise serves only the interest of elites, then the answer to Cant and Sharma's above question as to whether CAM can be understood as a reaction against EBM crimes of capitalist oppression is: no.

In sum, we have found little utility in Cant and Sharma's engagement with postmodernism or Marxism or "power". In short, it appears that as Cant and Sharma hold the world at arm's length as social constructivists, they are left with little upon which to base a claim which might result in a legitimate call to action. Again, they are unable to adjudicate the legitimacy of power. What we are left with is the same empirical questions about what people really believe: in particular what CAM patients believe, in terms of their anti-science, anti-medical beliefs and fears and how these cognitive artefacts interact with other operative medical beliefs about the efficacy of medical treatments, ontological commitments, and their pragmatic goals. These are questions we can and shortly will use the data to answer, but the answers will not tell us anything discernibly meaningful or important about Marxism, postmodernism or vaguely sinister notions of power relations²⁴.

²⁴ As we made clear in the case of Medical Anthropology, our interdisciplinary project relies significantly on our success in characterizing the consensus and epistemology of other disciplines -- in this case, Medical Sociology as well. We have found a wealth of writers who are willing effect agnosticism in terms of the relative

CAM in an EBM world: the establishment of pseudo-professionalization

Epistemically, CAM and EBM are incompatible. Overlap occurs in areas of marginal importance, like the extremely limited endocrine or close-intercellular effects of acupuncture, or the medical utility of herbs as a drug delivery system. Neither of these ideas is remotely well supported as a promising medical theoretical approach. Both of these areas of research are artefacts of the combination of CAM's popularity and enterprising scientists, and have turned up no dramatic findings. CAM, from the ground up, is built upon ontologically suspect notions like "subluxations" or "qi". When decisions are made on such pragmatically impoverished concepts, concrete goals are not advanced. All we get is the effects of whatever legitimate medical practices the CAM practitioner may be employing and medically inert practices which may contribute expectancy or placebo effects. We may also get a sense that the patient and practitioner inhabit a shared zone of magical, vitalistic or religious belief.

The CAM modality itself, however is a fantasy, and it is not in possession of an area of expertise to command. This is why we propose the notion of pseudo-professionalization. True experts have traditionally been capable of governing themselves as a profession. The profession thus understands the pragmatic problems it exists to overcome, the nature of the knowledge it has to shepherd and harness in that process, and the nature of the threat from non-expert pretenders as a threat to the integrity of the profession and to the vulnerability of the public. CAM organizations have no expert knowledge. Their expertise is an illusion. The more they approximate the global values of professionalism, honesty, integrity and the embrace of the best available knowledge about their modality -- particularly if their modality has been a major area of study by EBM itself -- the more they are confronted with evidence that what they are selling does not work.

Although as we shall see, a number of CAM ethics "experts" have emerged, arguing that CAM is under no obligation to accept EBM evidentiary standards, *we are*. Our universal pragmatic epistemic modal acknowledges only one way to understand clinical evidence. To choose to understand it otherwise it to merely engage in bad science or worse, wilful negligence or fraud. As such, CAM departs from the realm of real professionalism when it chooses to ignore the legitimate consensus of scientific expert authority, which for most CAM movements, is right at the start.

In spite of this complete epistemic incompatibility, there are a number of ways that CAM and EBM interact, and it is here that we will be afforded the most important insight into the nature of expertise, professionalism and its relationship to pragmatic medical power. As we mentioned before, CAM modalities generally begin with a small number of charismatic leaders, spreading CAM beliefs

legitimacy of CAM and EBM. We have yet to identify a sub-field of Realist Medical Sociology which accepts EBM's epistemic superiority and attacks CAM's legitimacy on this basis. In the interest of due diligence, the following references are provided, which characterize Medical Sociology and its conformity to social-constructivist epistemology (Collins & Pinch 2005; Foucault 1963; Leslie 1980; Loustaunau & Sobo 1997; Cant et al. 2012; Vohra et al. 2005; Sade 2003).

through informal trainings and the social networks of likeminded people. However, as each CAM modality becomes more prevalent, EBM scepticism and associated efforts to restrain, criticize and otherwise marginalize the "movement" spawn an urge among CAM practitioners and advocates to organize -- to create advocacy groups. This urge to organize becomes irresistible to most participants, either in the face of EBM attack, or for some other reason -- quite possibly the desire to emulate the professional trappings of EBM. As we shall see, the tendency for CAM practitioners to call themselves "doctor" in spite of this being against the law, is a strong argument for this latter theory.

As we shall also see, this increase in the level of organization often results in profound changes to the public face of the modality. Forming one formal organization provides a focal point for criticism. In the case of Chiropractic in particular, the difference between the way chiropractors publically advertised themselves, both individually and as a group, has changed so profoundly in the last 5 years that today's public face of Chiropractic bears little resemblance to its public face in the start of 2009. We will deal with this in greater detail in terms of the history of Chiropractic later in Part II. For now, we wish to explore what Sociology has to say about several ways in which EBM and CAM interact: 1.) The way CAM moderates its claims in response to EBM attack; 2.) the way CAM attempts to adopt EBM, methods, including "science"; 3.) the way EBM has reacted to CAM; 4.) to what extent EBM doctors have embraced CAM themselves. As we explore the sociological data, we ask the reader to keep in mind an idea we have already mentioned, but which is absolutely central to the thesis: CAM's existence in the medical marketplace and CAM's uptake by some doctors, medical schools and other universities, can best be understood as a failure of professionalism.

The tempering of knowledge claims is part of this process of pseudo-professionalization. according to Cant and Sharma (p.65)

Within chiropractic, a section of the profession believed that the manipulation of the spine had the potential to cure the whole range of mechanical and organic problems and in Britain the Druidic homoeopaths stressed the danger of biomedicine, the spirituality of the vital force, and the ability of homoeopathy to deal with all medical problems.

However, throughout the 1980's we witnessed the gradual curtailment of the therapy specific claims, although this may have been accompanied by the expansion of other skills -- for example, in the United States osteopaths and naturopaths have acquired skills of general practice (Baer, 1984b).

This passage is interesting in four ways, the first being because it illustrates the way that CAM itself is, in part, responsible for some of the anti-medical sentiment we see appearing in public attitudes. The second interesting point here is that it underscores that CAM beliefs are originally panacea in nature and, in some cases, involving questionable ontological commitments. The third point here is that therapy claims are curtailed gradually over time, presumably as the popularity of the modality grows. Finally, the forth point here is that some modalities successfully make a partial or complete transition to medicine without disappearing. We would like to add to this last point the proviso that if they make the transition completely, they cease to be a CAM modality in any meaningful sense.

CAM modalities do, to various degrees, take on medical techniques, criteria for referral, and other trappings of medical professionalism. Shortly, when we discuss the history of CAM we will take a look at the case of Osteopathy in the United States, which is now entirely integrated with EBM. The only remaining differences between American Osteopathy and EBM is that 1.) medical schools with an Osteopathic history are not as well respected and 2.) that there remain a few older die-hard professors at the Osteopathic schools who still attempt to maintain that there is something "better" about the osteopathic approach, although young medical school attendees are unable to identify what that might be. As we mentioned we will also look more carefully at the history of both osteopathy and chiropractic in the UK. For now, it is important to recognize that CAM movements begin to reign in their more outlandish claims and they often will try to appropriate EBM methods. In the case of Chiropractic in the UK, the combination of these two tendencies has become so extreme that Chiropractic is now impossible to tell apart from physiotherapy by what the British Chiropractic Association puts on its website²⁵:

Treatment consists of a wide range of manipulation techniques (some very gentle) designed to improve the function of the joints, relieving pain and muscle spasm. Such skilled manipulation is very specific, directed at individual joints in order to reduce strains and improve mobility in one area without disturbing another.

This is a significant retreat from the beliefs on display in the quote from Cant and Sharma: "that the manipulation of the spine" could "cure the whole range of mechanical and organic problems". As we shall see, as late as 2009, the British Chiropractic Association website was suggesting that problems as diverse as infant colic could be treated effectively with spinal manipulation.

It is, of course, of profoundly significant consequence that Chiropractors are, in fact, not professional physiotherapists, but rather a pseudo-profession, in possession of no legitimate extra-medical form of expertise, whose "manipulative techniques" may or may not be under a legitimate constraint to actually be pragmatically potent, depending upon the interpretation and/or enforcement of the General Chiropractic Council Code of Practice requirement that chiropractors be honest with patients about "the likely outcomes with or without care."²⁶ Unfortunately, the ability to know if this requirement is being uniformly ignored is completely beyond any available form of oversight. Studies which might allow us to divine what it is what these techniques actually look like or what chiropractors actual tell their patients about these techniques is one of the sociological "elephants in the room" of the debate over CAM. Ethical limitations against lying to CAM practitioners, while posing as patients -- techniques we believe are necessary as the only way to bait chiropractors into

²⁵ Retrieved on September 10th, 2014 from the following URL:
<http://www.chiropractic-uk.co.uk/For-You-5-General-Advice-0-ms.aspx>

²⁶ Retrieved on September 10th, 2014 from the following URL:
http://www.gcc-uk.org/UserFiles/Docs/COPSOP_2010.pdf

divulging what they actually do and say to patients -- has proven a significant obstacle to such research.

However, at least one, small study in New Zealand used emails to 13 randomly selected chiropractors, posing as a grandmother with a sick child and asking about treatments for asthma and an ear infection. 69% claimed they could treat asthma and 62% claimed they could treat an ear infection. 92% of the chiropractors suggested an appointment (Holt 2008) . Cant and Sharma's claim that Chiropractic has tempered its knowledge claims to the point of portraying itself as "...a specialist..." fitting into "...a gap in the medical market (back pain), an area in which the medical profession has had little success" appears to be an overly credulous acceptance of Chiropractic's public claims (Cant & Sharma 2004, p. 148).

This renders the most important difference between CAM and EBM: if it were to be determined exactly what EBM and CAM practitioners actually say and do to patients, the information about EBM would not likely be surprising to anyone, while we believe that this same information about CAM would likely prove revelatory. CAM has always been making claims to efficacy which bear on questionable ontological commitments. In the past their claims to efficacy were tolerated as largely insulated against the intrusion of medical standards of evidence. This has changed dramatically. The drive toward real professionalism, including universal evidentiary standards, is putting great pressure on at least the more mature and monetarily successful CAM modalities like Chiropractic. The evidence for this is in the disappearance of public claims of efficacy or public claims of ontological commitments. There are, however, still a great many people visiting Chiropractic clinics. Given the small amount of evidence available, it is hard to imagine that even though their public statements have been forced to undergo such profound and rapid change, that their private statements are necessarily mirroring this change.

The moderation of radical CAM claims was the first of the dynamics we wanted to explore. The next is the use of "science" by CAM advocates and researchers. From our perspective, CAM advocates who would like to appeal to science are at a distinct disadvantage. To embrace the progressive nature of Peirce's vision of the scientific progression toward the real, good CAM science will always result in the determination that CAM is ineffective, because CAM's premises are based on ontological misconceptions. In fact, methodologically robust clinical research literally always demonstrates that CAM treatments are ineffective. Badly designed or otherwise biased research always stands a chance of "confirming" efficacy. In fact, as we shall see, the worse the design, the greater the chance of confirming efficacy. Bad interpretations of biased research are then used to argue a "scientific basis" for the CAM modality in question. Unfortunately, these last two elements are enough to maintain a fairly robust CAM research culture. Cant and Sharma dealt with this issue only by suggesting that CAM researchers are more inclined to embrace anecdote as evidence as "biographical medicine" and suggested that there is a legitimate debate over what constitutes evidence of CAM efficacy in medical epistemology over "different kinds of biomedical knowledge, which was

becoming difficult to ignore" (p. 100.) As we shall see, this is a common trope among CAM advocates, and as we understand medical epistemology, this is simply wrong.

There are however, at least in the United States, a number of CAM researchers, still funded by the National Institute of Health and an ethnographic sociological study of them was published by Polich, et al. in 2010 (Polich et al. 2010) (Polich et al. 2010). Not surprisingly, they "found close personal ties to CAM to be a highly important motivating factor for entering the field of CAM research." This naturally explains the confirmation of efficacy so common among studies wide open to bias. It also confirms our epistemic characterization of robust clinical research methodology as the only means of overcoming human perceptual error. Three quarters of the researchers studied "attributed either a lifelong interest in or personal therapeutic experience with CAM as their original impetus for beginning CAM research." Also not surprisingly, most of the researchers in ethnographic interviews characterized outcomes of their research in terms of CAM-specific constructs such as 'chakra' or 'qi' in their responses, concepts lacking biomedical correlates. This suggests that the public statements of CAM practitioners in the UK, which are stripped of unusual ontological commitments, may say little about how CAM is functioning on the ground.

The study also found something very interesting about how CAM researchers interact with mainstream EBM scientific research colleagues. Something which strongly confirms both Cant and Sharma's findings about the moderation of CAM claims and our suspicions about what CAM practitioners tell their patients in private: CAM practitioners do not share their questionable ontological beliefs with EBM colleagues, in fact, they actively hide them.

Interviews secondly demonstrated CAM researchers' concern that close ties to alternative therapies could threaten their professional credibility. Consequently, to protect their reputation, CAM researchers had to be continuously diligent about self-presentation.

And:

CAM researchers in this cohort preempted the negative connotations associated with unorthodox work by carefully selecting and editing what was shared with colleagues. They sought to distance themselves from alternative medicine and reposition themselves nearer to the norms of biomedical research.

This is what we hypothesise is happening with the public statements of chiropractors in the UK. We will shortly prove that this is what is happening with the public statements of chiropractors in the UK.

The last key finding of this study only makes sense given an understanding of the way the researchers in this study described themselves to the ethnographers: radical, iconoclasts on the cutting edge of a fantastically valuable scientific discovery. The last key finding of the study concerns the conflict the CAM researchers find themselves in over methodology. They know that their

unconventional claims will only breakthrough EBM prejudice about CAM modalities, if they are able to use the most rigorous of clinical methods. At the same time, they are afraid that:

by adhering to conventional biomedical research practices, CAM researchers feared they were making methodological concessions that could severely distort, even undermine, the therapeutic value of the therapy under investigation.

It seems that these researchers, are running up against the very Peircian limits of reality that foretold the demise of CAM from the beginning: bad metaphysics. CAM is pragmatically impotent because it relies on mistaken ontological commitments. As such, good research will simply prove it does not work. Unable to extricate themselves from the original, metaphysical mistake, but cleaving close enough to medical expertise to at least see the persuasive utility in the results of robust randomised clinical trials, these CAM researchers are indeed in a serious bind. It is unfortunate, yet unsurprising, that they struggle with their desire to turn to what Cant and Sharma described as "biographical medicine", and what we suggested is a CAM advocacy trope: that if robust clinical methodology does not prove what must follow from mistaken ontological commitments, then the methodology must be wrong. Kuhn comes to mind in the sense that CAM researchers are so wedded to their ontological mistake, that they will simply never overcome their paradigmatic bias, no matter how long their research continues to demonstrate reality to them, over and over again.

The bind of CAM researchers says something very interesting about Peircian epistemology, about expertise, about scientific communities and about professionalism. To begin with, metaphysics matters and should not be excluded from scientific goings on. Secondly, since CAM modalities are conceived as radical panaceas, driven by ontologically suspect commitments and rejected by scientific communities, the whole enterprise is essentially either anti- or pseudo-professional. The distinction being simply whether a person rejects professionalism or attempts to affect professionalism, in the absence of a pragmatically powerful expertise. Unfortunately, the professional practice of scientific medicine is a vast enterprise and like any large enterprise, there are individual and sometimes collective lapses of professionalism. Which brings us to the third and fourth points of friction where CAM and EBM interact: the way EBM has reacted to CAM and to what extent EBM doctors have embraced CAM themselves.

In 1986, the British Medical Association responded to CAM with a substantial document entitled "Alternative Therapy" which we will deal with in the History of EBM and CAM section coming shortly in this part of the thesis. We will deal with this document in some detail at that point however we would like to note that it is a very carefully crafted treatise, firmly rejecting CAM as matters unsupported by evidence and claiming questionable modes of action. Plan to contrast this with EBM's current take on CAM as well. According to Cant and Sharma, however, EBM backtracked from its 1986 position and by 1993 had softened considerably toward CAM (p. 60). Cant and Sharma suggest that EBM shifted its concerns from efficacy to "trustworthiness and safety".

Cant and Sharma make a number of claims however, which appear to be very critical of EBM's response to CAM. They suggest that "biomedical responses to alternative medicine demonstrate contradictions in the position of biomedicine" and they cite examples including contradictions between holistic aspirations and a reductionist knowledge base; contradictions between an interest in continued professional dominance and the realities of CAM's popularity, both as expressed by consumers and by the State; and by differences of opinion between doctors in different positions within the health care system. We would note that none of these things are actually contradictions from our perspective, with the exception of the last one. There is nothing contradictory between holism and scientific realism (or a "reductionist knowledge base", to the best of our knowledge). Nor is there anything unusual, as Parsons would point out, about irrational medical beliefs popular among the public. For that matter, craven political pandering by actors in government is hardly less common. The pragmatic necessity of scientific epistemic authority is diminished by neither of these things. The suggestion that they should be is either an embrace of popularity as epistemic warrant or a reflection of anti-realist nihilism. What constitutes significant failures of medical professionalism, in the above list, is the embrace of CAM modalities by doctors.

Cant and Sharma are more quantitative in their efforts to document the percentage of doctors using CAM than they are at any other point in their book. They cite numerous studies showing levels high levels of CAM use referral and training, including one study of use of CAM by young doctors reporting that 21% of young doctors use CAM already with 80% wanting to train in some CAM modality (p. 97). They argue that the relatively strident statements issued by EBM authorities were not shared by doctors themselves. Cant and Sharma also mentioned a study by Ernst which suggest that doctors who are pro-CAM tend to be unaware of the bleak clinical evidence regarding CAM efficacy (p. 99). This is the operative point here. Doctors are in a profession, and there is a body of expertise they are responsible for. Doctors who are unaware that CAM is ineffective may have very large incentives to remain that way, if it makes dealing with patients easier. Unfortunately, as Ernst was clearly attempting to point out, understanding the consensus of EBM's epistemic experts is the responsibility of every physician. We could go into a great deal more detail about the attitudes of doctors regarding CAM, but we do not have to. Our point here is simple. When doctors embrace CAM they are in breach of their professionalism, they are ignoring the consensus of the relevant expert community and they are abandoning their ethical requirements to connect their patients treatment to the best available science.

The reader should keep in mind that our epistemic position on CAM has important implications for universities offering degree programs in CAM modalities, along with the way medical schools deal with the subject. The state and its interaction with CAM and EBM is subject to the same epistemic constraints and as we will document in due course, the state has failed the public substantially in this regard.

What CAM users believe

By far the most important empirical question in the thesis, after the efficacy issues surrounding CAM techniques themselves, is: what do CAM users believe about CAM? Within the answer to this question rests the answer to questions about the status of CAM in the context of medical choice, the lines along which we can understand CAM as a form of spirituality and most importantly, the question of fraud and negligence, so central to understanding CAM itself as an artefact of the failure of professionalism -- as a pseudo-profession. Of profound importance, as we shall see, there is a large percentage of CAM consumers who do not understand that the CAM modalities they engage are epistemically incompatible with the rest of the medical system. This is a crucial sociological finding for a number of reasons, not the least of which is that it essentially overturns the logic of the UK courts, who, as we shall see, place a great deal of importance on the "choice" to use CAM (or TCM specifically, in *Shakoor v Situ*), and that this decision on the part of each CAM consumer constitutes a rejection of EBM, a "rejection of the orthodox" in the words of the decision. The epistemic confusion on the part of medical consumers sheds a very sinister light on the fact that, as we shall see, CAM practitioners must be repeatedly threatened with prosecution to stop them from referring to themselves as "doctor"²⁷. A significant portion of CAM users are seeking pragmatic solutions to medical problems, from the wrong people, and as a result, there is no chance that those users will get what they are seeking. Beliefs like "acupuncture can cure infertility" read from high street store fronts as we write this, are passed through social networks by well meaning family, friends and colleagues, to desperate people with real medical problems, for which available medical solutions may be frightening or unlikely to help. Parsons understood medical professionalism as a means to protect desperately vulnerable, potentially irrational sick people from both the criminal fraud of the quack and the pernicious, negligent dogmatism of the crank. CAM has broken the tentative hold EBM had on a universal expression of the professional medical contract. The pernicious effect of allowing CAM into the NHS, of allowing it to exist as a parallel medical system in private enterprise including training and degree programs, can be seen in the continued proliferation of CAM beliefs among the public.

CAM beliefs are not benign. Furthermore, the list of harms cannot be reduced to the deaths resulting from Chinese herbs²⁸ or Chiropractic manipulation²⁹, or to the billions³⁰ spent on CAM

²⁷ We will shortly be documenting the McTimothy Chiropractic's reaction to a movement to complain about unsupported claims to efficacy on chiropractic web sites to the General Chiropractic Council. In a panicked email sent to all its member practitioners, the organization advised them to remove claims to efficacy from their web sites and to remove any reference to chiropractic practitioners as "doctor" specifically in an effort to avoid persecution.

²⁸ The only negligence case brought against a CAM practitioner in the UK so far is *Shakoor v Situ*, brought against a Chinese herbalist by the wife of a man who was killed by the herbal concoction provided by the herbalist.

²⁹ According to the Results of a 2010 *International Journal of Clinical Practice* study by Edzard Ernst (Ernst 2010) (Ernst 2010), "Twenty six fatalities were published in the medical literature and many more might have

world-wide, which certainly includes cases of desperate people spending money on CAM, when they need that money for other things. As we review the sociological research on what CAM users are thinking, we ask the reader to keep in mind the notion of the "culture of fixing belief" which we mentioned earlier in this section, and to consider the potential value of an *intact* medical profession, where the epistemic and expert basis of the medical contract is more fiercely and effectively guarded. This goes to the heart of the legitimacy of power issue traditionally so dear to sociologists. Cant and Sharma however, are clearly enthusiastic about CAM's proliferation. The subtext to our exploration of CAM user beliefs must be Cant and Sharma's thinly veiled advocacy.

We believe that understanding pernicious medical beliefs and their transmission through the lens of network science -- understanding pernicious medical beliefs like any other pathogen in an epidemiological sense -- is the most promising way to frame the relevant quantitative sociological research. CAM clinics and certification and degree programs function as network hubs through which pernicious ideas are more rapidly spread among the public around them. We will see examples of pernicious and very specifically spurious anti-medical and anti-science beliefs in the form of anti-vaccination programs, spreading most rapidly into areas where CAM is present -- areas which now, are showing an increased incidence of MMR epidemics. We will revisit research we have already mentioned, suggesting that mistaken medical beliefs often transmit more rapidly through populations than pragmatically powerful ones. We are however, confident that a network analysis approach is the way important future research on this topic should be framed in the continuing search for how to understand and deal with both medical pluralism, CAM and other potentially pernicious medical beliefs.

The data on CAM user beliefs is complex and in some cases contradictory. Cant and Sharma identify what they suggest are two strands of research about consumer medical beliefs. They are quick to point out that "lay people do not simply accept biomedical definitions of health and illness", that lay public views are complex and often contradictory (p. 22). The second strand of research address the "why me" question, which we have addressed before. Citing a number of ethnographic studies Cant and Sharma work to theorize the public interest in CAM as a rejection of science due to its "failure" and public dissatisfaction with medicine's perceived lack of time and attention to patients.

remained unpublished. The alleged pathology usually was a vascular accident involving the dissection of a vertebral artery." It is worth noting as well that the Conclusion also states: "Numerous deaths have occurred after chiropractic manipulations. The risks of this treatment by far outweigh its benefit."

³⁰ According to an American 2009 *National Health Statistics Report* : "In 2007, adults in the United States spent \$33.9 billion out of pocket on visits to CAM practitioners and purchases of CAM products, classes, and materials. Nearly two-thirds of the total out-of-pocket costs that adults spent on CAM were for self-care purchases of CAM products, classes, and materials during the past 12 months (\$22.0 billion), compared with about one-third spent on practitioner visits (\$11.9 billion). Despite this emphasis on self-care therapies, 38.1 million adults made an estimated 354.2 million visits to practitioners of CAM. About three-quarters of both visits to CAM practitioners and total out-of-pocket costs spent on CAM practitioners were associated with manipulative and body-based therapies. A total of 44% of all out-of-pocket costs for CAM, or about \$14.8 billion, was spent on the purchase of nonvitamin, nonmineral, natural products."

They acknowledge that it is both important to understand the difference in beliefs between users of EBM and CAM and that the methodological difficulties of capturing the cognitive antecedents of a decision as a research objective can be daunting (p. 28).

Cant and Sharma then move on to review the studies which attempt to characterize CAM users, in terms of gender, education, region, etc. Interestingly, a major finding from Cant and Sharma's own research confirms a higher level of CAM uptake in areas with a greater number of CAM practitioner training schools. They consider the relationship between the use of CAM by consumers and the density of CAM training institutions (p. 30):

in our study of reflexologists, chiropractors and homeopaths, it was noticeable that training schools were clustered in the South and that these attracted local students who then chose to practice in the same area. Even in the case of the largest college of chiropractic in Bournemouth (south coast of England), which attracts students from all over Europe, we found the highest concentration of chiropractors in this area (Cant and Sharma 1994). Considering, as we shall see later, that most patients make the decision to consult on the basis of recommendation, this suggests that it may be the supply of practitioners that encourages demand and the extent of usage may be greater if the number of practitioners were to increase and situate themselves in all parts of the country.

Cant and Sharma's take on this is that "the full extent of usage of these services may be artificially constrained." We believe that it is essentially correct and deeply troubling. From a network analysis standpoint, CAM schools are not just places where students are parted with their money in trade for pragmatically impotent medical beliefs, but rather they are places from which a synergistic culture of CAM normalization is created, which transforms the local native "belief fixation culture" by exploiting credulity and breaking down the existing protective faith in the culture of medical professionalism, until high levels of CAM uptake become the new normal with negative beliefs about EBM established in parallel. Given that CAM cultural elements are specifically critical, even libelling of medical values, the association between CAM usage and negative beliefs about medicine may be more than merely a correlation. There is every reason to believe that CAM training institutions are *creating* an enduring paranoia in regard to medicine, which may take decades and substantial research and public understanding of science resources to overcome. The lack of research on this topic is in desperate need of attention from academics.

Of course, demographics alone cannot really tell us what we need to know about the values and beliefs of CAM users. What we really need is to get at the differences in the values and beliefs between users and non-users. Not surprisingly, the first example of such a study that Cant and Sharma review is the finding that users of Homeopathy were found to be more critical and sceptical of biomedicine. Another being that users of acupuncture have lower levels of confidence in GPs and biomedical drugs than non-users (p. 31). In the end Cant and Sharma conclude that there appear to be modes of motivation which attract people to different CAM modalities, including scepticism or fear of biomedical drugs or a heightened awareness or level of concern about their health. None of these

findings surprise us, although we are particularly concerned about the possibility that CAM may be creating a market for itself, by spreading and now normalizing fear, scepticism and distrust of EBM.

Cant and Sharma also review studies revealing the actual reasons for seeking out CAM practitioners. A principal finding, supported by multiple studies and widely believed is that patients tend to seek CAM as an option when EBM is not successful with a chronic medical problem. This means that CAM users are not going first to CAM practitioners for acute conditions (p. 32), rather after a problem becomes chronic, and after the EBM options are presented and judged to be less than satisfactory, patients then tend to turn to CAM. In an ironic turn of phrase, Cant and Sharma describe this situation as "...where biomedicine cannot offer a panacea." Cant and Sharma enthusiastically describe the attraction to specific CAM modalities in regard to specific chronic conditions (p.32):

Sharma (1992) identified pain (especially back pain) and allergies as the most frequent presenting problems and certainly osteopaths and chiropractors are used extensively throughout the world for lower back pain (Thomas et al. 1991), this being the area that these practitioners have identified as their specialty (see Ch. 3 and Ch. 5). Acupuncture has been linked to the relief of chronic pain and migraines (Fulder 1996 -- an area that the medical profession recognize as acupuncture's forte, see Ch. 4), whereas homeopaths tend to see more non-specific conditions, allergies and stress related illness and diseases that relate to inadequate immunity.

It is perhaps this passage which most betrays Cant and Sharma's biases regarding CAM's efficacy. We will make a strong case that none of these modalities do anything but illicit expectancy and placebo effects. In any case, CAM patients are usually seeking relief from a concrete problem, it is usually not life threatening, and the evaluation of the severity of the condition is a matter of subjective perception, prone to perceptual distortion, as in conditions like pain or allergies. Cant and Sharma appear convinced that patients go to CAM practitioners because, in their words "biomedicine has failed them" (p. 32). We agree with Cant and Sharma, minus the gratuitous dig at EBM.

As we mentioned, a major finding of Cant and Sharma's review is that patients overwhelmingly go to EBM doctors first. These patients are then, in some cases inclined to take their bad news or dissatisfaction to CAM (p. 33). Cant and Sharma cite their own study to again confirm that patients "informed themselves about the CAM therapy and possible cures" but that the actual choice of which CAM practitioner to ultimately see was made on the basis of advice from "significant others". Aspects of social networking phenomena were discussed in some detail, including that satisfied CAM users "act as advisors to other users or actively bring new users to the practitioner" (p. 36). Women who are convinced of the utility of the CAM modality take their children to the practitioner as well.

The most troubling reading of the data that Cant and Sharma review, is the idea that patterns of use of CAM are dynamic and change as consumers "become more acquainted with the skills of the alternative practitioner" (p. 37). As we see it, alternative medical practitioners are pseudo-professionals, who as such, have no pragmatically useful "skills". The social networks along which

CAM users acquire more "knowledge" about their condition and what CAM modalities can be expected to do, are clearly informal breeding grounds for a-priori beliefs. The frequent mention, by Cant and Sharma of the "additional knowledge" and the "increased understanding" associated with CAM users may be an exceptionally misleading use of words. These beliefs may be entirely wrong, and thus the kind of thing only a sociologist could refer to as "knowledge". Furthermore, these beliefs may be even worse than simply erroneous ideas. They may actually be specifically erroneous anti-EBM ideologies, passed from self-interested, entrepreneurial CAM practitioners to patients, and from the patients to the patients other social contacts. The desperate need for less credulous and more adversarial sociological work in regard to these matters should be becoming extremely clear.

For all that the CAM problem stands in dire need of a more aggressive sociological quantification, many of the studies Cant and Sharma conducted and review are helpful in providing a more ethnographically intimate account of the experience of CAM users. An approach which is equally important. In particular, Cant and Sharma have provided several personal accounts of Homeopathic users which we find particularly enlightening. Homeopathy, of all the CAM modalities may be the most easily subject to clinical research and as a result, is the modality which has been most definitively proven not to work. Cant and Sharma provide examples of satisfied users, including a worried mother dealing with her child's eczema, who, after being frightened by a prescription of cortisone creams and steroids, took her child to a homoeopath who, in her words "dealt with the problem more or less" (p.38). Another user "really had bad ovulating pain" and describes what happened after her homoeopath provided her with a remedy (p. 39):

he gave me a remedy and it worked. That was what converted me. Homoeopathy worked in five minutes, no more pain. It was so miraculous.

These accounts are very disturbing in light of the profound inability of the users to evaluate the efficacy of the treatments involved. As if in an effort to achieve some balance, Cant and Sharma include one user who suggested (p. 39):

The remedy worked in a very gradual way, I mean you could be getting better yourself. That's the airy fairy nature of homoeopathy.

The direct consideration of regression to the mean as a likely explanation for the improvement in a medical condition is insightful indeed, but as the other two respondents indicate, Parson's observation of the epistemic vulnerability of sick people is clearly the mode for CAM consumers, whose perceptions are clearly overwhelmed by expectancy and placebo effects. Cant and Sharma, ever optimistic, go on to cite a *Guardian* survey from 1996, suggesting that only four of 386 respondents failed to experience improvement in their symptoms as a result of seeking a CAM treatment. From our perspective, one which firmly holds that none of the people in this study experienced a single

clinical effect beyond placebo, this same evidence is very powerful indeed, but as proof of Parson's thesis: that people are terribly vulnerable to perceptual distortion and indeed, irrationality. Completely unaware of the way we would consider the implications, Cant and Sharma then pose the following question: "Maybe the continued motivation to use this sector is the pragmatic one" (p. 39). That is of course, our point exactly.

It is at this point that we must turn to more recent and more quantitative sources of sociological data on the beliefs of CAM users. First, it is helpful to check in on the current level of uptake of CAM beliefs, as represented by CAM usage. The *Journal Clinical Medicine* published a review in 2013 in which:

Five databases were searched for English language, peer-reviewed surveys published between 1 January 2000 and 7 October 2011. In addition, relevant book chapters and files from our own departmental records were searched by hand. Eighty-nine surveys were included, with a total of 97,222 participants (Posadzki 2013).

This meta-analysis determined that in the UK, according to the "methodologically sound surveys" the average one-year and the average lifetime prevalence of CAM utilization was 26.3% and 44%, respectively. More specifically, "Herbal medicine was the most popular CAM, followed by homeopathy, aromatherapy, massage and reflexology" while "[a]nnual out-of-pocket expenditure in the UK on CAM has been estimated at £1.6 billion."

Having established that mistaken beliefs regarding CAM's efficacy must be widespread, given that nearly half the population in the UK uses CAM in their lifetime and a quarter have used it in the last year, we can return to the question of understanding: to what extent are CAM users aware that CAM's epistemic status is incompatible with EBM's, and do they differ from non-CAM users in this regard? As the reader will recall, Cant and Sharma mentioned that there was indeed a dearth of research comparing the beliefs of CAM users and non-CAM users. Fortunately, since the publication of their book, additional work in this area has appeared. Homeopathy is a good modality to focus on for two reasons: 1.) because as we can see from the above passage it is the second most commonly used form of CAM in the UK; but also 2.) because EBM maintains a uniform consensus in the belief that it does not work. Cant and Sharma suggests that CAM use is at least associated with negative attitudes toward EBM and that we can expect this to be the case in regard to public attitudes toward Homeopathy. A recent study, utilizing the 2009 Wellcome Monitor survey about beliefs surrounding homeopathy, identified the same potential for a science/anti-science duality regarding CAM that Cant and Sharma identified. It was in consideration of the growing evidence that a kind of bi-modality was developing in the population -- a polarization between pro-science and pro-CAM attitudes, which lead the researchers to propose the following question:

Do citizens adhere to a 'science versus CAM' binary opposition, or do they (at least in part) feel comfortable in supporting conventional and scientifically controversial alternative treatments simultaneously? If so, how can this apparent inconsistency be accounted for (Stoneman 2013)?

The study uses sophisticated statistical techniques (to "fit latent class models to a range of attitudinal indicators") in an effort to "identify a sub-group of the general population who maintain positive evaluations about both conventional medicine and homeopathy." In other words, to find if they could identify "a distinct group with ostensibly 'incommensurable' perspectives".

With a focus on the highly controversial case of homeopathy, we have sought to establish whether public attitudes can be characterised as falling into starkly oppositional 'camps', or whether citizens are, as in other domains, untroubled by combining apparently contradictory practices and beliefs.

They found that one third of UK residents fall into a clearly delineated pro-EBM/anti-CAM camp. However, by far the larger group identified maintained what the authors called "a 'dissonant' orientation", attitudes which simultaneously maintain "optimism about conventional medicine and science alongside use of CAM and belief in the efficacy of homeopathy." Given the dominance of the narrative of CAM being a reaction against EBM authority, callousness, etc., where the appeal of CAM is characterized as an outgrowth of dissatisfaction with EBM, this evidence suggests that the dynamics in play here are more subtle. According to this study, most of CAM's consumers are "quite comfortable in maintaining positive orientations toward both conventional and alternative forms of medicine." The researchers' explanation for the above finding, born of statistical analysis involving "a number of explanatory variables...included in a subsequent regression model to predict latent class membership" rests precisely in line with Peirce and Parsons: the public, who are largely ignorant of relevant expert scientific consensus or matters of medical epistemology, are vulnerable to pragmatically inferior, a-priori modes of fixing belief. It appears that most potential CAM consumers are simply unaware of the epistemic implications of CAM's ontological commitments:

These results raise questions about the validity of commonly advanced explanations for CAM use; that it is, in some simple manner, driven by anti-science attitudes and a rejection of conventional medicine [11,47]. On the contrary, while this may be true of some CAM users, many are clearly quite comfortable in maintaining positive orientations toward both conventional and alternative forms of medicine.

In order to account for this heterogeneity in orientation toward use of and beliefs about CAM, a number of explanatory variables were included in a subsequent regression model to predict latent class membership. This revealed that the 'dissonant' sub-group had lower levels of scientific knowledge and was less likely to have a qualification in a scientific discipline. This is consistent with the idea, then, that the public's positive evaluation of controversial CAM treatments such as homeopathy is, at least in part, due to a failure to properly understand the evidential basis of the conclusions drawn by the scientific community - that many of these treatments are not efficacious at all.

As we have seen, the narrative of CAM as an expression of badly needed medical democracy is dominant in medical sociology. Although this might well hold true for a smaller subset of the population, the bulk of the population is not in a position to make such a judgement. In fact, the epistemic confusion displayed by the bulk of the population fits with another narrative we would suggest more closely reflects the truth: the general population varies in its scientific education and overall level of credulity; however, most people do not understand what CAM is epistemically; when those who can be convinced, are drawn into CAM therapies through social networks, it is through these social contacts and interaction with CAM practitioners that they may be exposed to the ideology of CAM, including its unlikely ontological commitments and its opposition to EBM; however this is not a guaranteed outcome; while some people may then turn away or become ideological converts to CAM, many people will remain uncertain about what exactly CAM is epistemically in relation to EBM. To put this narrative into perspective we need to look again at the numbers from the Wellcome study:

We found evidence that around a third of the UK public appear to espouse what we have termed a 'conventional' orientation, in that they express optimism about and trust in conventional medicine and science, while also rejecting the use of CAM in general and the efficacy of homeopathy in particular.

If 33% of the UK population espouse a conventional, pro-EBM epistemic orientation, this leaves two thirds of the population vulnerable to non-scientific medicine. Half of these people identify as pro-CAM and understand the epistemic incompatibility between EBM and CAM. We can reasonably presume that all of those people have been to a CAM therapist in the past. 44% of the population have used CAM, 33% of the population have a pro-CAM epistemic orientation, which presumably account for three quarters of the people who have used CAM at some point in their lives. It therefore stands to reason that 11% of the population in the UK have used CAM as well, however, they were not in a position to choose CAM as a matter of medical "choice". They were simply in the dark, outside of the UK courts' understanding of the use of CAM as simply a matter of rejecting EBM. These people are innocent medical consumers with pragmatic needs, presumably, as Parsons' might suggest, going to whoever will promise them what they desperately want to hear. At large in the medical marketplace -- a medical marketplace wracked by a fractured medical contract born of the failure of professionalism -- 33% of the population does not understand the meaning of CAM, and fully one third of those people are taken in by CAM anyway, innocent victims of a pseudo-profession, and the failures of professionalism which failed to stop it.

It is clear that this research raises as many questions as it answers. However, from a network analysis perspective, the one third of the British public who constitute CAM's various forms of support, exist in a density sufficient to pose a significant threat to the "culture of fixing belief" which we see as the principle casualty of medical pluralism, after the fracturing of medical professionalism

which made CAM possible at all. Medicine involves a special pragmatic form of epistemology, one which we believe is less contentious or prone to charges of incommensurability by virtue of its intimate, phenomenological universality. This same urgent primacy to the human condition renders the fracture in the medical contract an even greater threat to our culture's communal capacity for critical thinking. If the basic elements of clinical medical proof are either doubted or not understood by two thirds of the UK population, we are in a crises of public understanding of science, one which requires that we look very closely at the epidemiology of pernicious beliefs and take a much larger, much more considered view of what CAM is doing to the very meaning of medical truth in our culture.

Sociology and Medical Pluralism: Summary

Peirce made his case about the source of the pragmatic power of the scientific community by connecting everything from metaphysics to ethical virtue and communal effort. Expertise, the product of the communal effort of scientific communities, has a pragmatic value which necessitates the development of professional structures to funnel the benefits of pragmatic power to the people that need them while protecting those people from the ravages of the ignorant, the irrational, the unscrupulous and the dogmatic. The sociology of medical pluralism, expertise, professionalism, medicine and CAM all line up with perfect clarity in regard to all of these concepts. The universalism, the authority and the ethical imperative of our pragmatic epistemic system has emerged from our engagement with sociology with no significant challenge other than that of anti-realism to realism itself.

In regard to sociology as a discipline, we will shortly revisit our concerns in this regard on Part III. For now, the reader should be aware that we find it frustrating to engage an interlocutor which issues a blanket disclaimer, affects detached agnosticism sometimes and then argues vociferously enough to be used by CAM advocates as evidence for their cause. We are unable to reconcile our frustration with sociology's failure to come to terms with CAM's lack of legitimacy. We continue to believe that if sociology is to be taken seriously in interdisciplinary contexts, it must seek to make its position, vis-a-vis its antirealism clear to interdisciplinary readers on a case by case basis. Otherwise, claims to legitimacy, which are based solely on popularity, will be misleadingly exported to interdisciplinary contexts -- where presumably, they will be interpreted as globally legitimate epistemic positions. To the extent that this is the case, sociologists are exploiting a tacit nihilism, which makes them available as "expert" advocates for any mistaken epistemic position they might choose to endorse. Intelligent design, global climate change denialism -- all of them the children of a-priori beliefs and "fashion" in the Peircian sense -- and all of them are fair game to sociologists who have chosen to throw their academic weight behind one side in a realist debate, without a sufficient

effort to disclose that: they have no capacity to make claims about the real; they deal only in social constructions.

The History of Medicine and Current Medical Epistemology, History of CAM, and the Evidence on CAM's Efficacy:

History: 1.) The History of EBM; 2.) History of CAM (TCM, Homoeopathy, Chiropractic and Osteopathy); 3.) The Metaphysical Lessons of CAM History; 4.) 20th Century Medical Science's Reaction to CAM; 5.) CAM's Continued Evolution in the UK: Statutory Regulatory Status, European Trading Standards Law and Anti-CAM advocacy; 6.) Current EBM Authorities and their Statements on CAM; 7.) Medical ethics; 8.) Summary.

1.) The History of EBM

The practice of EBM is a practice of life-long problem-based learning in which caring for our own patients creates the need for evidence about diagnosis, prognosis, therapy and other clinical and health care issues. In the EBM process, we 1) convert these information needs into answerable questions; 2) tack down, with maximum efficiency, the best evidence with which to answer them (whether from the clinical examination, the diagnostic laboratory, the published literature or other sources); 3) critically appraise that evidence for its validity (closeness to the truth) and usefulness (clinical applicability); 4) apply the results of this appraisal in our clinical practice; 5.) evaluate our performance (Sackett & Haynes 1995).

As Philip Kitcher has suggested, to understand an epistemology it can be helpful if not essential to understand the relevant history. In our case, we will start with what EBM currently *is*, by referring to the above 1995 quote from the American College of Physicians, and look backward to account briefly for a few of the most crucial objects of medical epistemology and their connection to EBM's historical narrative. We will focus on the things which help us understand the difference between EBM and CAM and the basis of these differences in professionalism, expertise, ethics and pragmatic power. The above quotation, from the American College of Physicians first issue of the journal, *Evidence Based Medicine*, in 1995, serves as the epistemic connection between the philosophy of Peirce, expertise and professionalism and the history of EBM, CAM and their interaction. The above passage illustrates the essentially inquiry based, scientific approach to pragmatic problems which rests, not coincidentally, at the heart of both our own Peircian pragmatic epistemic medical epistemology and that of EBM.

As we have mentioned before, EBM can be understood as the effort to overcome the constellation of human miss-perceptions, errors and systematic perceptual distortions which make understanding the pragmatic workings of medicine impossible without specific epistemic conventions

and technologies. Just as Peirce characterized science generally, medical science is progressive, as the community of experts build on layers and layers of inquiry to become inexorably more pragmatically powerful, by repeatedly filling in the pitfalls of human perception, with ideas and technologies designed to bridge our understanding. The essential ideas which underlay the difference between CAM and EBM, accumulated for EBM historically, and can be listed as follows: 1.) naturalism; 2.) statistics; 3.) the methodological architecture of clinical trials, including the development of double blinded RCTs; and 3). meta analysis of large groups of clinical trials (essentially a more complex and powerful combination of 2 and 3).

This is a short list. It is short because what we ultimately need to understand is the difference between EBM and CAM. The larger issues of professionalism and professional ethics are important here as well, but those things are ultimately predicated on what the quoted passage above describes as "...the best evidence with which to answer..." whatever question we may have about how to best treat the patient: in other words "efficacy". Beyond vitalism and naturalism, the point at which CAM and EBM diverge is precisely over the issue of efficacy. Now that the evidence of CAM's over-all lack of efficacy is as uncontested among EBM experts as it is, CAM and EBM's growing professional and ethical failures in regard to CAM can be understood to follow directly from their refusal to accept the evidence, or their refusal to aggressively take on a huge lobby and vociferous special interest, in light of the evidence respectively.

Still, we begin with naturalism. The metaphysical commitment to medical physicalism developed gradually as magical, spiritual and vitalistic theories were abandoned in a process that was not complete until the 19th or 20th century. Finally, the vitalism which still animates chiropractic and osteopathy was discarded entirely by biology, as 19th century mechanistic conceptions of the body gave way to modern physiology and biochemistry. It is of no small consequence that EBM would have difficulty finding the words to embrace naturalism. In the end, EBM metaphysics would be constrained by the Popperian convention of only speaking in the negative: that there is, *as yet*, no evidence for vitalistic forces like Qi or subluxations. Peirce would likely have been even more shocked by such metaphysical scepticism than Popper and Popper was known to have wished that Peirce's work had been available to him earlier in his career. Unfortunately, we believe we can trace much of EBM's reticence to condemn CAM more decisively, to the loosely Popperian positivist reticence to condemn suspect ontological commitments as a scientific warrant for rejecting theory.

As for statistics, Benjamin Rush, a prominent colonial American doctor and signatory to the Declaration of Independence, was also the first person to use British libel law in an effort to crush legitimate scientific criticism (Porter 1999). Rush's penchant for bleeding his patients was challenged on the basis of statistics collected by a local paper, comparing the deaths in bleeding and non-bleeding hospitals, in a way which made Rush appear as though he was wrong about bleeding as a universally beneficial treatment. We will shortly revisit the pernicious nature of British libel law and its role in the fight over medical epistemology, for now however, the reader should be aware that Rush won the

case. The embrace of statistics, the numerical means by which we look past our biases to see more clearly the effects of given treatments, would non-the-less become another essential element of medical epistemology.

The first RCT's or randomized clinical trials can be credited to James Lind, the Scottish royal surgeon who conducted the first one to determine the cause of scurvy on board the HMS *Salisbury* in 1754 (Porter 1999). Lind was able to see that citrus fruit was the best way to treat and avoid scurvy because the effects were dramatic enough to render potential sources of error like observation and selection bias irrelevant. But problems relating to systematic errors introduced in the selection of subjects into groups and systematic errors introduced by what the study participants were subconsciously expecting and communicating to one and other, soon became apparent in studies of more subtle phenomena. From the placebo effects of research subjects themselves, to the beliefs of study workers administering treatments to patients, who unwittingly and *subconsciously* communicate expectations to the subjects by mistake, to the vulnerabilities of observers' ratings outcomes to bias, the vagaries of human perception and bias would soon become areas of intense study themselves.

We will shortly deal specifically with two of these sources of error as they are central to our case regarding the efficacy of CAM. However, meta-analytical researchers have developed tools for rating multiple studies in terms of their openness to bias, and the resulting tools like the Jadad score for example, illustrate the full set of RCT methodological imperatives. Developed in 1996, the Jadad scale is a series of questions designed to be rapidly applied by researchers to large scale reviews of research studies (Jadad et al. 1996). The Jadad method of meta-analysis consists of 11 questions which are designed to ascertain crucial elements of study architecture, including: the areas of double blinding, randomization, subject withdrawals and drop outs, clarity of study objectives and outcome measures, the presence of a clear description of subject inclusion and exclusion criteria, statistical power based judgement of subject sample size appropriateness, the presence of a clear description of interventions, the presence in the study of a control group, the presence of a description of the method used to assess adverse affects, and finally, a description of the statistical analysis. These criteria are applied to produce a five point scale, which can be understood as a continuum from badly flawed to perfect.

There are two areas above which we believe are the most relevant to the way the clinical CAM research in particular has been misunderstood, and we will deal with those in detail. However, it is important to point out that all of the above factors matter, and that CAM researchers, historically, are prone to conducting poor studies, interpreting them poorly and using them as evidence for efficacy in various arenas including the media without qualification (Ernst & Seip 2011). The two areas we will look at in particular are however, both forms of blinding, the blinding of the participants and the blinding of experimenters.

Placebo effects are the subject of significant research and are generally defined as effects related to patient expectation, both positive (placebo) and negative (nocebo). Patients given an inert

medical treatment and told by a credible person it will help them, will often experience one or more of a range of positive effects, depending upon the mode of action they have been lead to expect.

Conversely, patients given an inert treatment and told they will experience bad effects, often experience negative effects and such effects are referred to as nocebo effects. Patients experiencing placebo effects, who are later told that they were given a placebo, usually experience a reduction in effects. A study of postoperative pain and placebo analgesia suggested that the population presents a bimodal response to placebos because not everyone responds to placebos, but those that do, respond strongly, thereby showing a significant improvement in perceived pain. Also patients who respond to placebos, respond more strongly the more significant the pain (Levine et al. 1979). This is of course consistent with what we see in CAM.

There is another effect, a characteristic of psychotherapy, which we believe is relevant here to the range of therapeutic effects associated with placebo. Psychologist meta-analysts observed that all forms of psychotherapy, from psychoanalysis to cognitive behavioural therapy, all perform exactly the same in terms of patient outcome. This finding was given a name in reference to Alice in Wonderland, "the Dodo bird conjecture", in regard to the Dodo's suggestion that "Everybody has won, and all must have prizes" (Wampold et al. 1997). Statistically powerful meta-analysis of multiple studies of psychotherapy have demonstrated an alarming fact about the way that different forms of psychotherapy compare in terms of efficacy: they do not differ at all. It appears to be a robust and stable characteristic of talk therapy that no matter what therapeutic modality is involved, as long as the therapist: 1.) believes in the efficacy of the technique; 2.) has a personality which make her an effective psychotherapist; and finally, 3). forms an affectionate alliance with the patient providing the patient with an empathetic response, while gaining the patient's collaboration, trust and motivation, then the usual therapeutic gains are realized. The similarity between this finding and the kind of personal relationship Cant and Sharma described between CAM therapists and users, and which is widely associated with CAM therapeutic approaches, is striking and relevant to a later point, but it is also essential to understanding placebo effects in terms of the trust required between the patient and the doctor in order for them to work.

At this point, it should be clear that both placebo effects and the Dodo bird conjecture, address a specific aspect of RCT methodology: blinding. Studies which fail to blind the patients as to the therapy they are about to receive, are wide open to systematic error, bias which will occur in the outcome data as a result of the patient expectations. However, placebo effects are also intimately tied to the nature of the relationship between the therapist and the patient, with the therapeutic relationship so strong, that baseline effects occur just from interaction with a caregiver, which at least in the case of psychotherapy, is far more significant than the therapy itself. The problem here is, there is no way to blind a patient from a therapeutic relationship. So the relative efficacy of a given therapy, which must be provided within the context of a therapeutic relationship, cannot be compared to a control group within an RCT, because a sham therapeutic relationship is impossible. This casts a very

significant shadow over the landscape of alternative therapies, under which the efficacy of CAM techniques cannot ever be definitively known.

Keeping these things in mind, there is one other aspect of RCT methodology which is made indispensable by another surprising human behavioural proclivity: the blinding of study workers, participants in clinical trials other than subjects. These clinical trial workers, are not necessarily the "actors" required for sham therapies required for blinding in some study designs. We are not exclusively referring to, for example, the person delivering either "real" acupuncture, or sham needles, which actually do not penetrate the skin but rather roll up in the needle like a theatrical sword -- in other words, people who must convincingly "act" in order to blind the patients in the study as to their status as either an experimental or control group participant. Rather, we are merely referring to anyone who must interact directly with the subjects at all during the course of the study. Research confirms that human research subjects are so sensitive to other people, that research workers may unwittingly convey their own expectations to research subjects, and systematically bias research outcomes. A study which recorded the pain rated by patients after oral surgery, involved patients who were told they would receive either a placebo, a narcotic, or something which would make their pain worse. The clinicians who inject the subjects IV's, were told that one group of patients was receiving narcotics or a placebo and the other group were receiving injections which would make them experience more pain -- in other words, the clinicians were made to believe that they were "in the know" about which patients were getting what, and instructed to keep this information from the patients. In reality, the clinicians were given only placebos to inject into the patient's IVs. However, the patients who were given a placebo by a clinician who thought she was injecting the patient with something to make the patient feel worse, did feel worse than the patients who the clinicians believed they were injecting with a placebo or narcotic (Gracely et al. 1985).

For the above reasons the methodological architecture of RCT's has become indispensable to the definitive determination of clinical efficacy. However, larger studies, or larger numbers of smaller studies, all other things being equal, are always more statistically illuminating than single, small studies by themselves. Sophisticated meta-analysis have solved a number of long standing medical disputes, by focusing the statistical power of multiple studies to illuminate subtle casual relationships which would have otherwise remained indiscernible. It is for this reason that meta-analysis is the third EBM element in our list of essential methodological tools which distinguish it from CAM.

In aggregate, these tools allow for human beings to overcome a host of pitfalls inherent in the way we think and interact with others, which otherwise make it impossible for us to see beyond our own biases and expectations, and into subtle shades of statistical information. These scientific tools, developed by communities of researchers and now deployed to establish the relationships between different forms of treatment and the outcomes we can expect from them under specific circumstances, form the backbone of contemporary EBM medical epistemology, expertise, professionalism and the pragmatic power which EBM exploits.

Unfortunately, as powerful as it can be, clinical research methodology bears its limitations. There is no way to blind the effects of human relationships and other forms of human interaction. There will never be a double blinded study of chiropractic techniques, acupuncture, or even homoeopathic therapies, which might occur in the context of a holistic therapist/patient relationship. All of the research ever conducted on acupuncture, chiropractic, reflexology and osteopathy, has been, at least to some extent, open to bias. It is not possible to design a clinical trial of these modalities, which is completely double blinded with a perfect Jadad score. Just as we noted with James Lind and his rudimentary, un-blinded clinical trials on scurvy, the lack of partial or complete blinding is not necessarily an obstacle to observing sufficiently dramatic effects, or even a sufficient lack of them, particularly when direct medical measurements cut out patient reporting bias. But niggling effects, marginal effects, or effects which appear significant only from powerful placebo effects, patient reporting bias, or researcher bias -- these kinds of results cannot be definitively dismissed as errors which are definitively unrelated to clinical effects, purely by the architecture of clinical methodology alone. These marginal effects may be error or they may be true clinical effects, but RCT's cannot ever definitively answer the question for us.

We have privately come to term the landscape of these methodological limitations, "the shadow", a term we will use as shorthand for this problem later in the thesis. We will also observe that CAM modalities caught in the bright light of double blinded RCT's, like Homeopathy, will claim in reaction to the complete absence of clinical effects apparent in robust clinical trials, that the *real* power of Homeopathy can only possibly be seen in the context of a close therapist/patient relationship. As we shall see, leading EBM medical authorities still entertain the possibility that some CAM modalities appear to be effective for some conditions. It is our contention that the marginal effects observed in such CAM clinical trials are exclusively the artefacts of bias, and that CAM is universally clinically inert. We will shortly review the clinical evidence on CAM and EBM's current adjudication. However, we will propose to expand EBM's current medical epistemic toolkit, to use inductive and metaphysical tools to resolve the deadlock³¹.

³¹ In this section we will rely heavily on two 2008 books written on CAM by the UK's leading clinical expert on CAM research, Edzard Ernst. A recognized EBM authority on meta-analysis and clinical research design, Ernst wrote for and edited the *Oxford Handbook of Complementary Medicine*, the most authoritative summary of clinical evidence currently available on CAM (Ernst et al. 2008). Simultaneously, Ernst worked with science writer Simon Singh to produce *Trick or Treatment*, a similar treatment of the evidence reworked for a lay audience, and with additional historical research (Singh & Ernst 2008).

2.) The History of CAM

Traditional Chinese Medicine (TCM)

We are now familiar with the sociological and anthropological framework from which CAM emerges. The specifics of just a few popular CAM modalities will serve our purposes for better understanding their status as pseudo-professions and as background for the account of recent events involving anti-cam activism, as well as other significant recent events. Lastly, we can clear up some popular misconceptions and highlight the extent to which erroneous CAM narratives find their way into the context of official decision-making, like the courts.

Traditional Chinese Medicine TCM, is often touted as an "ancient", therefore effective, form of medicine. As any pre-1950's medical anthropologist would whole-heartedly agree, the "ancient-equals-valid" claim deserves special status as a logical fallacy in medical epistemology. Uniformly, as we have seen, pre-scientific forms of medicine are shot through with religious, magical and vitalistic beliefs. Of course, as we have also seen, these beliefs do not preclude the development of useful medical technologies in tandem, however, as medical epistemologists, looking at medical practices individually, it is possible to track the use of magical, religious or vitalistic ideas when they appear as the *principle justification* for medical techniques. In such cases, ethnomedical experts tease apart the practice for underlying pragmatically utility, as in the case of some herbs -- while the shaman might see the herb as a means of manipulating spirit forces, a small number of herbs, due to the substances present in the plant, have significant pharmacological utility. There are however medical systems, in which the magical, religious and vitalistic framework dominates and the resulting pragmatic utility is very scarce. This is the case with TCM.

TCM has a rich and varied history in China and has been appearing sporadically in the West for hundreds of years. For our purposes, a historical turning point occurred in 1971 when a member of a preliminary team associated with President Nixon's upcoming 1972 delegation to China became ill and was treated with acupuncture and wrote about it in the New York Times (Singh & Ernst 2008, p. 64). What followed were a string of incidents involving dramatic demonstrations in China of now clearly suspect displays like open heart surgery with acupuncture alone as aesthetic. Later, it would become clear that these demonstrations were faked (Singh & Ernst 2008, p. 66). At the time, these events cause the sensation which undoubtedly motivated Feyerabend and Rorty to mention Chinese Medicine and acupuncture in their work. Such wonders also initiated a "massive" amount of research, unfortunately, extremely rudimentary research not any more sophisticated than what James Lind used to understand scurvy (Singh & Ernst 2008, p. 69). Unlike James Lind, acupuncture researchers were not dealing with a clearly effective clinical treatment, they were being confused by a powerful and dramatic placebo. By 1979, the World Health Organization had issued a statement summarizing the "evidence" on acupuncture resulting from the enormous flurry of research suggesting that there were

more than 20 conditions "which lend themselves to acupuncture treatment" including I (Singh & Ernst 2008, p.69):

... sinusitis, common cold, tonsillitis, bronchitis, asthma, duodenal ulcers, dysentery, constipation, diarrhoea, headache and migraine, frozen shoulder, tennis elbow, sciatica, low back pain and osteoarthritis.

Of course, putting needles in people to direct the flow of "qi" is not a treatment capable of clinical impact on any of the conditions listed above. The world's leading EBM authority had misinterpreted the research, and was profoundly mistaken. A failure of professionalism, caused, arguably, by an understandable failure of meta-analysis: to properly interpret the placebo effects occurring in the enormous body of research. We would however, note another possible interpretation of this failure -- it may have been more of a failure of metaphysics. It could be argued that the *research itself* was a terrible waste of resources and the start of a long and costly period of confusion.

The basis of Chinese Medicine depends somewhat on the "school". All acupuncture schools have some notion of "Qi", a vitalistic force which is what developed in the explanatory vacuum of the Chinese prohibition against dissection (Singh & Ernst 2008, p.70). The twelve "meridians" dictating the flow of "Qi" were likely a reference to the twelve great rivers of China and the 365 body "parts" through which "qi" can be directed, likely mirror the number of days in a year (Singh & Ernst 2008, p.70). It was not in dispute in 1970, that "Qi" has never been shown to exist and is ontologically antithetical to EBM. Doctors at the time were willing to consider that needling might be effective, but they were "highly sceptical about the existence of meridians or the flow of Chi" (Singh & Ernst 2008, p. 70). Still, the loosely Popperian prohibition against metaphysical claims regarding the non-existence of "meridians" or "Qi" appears to have won the day. The scientific speculation about the biological mechanism of action for acupuncture, which never found anything significant, was not similarly restrained. After all, this research too was understandably predicated on the idea that the use of acupuncture for pain was like Lind's citrus for scurvy -- that it was clearly, visibly, indisputably effective. There was a possibility that needling was as effective as it appeared to be, even if the mechanism suggested by the Chinese was not tenable.

By the 2000's, the number of clinical studies on acupuncture was in the hundreds, with methodological quality ranging from outright fraud in the case of many of the Chinese studies now understood as propaganda, to carefully controlled RCT's with sham needle blinding (Singh & Ernst 2008, p. 67). Meanwhile, by 2002, over half of UK doctors were sending patients to acupuncturists (Singh & Ernst 2008, p. 70). What is clear is that the more clinical trials of acupuncture approach robust levels of blinding and otherwise conform to the requirements of the Jadad scale, the more likely they are to show no effects (Singh & Ernst 2008, p. 107). To Edzard Ernst, the leading clinical researcher in the UK, who made the above observation, the findings on acupuncture in particular are "neither consistent nor convincing -- its borderline" (Singh & Ernst 2008, p. 107) Of course, we

disagree. If the Jadad scale data for all of these studies were plotted on a graph in the vertical axis, with the resulting effects of acupuncture plotted on the horizontal, we would see an obvious trend: the effects drop off, with every additional reduction in bias. The induction associated with following the slope of the line on down to where the effects would be, if an imagined perfectly blinded trial were possible and was plotted on the graph, is compelling: a study on acupuncture perfectly free of bias, would show no effects at all. This induction converges where the ontological metaphysics had lead us all along.

Homoeopathy

Homoeopathy is interesting because it is the CAM modality most vulnerable to efficacy testing through RCTs. Invented by Samuel Hahnemann in the late 18th century, homoeopathy was developed after a series of observations based on the idea that substances which cure certain ailments in sick people, cause the same symptoms as the illness, when given to healthy people (Singh & Ernst 2008, p. 119). Hahnemann then imagined what he termed the universal "law of similars": that a substance that causes symptoms in a healthy person, can be used to treat a sick person with the same symptoms (Singh & Ernst 2008, p. 120). He developed his ideas further, imagining that diluting the substance made its effect stronger, eventually happening upon the idea that the strongest remedies could be produced by diluting the solution until, functionally, *all* of the original substance was gone (Singh & Ernst 2008, p. 123). A huge variety of substances could be used, including plants, crushed insects, other live animals, and some bodily substances from human beings (Singh & Ernst 2008, p.122). Homeopaths are religiously fixated on Hahnemann's original texts and ideas (Singh & Ernst 2008, p. 121). The UK based Faculty of Homoeopathy has 1400 registered members (Singh & Ernst 2008, p. 117).

The mechanism of action is not a motivator for significant research, as in the case of acupuncture. The clear infeasibility of homoeopathy conceptually is widely recognized. The clinical research is in some respects, is very straight forward. Homoeopathy remedies, can be double blinded and thus achieve the highest levels of validity for an RCT, obtaining a judgement of efficacy, free from bias. In 1999, Ernst plotted some data from a previously published meta-analysis of Homoeopathy studies, including enough perfect "five" Jadad scores to meet the criteria for statistical sufficiency (Ernst & Pittler 2000). The resulting graph is precisely what we proposed would result from plotting the data of acupuncture on a similar graph, with one exception, the perfect Jadad scores appear to deviate from the otherwise perfect slope along which the remainder of the data sits (except the "zero" score line which did not have enough studies and can be ignored). Visible in figure 1

below, the perfect "five" Jadad scoring homoeopathy trials appear to show more effects than we would expect. Ernst summarises the likely explanation thusly:

The fact, however, that the average result of the 10 trials scoring 5 points on the Jadad score contradicts this notion, is consistent with the hypothesis that some (by no means all) methodologically astute and highly convinced homeopaths have published results that look convincing but are, in fact, not credible. Viewed in this way, the re-analysis of Linde et al. [1] can be seen as the ultimate epidemiological proof that homeopathic remedies are, in fact, placebos.

This is important because it mirrors the fakery inherent in the Chinese acupuncture studies and suggests that the meta-analytic picture, even with the fakes can still be interpreted in a definitive way.

Figure 1:

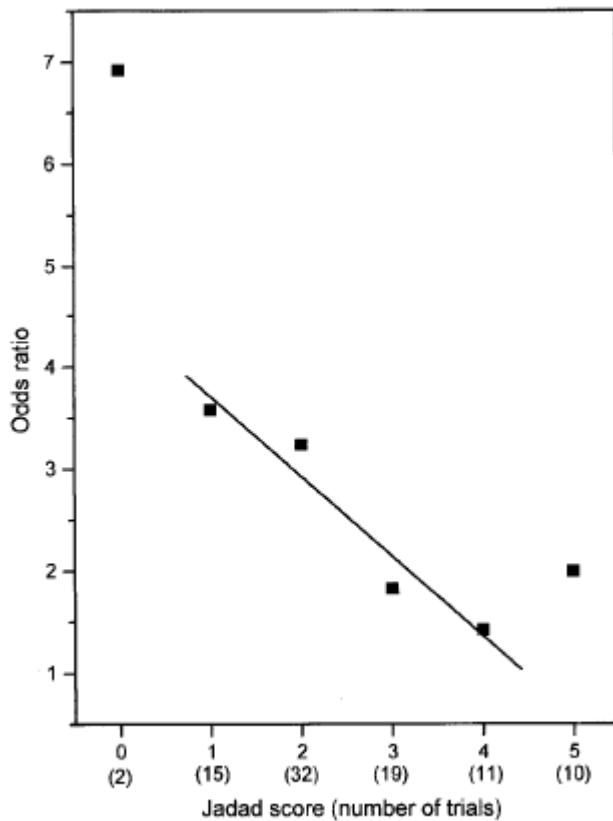


Fig. 1. Relation between quality score and odds ratio in controlled clinical trials of homeopathy.

It is important to acknowledge that Ernst is not an ideologue. Ernst takes a very carefully crafted, open minded, even generous view of Homeopathy, characteristic of EBM's current position on these matters. We will shortly be criticising Ernst's position regarding metaphysics as a warrant for rejecting Homoeopathy. In regard to potential breaches of professionalism, the history of

homoeopathy mirrors acupuncture in two important ways: 1.) it was another medical modality which presented itself as having dramatic effects; and 2.) the quantification of said effects was complicated by poor quality studies and purposeful fraud. Although fraudulent Chinese propaganda may have made been pivotal in the uptake of acupuncture in the West, this is far from certain. In both cases, it is plausible that research fraud is only a contributing factor, and that honest misperception, expectancy and placebo effects would have been enough to sway EBM authorities and make these modalities the hugely successful objects of intensive clinical research they have become. What is different for homoeopathy, is that needling is at least plausibly an endocrine stimulant, so that one could argue that there was a case to be made that although "Qi" was clearly not plausible, but the effect might be due to something else like the stimulation of natural opioid release. Homoeopathy is ultimately just a matter of giving people pure water with nothing in it. There was no case for some undiscovered mechanism other than placebo effects. This has important implications for the adjudication of EBM reactions to homoeopathy, and suggests there is a greater failure of professionalism here.

Chiropractic and Osteopathy

We will present the histories of Chiropractic and osteopathy simultaneously, not only because they are similar, and that they have both achieved statutory regulatory status in the UK, but also because in the United States, osteopaths have made the transition to EBM, arguably, completely, and are now indistinguishable from EBM and as such, no longer exist as CAM, while in the UK they have retained their CAM status. Furthermore, these are very similar modalities, and it has been suggested "...that both osteopaths and chiropractors engaged in a greater cross-fertilization of ideas and techniques than most of them have cared to admit" (Baer 1987).

Both modalities begin in the late 19th century, each with a single charismatic figure, espousing a pre-germ-theory mechanistic explanation for literally *all* disease, "who claim divine revelation as the source of their medical doctrines" and combined what was essentially a form of bone setting with their shared interest in "magnetic healing" (Baer 1987). Andrew Taylor Still, (1828-1917) the inventor of Osteopathy was a doctor who, failed to save three of his children from meningitis, and perhaps as a result, never accepted germ theory (Baer 1987). Subsequently, he rejecting the opportunity to continue in medicine, and commenced to fail in the effort to persuade his local medical school to go along with his unorthodox ideas. Finally, he opened his school of osteopathy in 1892. It is worth quoting Still's 1897 autobiography to get a feeling for: 1.) the

mechanistic nature of his ideas; 2.) the clear intention that his system would be a panacea; and 3.) the profound implausibility of what Still had in mind, in terms of current medical epistemology³² (p. 107).

I have concluded after twenty-five years' close observation and experimenting that there is no such disease as fever, flux, diphtheria, typhus, typhoid, lung-fever, or any other fever classed under the common head of fever. Rheumatism. sciatica, gout, colic, liver disease, nettle-rash, or croup, on to the end of the list of diseases, do not exist as disease. All these separate and combined are only effects. The cause can be found and does exist in the limited and excited action of the nerves only, which control the fluids of parts or the whole of the body. It appears perfectly reasonable to any person born above the condition of an idiot, who has familiarized himself with anatomy and its working with the machinery of life, that all diseases are mere effects, the cause being a partial or complete failure of the nerves to properly conduct the fluids of life.

Still's autobiography includes many accounts of miraculous cures as well as on page 113, the vindication he felt after sceptics who initially felt he was crazy, came back to him, after he had opened his osteopathic school, realizing "There's money in it, I want to study osteopathy!"

Daniel David Palmer, was not a doctor, but began conducting chiropractic treatments in 1895, claiming to have first cured a man of deafness, by ridding him of "subluxations" which were interfering with neural transmission, thus returning the normal function of "nerve force" and apparently, his hearing (Baer 1987). He claimed such spinal adjustments could restore the functioning of "various bodily organs" (Baer 1987). He opened the Palmer Infirmary and Chiropractic Institute in 1897, which grew into "one of the largest institutions that trained health practitioners in the United States, graduating its first 'One Thousand Class' in 1921" (Wardwell 1982:215)" (Baer 1987).

During their time, both Still and Palmer were rejected by mainstream medical authorities, who were, in contrast, locked in the process of absorbing germ theory and what would be an avalanche of pragmatically powerful medical advancements. There can be no doubt that massive improvements in the pragmatic power of medicine took place within the lifetimes of both Still and Palmer. It is crucial to recognize that these men, in so much as that their ideas were rejected by mainstream medicine at the time, were fundamentally pseudo-professionals by virtue of this. They failed to participate in the community of medical scientists who would produce such pragmatically significant breakthroughs during this time. They existed outside of the Peircian community of scientific practice. As such, they were neither legitimate experts nor legitimate professionals.

The commonalities between these modalities, both panaceas, both with claims to divine revelation, both depending on vitalism and elements of magnetic therapy for mechanisms of action, were indisputably a step backward and away from the Peircian community of medical scientific experts of the day. In hindsight, it is clear that these men were not eccentric scientists or visionaries,

³² Andrew Taylor Still's autobiography has been graciously made available electronically by the openlibrary.org and was retrieved from the following location on September 18, 2014: <https://archive.org/stream/autobiographyand00stilia#page/106/mode/2up>

they were precisely a medical crank and a quack respectively, or something in between, with no pragmatic value to their theories at all. But we know this now, due to the meta-analytical evaluation of huge volumes of clinical testing, which the medical establishment of Still and Palmer's time did not have as a reference. The medical authorities of that time however, excluded osteopathy and chiropractic as non-scientific anyway, on the basis of something else -- something other than robust clinical evidence. Something which was not only a part of the communal consensus of scientific medical experts at the time, but something which would be tragically de-emphasised in medical epistemology by later developments in the philosophy of science of the 20th century: metaphysics.

The fundamentally mistaken nature of osteopathy and chiropractic, will not, as it turns out, be enough to stop it from spreading both independently of and later in the context of a certain level of acceptance by, mainstream scientific medicine. The vitalistic, panacea nature of these modalities would in fact persist in their practice, right up to the present time, to a much greater extent than Cant and Sharma would naively suggest. Central to our argument, the mistaken core of both of these modalities and the associated pseudo-professional values which have evolved by necessity to keep these erroneous modalities alive in the context of continuing, evolving challenges from medical science, will remain the only thing which distinguishes these practices from the physiotherapy or medicine they will, to varying degrees on both sides of the Atlantic, come to resemble. In the case of both of these therapies, more than acupuncture and homoeopathy, we must carefully consider what it means for a fundamentally mistaken, pseudo-professional medical system, to struggle to conform to, to superficially copy, to illegitimately co-opt the identity of EBM in certain respects for some clients, while simultaneously holding on to, in various degrees, a profoundly pseudo-professional, in some cases even quasi-religious identity at the same time. We believe that questions relating to ethics, fraud, negligence, and patient consent, flow from this process. But perhaps more importantly, we will ask if the professionals already charged with Parson's mission to safeguard the interests of the sick and other medical consumers, medical and state authorities, could, or should have done things differently, at least from the time of the Flexner report³³, early in the 20th century, forward to the present, in regard to osteopathy and chiropractic.

3.) The Metaphysical Lesson of CAM History

One of our primary criticisms of EBM's current position, will turn on what we identify as the tendency for EBM to exaggerate the epistemic utility of the goals of clinical efficacy testing, by

³³ The publication of the Flexner Report in 1910, was the single most profoundly influential event in the modern history of North American medicine. Published under the auspices of Carnegie Foundation, the report strongly criticized the state of medical education for its entrance and graduation standards and lack of adherence to scientific knowledge, resulting in half the existing schools closing or merging as a result.

attempting to adopt the idea that clinical trials are the *only way* to consider efficacy as a kind of global epistemic position. Late 19th century medical authorities initially rejected Osteopathy and Chiropractic on ontological grounds, and we think this was, and is, all the justification required. However, medical authorities in the 20th century, would be at least partially swayed by the argument that ontological evaluations of medical modalities are irrelevant to evaluating efficacy. Edzard Ernst recently summarized this position by suggesting that the "sheer oddity" of questionable modalities is "irrelevant" because the critical test is not how "bizarre" the modality is, but "whether or not it is effective" (Singh & Ernst 2008, p. 131). This idea, that efficacy can only be determined by testing, and that metaphysics is not a consideration, is not only inconsistent with the evaluations of medical authorities in the late 19th century, but inconsistent with the enduring tendency to consider the face value legitimacy of the "underlying theoretical basis" of medical treatments which is common to medicine both historically and today. In fact, as we shall see, the conflict between the very narrow idea Ernst articulates above and the enduring medical tendency to use more nuanced metaphysical scientific judgements about the "scientific basis" of a modality is readily visible in a BMA publication about CAM in 1986 and in current NHS web pages addressing CAM and its efficacy today. The narrow view, on the other hand, makes perfect sense as an *intra-disciplinary* methodological rule. A clinical researcher today, when working *only* as a clinical researcher, is well advised to keep to such constraints. However, in one of the more profound lessons of this thesis, when, like sociological anti-realism, such a discipline-specific methodological convention is extended as a global epistemic *world-view*, a disastrous philosophical mistake is realized. When considered as a global epistemic medical claim, Ernst's position here is quite *philosophically* untenable. The unmistakably erroneous nature of osteopathy, chiropractic, homeopathy and acupuncture *are* relevant to medical epistemology -- because as Peirce observed -- metaphysics *is* continuous with science. Ironically, we have finally arrived at the point that sociologists are so prone to prematurely identifying: the point at which we can identify that the practice of science is flawed by an excessive ideological commitment to a mistaken, anti-metaphysical misunderstanding of Popperian positivism, characteristic of mainstream scientific discourse. Surprisingly, we find ourselves in agreement with what has become a trope for sociologists studying science: that EBM's naive positivism has indeed lead it to make a dire mistake. However, very much to the contrary of sociology's usual conclusions about EBM and medical pluralism, in our view, EBM is not currently strident *enough* in opposing the glaring absurdity of CAM on metaphysical grounds.

4.) 20th Century Medical Science's Reaction to CAM

The Flexner report was a watershed moment in American Medical education. Published in 1910, the Carnegie Foundation report identified low standards and a weak commitment to scientific

knowledge in many of American medical schools and half of existing schools closed or merged within a decade. The production of American doctors went down, a scarcity of doctors resulted, the status of doctors went sharply up. Since by the late 30's, osteopaths had already adopted mainstream medical doctor training program curricula, it was natural for them to move into the vacuum in rural medicine left by the disappearance of lower status medical schools (Baer 1987). Chiropractic moved in to take up the musculoskeletal complications that the osteopaths abandoned for general practice. It is in this context that Osteopathy and Chiropractic dealt with the continuing scepticism of mainstream, scientific medical authorities, toward these alternative modalities.

In the first half of the 20th century, neither Osteopathy nor Chiropractic had any standing with mainstream medical authorities (Baer 1987). Often referring to both as quacks, this kind of criticism from medical authorities prompted public attacks on mainstream medical authorities by Palmer himself (and later his son), who, while not having been doctors previously like Still, were full throated in their attack on mainstream medicine, setting an adversarial, anti-medical tone which is still reflected in Chiropractic's culture today (Baer 1987). Medicine, for its part, responded with a series of systematic attacks on chiropractic and osteopathy, designed to "destroy or restrict their practice" including (Baer 1987):

Opposition to licensure or campaigns to restrict practice rights, denial of membership in local medical societies, refusal to extend admission privileges to allopathic hospitals, and prohibition of professional cooperation between M.D.s and heterodox health practitioners have all been used in this effort.

Osteopaths, likely through their less obstreperous attitude toward medicine had managed to procure the right to practice in many states and avoided the frequent arrests which plagued the chiropractors of that period for practicing medicine without a license (Baer 1987). Osteopaths were confined to "marginal hospitals" or were kept out entirely by the medical establishment. By the 50's the AMA was officially opposed to osteopathy and chiropractic. In 1938, the AMA made it a professional offense to consult with either and prevented them from serving as doctors in the armed forces (Baer 1987).

By the end of 50's, osteopaths had won the right to at least practice in most states in spite of the efforts of the AMA. By way of the path of least resistance, the AMA then sought to absorb osteopathy, offering them access to medical hospitals for internships and residencies, in a long term strategy of "co-optation" so successful, osteopaths fought to avoid the wholesale absorption of their profession (Baer 1987). Osteopathic education became nearly identical to, if lower status than, mainstream medicine. To this day in the US, "osteopathic physicians" as they are currently know, suffer from a pronounced difficulty to identify what exactly makes them different from EBM, choosing essentially to focus, questionably, on the value of vague concepts like osteopathic "manipulation therapy" to attempt to distinguish themselves. It would be a fair characterization to

suggest that osteopathy has functionally died in the US, with its only remnant a relatively harmless fixation with manipulative therapy.

Chiropractic in the US grew rapidly in spite of AMA efforts to stop it. Early numbers are not available, but by 1982, there were 25,000 chiropractors employed in the United States (Baer 1987). There were no new chiropractic colleges started between 1941 and 1972, however the end of that decade saw six new schools, bringing the total number to 17. The state granted chiropractors a long string of success in the 20th century, including (Baer 1987):

(1) coverage for chiropractic care under Workmen's Compensation in all 50 states and the District of Columbia, (2) granting of sick leave based upon certification from a chiropractor for Federal Civil Service employees, (3) allowance of federal income deductions for chiropractic care, (4) coverage for chiropractic under Medicaid and Medicare in nearly all states, and (5) funding to the chiropractic profession from the National Institutes of Health for research on musculoskeletal complications. Furthermore, in 1974 the United States Office of Education authorized the Council on Chiropractic Education to act as the official accrediting agency of chiropractic colleges.

The success of chiropractic is particularly surprising given the posture of uniform negativity with which the AMA has opposed chiropractic. "Incompetent quacks and cultists" famously characterises the AMA's position (Baer 1987). Doctors commonly provoked local law enforcement to prosecute chiropractors for practicing medicine without a license. The AMA can reasonably be said to have struggled mightily against the successful lobbying efforts of chiropractic in the name of scientific medicine, until the AMA lost an anti-trust suit in 1981, which forcibly reversed the AMA's previous commitment to sanction doctors who worked with chiropractors.

In stark contrast to osteopathy, chiropractic in the US has maintained its original anti-science attitude, complaining that even the teaching of the basic science curriculum required to help students pass state licensing requirements was threatening to "medicalize" the "profession" by forcing it to accept the "scientific medicine model" (Baer 1987). Such purists, called "straights" in the chiropractic world, in contrast to "mixers" who claim to have incorporated certain scientific medical principles into their practice, are not an indication that there is a spectrum of legitimate medical professionalism emerging from chiropractic culture. It was 1969 before the major chiropractic organization in the US were willing to state publically that "that subluxation was not the sole cause of nearly all disease, as D. D. Palmer had claimed" (Baer 1987). Chiropractic continues to function entirely outside medicine, as "the antithesis of medicine" (Baer 1987). The chiropractic curriculum has not conformed to medicine to the extent osteopathy has in the US, and as such, chiropractic continues unlike osteopathy in the US, to be a pseudo-profession, conforming to medicine only superficially.

In the UK prior to the 1990's, chiropractic was similarly rejected by the BMA. Osteopathy however, unlike its fate in the US, was also similarly rejected, losing their lobbying efforts in the 30's, for formal recognition. A permissive regulatory atmosphere in the UK allowed for the expansion of

CAM practises, including chiropractic and osteopathy, as a parallel medical system, unimpeded by the arrests for practicing medicine without a license that occurred in the US. Volleys of letters in the British Medical Journal between BMA members, the osteopathic college and advocates from 1946, suggest an ongoing argument which is a microcosm of the metaphysical debate over CAM, with some suggesting of osteopathy that the "the basic principles are erroneous", while osteopathic advocates already arguing that concerns over "basic principles" should be subordinated to "good results"³⁴.

The watershed moments in the UK interaction between CAM and EBM begin with the 1986 BMA publication of "Alternative Therapy" which was, no doubt, intended to be the final scientific word on CAM from the BMA (Association & others 1986). It is a deeply sincere treatise on the history of medicine and its warrant for the authority of scientific medicine in medical epistemology. It is also a sole-searching account of the perceived need for more attention to narrative medicine which the authors speculated might explain some of the attraction to CAM. Unfortunately, the metaphysics in their work reflects a fundamental confusion, in that the authors attempt to maintain an openness to any therapy and adjudicate therapies only on the basis of efficacy while simultaneously professing to embrace naturalism and reject therapies on a metaphysical basis at the same time.

There are a number of points in "Alternative Therapy" where the authors betray their commitment to naturalism. At the start, disturbingly, they observe an "underlying hostility to technology and science", they worry about the irrational demand for "instant cures for the currently incurable diseases of mankind" and the "ill-founded suspicion that nothing is being done to attack these problems" (British Medical Association, 1986, p. 4). This is not just a relevant observation, but it serves to contextualize a moment when the authors vaguely give voice to fears that the public could be on the verge of a "turning back to primitive beliefs and out-moded practices, almost all purposeless and without a sound base, however well meaning." The authors begin their history of medicine with the claim that by the time of the birth of Hippocrates, medicine had already "largely discarded the concepts of magic and religion" (British Medical Association 1986, p. 6). The authors note accurately that some alternative modalities like chiropractic and "classical acupuncture" (TCM) "stem from particular beliefs about the nature and causation of disease" (British Medical Association 1986, p. 35). The authors observe that such practitioners "reject the views of orthodox medicine" and as such can be said to offer "alternative medical systems." The authors then go on to assert: "These systems are incompatible with the corpus of scientific knowledge, and must be rejected by anyone who accepts the general validity of the latter." The authors tell us that science itself has "become inevitably and increasingly separated from doctrines embracing superstition, magic and the supernatural" (British Medical Association 1986, p. 61). At this point the author's tone becomes more emphatic (British Medical Association, p. 62):

³⁴ The British Medical Journals are archived here and were retrieved from the following URL on September 18, 2014:
<http://www.ncbi.nlm.nih.gov/pmc/articles/PMC2054114/pdf/brmedj03796-0028d.pdf>

Thus, herein lies the first and most important difficulty that orthodox medical science has with alternative approaches. So many of them do not base their rationale on any theory which is consistent with natural laws as we now understand them.

It is "simply not possible" the authors claim, to "accept" the principles of homoeopathy. They go on to indict the theory behind homoeopathy again, pronouncing that there is "no evidence for this belief" (British Medical Association, 1986, p. 66). They indict the vitalism of Osteopathy and Chiropractic in the same global fashion, pronouncing that they could "detect no physio-pathological basis for these theories" (British Medical Association, 1986, p. 67). There can be no doubt about the visceral level of commitment to naturalism the authors felt they need to convey.

Unfortunately, the authors also committed themselves to agnosticism in this regard, when considering efficacy. If a "prima facie" case for a treatment can be made, the authors tell us:

...orthodox medicine will not exclude a treatment because its mode of action is not understood. There is thus no logical class of 'alternative therapies': there are only therapies with and without good evidence for their efficacy (British Medical Association, 1986, p. 27).

The above passage seems remarkably clear in categorically dismissing all the metaphysical talk elsewhere in the book. This is precisely the argument we observed Ernst articulating when he suggested that the "bizarre" nature of CAM modalities was irrelevant to the evaluation of efficacy. Clearly contradictory to the huge volume of tacit and explicit scientific medical metaphysics apparent from the earlier quotations we selected from the same text, this argument has been the source of significant epistemic confusion for medical science in 1986 and it continues to be a source of the same confusion for EBM now. Scientific medicine, at both these points in time, appear to be struggling with what they instinctively know to be part of their tacit, scientific metaphysical, Peircian DNA, and the way that such metaphysics inherently contradicts the mistakenly global application of such a narrow, clinical methodological rule.

5.) CAM's Continued Evolution in the UK: Statutory Regulatory Status, European Trading Standards Law and Anti-CAM advocacy

The Osteopaths Act of 1993 provided for the creation of the General Osteopathic Council (GOsC). This requires the council to publish a code of practice, and protects the name "osteopath", by requiring that the only people who can use the name "osteopath" are those registered with the authority. This legislation was followed by the Chiropractors Act of 1994, which was very similar. The passage of these laws was greeted with enthusiasm by the respective CAM communities and capped decades of lobbying for such recognition. There were however, unforeseen consequences associated with these victories, including problems associated with additional conventions of *real*

professionalism, which were now operative in ways that these two pseudo-professions would find in significant conflict with their previous modes of operation. The most important of which would prove to be a requirement for sufficient clinical evidence to be produced in support of all medical claims to efficacy -- a requirement which would find its way into the mandatory Codes of Conduct statutorily required by both of the new acts.

On another front, new European trading standards regulations had been in development for some time and finally came on line, in the UK, in 2008. This law also had ramifications for proof of clinical efficacy which were detailed with considerable clarity and clearly applied to what CAM practitioners were able to say to the public in their advertising. The Consumer Protection from Unfair Trading Regulations 2008 legislation is designed to prevent advertising which "materially distort the economic behaviour" by "appreciably" impairing "the average consumer's ability to make an informed decision thereby causing him to take a transactional decision that he would not have taken otherwise". This goes for messages which contain "false information and is therefore untruthful" and which might mislead the consumer about the "benefits" of the product or "results to be expected from use of the product".

Oddly, the law is normally "enforced" only by the Advertising Standards Authority (ASA), a self-regulatory organization operated and paid for by the advertising industry itself. However, the ASA has ruled on a large number of CAM advertising cases including cases of Chiropractors referring to themselves as "doctor"³⁵. This opportunity has motivated an activist movement to search for and complain about erroneous CAM claims to efficacy, in part facilitated by groups including the Nightingale Collaboration, a web based clearinghouse for information facilitating the filing of such complaints about CAM advertising with the ASA. The Nightingale Collaboration keeps a running tally of its success against CAM with the ASA and with complaints to the Medicines and Healthcare products Regulatory Agency (MHRA), all of which are available on their web site³⁶.

As we have mentioned, in 2009, Edzard Ernst published his meta-analysis on CAM's clinical efficacy in two books, the one that was published for the lay market, which we have used extensively, was written along with Simon Singh (Singh & Ernst 2008). One of the outcomes of the publication of the book, was a Guardian article, by Singh entitled "Beware the Spinal Trap" which mentioned the panacea nature of chiropractic beliefs and referred specifically to claims that chiropractors "can help treat children with colic, sleeping and feeding problems, frequent ear infections, asthma and prolonged crying" and referred to such claims as "bogus"³⁷. Singh was later sued for libel by the

³⁵ This is a web page listing information on the ASA adjudication of The Chiswick Chiropractic Clinic at the following URL ad was retrieved September 14, 2014:

http://www.asa.org.uk/Rulings/Adjudications/2013/12/The-Chiswick-Chiropractic-Clinic/SHP_ADJ_240760.aspx#.VF_BYvI_vpg

³⁶ The Nightingale Collaboration's web site is available at the following URL

<http://www.nightingale-collaboration.org/>

³⁷ A Guardian web page with the article was retrieved from the following URL on September 15, 2014:

British Chiropractic Association, an action which attracted support for Singh from the British charity Sense About Science, and ultimately resulting in a court victory for Singh. One additional outcome of the intense backlash directed against the actions of the British Chiropractic Association involved an activist movement against Chiropractic dubbed "quacklash".

In 2009, the GCC, the chiropractic statutory regulatory authority Code of Practice required that all members hold evidence for any claim they make. Complaints to the council about potentially fraudulent claims to efficacy, would trigger demands for the evidence from the offending chiropractor and possibly a judgement against them. Exploiting this rule, and in response to the Singh case, two anti-CAM activists independently wrote automated computer programs to find spurious claims on chiropractic web sites and file hundreds of claims against the offending practitioners with the statutory regulatory board in a single day. The blog of the science journal *Nature* published an account of the incident including a leaked copy of an email sent from the chair of a leading chiropractic advocacy group, the McTimoney Chiropractic Association, to all of its members³⁸. The email read in part:

Dear Member

If you are reading this, we assume you have also read the urgent email we sent you last Friday. If you did not read it, **READ IT VERY CAREFULLY NOW** and – this is most important – **ACT ON IT**. This is not scaremongering. We judge this to be a real threat to you and your practice.

Because of what we consider to be a witch hunt against chiropractors, we are now issuing the following advice:

and

The target of the campaigners is now any claims for treatment that cannot be substantiated with chiropractic research. The safest thing for everyone to do is as follows.

1. If you have a website, take it down NOW.

and

1. If you use business cards or other stationery using the 'doctor' title and it does not clearly state that you are a doctor of chiropractic or that you are not a registered medical practitioner, **STOP USING THEM** immediately.

and finally,

<http://www.theguardian.com/commentisfree/2008/apr/19/controversiesinscience-health>

³⁸ Nature's blog page covering the incident was retrieved here on September 15, 2014:

http://blogs.nature.com/news/2009/06/chiropractic_group_advises_mem_1.html

IF YOU DO NOT FOLLOW THIS ADVICE, YOU MAY BE AT RISK FROM PROSECUTION.

Another unfortunate disclosure divined from the text of this email, involved evidence of an institutional commitment on the part of chiropractic to keep all of this a secret. This presented itself in the form of explicit instructions to keep the nature of the crisis a secret from patients: "Finally, we strongly suggest you do NOT discuss this with others, especially patients,..." Naturally, the email also warned that the information must also be kept from the public at large as well: "Most importantly, this email and all correspondence from the MCA is confidential advice to MCA members alone, and should not be shared with anyone else." Given the dearth of data about what chiropractors actually tell their patients, as well as their institutional history as anti-science and pseudo-professionals, we believe, like the journal *Nature*, that this very rare glimpse into the world of chiropractic intentions is extremely important. There can be no doubt that one of the primary rules of professionalism -- honesty -- whether in the form of patient consent or in good scientific practice, has been exploited as a fundamental deception by CAM's pseudo-professional culture, as an integral part of its fundamental make up. The issue of CAM's ontological commitments is now substantially confused by the way CAM has elected to actively *hide* relevant aspects of both its theoretical and evidentiary basis. As we have seen in the sociological data on the way CAM researchers hide their more bizarre beliefs from their EBM colleagues, as well as from the McTimothy email, the culture of CAM is deeply mired in secrecy and deception.

It should also be clear precisely how overly optimistic Cant and Sharma were when they suggested that CAM generally and chiropractic specifically, were "moderating" their "knowledge claims" prior to 2009. But far more importantly, it must be understood that the "moderation in knowledge claims" which have become so dramatic in the last five years are absolutely not a product of evolving internal chiropractic beliefs. Chiropractic is being acted upon by outside forces which it perceives as utterly hostile, as the "witch hunt" quotation above was selected to illustrate. There is no evidence to suggest that chiropractors are actually changing their beliefs about anything, other than what is required to avoid prosecution.

The lack of information about CAM practitioner beliefs and attitudes has been acknowledged by other researchers. One 2011 study attempted to get at the attitudes of UK osteopaths by collecting and thematically analysing public documents like editorials, letters, forum threads and essays from 2003 to 2009 (Humpage 2011). This is a useful time frame as the data was collected just prior to "Quacklash". The study found that the primary thematic concept which emerged from this analysis, was "...uniqueness,' relating to respect for osteopathic principles and [the original teachings] of Andrew Taylor Still, and the profession maintaining autonomy from mainstream medicine;..." This is a profoundly worrying finding, because it suggests that modalities like chiropractic and osteopathy in the UK still cling to the vitalistic, pre-germ theory ideas upon which their pseudo-professions were

predicated. This issue is extremely important in the context of current EBM authority beliefs about CAM. There are those who argue from within EBM, similar to the way Cant and Sharma did, that CAM is becoming increasingly "scientific". The evidence, scant though it is, profoundly contradicts this optimistic assumption.

Perhaps the single most well documented harm associated with anti-science CAM beliefs are the increasing disease outbreaks associated with the anti-vaccination movement (Ernst 2001). The social networks of CAM practitioners are clustered as we have seen from the sociological data. Vaccine refusal, resulting from CAM ideology, mirrors CAM practitioner density in the geographic clustering of refusals that result in outbreaks (Omer et al. 2009). One study clearly demonstrated that children whose parents take them to CAM practitioners, are less likely to be vaccinated and "were significantly more likely to be diagnosed with a vaccine- preventable disease" (Downey et al. 2010). Anti-vaccination is now a well documented form pernicious idea transmission, to be accounted for in the context of the study of the unique and sometimes counter intuitive dynamics of pernicious health belief transmission (Tanaka et al. 2009). The new and continuing occurrence of vaccine preventable disease outbreaks is not only a serious harm for which there can be no doubt about CAM's responsibility, it is another indication that Cant and Sharma were mistaken, that CAM is not "moderating its knowledge claims", that pernicious beliefs still reside at the heart of CAM practice and that the transmission of these beliefs to the surrounding community is ongoing.

We do not think that the difference between negligence and fraud matters to our case for CAM anti-professionalism. However, the question perhaps deserves one caveat. There is evidence that chiropractors frequently espouse the belief that chiropractic cannot be vindicated by clinical testing, even though it is actually effective (Association & others 1986, p. 71). If this is truly what they believe, then the "witch hunt" notion could be read another way by CAM ideologues: that EBM has the evidentiary picture wrong, and that chiropractors are being unfairly persecuted. This belief is mistaken, but does bear on judgements regarding negligence and fraud. Whether holding such beliefs is negligent or not, at least currently in the courts depends on the interpretation of the conceptual legitimacy of the modality in question. As we shall see, there are specific provisions in the UK courts' position on CAM, which allow that if the court can be persuaded that the entire conceptual basis of a CAM modality is untenable, then the beliefs endemic to that medical system's ideology may be considered negligent. In this case, we believe the common CAM belief that CAM efficacy cannot be tested by RCTs is so profoundly mistaken, that it should not be a defence against negligence or fraud.

Fraud connects directly to the way in which medical professionalism in the wider medical marketplace protects people with prohibitions against using the term "doctor" without actually being an EBM professional. Any remaining doubt that chiropractors continue to appropriate the term "doctor" in violation of the law in the UK, as a matter of culture wide convention, should now be dispelled by the text of the McTimothy emial. Illegally appropriating the word "doctor" is essential to

understanding chiropractic as a pseudo-profession, regardless of the sense in which this deception could, in some ways, be considered either a kind of wilful negligence or fraud.

Elements of pseudo-professionalism are under great pressure -- are clearly being impacted, by the changing legal environment in conjunction with anti-CAM activism, which is arguably reducing the opportunities for some modes of transmission of the mistaken medical ideas CAM continues to offer for sale. However, we also believe that the social networks along which CAM transmits its fundamentally anti-science anti-EBM messages is still largely intact, along with other fundamental elements of specific forms of CAM's ideology. Furthermore, the medical contract -- Parsons' basis of medical professionalism -- is badly damaged by CAM's continued pseudo-professional actions. It is for this reason we turn to EBM once again -- to EBM and their current position on CAM. EBM is the professionally responsible party here. We will now analyse their output on CAM and the nature of the medical pluralism question.

6.) Current EBM Authorities and their Statements on CAM.

NHS

The NHS web site contains information on various CAM modalities. We have selected a passage on osteopathy to quote at length, but will characterize the statements on chiropractic, acupuncture, and homeopathy in keeping with the examples we have used previously³⁹.

Osteopathy is based on the principle that the wellbeing of an individual depends on their bones, muscles, ligaments and connective tissue functioning smoothly together. Osteopaths believe their treatments allow the body to heal itself. They use a range of techniques but do not use drugs or surgery.

Most people who see an osteopath do so for help with back pain, neck pain, shoulder pain or other problems related to muscles and joints. Some osteopaths also claim to treat a wide range of health conditions, including asthma, digestive problems and period pain.

The meaninglessness of the first paragraph is striking. The meaning of the second paragraph is unclear and, unfortunately, only makes sense in the context of this final quotation which attempts to answer the question: "Does osteopathy work?"

There is good evidence that osteopathy is effective for the treatment of persistent lower back pain. The National Institute for Health and Care Excellence (NICE) recommends it as a treatment for this condition.

³⁹ The NHS web page on Osteopathy was accessed at the following URL on the 16th of September 2014: <http://www.nhs.uk/Conditions/Osteopathy/Pages/Introduction.aspx>

There is limited evidence to suggest it may be effective for some types of neck, shoulder or lower limb pain and recovery after hip or knee operations.
 There is no good evidence that osteopathy is effective as a treatment for health conditions unrelated to the musculoskeletal system (bones and muscles).

We were unable to locate the NICE review apparently responsible for the adjudication on efficacy here. There is nothing new to understand from this web page, unless of course, it is that there is no information about what exactly an osteopath might actually do to a patient in order to realize the pain reduction effects described above.

The NHS page on chiropractic is similarly evasive although it does disclose that chiropractors "use their hands to treat disorders of the bones, muscles and joints"⁴⁰ Judging the evidence that Chiropractic is effective in treating back pain as "good" and the evidence that spinal manipulation may be useful for treating other musculoskeletal conditions as being "poor quality", the page does state more emphatically:

There is no evidence that treatments offered by chiropractors are effective for other conditions.

There is also no scientific evidence to support the idea that most illness is caused by misalignment of the spine.

The above statements are in stark contrast to what chiropractors were telling the public five years ago. However, we would pose the following question regarding the NHS pronouncements on these modalities: what is the difference between physiotherapy, an ostensibly EBM area of expertise, and either osteopathy or chiropractic? If the NHS itself is unable to articulate an answer to that question, this, in itself, suggests a potential failure of EBM professionalism.

The NHS acupuncture page suggest both that acupuncture is an "ancient Chinese medicine" and that "western medical acupuncture is the use of acupuncture after a proper medical diagnosis" and that it is "...based on scientific evidence that shows the treatment can stimulate nerves under the skin and in muscle tissue"⁴¹. This web page deftly avoids the ontological problems of TCM, and reduces needling to the discovery of an "ancient" entho-medical technological method. As we mentioned before, "old" is not a warrant for efficacious. Disturbingly, the page suggests that acupuncture is used "to treat a wide range of health conditions" including headache, lower back pain, osteoarthritis, infertility, anxiety and asthma. The page adjudicates efficacy by suggesting that NICE only recommends "considering" acupuncture for "chronic lower back pain, chronic tension-type headaches and migraines" adding, "NICE makes these recommendations on the basis of scientific evidence." The page goes on to claim that there "... is also some evidence that acupuncture works for a small number

⁴⁰ The NHS web page on Chiropractic was accessed at the following URL on the 16th of September 2014:
<http://www.nhs.uk/conditions/chiropractic/pages/introduction.aspx>

⁴¹ The NHS web page on Acupuncture was accessed at the following URL on the 16th of September 2014:
<http://www.nhs.uk/conditions/acupuncture/pages/introduction.aspx>

of other problems, including neck pain and post-chemotherapy nausea and vomiting." Finally the page concludes with "Acupuncture is sometimes used for a variety of other conditions as well, but the evidence is not conclusive for many of these uses." We are left wondering about things like infertility and asthma. Are these "uses" for which the evidence is "conclusive" or not? The page is fundamentally, and we think, perniciously contradictory and equivocal.

The NHS web page on Homoeopathy was an abrupt departure from posture of the other three NHS web pages we have surveyed⁴². At the very top of the page beneath a very brief and non-committal explanation is a starkly worded metaphysical statement:

A 2010 House of Commons Science and Technology Committee report on homeopathy said that homeopathic remedies perform no better than placebos, and that the principles on which homeopathy is based are "scientifically implausible". This is also the view of the Chief Medical Officer, Professor Dame Sally Davies.

The rest of the page's sections are uniformly negative in terms of efficacy: "There is no good-quality evidence that homeopathy is effective as a treatment for any health condition." In reference to NICE research the page explains: "NICE currently does not recommend that homeopathy should be used in the treatment of any health condition." The page does have a lot of additional confusing content however, suggesting, for example: "Many practitioners believe that homeopathy can help with any condition." The page claims that "homeopathy is used for an extremely wide range of health conditions" listing asthma, ear infections, hay fever, mental health conditions, such as depression, stress and anxiety, allergies, such as food allergies, dermatitis (an allergic skin condition), arthritis and high blood pressure." This list is again followed up with an incongruous rejoinder: "There is no good quality evidence that homeopathy is an effective treatment for these or any other health conditions." Perhaps the strangest thing on the page is the an evaluation of the availability of Homoeopathy on the NHS:

Homeopathy is not available on the NHS in all areas of the country, but there are several NHS homeopathic hospitals and some GP practices also offer homeopathic treatment. Homeopathy is usually practised privately and homeopathic remedies are available from pharmacies. The price for an initial consultation with a homeopath can vary from around £20 to £80. Homeopathic tablets or other products usually cost around £4 to £10.

It is hard to interpret the conflicting ideas on the above web page as anything other than the result of a continuing power struggle. The aggressive reference to the pronouncement of the 2010 House of Commons Science and Technology Committee report, as well as the opinion of the Chief Medical Officer, juxtaposed to the list of ailments, the fact that Homoeopathy is still available in some places on the NHS, that it otherwise costs £20 to £80 and that the pills are available over the counter -

⁴² The NHS web page on Homoeopathy was accessed at the following URL on the 16th of September 2014: <http://www.nhs.uk/conditions/homeopathy/pages/introduction.aspx>

- in aggregate, these statements are incoherent. The conflict on the web page is an artefact of an unusual point in time. The dynamics between professionalism and pseudo-professionalism, naturalism and vitalism, clinical evidence and its rejection, are all present, but this time along with the sudden and unusual appearance of metaphysics -- "...the principles on which homeopathy is based are 'scientifically implausible'. The sense that the page is a battlefield of claims and ideas, rather than the distilled beliefs of a group of appropriate experts, is overwhelming.

In summary, the NHS is caught in the same trap it has been for decades. Unable to justify its naturalist proclivities, due to the almost uniform positivist prohibition against metaphysics, EBM has allowed the pseudo-professionals access to the medical system and the blessing of EBM authority by default. By failing the scientific process itself at the metaphysical level, the structure of expert authority and professionalism collapsed with its foundations. With the pseudo-professionals now raiding the market of the venerable that Parson's described, social networks transporting pernicious anti-scientific and anti-medical ideas continue to proliferate with expanding CAM training institutions, creating a greater market for CAM and directing ever larger numbers to the ideology of CAM, or simply into a pseudo-professional market zone, where fears can be exploited whether people are capable of understanding the fundamental epistemic and ontological incompatibilities between CAM and EBM or not. It should be occurring to the reader by now, that even though there is a dynamic element to the regulatory landscape, especially when augmented by anti-CAM activism, the fundamental metaphysical problem is entrenched. The NHS Homoeopathy page is how this struggle ends without revamping scientific metaphysics: in a confused stalemate, where CAM advocates can still present their pernicious beliefs, unscathed by anything other than what might appear to be contested, evidentiary quibbling -- while frustrated medical authorities perpetually fail to overcome the insurmountable limits of clinical methodology.

Edzard Ernst

As we mentioned, Edzard Ernst is the leading clinical medical expert on CAM efficacy in the UK. In his book *The Oxford Handbook of Complementary Medicine*, Ernst engages the philosophy of science around these issues for medical professionals in a section where he responds to perceived misconceptions about CAM, beginning with the first misconception: that CAM is unscientific (Ernst et al. 2008).

Conventional medicine is struggling to be scientific. Yet the application of the scientific method to medicine is relatively young and not all the procedures of conventional medicine have sound scientific bases. Undoubtedly, the scientific foundations of CAM are much more shaky than those of conventional medicine. However, given the paucity of resources and the backing from

the scientific community, CAM is increasingly successful in becoming scientific as much of this book demonstrates.

The idea that the CAM pseudo-professions are "becoming scientific" strikes us as substantially contradicted by the evidence. Other than through the efforts of other, related professionals, EBM actors more willing to deploy metaphysics against CAM, the ASA, the anti-CAM activists -- who we believe are very much professional in the sense that Parson's had in mind, even though the activists are not being monetarily compensated -- CAM pseudo-professionals show every sign of continuing to conform only publically, and only superficially to professional rules imposed from the outside. Again, there is more research needed in this area, but the McTimothy Chiropractic email suggests that secrecy is a major strategy for CAM in the current rhetorical climate and that it is precisely secrecy which they are actually turning toward, and not any major defection from their core ontological or ideological beliefs, as suggested by the thematic analysis of osteopath public statements.

The issue of the scientific nature of "conventional" medicine is in reference to the cost and methodological limitations within EBM itself and the large number of decisions which must be made, in the everyday application of EBM, without firm evidence. The claim that EBM and CAM are similar in their posture toward science, except for, on CAM's part, a relative lack of resources and the lack of EBM's "backing", makes us think that Ernst is talking about something different than we are in terms of what he means here regarding "CAM". Perhaps Ernst is referring here to CAM researchers, rather than CAM culture more generally. In any case, we do not share Ernst's optimism here. We are quite certain that CAM has no "scientific foundations" at all. The "shaky" nature of CAM's relationship with science is motivated by two things in our opinion: 1.) a historical and deep cultural antipathy toward science; and 2.) opportunism associated with adopting "science-y-ness" exemplified by the wearing of white lab coats and appropriating the word "doctor". To us, the culture of CAM is fundamentally opposed to naturalism, an elemental, and as we have seen, difficult to repress ontological component of science. The appearance of thoroughgoing EBM scientists, like Ernst himself, from out of CAM-positive backgrounds, is in no way the norm here. The passage quoted above sounds more like Ernst himself, than CAM culture generally.

The nagging issues of medical negligence go unanswered by EBM epistemologists like Ernst. As we have mentioned, there appears to be something metaphysically mistaken in the manner of the positivist refusing to accept that CAM is absurd on its face. There is something unacceptable in that almost half of the British population will see a medical practitioner in their lifetime who will do nothing but deceive them *by design*.

Ernst continues by addressing our point head on, albeit, as another in his list of misconceptions. The misconception: "That CAM is known to be worthless."

Some CAM approaches have indeed been shown to be wrong and ineffective. Yet to generalize in the above manner is not correct. As long as a remedy has not been tested, it cannot be labelled to be either effective or ineffective. Furthermore there are many areas within CAM which have shown much promise—and this book provides ample evidence in support.

Again, this is a methodological rule, central to clinical medical research practice, mistakenly exported as a global epistemic world view. Contrary to the extremely reasonable metaphysical claims about the relative validity of the "basis" of scientific medicine vs. CAM, many of which we identified in the 1986 BMA report "Alternative Therapy", and contrary to the start of the NHS web page on Homoeopathy, proclaiming that the basis of homoeopathy was "scientifically implausible" -- such essential, foundational, metaphysical assertions are too much a part of the scientific position here to be dismissed so easily. These basic elements of rationality cannot be dispatched by a blithe assertion that no judgement pertaining to efficacy can *ever* be legitimate, except to test for efficacy. This is scepticism on a Cartesian level. To imagine such an assertion to be true would be to attempt to live in a way which is impossible. Peirce specifically rejected such Cartesian scepticism for this reason. We do not collect every known medical technology from the vast universe of non-scientific practices that has even been said to work, assign them numbers and put them, randomly, in the queue for testing. Instead we use an interlocking set of values, beliefs, social conventions and political evaluations to decide what to write grants for, in order to get funded for research. A process which by any estimation, has resulted in billions of pounds of expenditure on research into the efficacy of CAM techniques which are burdened with unlikely ontological commitments and no efficacy beyond placebo effects. There would be, in other words, profoundly pragmatically pernicious consequences to a ban on metaphysical judgements relating to, at the very least, prioritizing what research to fund. There are, other negative consequences for medical epistemology, expertise, medical professionalism and human flourishing as well. Not the least of which is the lack of a coherent position from which to address the rest of the world in terms of medical pluralism.

Ernst continues, however, moving on to metaphysics proper, as he addresses his third misconception, that: "CAM is implausible".

True, CAM often lacks a sound theoretical basis and tends to substitute this lack with a 'philosophy'. Philosophies, like religions, are difficult to prove right or wrong. This, however, is not to say that the remedy associated with such a philosophy is useless. The history of medicine abounds with examples of therapies that were once used on the basis of a totally false rationale. Eventually the concepts were corrected and the therapy, which, of course, had to have a certain level of effectiveness to begin with, became established for defined conditions. In other words, just because the philosophy of a CAM approach, e.g. traditional Chinese medicine, is in conflict with science, a treatment, e.g. acupuncture, is not necessarily ineffective.

Before getting onto the metaphysics, in the second half of the quotation, Ernst points out that effective therapies, with initially false rationales, are still effective therapies. The problem, of course, is not effective therapies, of which there are a *limited* number. The problem relating to our previous point, is

the unlimited number of potentially *ineffective* therapies. We are happy to do research on effective therapies. In cases like blood-letting, or thalidomide, we are even happier to do research on apparently effective therapies with dangerous or deadly side effects. In the case of CAM today, the effects are *never* enough in more robust studies to qualify for the "certain level of effectiveness" he is referring to here. Yes, we used to think that opium worked the way it did because of its "dormative" nature. Perhaps less a false rationale than a tautology, but still, opium actually works as more than a placebo. We are convinced that CAM practices do not perform beyond placebo, in part because uniformly, the only robust double-blinded RTCs ever performed on them, demonstrate exactly that. We would argue that humanity has already identified the really useful stuff from the universe of traditional medical concepts within our immediate grasp. There might be some useful plants out there still, in enthnomedical obscurity. However, the proposal to hold ourselves permanently hostage the hope, that the remaining traditional or CAM medical treatments close at hand in Western Culture -- which however frequently studied, have *still* not been identified by scientists as powerfully useful but previously unknown new medical concepts, lying undiscovered and hidden beneath CAM's "totally false rationale" -- is without any measured justification.

More importantly, even if one or more of CAM therapies is effective in a way which is not due to the truth of CAM's ontological commitments *or* to placebo effects, such CAM therapies are not *very* effective. There is a matter of degree here, which also does not fit well with the blind, dogmatic assertion that the question is always : effects, or no effects? There is no reason to believe that the huge resources that are directed to CAM annually are justified on the basis of an adequately pragmatically powerful, yet presently unknown mechanism of efficacy, if CAM does work beyond placebo, the effects are extremely subtle. At least in the CAM/EBM era, in the last 20 years, not a single one of these alternative mechanisms has emerged as a pragmatically powerful treatment, as Ernst describes in the above quotation. More importantly, it is unlikely that this will *ever* happen. What Ernst is actually proposing, is that we *never stop* researching pragmatically marginal therapies. Because, in using the methodology Ernst has described, there will never be a reason to stop researching pragmatically marginal therapies. As long as methodological limitations ensure a lack of blinding, and as long as a lack of blinding will then insure the presence of bias and placebo effects, and as long as the presence of bias and placebo effects cannot be distinguished from evidence of efficacy, the question of efficacy will never be answered -- and since the question of efficacy cannot be answered any other way, we must continue the clinical research. This is not the sedimentary layers of inquiry Peirce had in mind for scientific progression. Peircian scientific processes move forward over time. Ernst's proposal never moves, and as such, is merely a waste, continuing indefinitely, or until the scientific metaphysics return to a more pragmatically valid state, so Ernst's costly and pragmatically impotent proposal can be abandoned.

Ernst was addressing metaphysics directly in the first part of the quotation. We believe that had Ernst been using philosopher speak, instead of medic speak, he might have said essentially: "all of

metaphysics, just like ontological metaphysics in particular, needs to be ignored, because the certainty of claims cannot be definitively derived." Ernst did articulate a reason for rejecting metaphysics, that such things are difficult to prove right or wrong. In a sense, this sounds a little like demarcating right from wrong, or what works as a proxy for this idea in the received view of science, demarcating science from pseudo-science. As we know, demarcation is an extremely vexed question in the global sense, if not a permanently lost cause, and thus cannot actually be used as a convenient, certain and unquestionable means of distinguishing globally between right and wrong as Ernst has suggested.

As such, Ernst's view here, sounds like it could be a mistaken articulation of Popper's positivism. What Ernst fails to grasp about Poppers's falsificationist world, is that nothing can ever be *proved* right or wrong. The only thing falsificationism sets out to help us determine, is the difference between a scientific and a non-scientific claim. For a claim to be a scientific claim, then the claim must be falsifiable, and a claim can only be falsifiable if there is an action that can be taken to demonstrate the fact that the claim is false -- in the event that it actually is false. This is an interesting idea and it is reflected strongly in clinical medical experimental design technologies. The problem is that falsificationism, as influential and beneficial to experimental science that it has been, is really an *insufficient* means of demarcating science from other stuff. The point we are attempting to make here, is that, like Ernst's epistemic claims, when falsificationism is scrupulously applied to the experimental process, it ultimately produces nothing other than a stream of dualistic answers: false, false, not necessarily true or false, false, not necessarily true or false, etc. This is not enough to construct a workable medical epistemic system either. For that, we need metaphysics. Thus the failure of positivism as a global solution to the demarcation of science. Neither Popper nor Ernst's narrow clinical methodologies offer a viable alternative scientific metaphysics. Ernst has not discovered a shortcut to the global demarcation of scientific right and wrong in the form of a simple methodological rule. What Ernst is doing, is mistakenly misapplying a narrow methodological rule -- a rule which is useful when applied specifically to doing clinical medical researchers -- as a global epistemic world view.

7.) Medical ethics

Medical professionalism, by virtue of its commitment to the pragmatic exploitation of medical scientific knowledge for the benefit of humanity, must explore the ethical dimensions of this vast project. Medical ethics as a field is both proof of medicine's inherent altruism, the same altruism so easily questioned or dismissed by post-modern sociologists, and a source of understanding to demarcate CAM's anti-professionalism. Tom Beauchamp and James Childress in their 2001 edition of *Principles of Biomedical Ethics* (Beauchamp & Childress 2001), list four principles of biomedical ethics: respect for autonomy; non-maleficence; beneficence; and justice (p. 39). They suggest these

correspond essentially to what they call "the five focal virtues": compassion; discernment; trustworthiness; integrity; and conscientiousness (p. 32 - 38). Beauchamp and Childress tie all of these things to "professional roles and practices" arguing precisely as Parson's would, that "each organized body of professional practices" has "standards of virtue" which "incorporate criteria of professional merit" and that "possession of these virtues disposes a person to act in accordance with the objectives of the practices" (p. 30).

Fundamental aspects of CAM practice are predicated on the violation of a number of these principles and virtues. The most vexed is probably autonomy. The sociological narrative, holding popularity as an epistemic warrant, sees CAM as liberating. As realists, we are aware of the role CAM plays in the transmission of pernicious anti-medical science beliefs (Ernst 2009b). There is an argument to be made that beliefs which sociologists identify as the warrant for CAM as a democratic movement, would be better interpreted as the by-product of CAM's anti-professional *modus operandi*. By this reading, the anti-medical beliefs attributed to CAM's legitimacy are not legitimate, independent, spontaneous, organic, phenomenological perceptions or reactions to the EBM experience, but rather just what CAM has had to convince people of in the course of making money through pseudo-professionalism. The pseudo-profession's entire purpose is to demonize the professional, and exploit the resulting perception of a breach in the medical contract.

The issue of placebo effects bears on autonomy as well. Trusted doctors always inspire placebo effects, just as psychotherapists do, and CAM therapists do, regardless of the modality. Doctor inspired placebo effects supplement otherwise effective therapies. CAM therapists *only* inspire placebo effects. CAM treatments are predicated only on deception. Patients, regardless of their level of desperation, deserve for the practitioners treating them to respect them by defaulting to telling them the truth, unless that is, for some reason, impossible. Even some additional suffering is justified in deference to autonomy and truthfulness, as people are more likely to adjust adaptively to bad news, than they are to deception.

The notion that CAM practitioners too *believe* they are providing effective treatments to patients, makes them guilty of negligence rather than fraud, in the exploitation of placebo effects through deception. In doing so, they violate discernment, trustworthiness, integrity, and conscientiousness. In rejecting EBM evidentiary standards CAM practitioners continue to practice outside the narrow confines of what, for example the NHS suggests are the very few conditions for which any argument can be made for their efficacy. The due diligence associated with any understanding of conscientiousness, would at least require that CAM practitioners consider the evidence. In the same way, so would the fair adjudications associated with justice. Furthermore, trust in a medical practitioner's commitment to honesty requires the disclosure of the verdict delivered on efficacy by EBM, the integrity to ensure that their patients actually understand the ontological commitments integral to their modality and how this differs from EBM, and finally the discernment

which would, like conscientiousness, in any legitimate form, preclude becoming a CAM practitioner in the first place.

CAM practitioners are aware that EBM, and other regulatory authorities like the ASA, hold their ontological beliefs and evidentiary standards in contempt. Their reaction, to hide these beliefs both from patients and the rest of the public until it is safe and profitable to disclose them in private, adds to the above concerns. Finally, non-maleficence and beneficence, are violated by dispensing dangerous advice, from anti-vaccination to more general, erroneous anti-EBM claims. CAM is doing undeniably serious harm, both to the population physically, to the integrity of the medical contract and to the overall "culture of fixing belief."

EBM's complicity in all of this hangs largely , but not completely, off of its refusal to recognise the centrality of scientific metaphysics to the evaluation of efficacy. Although EBM has, in many ways struggled successfully to maintain some metaphysical components to its adjudication of CAM , as in the case of the NHS web page on Homoeopathy, their failure to cleave more closely to their metaphysical critique has proven a profoundly damaging mistake and professional failure.

Independent of the efficacy issue, EBM and the state should never have allowed the commercial operation of CAM as a parallel system. The externality of CAM to EBM is primarily responsible for CAM's evolution as a secretive, duplicitous, destructive, entrepreneurial anti-profession. If doctors themselves practice CAM, as many of them do, their professional superstructure at least provides legitimate oversight and a set of institutional values which, had they been the only people providing CAM, would have prevented the wholesale fraud associated with CAM practitioners referring to themselves as doctors and exploiting their freedom from the constraints of legitimate medical professionalism to exploit patients in ways we may never know about without much more aggressive forms of sociological research. Far worse, the external growth of CAM culture has metastasised to the level of a profound threat to the dominance of medical professionalism itself, while EBM has clearly failed to recognize this.

CAM's continued growth and political success has provided it with a power base now so formidable that CAM is breaching the walls of EBM professionalism organizationally, in ways that are far more pervasive and damaging than would have been possible if doctors had remained the only gateway for research or experimentation with CAM practices. According to Ernst, "The muddled and regressive thinking" endemic to CAM culture "is currently being integrated in medical education, in medical practice, and in research capacity development" (Ernst 2009a),(Ernst 2009c). Primary care is engaging CAM to the point that "a survey of chairpersons of 72 primary care organisations in England showed that 85% of these organisations had discussed CAM at board level" (Ernst 2009a). The dire state of the CAM situation from a network analysis standpoint, is only exceeded by the predictors that the situation will continue to deteriorate (Ernst 2009a):

A survey of CAM courses offered to healthcare professionals by 25 institutions of higher education in the UK showed that the aims of these courses were often not well- defined, included themes such as 'holistic study skills', the 'philosophical basis of Chinese medicine', 'heteropathy of Chinese herbal treatment' and 'theory of energy', and were devoid of any sign of critical analysis. If the next generation of 'CAM experts' is systematically brainwashed, how are they going to make a meaningful contribution to the future health of the nation?

As we have seen, the harms associated with CAM proliferate mainly through the operation of CAM training institutions. The damage these institutions are doing is only exceeded by the professional failure of the authorities regulating medical education in the UK.

9.) Summary

In many ways, the CAM modalities which EBM insists, wrongly, on adjudicating as "effective", can be seen in a Darwinian light. As the CAM claims which can be double-blinded are definitively seen to be not effective at all beyond placebo, only people who reject EBM evidentiary standards completely are likely to continue to believe they are effective medical treatments. This change in CAM's environment has proved beneficial for forms of CAM which cannot be blinded and to some degree, for those which can, but only partially by sham treatments performed by actors. As we have seen, even the "actors" or other workers in a study are a significant avenue for the introduction of bias into a trial design. All of these methodological limitations and vulnerabilities to bias have been interpreted too optimistically by too many EBM meta-analysts. "The Dodo bird conjecture" demonstrates that any trusted therapist will generate a certain kind of effect, no matter what she does, as long as her relationship with the patient meets certain conditions. In addition we must recall, where the line on the Jadad graph, of efficacy and study bias, meets the point where perfectly blinded studies would be if they were possible for all CAM modalities: at zero effects. We must also recall that there has never been a series of robustly double-blinded studies which have ever shown a significant CAM effect beyond placebo: every time we can see CAM efficacy clearly, nothing actually appears.

CAM's inherent connection to deception, born of the absurdity of its origins and its basis in ontological nonsense has made it an object of scientific medical professional attack since it appeared in the form of communist propagandists, quacks, cranks and deluded mystics. But scientific medicine has been caught between the metaphysical and the Popperian, in a confusion born of the failed project of positivism, to try and rid itself of its reliance on the scientific metaphysics so central to the naturalistic medical understanding of the world. By mistaking a methodological rule for clinical researchers, for a global epistemic rule, scientific medical epistemologists have been alternately making and invalidating the strong metaphysical case against CAM in a process of confusion which continues to this day.

As CAM's own statutory regulatory recognition and new European trading standards legislation would open up new avenues for anti-CAM advocates to attack CAM's duplicity, the results of Quacklash would reveal a deeply anti-professional culture, driven by the vast and rapidly growing architecture of CAM training facilities and CAM culture's entrenched anti-medical recalcitrance. EBM would struggle with its internal epistemic conflicts and fail, with the medical state, to check CAM's rapid external growth, to come to terms with CAM's fundamentally unethical, anti-professional nature or to understand the threat that it clearly posed to the state of the professional medical contract.

The final point, which can only be made as we arrive at the end of this substantial review of medical history and epistemology, is that by far the most parsimonious explanation for the existence of CAM is the very reason that Parson's gave us for medical professionalism: vulnerability and irrationality. On this reading, CAM itself -- its intrinsic harms and fundamental orientation to deception -- is the most powerful argument for the resolution of the medical pluralism debate in a universal pragmatic realist epistemology. Even then, if CAM and other forms of non-scientific medicine are merely manifestations of our incapacity to evaluate efficacy by ourselves and the inability to cope gracefully with the fear of death and the pain of illness, then the highest expression of medical professionalism will be to protect ourselves from CAM and other forms of non-scientific medicine. In that case, we must not just accept that there is only one, pragmatically potent, universal, naturalistic, scientific medical epistemic system. We must also accept that we are obligated to develop our scientific metaphysics for application to a more coherent medical epistemology. In the wake of the failure of Positivism, we must apply this more robust epistemic understanding with renewed confidence -- to ridding our medical marketplace of the mistaken artefacts of previous failures of expert medical professionalism.

Part III: Epistemic problems affecting medical pluralism

Introduction

Sociology and Anthropology, as we have suggested, are central to the medical pluralism question, in that it would be impossible to understand medical pluralism without understanding how various actors think and what they believe. Yet as soon as we embarked on the quest to consult these two disciplines, we were met with an astonishing claim from Byron J. Good, in his 1990 book *Medicine, Rationally and Experience*, the same book that we frequently referenced at the beginning of Part II (Good 1994). Good quotes the anthropologist Rodney Needham as suggesting that the very idea of a "belief" or "the supposed capacity for belief" was something which "philosophers had failed to clarify" and "were unlikely to do so" (p. 14). We were surprised to hear an Anthropologist

weighing in with such confidence, with a claim which was so wrong in any meaningful sense. This unfortunate proved pervasive however, and we eventually realized that Anthropologists and Sociologists appeared to be involved in a failed philosophical project spanning multiple decades and the vast bulk of the ethnographic territory in their field relevant to our research. The project involved doing philosophy badly and doing it badly mostly by invoking mistaken or long discarded interpretations of philosophers with the kind of repetition and certainty which accompanies dogmatic ideology generally. It appears that anti-realist ideas which had never had more than peripheral support among analytic philosophers at any point in the 20th or 21st centuries, had become essential to both later 20th century Anthropology and Sociology, as the result of intra-disciplinary motivations or methodological tools. The bad philosophy was realized however, only after these fields elected to export their internal habits of mind as a global epistemic claim.

It is not just us who find this kind of thing disorienting. In fact, beyond the matter of Anthropological and Sociological anti-realism's fundamental incoherence, the principle non-philosophical problem we have is that they fail to tell their realist interlocutors about their epistemic position. Edzard Ernst, nothing if not prolific, fearless and committed to engage all of the academic nonsense relating to CAM that he turned up in his research, labelled sociology's input on the subject as "postmodernism" and rightly, dismissed it as such:

Post-modern ideas have infiltrated CAM. Science's quest for truth is seen as a politically motivated 'struggle for power', in which the 'societal approach is pitted against reductionism'. The claim is that 'CAM poses huge challenges not only to the dominance of biomedicine, but also to the fruits of dominance — status and power in decision making about health care and the livelihood of physicians' or that CAM poses a 'threat ... to the long-standing hegemony of biomedicine in the West'. In order to fend off this threat, conventional medicine is said to 'attack the medical competition, show no intellectual tolerance, and only take those prisoners, who can be converted'.

This is a much better example of Cant and Sharma's invocation of the "double hermeneutic". Here Ernst has piled up the familiar tropes of sociology as he has run across them in what must have seemed like a series of arguments against science. What he failed to understand of course is that the people who originally made these claims were social constructivists, and as such, they do not believe that what they are saying refers to the 'real', or that what *anyone* is saying refers to the real. The sociologists making these statements were anti-realists and to them nothing anyone says has purchase on any aspect of a real world. The telling part of Ernst's quotation here is his initial assertion that "Post-modern ideas have infiltrated CAM." This is the confirmation of the double hermeneutic -- a double hermeneutic in which CAM advocates have taken statements made by anti-realists and begun using them, erroneously, in realist a context. Anti-realism is simply not understood (or even imagined) in the minds of the scientific realists generally involved in the interdisciplinary discourse of medical matters. Ernst and his CAM advocate interlocutors have no idea what the epistemic status of these

ideas might be, only that these statements sound as if they are making a very academically authoritative claim that medical authority is bad and can be ignored, and that is extremely valuable for CAM advocacy.

We are at a loss as to what to do about this, other than to sound the alarm. To us and the rest of the disciplines continuous with pragmatic realist medical discourse, the export of such epistemically obscure statements, for use ultimately as realist arguments for ontologically mistaken medical systems, constitutes a pernicious breach of academic professionalism. This is yet another one of the breaches of professionalism which seem to so consistently attach themselves to CAM. To use social constructivism this way could only be justified, unfortunately, by a social constructivist. In sounding the alarm, Sociology and Anthropology may attach our criticism to the nascent movement emerging from Harry Collins's work on the Third Wave of Science Studies. Yet we are not sympathetic to Collins' efforts to both be relativists and not be relativists, nor his use of Wittgenstein as a talisman to wave repeatedly and without any coherent explanation at unwanted realist difficulties like the universal commensurability of certain very central aspects of human experience, like pragmatic epistemic medical power.

In the end, we are compelled to refer back to Peirce and his grounding of scientific epistemology on an immediately recognizable "common sense" understanding of belief. What connects Peirce's everyday notion of belief so powerfully to the pragmatic success that Peirce's vision of scientific inquiry guarantees, is the way Peirce specifies that in order for the this very ordinary experience of doubt and belief to function as part of scientific inquiry, it necessarily must take place in certain kinds of individuals and those individuals must necessarily be part of a certain kind of group. Specifically, these ordinary belief processes best constitute scientific inquiry, when they take place in the minds of trained, expert scientists, who are not only experts in their field, but who are also trained as scientists generally, to understand the rules of logic. Furthermore, the scientific mode of fixing belief, and the pragmatic power possible through it can still only be realized if these very specifically scientifically trained individuals are part of a group of scientists which functions in a pro-social manner, honestly and earnestly subordinating their private interests in the pursuit of the knowledge required to solve the problem at hand.

It is on these terms that Peirce specifically attacks Cartesian scepticism. He does so by dismissing those who would suggest that we embrace awkward and obscure foundationalist claims as more certain than those characteristic of common sense. Such sceptics he asserts, are not engaged in the everyday doubt and belief process that must function at the heart of scientific inquiry, rather they are merely fooling themselves -- engaged in a "self deception" (Peirce 1955, p. 228). These counterintuitive forms of scepticism, which call into question assumptions about the real world that are essential to both our everyday functioning and to scientific inquiry, are useless exercises which contribute nothing while blocking normal inquiry.

Sociology attempts something similar to Peirce's Cartesian interlocutors. Arguing that common sense, realist scientific beliefs should be replaced by the supposedly more rigorous social constructivism. Peirce, like Hume, would have rejected this attack on everyday beliefs as mistaken, because it undercuts the everyday doubt and belief process which powers the most pragmatically powerful forms of scientific inquiry. The "hypothesis of reality" that results from the Peircian synthesis of everyday belief processes and carefully prescribed scientific values and behaviours is far more pragmatically powerful than either Cartesian scepticism or social constructivism.

Legitimacy

Before we continue on to Collins and Peirce, we need to mention what we see as one of the most profound failures of Sociology emerging from the sociological research on medical pluralism, after the turn to sociological non-realism. The failure to adjudicate CAM's legitimacy has proven a glaring omission from the traditional sociological prevue. As if CAM has suddenly become untouchable, the obvious wrongdoing of CAM's anti-professional exploitation has so far been a third rail for sociologists writing on medical pluralism. One wonders what it is about the medical pluralism debate which prevents sociology from pursuing the rich narratives of unfairness available in frightened sick people paying quacks and faith healers for deception.

Of course, medical professionalism is necessary because of the vulnerability of patients, and as such, Sociology has done well to identify the potential in doctor patient relationships for a breakdown in that professionalism. The evolution of a medical culture characterized by the abuse of professional autonomy, or the failure to self regulate, is certainly worth remaining vigilant for. For that matter, concerns about whether everything possible is being done to reduce patient vulnerability are totally legitimate. Every effort must be made to monitor the extent to which EBM empowers patients by facilitating a culture of openness, where doctors are respectful of patients and their levels of understanding and are committed to taking the necessary time to clearly and thoroughly explain the nature of medical procedures and the medical process in general. Sociology plays an indispensable role in overseeing EBM: investigating, measuring, and critiquing EBM's compliance with their own professional mandates, including the wider public of understanding of science issues which allow EBM to educate the wider public regarding the nature of public health initiatives, medical procedures, and the medical process in general.

Unfortunately, many sociologists abandon this legitimate role, and steer their work from oversight to dogmatic condemnation. Such sociological analysis then adopts a posture of aggressive confrontation, and without justification, reduces EBM's motivations entirely to self interest. Abusive aspirations to power then become the only explanation for EBM behaviour, while any consideration of EBM's self-stated and by all rights deeply felt commitment to the interests of patients and to the

wider public, is neglected. The contrast between the excessively pessimistic nature of Sociology's attitude toward EBM and Sociology's posture toward CAM is striking and inexplicable. CAM is assumed to be unwaveringly dedicated to patient interests. While EBM has been writing extensively and with great sincerity about ethics and narrative medicine for decades, just as we saw in the 1986 BMA report on CAM, and clearly works to address its failings and weaknesses, responding both to internal and external criticism, CAM does none of this. Yet, EBM's efforts and CAM's uniform failing in this regard are ignored by Sociology. More importantly, the obvious pragmatic superiority of EBM's professional approach to patient welfare, in comparison to CAM's specifically anti-professional business model, goes ignored. Missing the undeniable fact that patient interests are ultimately served by dedicated professionals working in the context of EBM, than the loose network of anti-professional entrepreneurs which characterize CAM.

As philosophers have been at pains to argue from Plato onward, the truth of a proposition is not to be determined by the number of people who believe it to be true. The suggestion that because lots of people believe in CAM, that it is therefore legitimate, is the most appalling possible abuse of the notion of legitimacy. The idea that human beings are uniformly rational, strikes us as the kind of rabidly libertarian position which has destroyed the legitimacy of orthodox economics. What we have come to term "popularity as epistemic warrant," was a common tool of Cant and Sharma and a transparent attempt at CAM advocacy. Again we are uncertain what it takes to castigate a non-realist. The notion of incommensurability which underlies social constructivism, can be simply turned on philosophical arguments to argue that realist epistemologies and ontologies are so radically different from non-realist ones that they too are incommensurable. Sociologists listening to realist epistemic arguments hear them as naive -- naive realists missing the point. There is no common ground, in the non-realist's mind upon which realist arguments can be compared or assessed. As pragmatists, writing about sociologists, we simply argue that in this case the only ground is the common ground of our pragmatic medical epistemology, where CAM beliefs do not work.

Collins and the Second and Third Waves of Science Studies

As the reader will recall, Collins acknowledges in his Third Wave of Science Studies that sociology has created serious problems for itself by uncoupling epistemic authority and expertise (Selinger 2006, p. 11). Although Collins suggest that there is an important need for sociologists to begin to better understand the difference between experts and non-experts, and that ultimately, sociology needs to export normative claims about how non-experts should identify experts and engage

them for problem solving, Collins means to do this without abandoning the social constructivism inherent in the Second Wave of Science studies⁴³. We do not believe this is possible.

As we mentioned in Part II, Collins only evidence for the abandonment of the epistemic superiority of scientific knowledge is to suggest that because of Wittgenstein's suggestion that words mean different things when used in different contexts -- different forms of life -- then there is no objective or fixed meaning of language and that "truth" becomes a meaningless term, with nothing to attach itself to.

We believe this is a radically mistaken reading of Wittgenstein. We believe that Wittgenstein was not attempting to make such an anti-realist point. We believe that Wittgenstein was more or less in agreement with his contemporary and close colleague Frank Ramsey and Ramsey's *deflationary theory* of truth, which happens to fit well with our pragmatic medical epistemology (Horwich 1996). The deflationary theory of truth flattens the arguments about the nature or the meaning of truth by suggesting that "truth" is nothing other than "a logical device enabling simple formulations of certain sorts of gen-realization". In other words, to say for example, "it is true that, that tree is an oak" is the same as saying "that tree is an oak." there is nothing more to "truth" than that. The nature of "truth" then is not really a matter of dispute. To Ramsey, people who are trying make a warrant for anti-realism out of worrying about the nature of the word "truth" are not making sense because "truth" is just a logical device for making certain kinds of generalizations, not a key to the breakdown of realism.

The deflationary theory of truth is consistent with Peirce's pragmatism. Even though the truth of a proposition can be disputed and that dispute may legitimately involve a matter of the context, "language game" or "form of life", in which the dispute occurs, Peircian reality is not derailed by such semantic difficulties. The communal nature of scientific practice will still get to the pragmatic "real" in the long run. For that matter, in Wittgenstein's terms, confusions relating to "language games" and "forms of life" inhibit our ability to "go on" in our everyday pragmatic prosecution of the pragmatic objectives which make up our lives. As such, we still get there in the end, in precisely the way Peirce does, regardless of how the matter has been socially constructed.

Paul Horwich in an argument with Crispin Wright about certain versions of deflationary theory helps us out by observing something about Wittgenstein and Ramsey in the course of his argument: "Neither Ramsey nor Wittgenstein engage the issue of whether truth is a property: their only point is that truth is entirely explained by the disquotation schema⁴⁴" (Horwich 1996). We are not

⁴³ The idea with the waves of science studies is that the first wave is the "received view" of science, in other words social studies of science consistent with realism. The Second Wave was social constructivism, which exploded the epistemic authority of science and made it just another form of knowledge with no warrant for epistemic superiority. The Third Wave Seeks to acknowledge the problems inherent in the second wave without denying it but while maintaining the rejection of the First Wave.

⁴⁴ Deflationism is a popular theory and its formulation has been attempted in indifferent ways resulting in multiple deflationary theories of truth, often referred to by different names, including: redundancy theory, the

certain why Wittgenstein is so commonly misconstrued as an anti-realist when the essential notion that sociological anti-realism turns on -- the idea that "truth" is so problematic as to justify the abandonment of universalism in favour of incommensurability -- is inconsistent with the way he understood the function of the word "truth".

We are hopeful that Collins' efforts to restore a means of connecting expertise to epistemic authority will prove successful. However we are sceptical that this can occur without overturning the fundamental anti-realist claims of the Second Wave. As such, the problems with sociological anti-realism in interdisciplinary settings will continue. We should reiterate that the non-realist levelling of epistemology, does have its advantages within sociology. All scientists, whether they turned out to be right or wrong in the eyes of history can be more easily treated objectively, if such normative evaluations are suspended. It is in the choice to export non-realism as a global epistemic position that the philosophical mistake is realized. Sociologists sympathetic to CAM will hopefully be more sensitive to the confusion caused when social constructivism is delivered without any disclaimers into realist settings. Unfortunately, we are unable to think of any reason why a social constructivist sociologist would necessarily feel compelled to see this as the case.

Peirce, scientific metaphysics and the failure of scepticism

EBM epistemologists often assert that at the heart of the EBM project is a radical commitment to more data based decision-making. The idea is that the old habits, shortcuts and pet-theories of doctors are pernicious obstacles to good clinical practice and need to be replaced by something more scientific. The name they often reach for as their patron saint, is Karl Popper (Goldenberg 2010):

External clinical evidence both invalidates previously accepted diagnostic tests and treatments and replaces them with new ones that are more powerful, more accurate, more efficacious, and safer, which is as close as it could be to Popper's criterion of demarcation based on falsifiability.

disappearance theory, the no-truth theory, the disquotational theory, and the minimalist theory. Often presented with the help of the schema below, this is sometimes referred to as the equivalence schema or the disquotation schema, as in the passage to which this footnote is addressed:

(ES) <p> is true if and only if p.

This information was taken from the online Stanford Encyclopedia of Philosophy and retrieved from the following URL on September 16, 2014:
<http://plato.stanford.edu/entries/truth-deflationary/>

The problem is that this is a bad reading of Popper and of EBM. By Popper's lights, theories can only be falsified. Worse, Popper was hoping to side step the "problem of induction" and so he suggested, very radically and absolutely antithetically to the daily practice of EBM, that the scientific process *never* confirms theories at all. That is a tough limitation for a profession which every day tells people "we think this is what you should do", in other words, a profession, which by its essential task, *confirms* theories every day.

What EBM epistemologists tend to miss is that Popper was focused exclusively on the context of discovery. EBM does do research and it does discover things, but it also has to *evaluate* competing theories against each other to make decisions. One of the big problems and limitations of Popper's ideas is that they provide no grounds for this process and render such actions -- for example, replacing an old theory with a new and better one -- *not science*, but rather only conjecture (Goldenberg 2010). This is "a dramatic break from the common picture of science as an *inductive* process" (Goldenberg 2010). But it also badly undercuts the narrative of Popper-as-patron-saint-of-the-received-view-of-science-and-therefore-EBM-as-well. It is important that EBM's frequent invocation of Popper be understood as more of a chant that means "we are dedicated to rationality and science!" In reality, and quite to the contrary, it is widely recognized that Popper's proposal -- a theory of scientific reasoning that just *leaves out* any means of theory confirmation -- is of little use for an actual scientific practices like EBM. For EBM, Popper is utterly inadequate. A clinician faced with two competing theories can rest assured that Popper believes the clinician has no logical grounds what-so-ever for choosing one theory over the other (Goldenberg 2010). EBM, by Poppers light's, is by far mostly *not* scientific.

On the other hand, Popperian *values* do tend to resonate with EBM. EBM, reflects Popper when it encourages doctors not to resist disconfirming evidence or to be dogmatic in decision-making, and when it encourages doctors to be creative, while using critical thinking and open- mindedness -- these are all Popperian values (Goldenberg 2010). What EBM needs to keep in mind is that Popper was not a medical epistemologist attempting to outline the basis of clinical practice. For that matter, it is very important to acknowledge that Popper was not in agreement with the Positivists who argued that the world of human goings on could be clearly divided into science and non-sense. Popper believed that the kind of grand theorizing that Marx, Freud and Adler were up to was undeniably an essential metaphysical element of inquiry, just not science. Popper desperately wanted to knock Marx, Freud and Adler's un-testable, un-falsifiable, arm-chair pontifications off of the scientific podium, and in doing so he has caused generations of scientists to be paranoid over the status of their beliefs about "theory". This is certainly one instance in which Peirce would be very helpful in his capacity to sooth and coherently resolve such Popperian anxieties. But these anxieties ultimately originate from the failure of the EBM narrative to entrain an understanding of the mismatch between what Popper was doing and what EBM actually does. The gap between those two things constitutes a great deal of epistemological space, un-adjudicated by Popper, including all of the ontological judgements about

CAM. We, like Peirce, see EBM's original ontological judgements about CAM as firmly "scientific". EBM itself is making a huge mistake invoking Popper to assert otherwise.

Peirce conceived of metaphysics as continuous with scientific epistemology. Arguing essentially in opposition to what would eventually become the Positivist project, Peirce was attacking the common philosophical positions involving radically sceptical attitudes in his day. He argued that such arguments were not practical ways of looking at the world and that they amounted to an incoherent position which did not involve the essential processes of doubt and belief. As such, they were not legitimate (Peirce 1955, p. 228).

In some, or all of these respects, most modern philosophers have been, in effect, Cartesians. Now without wishing to return to scholasticism, it seems to me that modern science and modern logic require us to stand upon a very different platform from this.

1. We cannot begin with complete doubt. We must begin with all the prejudices which we actually have when we enter upon the study of philosophy. These prejudices are not to be dispelled by a maxim, for they are things which it does not occur to us can be questioned. Hence this initial scepticism will be a mere self-deception, and not real doubt ; and no one who follows the Cartesian method will ever be satisfied until he has formally recovered all those beliefs which in form he has given up. It is, therefore, as useless a preliminary as going to the North Pole would be in order to get to Constantinople by coming down regularly upon a meridian.

Peirce's assertion is immediately relevant to our claims regarding the conflict in EBM medical epistemology over CAM. There is a fight raging between those in EBM who would privilege the time-honoured scientific metaphysical evaluations of "the scientific basis" of a treatment as a necessary, if not sufficient, element of efficacy evaluations, and those who would advocate that all such analysis should be scrapped, at any point, if it is suggested that the only thing that can ever be used in the evaluation of efficacy is an experiment -- specifically, an RCT.

The similarity between the narrow EBM belief that RCT efficacy testing must be exclusively reserved as the only admissible form of efficacy evidence, and the positivist desire to erect science exclusively on the foundation of experience, should be clear. In the same way, the more elegant, intuitive, historically faithful and essentially functional scientific metaphysics that Peirce was affirming in the rejection of Cartesian scepticism can be seen to be getting at the same issues.

Our argument is in line with Peirce, that scientific metaphysics is essential, it is historically part of the EBM's tradition, and it is the key to understanding the nature of CAM as ontologically, metaphysically, unscientific, implausible and not nearly as apparently effective as it would have to be to overcome such dire metaphysical flaws. A miracle would, after all require equally miraculous evidence to be considered. The confident embrace of metaphysics is the way out of EBM's epistemic conflict over CAM's efficacy, the way to see CAM for the failure of professionalism it is.

Part IV: The implications of our epistemology for public health, regulatory and higher education policy

Introduction to Part IV

It is our belief that CAM can best be understood as an artefact of the failure of medical professionalism. In this section of the thesis, we consider the changes in the status quo warranted by our findings. To use scientific metaphysics with authority, is to engage with Peirce's pragmatic, communal basis of scientific practice and expertise, and in doing so, adjudicate CAM as an understandable but pernicious error, and to move forward to rebuild the medical marketplace and the medical contract in the form of Parson's vision of medical professionalism. First however, we must carefully articulate exactly what we believe we have a warrant to change. We must consider the CAM phenomena as an artefact of the failure of professionalism by articulating the changes indicated by our universal, pragmatic medical epistemic ideal -- an action also understandable as articulating what we would suggest is required to fix the harms caused by CAM's anti-professionalism.

The adjudication of CAM efficacy

At least in terms of public statements, EBM has held onto its ontological commitment to naturalism. There is no sign in any of its public statements, that any of CAM's mistaken ontological ideas are validated by EBM. On the NHS website on Homeopathy, the ontological "principles upon which homoeopathy are based" are rejected outright and dismissed as "scientifically implausible"⁴⁵. But other modalities, including those with statutory regulatory status have negotiated a politically savvy compromise with EBM in regard to their questionable ontological commitments. The NHS web page on Chiropractic is extremely interesting in this regard in that the ontological issue is pointedly absent from the introduction, and the first three subsections including: "*Uses*"; "*Chiropractic and the NHS*"; and "*Does it work?*" It is only in the last subsection, entitled "*The History of Chiropractic*" that Chiropractic's origins as a vitalistic panacea are revealed⁴⁶.

Palmer argued that most human disease is caused by misalignments of the spine that apply pressure on surrounding nerves. He called these misalignments "subluxations" (a term also used in conventional medicine, where it has a different meaning) and believed that they blocked the flow of a natural energy, or "life force", through the body. Correcting these subluxations, he

⁴⁵ The NHS web page on Homeopathy is available at the following URL and was accessed on September 20, 2014:

<http://www.nhs.uk/conditions/homeopathy/pages/introduction.aspx>

The NHS web page on Chiropractic is available at the following URL and was accessed on September 20, 2014:

⁴⁶ <http://www.nhs.uk/conditions/chiropractic/pages/introduction.aspx>

argued, could restore the proper flow of energy, and so restore health. Thus, he saw chiropractic spinal manipulation as a treatment for 95% of all health conditions.

There is no specific rejection of these ideas on the web page, however, like there is at the top of the NHS Homoeopathy page. Instead, the rejection is implied, like a kind of naturalist dog-whistle, directed only to people who are already inclined to scientific metaphysics, while the page's content remains circumspect in regard to any judgement of Chiropractic's ontological status, implying some kind of lawyerly victory in negotiations over the text, achieved for Chiropractic -- but failed for Homoeopathy. Meanwhile, not only is a clear adjudication of Chiropractic's ontology utterly missing from the whole page, things get even more confusing as the "History of Chiropractic" subsection continues:

More recently, elements within the profession have sought to place chiropractic on a more scientific footing through research to establish an evidence base for its principles and practice. Today, Palmer's ideas do not always form the basis on which chiropractors practise, but this varies widely between individual chiropractors. The GCC says the idea that subluxations are responsible for illness "is not supported by any clinical research evidence" and that this idea should be taught as a historical concept and not a current theoretical model.

The second half of the passage here is citing the Chiropractic statutory regulatory entity under the Department of Health, the General Chiropractic Council (GCC). It is interesting in itself that apparently this government body has admitted that the ontological basis of Chiropractic as a panacea "is not supported by any clinical research evidence". It is equally interesting that there is no link to the content referred to in this regard and we have not been able to find it on the GCC web site, this is a laps in protocol uncommon to the NHS web pages and a warning sign that the information in question is effectively being hidden from the public.

The relatively weak and very specifically non-metaphysical claim "not supported by any clinical research evidence" appears to be an EBM capitulation to one of the two mistaken ideas we identified, that EBM tends to entertain regarding these matters: that metaphysics must be strictly barred as evidence in adjudications of clinical efficacy. As we have mentioned, this is mistaken, and by contrast EBM has it right on their Homoeopathy page in that regard.

We would argue that this inconsistency itself is a problem, however, the inconsistency appears to be connected to the first part of the passage where it is claimed that "elements" within chiropractic have sought to make Chiropractic "more scientific" by using research to establish an "evidence base" for its "principles and practice." It is important to unpack this claim and be clear that what is being argued here is a species of the second kind of error that EBM is prone too with regard to efficacy testing: the idea that since there could be some kind of as yet undiscovered, but pragmatically legitimate, scientifically identifiable mechanism underlying Chiropractic's efficacy, that we should continue to test it indefinitely, and by extension apparently, *use* it as well. Unfortunately, it is not clear what "principles" refers to here. Are these Palmer's original vitalistic ideas already dismissed by the

GCC or something else? The next sentence confuses matters more by introducing additional uncertainty. We are informed that currently: "Palmer's ideas do not always form the basis on which chiropractors practise, but this varies widely between individual chiropractors."

Is it acceptable that some Chiropractors cling to Palmer's original vitalistic panacea, while the GCC maintains there is no evidence for it? What has replaced these beliefs as "principles" in the minds of the rest of the Chiropractors? What is the ontological status of the replacement beliefs? Is it acceptable that Palmer's disturbing set of ontologically mistaken beliefs are widely embraced by the community of Chiropractors, even if the extent to which theses mistaken beliefs are embraced "varies" widely as well? What is the status of the research into the "evidence base for its principles"? What makes chiropractors different, better or worse than EBM physiotherapists?

The last two paragraphs on the NHS web page on Chiropractic continue to read like a gruelling negotiation between teams of attorneys. After a circumspect quotation from the GCC which purposely dodges the ontological question by suggesting vaguely that Chiropractors are "concerned" with the "musculoskeletal system", the last paragraph suggests: "Nonetheless, some UK chiropractors continue to claim that they can improve a range of health conditions by correcting subluxations". "Subluxations" as it is used here, is a direct reference to Palmer's original vitalistic panacea, while the "range" of "conditions" referenced here can and should be read as confirmation that the "panacea" nature of those mistaken beliefs is, in fact, intact in the beliefs of some practicing Chiropractics.

There are two salient observations to be made here: 1.) EBM holds firm, uniformly and *professionally* to its ontological commitment to naturalism; and 2.) Chiropractic, *still*, does not, with the GCC specifically failing to commanding uniformity of belief among its members -- a characteristic which must be recognized as a fundamentally anti-professional characteristic. Importantly however, the only way to make sense of the NHS web page on Chiropractic is *not* to read it as an EBM adjudication, but rather to read it as *an argument between* EBM and the Chiropractic establishment, now bolstered by statutory regulatory status and as such, entitled to *speak for itself through* the official conduits of EBM communications to the public.

This invasion of EBM authority, by what can only be understood as the official Department of Health, Statutory Regulatory Authority representing Chiropractic in their ongoing quasi-legal battle over what is said on NHS web pages, is a problem distinct at this point from EBM's official position regarding CAM. We will address this issue shortly, however for now, we believe that in spite of the confusion of the NHS web pages, EBM has never backed down from its native ontological commitments. EBM medical authorities in the UK have never made any official statements we are aware of, proposing to accept the fundamental premises of medical pluralism: the idea that multiple, incompatible ontological epistemic systems should be regarded within the universal medical epistemic framework, as epistemic equals.

EBM's native confusions about CAM do involve some, but not all, EBM metaphysical adjudications, being arbitrarily subjected to invalidation, by one or both of two mistaken beliefs: 1.)

the belief that some aspect of the scientific system should necessarily prioritize the infinitesimally small possibility, that currently undiscovered in CAM modalities, there may be some kind of pragmatically powerful mechanism, and that this belief is then taken to warrant the eternal study of CAM techniques, including modalities which, can never be definitively proven not to work by the methodological limitations of clinical experimental architecture⁴⁷; and 2.) the mistaken application of a clinical research convention, the belief that only clinical efficacy testing itself may ever be used, under any circumstances, to determine pragmatic reality. An associated belief can be expressed in the form of another rule: that ontology and, according to some, induction, are strictly prohibited as sources of evidence in the evaluation of efficacy.

These two mistaken beliefs are responsible for all of the confusion in EBM's pronouncements about CAM. These ideas appear to be both native to EBM as well as imposed by CAM's advocates, who were levelling this argument in the BMA journal almost 100 years ago. These two mistaken beliefs have resulted in an erroneous, conflict ridden adjudication on CAM's efficacy by EBM authorities on CAM.

By way of contrast, we would offer our version of a correct EBM adjudication of CAM as follows:

- 1.) The defining characteristic of CAM modalities is that they are initially conceived with dubious, usually non-scientific, ontological commitments. Even if scientific theories are later introduced as possible mechanisms, legitimate scientific theories are not the "basis" for these therapies initially.
- 2.) Because of scientific medical professionalism's historically and pragmatically grounded naturalism, these therapies, when they first came into contact with scientific medicine, were immediately adjudicated as mistaken and appropriately rejected.
- 3.) There has *never* been any perfectly blinded, bias and error free clinical trials which have proven a CAM modality is effective beyond placebo, in spite of the fact that there is at least one one-million dollar prize in place for doing so today which has been in place for several years.⁴⁸

⁴⁷ We should also keep in mind that there is an ongoing dispute regarding the potential to confirm the null hypothesis at all, as there is a concern that this involves the use, and therefore the justification of induction. As a result, there are those in EBM who argue that by their reading of the limitations of the experimental method, it is *impossible* to *ever* prove that a medical technique is wholly ineffective as the experimental method never confirms the null hypothesis.

Stanford Encyclopedia of Philosophy page on Philosophy of Statistics contains a discussion about this under the subheading "1. Statistics and induction". We were able to retrieve the page from the following URL on September 18th, 2014.

<http://plato.stanford.edu/entries/statistics/>

⁴⁸ One Million Dollar Paranormal Challenge

http://en.wikipedia.org/wiki/One_Million_Dollar_Paranormal_Challenge#History

We've always said that homeopathy is eligible for the million-dollar prize

<http://archive.randi.org/site/index.php/swift-blog/581-homeopathy-qualifies-for-the-million-dollar-challenge.html>

4.) All the clinical trials ever cited as proof of CAM efficacy, were fundamentally open to multiple forms of bias and error.

5.) All of the multiple sources of bias and error which are fundamentally inherent in the limitations of the research design architecture which makes the double-blinding of CAM modalities impossible, have in various circumstances been proven to be sources of error sufficient to account for all of the data that EBM is currently reporting as proof of CAM's efficacy. In other words, all of the data can be better accounted for solely as error.

6.) There is no existing evidence, which currently contradicts the following hypothesis: CAM is uniformly clinically ineffective. EBM judgements to the contrary are the result of arbitrary, overly optimistic meta-analytic judgements.

7.) Taken in aggregate, these factors constitute a warrant for changing EBM's currently confused and inconsistent adjudications of the popular CAM modalities. Instead, EBM should universally deploy the unequivocal pronouncement at the top of the NHS homeopathy web page: "[X CAM therapeutic modality] remedies perform no better than placebos, and the principles on which [X CAM therapeutic modality] is based, are scientifically implausible."

Further EBM actions necessitated by medical professionalism

As both Parsons and Peirce would undoubtedly observe, the failure to properly adjudicate CAM efficacy constitutes a significant professional failure on the part of EBM. However, the adjudication of CAM's efficacy, by itself, is a very narrow, if central and indispensable aspect of EBM's wider responsibility regarding CAM. Having proposed this change to EBM's epistemic posture to CAM efficacy, we can now explore what this change means for the other obligations extending from medical and other forms of professionalism. CAM is a massive industry in the UK and the Western world. The fact that CAM is fundamentally mistaken and does not work, necessarily resolves confusion about CAM's place in the medical marketplace and in EBM. This in turn, constitutes a warrant for radical changes in public health policy, the regulation of trading standards and the regulation of higher education. We will take each of these up in turn, exploring the changes required to bring the various actors back in line with a prudent expression of their respective forms of professionalism. As a consequence, we will expose the wide ranging failures of professionalism which made CAM possible.

Public Health Policy

Our scope of "public health policy" for the purposes of this section of the thesis, is bounded by the reach of the UK Department of Health. Realizing that the ideal *means* by which the state utilizes experts in the prosecution of its duties in a democracy is a matter of significant philosophical contention, as well as an unresolved and inconsistent matter in practice, we will, in setting the terms of this discussion, collapse those difficulties into an assumption of rationality and assume both Perician values and conventions of functioning scientific communities, and Parson's vision of medical professionalism. For the purposes of this discussion, we will consider *hypothetically* idealized state actors -- actors conforming to Peirce's scientific modes of fixing belief and Parsons medical professional behaviours. As such, our hypothetical state actors are essentially continuous with EBM, and by extension, our universal pragmatic epistemic medical model. In other words, we are exploring *the ideal public health policy* in regard to CAM, using our epistemic preferences.

Following our re-adjudication of CAM efficacy, CAM can now be seen as inconsistent with EBM professionalism. CAM statutory regulatory status can now be seen as the mistaken elevation of non-expert, non-professionals to positions of state power and to some extent, to scientific authority. As such, the General Chiropractic Council (GCC) and the General Osteopathic Council (GOC) should be immediately abolished. So too, all CAM pseudo-professionals should be excluded from any involvement in the entirety of the UK medical system under the UK Department of Health. A similarly wide ban on the use of any CAM techniques should be extended to the UK medical system. However, since we are considering the consequence of the EBM wide, uniform embrace of our epistemology, a natural consequence of this would include the voluntary cessation of a wide range of Doctor behaviours now common in the status quo, including: referral patients to CAM therapists, advocacy for CAM to patients, including the communication of personal approval of CAM to patients and the communication of personal agnosticism toward CAM to patients.

All this activity, upon the adoption of our epistemic position, would of course, voluntarily cease. All medical personnel would instead be uniformly trained in the narrative medical skills required to sensitively, yet effectively convey to patients the mistaken status of CAM beliefs, being uniformly compelled to do so as a matter of medical professionalism. The reader should keep in mind that we are in the process of describing public health policy in terms of an epistemic ideal, not a dystopia authoritarian regime. We understand the practical limits of attitude change, although there is some reason to expect epistemic uniformity in a professional context. All we are doing here, is exploring a failure of professionalism. In fact, the very idea that we might not, in the real world, immediately get 100% compliance out of doctors in regard to the list of behaviours above, is evidence of the depth of the failure of medical professionalism. Similarly, our reflexive uneasiness with

assuming uniform compliance to naturalism in the practice of medicine, highlights the political vulnerability of a uniform opposition to pernicious ontological beliefs -- a political vulnerability which may be as much of a reflection of the pernicious social network influence of the massive CAM entrepreneurial complex, as anything else.

Trading Standards

This brings us to European Trading Standards law, and the failure to curtail the sale of ineffective medical treatments and products. Historically, the UK has been extremely permissive in this regard. The current system, although it appears to be having significant success in curtailing false *public* claims made by CAM in the course of commercial activity, is not addressing the *private* communications of CAM therapist and patients -- a concern exacerbated by the evidence that CAM practitioners are not actually coming around to science as the likes of Cant and Sharma would have us believe. From a network science standpoint, there are no major obstacles to the continued maintenance and growth of the massive social network which functions to propagate pernicious CAM beliefs -- in particular the general anti-science attitudes which are propagated as a by-product of CAM commercial activity. Aside from grave material harms like the clear link between CAM commercial activity and vaccine preventable disease epidemics, we have developed a strong case for the claim that CAM's continued commercial activity is doing significant harm: 1.) to the fabric of the medical contract; 2.) to the promise implicit in medical professionalism that medical professionals have integrity, will not lie to patients, are not negligent, and offer only the most pragmatically potent therapies to patients, or are honest with them if none are available. Of course, violating such promises and professional expectations is not just harmful, but also wrong, and there is every reason to believe these anti-professional violations of the medical contract are inherent in CAM's practice and are constantly employed in the exploitation of almost half the UK population. Included in this number are the 11% of UK residents who are CAM users, but to whom CAM has failed to explain what CAM is, and as a result, has exploited these people by accepting their money, while they had no understanding of what CAM is vis-a-vis EBM.

The fundamental nature of CAM as a vast and growing anti-profession, constitutes the single greatest existential threat to medical professionalism itself. By the professional obligations of all relevant actors, regulatory or otherwise, CAM's anti-professional status constitutes a warrant for a much more aggressive effort to eliminate CAM commercial activity. In full recognition of the massive economic and political upheaval undoubtedly associated with such a radical step, the very same epistemic warrant we used to rid the NHS of CAM must also be employed to stop CAM commercial activity, where, it is arguably doing a great deal more harm. The public understanding of science project, which would be required to help the public understand such a radical regulatory move, would

be considerable. However, the harms associated with the status quo are intolerable, entrenched and growing. Radical action is the only professional choice in this regard.

Higher Education

In light of our adjudication of CAM's efficacy and CAM's anti-professional status, it should be clear that CAM university degree programs and training schools are unethical, and the single greatest driver for the expansion of the CAM social network, along which pernicious medical beliefs are transmitted. However, UK medical school curricula should be also be immediately reviewed, an action we plan to take up subsequent to the completion of this project. Medical school students, like doctors, in our ideal epistemic universe would be immediately agreeable to a revamp of the curriculum which would clear up any of the confusions regarding CAM which are now widespread in EBM.

Conclusion to Part IV

Worldwide, if medical pluralism is not opposed, governments looking to save money will continue to choose healthcare development proposals which exploit nationalism and ignorance by replacing pragmatically potent EBM resources with cheaper potentially impotent or even dangerous Traditional medical options. This policy, known as "supporting traditional medicine" or "intercultural health", is a widely heralded public policy position specifically endorsed this year by epistemic authorities as diverse as The World Health Organization⁴⁹ and international development banks⁵⁰. It is

⁴⁹The web page delineating the WHO traditional medicine strategy: 2014-2023 states: "The strategy aims to support Member States in developing proactive policies and implementing action plans that will strengthen the role traditional medicine plays in keeping populations healthy." The content was retrieved on September 15, 2014 at the following URL:

http://www.who.int/medicines/publications/traditional/trm_strategy14_23/en/

⁵⁰The whitepaper entitled *The Best Practices in Intercultural Health* by John O'Neill, Judith Bartlett and Javier MignoneInter, published on line by the Inter-American Development Bank, Washington, D.C., Sustainable Development Department, Best Practices Series Publication, June 2006. States the following on page 3: "The Bank's Strategy for Indigenous Development, which accompanies the policy, states that, in order to narrow the quantitative and qualitative gap in health services received by indigenous peoples, the Bank should support specific affirmative and socio-culturally appropriate actions that take into account the importance of strengthening indigenous traditional practices, breaking down cultural barriers that limit access to public health services, and coordinating allopathic and indigenous health care systems. In addition, it encourages pilot project initiatives to 'support intercultural health systems.'" By way of a definition, the following quote from the same page in the next paragraph of the white paper, defines what they mean by "intercultural health":

also epistemically incoherent. Just as in the West, non-scientific medical special interest groups will lobby public officials in an attempt to gain more lucrative arrangements and in order to preserve their status as "healers" without professional constraint.

Our universal, pragmatic medical epistemic system is the only way to embrace an altruistic commitment to the fundamental human needs associated with health care. Still, it could not be more clear that as we continue to work through the long process of these inevitable changes internationally, we must consult Anthropology to better understand the extent which pernicious health beliefs are part of what Traditional Medical practitioners actually communicate privately with patients. The adversarial nature of this essential change in the approach to Anthropology, requires that Anthropologists come to terms with the ontological metaphysics required to appropriately adjudicate the efficacy of medical techniques, without conducting RCT's on every single medical practice on earth. This extremely radical shift in Anthropology is unavoidable if Anthropology is to contribute meaningfully to our universal pragmatic epistemology. In fact, if Anthropology continues to cleave to non-realism the extent that they are unable to do this important work, another discipline will simply have to be drafted to do it for them.

As the anti-vaccination research clearly shows, CAM is not always 100 % materially benign in the West and Traditional Medicine will not likely prove to be so in the rest of the world. Neither will CAM, which is already expanding rapidly into the unregulated medical markets in under-developed countries. Robust social network analysis should be combined with more aggressive ethnography to determine what kinds of beliefs are in play and what the associated harms might be. In any case, the same universal methods should be employed by EBM around the world: sufficient Anthropological research should be combined with culturally applicable narrative medical approaches which allow for the communication of EBM's naturalistic position to be emphasized in the way which promotes the best possible patient understanding and autonomy.

Thesis Conclusion

EBM's original, metaphysical adjudication of CAM was correct. Only two mistaken beliefs have interfered with what would otherwise be a perfect understanding of CAM's pragmatic nature, derived from a metaphysical analysis of CAM's basis for action. The first involves the over-emphasis of a clinical methodological rule. The second involves and the over emphasis of the potential importance of future pragmatic medical discoveries within CAM treatments and the failure to

"...intercultural health practices to promote indigenous peoples' access to allopathic health as well as to strengthen those traditional health practices based on indigenous peoples' own knowledge, culture, social networks, institutions and ways of life, that have shown their effectiveness."

This paper was retrieved on line on September 15, 2015 at the following URL:

<http://www.iadb.org/wmsfiles/products/publications/documents/1484415.pdf>

articulate when such hopes and the associated research programs into failed CAM modalities may be abandoned. Due to these simple errors, the EBM conclusion on CAM's efficacy is mistaken. The meta-analytic criteria which EBM has established for "efficacy" in the case of CAM has been established in error and presumably as a result, the language of EBM epistemic authorities, for example, on the NHS web site -- including, that there is "good evidence" that Chiropractic "can be an effective" treatment for some medical problems -- is misleading and must be changed.

We identify that the first two mistakes made by EBM in contradiction of its initial metaphysical adjudication of CAM modalities have given rise to a broader failure on the meta-analytical level, involving the use of the language of certainty to describe the outcome of multiple meta-analytic evaluations of clinical experiments, the designs of which cannot possibly be understood to demonstrate certainty in this regard. The lack of blinding inherent in the clinical evaluation of most CAM modalities casts a "shadow" over the line separating the two possibilities other than dramatic and indisputable efficacy regarding effects beyond placebo: *marginal effects*; and *no effects*. Under this "shadow", the line between marginally effective and ineffective is forever and irretrievably obscured.

This confusion related to CAM efficacy has concentrated on conditions which intersect at four criteria: common; non-life threatening conditions; which also happen to be medically intractable; and are expressed only as pain or discomfort. These characteristics are often contained in lists of the characteristics of conditions most commonly treated by CAM modalities, as understood by a wide variety of CAM analysts, from medics to sociologists.

The most subtly operative characteristic of the above four is "intractability". The fact that there is nothing in the medical cannon available to help people with certain problems, operates here in a very specific way. People suffering from back pain, for example, which can often only be helped with powerful and thus dangerously addictive narcotics, are badly frustrated by the total lack of tolerable medical solutions. This frustration, more than creating a market for CAM, also lowers the criteria for "efficacy" in the minds of EBM clinical meta-analysts. Urgent motivations to help patients by detecting even the most marginal levels of relief⁵¹, in combination with the powerful placebo and expectancy responses likely associated with CAM therapies -- therapies which cannot be blinded in *any way* in clinical trials -- have resulted in the criteria for "proof" of efficacy being located well into the wide band of obscurity created by methodological limitations. Adding even further to this obscurity is the additional source of systematic error inherent in the self-reporting bias of patients. Such studies boil down to asking people if Chiropractic made their back feel better or not. When all of the sources of error characteristic of this "shadow" over the terrain of modalities subject to clinical

⁵¹ At this point in the growth of the CAM medico-industrial complex, we would be naive to fail to acknowledge that EBM meta-analysts are under terrific pressure to be generous with their placement of the criteria for efficacy in their meta-analytic decision-making. Although we believe that the altruistic values endemic to EBM professionalism can be reasonably understood to be the basis of the motivation for this meta-analytic interpretive error, clearly pressure from CAM advocates, must also be a factor.

research are held simultaneously in perspective, EBM's mistake here becomes undeniable. Whether the mistake is conservatively phrased in the negative -- it is wrong to label something so clearly unknowable as certain -- or in the more metaphysically integrated claim -- CAM is almost certainly, uniformly a placebo -- EBM's current posture is mistaken.

As we have repeatedly observed, even if there are marginal effects associated with CAM treatments, which are unrelated to placebo effects or suspect ontological commitments, the likelihood that these effects will in the future be discovered and become the basis for a revolutionary, pragmatically powerful collection of medical techniques, is vanishingly small. Yet this, and the misapplication of the narrow ideology of clinical testing as a global epistemic claim, are the only arguments invoked against the vast web of scientific metaphysics which justly adjudicated CAM modalities like Osteopathy and Chiropractic the moment they emerged as whole-cloth panaceas from the minds of their crank and quack originators respectively. Scientific medical metaphysics has already adjudicated the most popular CAM modalities as pernicious myths. In leaning too far in the direction of only two other mistaken ideas, EBM has made a grave error in choosing to endorse the mistaken notion that CAM modalities are effective.

This profoundly unfortunate decision, exacerbated by the failure of the medical authorities to persuade the state or the courts to be less credulous and to more aggressively restrict the practice of non-scientific medicine, has allowed for the continued and now massive growth of the most anti-professional forces in the medical marketplace. The harms associated with this process are not limited to the physical harms of dangerous or ineffective treatments supplanting effective ones, rather they extend to the damage done to the medical marketplace via the vast and growing social network of people transmitting pernicious CAM beliefs. CAM's entrepreneurial anti-professionalism is destroying the fabric of the medical contract and the core principles of medical professionalism itself. This process is accelerated even more by the proliferation of CAM training schools, university CAM degree programs and the integration of CAM ideas into medical schools and medical practice.

The failure of Sociology and Anthropology to seriously consider the issue of efficacy, to default reflexively to the attack of EBM and advocacy for CAM, is as deeply damaging as it is professionally unacceptable. The realist disciplines working earnestly on the CAM and medical pluralism problem, some of which, like experts on clinical research, are already confused by their own debilitating epistemic shortcomings, are baffled by Sociology's unrestrained attacks on basic medical values like altruism, honesty and expertise. Sociology's self-identified failure to connect expertise and authority is realized as a license to ignore medicine's profound pragmatic medical powers, to libel their motivations and place the full force of academic authority behind the flagrant advocacy for EBM professionalism's most dire existential threat.

There can be no doubt that non-realism in academia, deployed in realist domains like medicine, must face the reckoning inherent in the realization that it is fundamentally incommensurable with any working notion of academic professionalism. As such, non-realist

academics, writing in realist domains, exist outside of the professional expectations, values and virtues observed as the basis for the understanding of academic rigour in our society. This understanding, by our lights, is not merely a social construction, but precisely the larger communal effort Peirce identified as the universal, collective, and *the only* process by which humans can best accomplish their pragmatic goals. non-realist academics, Sociologists, Anthropologists and others, currently work outside of this communal effort, rejecting its values as well as its authority. All the while, anti-realists, unrestrained by any realist interpretation of professional academic values, unencumbered by any professional commitment to respect the expert authority of other disciplines, persist in their effort to influence realist debates, taking pride in the "double-hermeneutic." We are tempted by these matters to level an accusation of anti-professionalism at these disciplines. Instead, for now, we observe that the "science wars" were merely a failed effort to come to terms with non-realism's incoherent legacy in Western academia. We are committed to continuing research in furtherance of a better understanding of this problem.

Non-realism however, does have its place. The religious elements of CAM, realized on a personal level, are clearly tied to fundamental human spiritual needs which, as Don Cupitt was so helpful in pointing out, cannot be adjudicated by anything other than pragmatic means. Religious non-realism reserves a space for the full variety of the expression of human spirituality, safeguarded by the commitment to scrutinize such beliefs for their real-world implications and effects. The disturbing tendency of CAM ideology to pin the responsibility for illness on the sick, engaging its bizarre ontological pantheon to identify hidden spiritual or personal failings as the explanation for illness, rivals the pernicious Christian trope Cupitt indicted, that events like giving birth to a child with deformities, are the wages of sin. The despicable use of such spiritual accusations as answers to the "why me" question, when used for commercial purposes by CAM "healers" in the sale of some form of redemption, must be criticized in a similar way. Whether in the form of a "juice cleanse" after a weekend of partying or in the form of expensive "spiritual healing" in the face of a cancer diagnosis, the narrative of CAM's business strategy must be unpacked and examined for such deeply pernicious elements. Certainly, the field of narrative medicine, or a similarly professionally restrained Sociology, could help to illuminate and adjudicate such atrocious abuse of the vulnerable. In the end, by our lights, new age religious ideas, if they can pass Cupitt's test, have a place in the human struggle for meaning which is in no way opposed to our pragmatic medical epistemic project.

For the non-Western world, scientific medicine holds the same universal pragmatic promises it does here. As we observed above, religious, spiritual, even magical and vitalistic components of indigenous belief systems can be accommodated by narrative medical analysis designed to funnel the benefits of EBM to those who for whatever cultural reason, may require some kind of specialized cultural means of accessing EBM. The point here is that EBM's narrative medical knowledge is expanding and can be directed interculturally just as easily. The dearth of clinical efficacy research that Anthropologists acknowledged regarding Traditional Medicines around the world should not be

interpreted as a license for vast CAM-style clinical trials directed at every traditional medical technique identified. Although this is precisely what is suggested by the statements some EBM epistemologists make regarding efficacy, the useless testing of an unlimited number of indigenous medical ideas would be a terrible mistake. Realist ethno-medicine is more than capable of looking through these traditions for something promising which might require testing. The idea that our pragmatic epistemic model is universal, is central to the thesis. We strongly believe that we all *owe* EBM to each other.

Finally, the Pragmatism of C. S. Peirce rests powerfully at the centre of our universal epistemic appeal. The heart of medicine is professionalism and the basis of professionalism is expertise. The origin of expertise is scientific knowledge. Pragmatically powerful scientific beliefs are derived by two things: 1.) scientific modes of thought, guided by expertise in scientific thinking as well as scientific knowledge pertaining to the field in question along with professional scientific values and behaviours; and 2.) the communal participation of scientists working together, in line with scientific professional values and modes of conduct. These two, easily understandable ideas, ground our epistemology in a universally phenomenologically accessible form of common sense. Such scientific means of "fixing belief" will eventually put us in touch with a pragmatic understanding of the real. This modest, metaphysically integrated pragmatic fallibilism fits better with EBM's values and professional goals than its current epistemology, by merely integrating its native scientific metaphysics. Although we have identified two, easily understandable epistemic errors EBM has made in regard to CAM, and the way these have contributed to mistaken meta-analytic criteria for efficacy, it could be argued that these errors are not significantly impacted by the potential integration of scientific metaphysics via our Peircian model. The mistaken adjudication of CAM's efficacy is much simpler than that.

We too, are part of the universal, communal Peircian epistemic project. As such, we will continue to work toward our goals of improving the world's only scientifically pragmatic medical system: EBM. As to medical pluralism, we believe we have answered all of the relevant questions. Medical pluralism, as a concept, is a product of the use of non-realism in a realist domain, and as such, is a mistake.

Bibliography

Abbott, A., 2009. Psychology: A reality check. *Nature*, 461(7266), p.847.

Aboulafia, M. (ed.) (2002) *Habermas and Pragmatism*. Routledge.

- Apel, Karl-Otto (1995) *Charles S. Peirce : From Pragmatism to Pragmaticism*. New York: Prometheus Books.
- Baer, H.A., 1987. Divergence and convergence in two systems of manual medicine: osteopathy and chiropractic in the United States. *Medical Anthropology Quarterly*, 1(2), pp.176–193.
- Baer, H.A., 1995. Medical pluralism in the United States: A review. *Medical anthropology quarterly*, 9(4), pp.493–502.
- Bakx, K., 1991. The “eclipse” of folk medicine in western society. *Sociology of Health & Illness*, 13(1), pp.20–38.
- Beauchamp, T.L. & Childress, J.F., 2001. *Principles of biomedical ethics*, Oxford university press.
- Berger, P.L. & Luckmann, T., 1991. *The social construction of reality: A treatise in the sociology of knowledge*, Penguin UK.
- Birx, H.J. & Birx, H.J., 2006. *Encyclopedia of anthropology*, Sage.
- Boghossian, P., 2006. *Fear of knowledge: Against relativism and constructivism*, Oxford University Press.
- British Medical Association, 1986. *Alternative therapy: report of the Board of Science and Education*, BMA.
- Cant, S. & Sharma, U., 2004. *A New Medical Pluralism: complementary medicine, doctors, patients and the state*, Routledge.
- Cant, S., Watts, P. & Ruston, A., 2012. The rise and fall of complementary medicine in National Health Service hospitals in England. *Complementary therapies in clinical practice*, 18(3), pp.135–139.
- Chiasson, P. (2001) *Peirce's Pragmatism: The Design for Thinking*. Rodopi Editions.

- Clark-Grill, M., 2007. Questionable gate-keeping: scientific evidence for complementary and alternative medicines (CAM): response to Malcolm Parker. *Journal of Bioethical Inquiry*, 4(1), pp.21–28.
- Clark-Grill, M., 2010. When listening to the people: lessons from complementary and alternative medicine (CAM) for bioethics. *Journal of Bioethical Inquiry*, 7(1), pp.71–81.
- Collins, H.M., 1981. Introduction: Stages in the empirical programme of relativism. *Social studies of science*, pp.3–10.
- Collins, H. & Pinch, T., 2005. *Dr. Golem: how to think about medicine*, University of Chicago Press.
- Crowley, V., 1999. *Jung: A journey of transformation: Exploring his life and experiencing his ideas*, Quest Books.
- Debrock, G and Hulswit, M. (eds.) (1994) *Living Doubt: Essays concerning the Epistemology of Charles Sanders Peirce*. Dordrecht and Boston: Kluwer Academic Publishers.
- Derkatch, C., 2008. Method as argument: Boundary work in evidence-based medicine. *Social Epistemology*, 22(4), pp.371–388.
- Downey, L. et al., 2010. Pediatric vaccination and vaccine-preventable disease acquisition: associations with care by complementary and alternative medicine providers. *Maternal and child health journal*, 14(6), pp.922–930.
- Edgar, A. (2011). Professional values, aesthetic values, and the ends of trade. *Medicine, Health Care and Philosophy*, 14(2), 195-201.
- Edgar, A. (2014). Professionalism in Healthcare, unpublished manuscript.
- Ember, C.R. & Ember, M., 2004. *Encyclopedia of Medical Anthropology: Health and Illness in the World's Cultures Topics-Volume 1; Cultures*, Springer Science & Business Media.
- Ernst, E. & Pittler, M., 2000. Re-analysis of previous meta-analysis of clinical trials of homeopathy. *Journal of clinical epidemiology*, 53(11), p.1188.

- Ernst, E., 2001. Rise in popularity of complementary and alternative medicine: reasons and consequences for vaccination. *Vaccine*, 20, pp.S90–S93.
- Ernst, E., 2008. The recent history of acupuncture. *The American journal of medicine*, 121(12), pp.1027–1028.
- Ernst, E., 2009a. Complementary and alternative medicine: between evidence and absurdity. *Perspectives in biology and medicine*, 52(2), pp.289–303.
- Ernst, E., 2009b. Complementary/alternative medicine: engulfed by postmodernism, anti-science and regressive thinking. *British Journal of General Practice*, 59(561), pp.298–301.
- Ernst, E., 2009c. Complementary/alternative medicine: engulfed by postmodernism, anti-science and regressive thinking. *British Journal of General Practice*, 59(561), pp.298–301.
- Ernst, E., 2010. Deaths after chiropractic: a review of published cases. *International journal of clinical practice*, 64(8), pp.1162–1165.
- Ernst, E. et al., 2008. *Oxford handbook of complementary medicine.*, Oxford University Press.
- Ernst, E. & Pittler, M., 2000. Re-analysis of previous meta-analysis of clinical trials of homeopathy. *Journal of clinical epidemiology*, 53(11), p.1188.
- Ernst, E. & Seip, R., 2011. An independent review of studies of “energy medicine” funded by the US National Center for Complementary and Alternative Medicine. *Focus on Alternative and Complementary Therapies*, 16(2), pp.106–109.
- Ernst, E. et. al., 2008. *Healing, hype or harm? A critical analysis of complementary or alternative medicine.*, Societas Imprint Academic.
- Evans-Pritchard, E.E., 1929. Witchcraft (mangu) amongst the A-Zande. *Sudan Notes and Records*, pp.163–249.
- Favret-Saada, J. & Cullen, C., 1980. *Deadly words: Witchcraft in the Bocage*, Cambridge University Press Cambridge.

- Feest, U. & Sturm, T., 2011. What (good) is historical epistemology? Editors' introduction. *Erkenntnis*, 75(3), pp.285–302.
- Feyerabend, P., 1975. *Against method: Outline of an anarchistic theory of knowledge*. Atlantic Highlands.
- FitzPatrick, M., 2010. The Greatest Benefit to Mankind. *BMJ*, 341.
- Foucault, M., 1963. *The birth of the clinic*, Routledge.
- Freidson, E., 1988. *Profession of medicine: a study of the sociology of applied knowledge*, University of Chicago Press.
- Freidson, E., 2001. *Professionalism, the third logic: on the practice of knowledge*, University of Chicago Press.
- Gabe, J.B.M.E.M.A., 2004. *Key concepts in medical sociology*, Sage.
- Goldenberg, M.J., 2010. From Popperian science to normal science. Commentary on Sestini (2009) "Epistemology and ethics of evidence-based medicine." *Journal of evaluation in clinical practice*, 16(2), pp.306–309.
- Good, B., 1994. *Medicine, rationality and experience: an anthropological perspective*, Cambridge University Press.
- Gordon, D., 1991. Female circumcision and genital operations in Egypt and the Sudan: A dilemma for medical anthropology. *Medical Anthropology Quarterly*, 5(1), pp.3–14.
- Gracely, R. et al., 1985. CLINICIANS' EXPECTATIONS INFLUENCE PLACEBO ANALGESIA. *The Lancet*, 325(8419), p.43.
- Green, E.C., 1999. *Indigenous theories of contagious disease*, Rowman Altamira.
- Haack, S.(1995) *Evidence and Inquiry Towards Reconstruction in Epistemology*. Blackwell Publishers.

- Harris, P. et al., 2012. Prevalence of complementary and alternative medicine (CAM) use by the general population: a systematic review and update. *International journal of clinical practice*, 66(10), pp.924–939.
- Hausman, Carl R. (1993). *Charles S. Peirce's Evolutionary Philosophy*. Cambridge University Press.
- Holt, S., 2008. The responses of alternative practitioners when approached about common childhood illnesses. *J New Zealand Med Association*, 121, pp.114–16.
- Hookway, C. (2003) *Truth, Rationality, and Pragmatism: Themes from Peirce*. Oxford University Press.
- Horwich, P., 1996. Realism minus truth.
- Hufford, D.J., 2003. Evaluating complementary and alternative medicine: The limits of science and of scientists. *The Journal of Law, Medicine & Ethics*, 31(2), pp.198–212.
- Hume, D., 1739. *A treatise of human nature*, [Online], Available: <http://www.web-books.com/Info/1053>[24 Aug 2014].
- Humpage, C., 2011. Opinions on research and evidence based medicine within the UK osteopathic profession: A thematic analysis of public documents 2003-2009. *International Journal of Osteopathic Medicine*, 14(2), pp.48–56.
- Ingold, T., 1994. *Companion encyclopedia of anthropology*, Taylor & Francis.
- Ingold, T., 1996. *Key debates in anthropology*, Psychology Press.
- James, W., 1912. *The Will to Believe: And Other Essays in Popular Philosophy*, Longmans, Green, and Company.
- Jadad, A.R. et al., 1996. Assessing the quality of reports of randomized clinical trials: is blinding necessary? *Controlled clinical trials*, 17(1), pp.1–12.
- Kapferer, B., 2002. Introduction: outside all reason: magic, sorcery and epistemology in anthropology. *Social Analysis*, pp.1–30.

Ketner, K. L. (ed.) (1995) *Peirce and Contemporary Thought*. Fordham University Press.

Kidd, I.J., 2013. A pluralist challenge to “integrative medicine”: Feyerabend and Popper on the cognitive value of alternative medicine. *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences*.

Kitcher, P., 2011a. Epistemology without history is blind. *Erkenntnis*, 75(3), pp.505–524.

Kitcher, P., 2011b. Philosophy inside out. *Metaphilosophy*, 42(3), pp.248–260.

Kleinman, A., 1997. Writing at the margin: discourse between anthropology and medicine, Univ of California Press.

Kleinman, A. & Gale, J.L., 1982. Patients treated by physicians and folk healers: a comparative outcome study in Taiwan. *Culture, Medicine and Psychiatry*, 6(4), pp.405–423.

Kunda, Z., 1990. The case for motivated reasoning. *Psychological bulletin*, 108(3), p.480.

Latour, B., 1993. *The pasteurization of France*, Harvard University Press.

Leslie, C., 1980. Medical pluralism in world perspective [1]. *Social Science & Medicine. Part B: Medical Anthropology*, 14(4), pp.191–195.

Levine, J. et al., 1979. Role of pain in placebo analgesia. *Proceedings of the National Academy of Sciences*, 76(7), pp.3528–3531.

Loustaunau, M.O. & Sobo, E.J., 1997. *The cultural context of health, illness, and medicine*, Bergin & Garvey Westport, CT.

Mareschal, D., Butterworth, B. & Tolmie, A., 2013. *Educational neuroscience*, John Wiley & Sons.

Martin, S.C., 1993. Chiropractic and the social context of medical technology, 1895-1925. *Technology and culture*, 34(4), pp.808–834.

- Murphey, M. G. (1993) *The Development of Peirce's Philosophy*. Hackett Publishing Company.
- Nahin, R.L., 2010. *Costs of Complementary and Alternative Medicine (CAM) and Frequency of Visits to CAM Practitioners: US 2007*, DIANE Publishing.
- Nussbaum, M.C., 2002. Moral expertise?: Constitutional narratives and philosophical argument. *Metaphilosophy*, 33(5), pp.502–520.
- Omer, S.B. et al., 2009. Vaccine refusal, mandatory immunization, and the risks of vaccine-preventable diseases. *New England Journal of Medicine*, 360(19), pp.1981–1988.
- Parker, M., 2007. Two into one won't go: Conceptual, clinical, ethical and legal impedimenta to the convergence of CAM and orthodox medicine. *Journal of Bioethical Inquiry*, 4(1), pp.7–19.
- Parsons, T., 1951. *Social system*, Routledge.
- Peirce, C.S., 1955. *Philosophical writings of Peirce* Justus Buchler, ed., Courier Dover Publications.
- Peirce, C.S., 2009. *Writings of Charles S. Peirce: A Chronological Edition, Volume 6: 1886-1890*, Indiana University Press.
- Polich, G., Dole, C. & Kaptchuk, T.J., 2010. The need to act a little more “scientific”: biomedical researchers investigating complementary and alternative medicine. *Sociology of health & illness*, 32(1), pp.106–122.
- Popper, K.R., 1963. *Conjectures and refutations*, Routledge & Kegan Paul London.
- Porter, R., 1999. *The Greatest Benefit to Mankind: A Medical History of Humanity (The Norton History of Science)*, WW Norton & Company.
- Posadzki, P. et al., 2013. Prevalence of use of complementary and alternative medicine (CAM) by patients/consumers in the UK: systematic review of surveys. *Clinical Medicine*, 13(2), pp.126–131.
- Putnam, H. (1995) *Pragmatism*. Blackwell Publishers.

- Rivers, W.H.R., 1924. *Medicine, magic and religion: The Fitzpatrick Lectures delivered before the Royal College of Physicians of London in 1915 and 1916*, Psychology Press.
- Rorty, R., Williams, M. & Bromwich, D., 1980. *Philosophy and the Mirror of Nature*, Cambridge Univ Press.
- Rosenthal, S. B., Hausman, C. R. and Anderson, D. R. (eds.) (1999) *Classical American Pragmatism: Its Contemporary Vitality*. University of Illinois Press.
- Sackett, D.L. & Haynes, R.B., 1995. On the need for evidence based medicine. *Evidence-Based Medicine*, 1(1), pp.5–6.
- Sade, R.M., 2003. Complementary and alternative medicine: foundations, ethics, and law. *The Journal of Law, Medicine & Ethics*, 31(2), pp.183–190.
- Selinger, E., 2006. *The philosophy of expertise*, Columbia University Press.
- Selinger, E., Thompson, P. & Collins, H., 2011. Catastrophe ethics and activist speech: Reflections on moral norms, advocacy, and technical judgment. *Metaphilosophy*, 42(1-2), pp.118–144.
- Sinclair, U., 1935. *I, candidate for governor: and how I got licked*, University of California Pr.
- Singh, S. & Ernst, E., 2008. *Trick or treatment: The undeniable facts about alternative medicine*, WW Norton & Company.
- Stoneman, P. et al., 2013. Incommensurable worldviews? Is public use of complementary and alternative medicines incompatible with support for science and conventional medicine? *PloS one*, 8(1), p.e53174.
- Stringer, M.D., 1999. Rethinking animism: thoughts from the infancy of our discipline. *Journal of the Royal Anthropological Institute*, pp.541–555.
- Sturm, T., 2011. Historical Epistemology or History of Epistemology? The Case of the Relation Between Perception and Judgment. *Erkenntnis*, 75(3), pp.303–324.

- Sugarman, J., 2003. Informed Consent, Shared Decision-Making, and Complementary and Alternative Medicine. *The Journal of Law, Medicine & Ethics*, 31(2), pp.247–250.
- Tambiah, S.J., 1990. Magic, science, religion, and the scope of rationality.
- Tanaka, M.M., Kendal, J.R. & Laland, K.N., 2009. From traditional medicine to witchcraft: why medical treatments are not always efficacious. *PLoS One*, 4(4), p.e5192.
- Thayer, H.S., 1968. *Meaning and action: A critical history of pragmatism*, Bobbs-Merrill Indianapolis.
- Ventola, C.L., 2010. Current issues regarding Complementary and Alternative Medicine (CAM) in the United States: Part 1: the widespread use of cam and the need for better-informed health care professionals to provide patient counseling. *Pharmacy and Therapeutics*, 35(8), p.461.
- Vohra, S. et al., 2005. Integrating complementary and alternative medicine into academic medical centers: experience and perceptions of nine leading centers in North America. *BMC Health Services Research*, 5(1), p.78.
- Wampold, B.E. et al., 1997. A meta-analysis of outcome studies comparing bona fide psychotherapies: Empirically, “all must have prizes.” *Psychological bulletin*, 122(3), p.203.
- Weldon-Linne, M., 2000. Complementary and Alternative Medicine: Legal Boundaries and Regulatory Perspectives (review). *Perspectives in Biology and Medicine*, 43(4), pp.612–613.
- The World’s Religions: A Lion Handbook, 1984., Herts, The Lion Publishing