The Epistemological Aspects in the Historical Works of Don Bates

For Don, from one unremitting Kuhnian to another

KENNETH ROCHEL DE CAMARGO, JR.

Abstract. The object of this paper is to systematize an epistemological framework of analysis derived from Don Bates’s extended essay “Medicine and The Soul of Science,” and apply that framework to a number of problems connected to medical knowledge, addressed in previous research by the author. The paper also draws from Bates’s earlier work, especially the two-part “Closing the Circle” on William Harvey and the reception of his ideas by his contemporaries, and from contrasting and comparing it to the work of philosophers and historians of science who tackled similar problems, most notably Ludwik Fleck, Thomas Kuhn, and Ian Hacking. The resulting framework is based on three main concepts: constructed coherencing, the unproblematic background knowledge (UBK), and the mechanical mind. The paper closes with an application of that framework to the discussion of knowledge in medicine and the definition of diseases.

Keywords. epistemology, science studies, history and philosophy of medicine, diseases, causality

Résumé. Le but de cet article est de systématiser un cadre épistémologique d’analyse, tiré de l’essai « Medicine and The Soul of Science » de Don Bates, et de l’appliquer à un certain nombre de problèmes, en lien avec la connaissance médicale, soulevés par l’auteur dans le cadre de recherches récentes. L’article s’appuie également sur les travaux précédents de Bates, plus précisément, « Closing the Circle » sur William Harvey, afin d’évaluer la réception de ses idées par ses contemporains et de les comparer au travail des philosophes et des historiens de la science qui ont abordé des problèmes semblables, notamment, Ludwik Fleck, Thomas Kuhn et Ian Hacking. Le cadre résultant est basé sur trois concepts principaux : « constructed coherencing », les connaissances tacites de base (UBK) et l’esprit mécanique. L’article propose finalement une application
de ce cadre d’analyse sur le discours sur la connaissance médicale et la définition des maladies.

Mots-clés. épistémologie, études de la science, histoire et philosophie de la médecine, maladies, causalité

INTRODUCTION

The purpose of this paper is to present the epistemological framework sketched in the later works of Don Bates—particularly his two-part paper on Harvey and the essay which forms the cornerstone of the present collection “Medicine and the Soul of Science”—and to demonstrate its application in a different context, namely to help analyze medical knowledge. It should be noted however that whereas his work is based on a thorough historical approach, this paper is focused on the epistemological implications of such studies, and as such, most (if not all) of the actual historical material will be left out. Since Bates wrote himself that the nature of scientific knowledge was one of his main concerns, I believe that addressing these specifically is a minor deviation that he would not disapprove of.

It was early September 2000 and I had barely begun the year I would spend as a postdoctoral fellow at McGill’s Department of Social Studies of Medicine, working under Allan Young. As per Professor Young’s suggestion, I was trying to contact the other professors of the Department in order to explore possible common interests.

Don’s office was right next door to mine, and he was one of the first appointments I had in those early days. We talked briefly and I handed him my research proposal and a related paper I had co-authored with my former thesis supervisor a few years before. I have to say I did not feel particularly welcome or encouraged in that brief first contact, but I was in for a memorable surprise.

One or two days after that, Don showed up at the door of my office, positively beaming, and really enthusiastic about the parallels he saw between what I was interested in researching and what he was working at that moment. He presented me with hardcopies of almost all of his recent works, including the seminal two-part paper on Harvey’s ideas and their reception. That pleasant moment extended into a number of agreeable lunches at the Thomson House on McTavish Street, where we discussed the history and philosophy of science and medicine, the structure of academic institutions and work in Canada and Brazil, world politics and almost every other subject.

It was during one of those lunches that Don told me he had recently been diagnosed with ALS, and he told me with grief that he felt he
would not be able to write the book that he was preparing in the previous years. And there what was already admiration for Bates the scholarly author became awe for Bates the man; where others would probably falter, Don went on to write the extended essay “Medicine and The Soul of Science” and began planning a seminar to discuss his ideas; he wanted others—his colleagues—to finish what he wouldn’t be able to. And it was then that I received probably the highest honour in my career, when Don himself invited me to take part in that future seminar.

Don’s illness was brutal, and swift. I had barely begun to learn from him and he was gone. While I was writing the paper I came back to George Canguilhem’s work and ended up modelling the title of my own paper after a text he wrote in honour of Gaston Bachelard, his former professor and mentor (“L’histoire des sciences dans l’ouvre épistémologique de Gaston Bachelard”). In retrospect, I believe that was a way of stressing how much I learned from and felt indebted to him, despite the briefness of our contact.

A few years later the seminar took place at the new location of the Department at Peel Street, with a thorough and pointed discussion of the arguments that Don presented in his essay. I believe Don would have enjoyed it.

The paper is structured in three parts. The first delineates the aforementioned framework, quoting extensively from the two texts. The second presents a discussion on the definition of knowledge, and how Bates’s own work can be used to provide a more precise and workable concept. Finally, the last part deals with the application of the preceding discussion to some problems in the study of medical knowledge.

BATES’S EPISTEMOLOGICAL FRAMEWORK

Coherencing: Harvey and the Game of Truth

Was the circulation of the blood “discovered” or “invented?” While accepting that scientific concepts such as that of circulation are indeed constructions, Bates points out the insufficiency of current constructivist accounts in explaining phenomena such as rapid consensus and stability. He proposes an alternative approach both to strong constructivism and naïve realism and for doing so he focuses on the reception of Harvey’s ideas by his contemporaries. His alternative points to an intellectual component that he sees as usually left out from accounts from both sides, and which could in his opinion bridge the gap between philosophical and sociological accounts of scientific endeavours. He calls that component “constructed coherencing,” and his two-part paper on Harvey can be understood as a presentation of that concept followed by a demonstration based on a detailed historical study; the latter part, however, will not be presented in this brief summary.
After presenting the debate between Harvey and some of his contemporaries, Bates stresses once again the importance of the intellectual component in that discussion. Referring to a taxonomy originally proposed by Ian Hacking, he points out there were no “marks” and a limited assortment of “things”; the central role played by “ideas” in this context also underlines the importance of agency, since humans actively decide when a satisfactory level of coherence was achieved.

Another important point, stressed in the summary presented in the opening of the second part, is that the term “rational” is of limited use in understanding the tenor of the debate, since both Harvey and his opponents subscribed to the same underlying intuitive rationality, particularly a form of “folk physics” that worked as an analogical model for the ideas being debated.

After presenting the case for coherencing in the first part, the second part further details the concept. Bates describes his methodological approach as a “willing suspension of belief,” pointing out that heuristic relativism often reintroduces the same normativity present in its realistic counterparts. It thus follows that “conventional” in that framework is not the same as “merely conventional;” as Bates himself put it, “there may be, or may not be, a piece of reality in the cultural envelope.”

At this point, another important piece of Bates’s model is introduced: the unproblematic background knowledge, or UBK. Although Bates does not provide an analytic description of this key element, its characteristics can be abstracted from the references made throughout the second part of the paper. The UBK is a network of previously established coherences that include analogical models (such as the folk physics already mentioned) as well as the accepted methods for checking new items against this background: “In short, coherences come from coherences.”

This concept is not very unlike Kuhn’s paradigms or Fleck’s thought style, in that it is a general, pervasive element that shapes the activity of inquiry and to some extent restrains it. The UBK is, however, devoid of the psychological aspects present both in Fleck’s and Kuhn’s works that still make some scholars cringe to this day. It also has some elements of Latour’s black boxes; for instance, Bates refers to the cost of accepting new ideas (in this case, Harvey’s) as having to give up large portions of a seemingly stable UBK—and, in the end, of a segment of reality itself, perfectly paralleling Latour’s description of a black box. And this cost had social implications as well, as Bates states in terms that are particularly relevant to this paper: “reality that felt like truth because he believed it ‘worked,’ because it made his professional conduct rational and coherent, and because it underwrote his credibility and standing with his clients on a very competitive medical market place.”

Coherences are, then, a source of constraint, both from “nature” and “society:” “Therefore, it seems that if one is committed to feeling, and to
being perceived as both rational and ‘in touch with reality,’ i.e., committed to an account of the world that is granted coherence by oneself and by one’s peers, then the importation of particular patches of that reality into the discussion can work both ways. Commitment to coherence, even conventional coherence, brings with it intellectual responsibilities as well as opportunities, constraint as well as freedom. Along with being socially sanctioned, then, a coherence is a fettered association.”

Such constraints are not absolute, however. Players in the game of truth enjoy a latitude of movement within those constraints, to the extent that different segments of the UBK can be leveraged in support of a threatened segment, such as falling back on ontological or methodological a priori that are far more difficult to constrain or contradict.

As for constraints, conventions also play an important part. Present day conventions, for instance, exclude resort to miracles, magic and the like, but “no amount of evidence could ever prove that such factors are not involved, and yet consensus seems almost universal within today’s scientific community.”

This, however, does not fully account for the stability achieved by certain ideas, such as the paradigmatic case in question, circulation, which has withstood four and half centuries. As Bates put it, “something else” is needed to account for that, something that he is inclined to think that, in principle, “will never be susceptible of rigorous proof because it is, in the final analysis, an historical event, with all the contingencies and complexities that that implies.” Nevertheless, it is possible to present a list of conditions that, when met, can account for most, if not all, of the ways in which a group of inquirers can achieve a particularly stable coherent idea:

1. An extensive UBK which has nevertheless a number of clearly identified problems;
2. A high degree of focus on a particular topic;
3. Consensus as to the most fundamental criterion of success;
4. A rather special historic setting (hence the contingent element).

Coherencing is, thus, a personal and social activity that is (a) cognitive, but (b) interactively cognitive (through the interplay of human beings and bits and pieces of nature) in order to produce (c) a collective assemblage of fettered associations privileged by the community because they seem “justified.” As this consensus evolves over time, components that owe more to individual participants tend to be phased out, leaving behind the more generally (i.e., collectively accepted as such) coherent aspects. But this does not imply that all coherences reach the same status or have the same constraining power; although coherences are constructed, they are not all created equal.

Bates closes the paper stating that coherencing allows for a description of construction that is not fabrication, doing away with the need to dis-
cuss relativism or truth. This, in my view, is precisely the strength of this notion, born out of an empirical historic study.

The Mechanical Mind

In what is also a demonstration of Kuhn’s idea of incommensurability, Bates demonstrated how terms like “mechanical” and “organic” can be misleading, particularly when looking at texts written in a context entirely different from that which obtains today. It is not just a problem of translation, but of apprehension of a complex mindset from the past, where the meaning of the corresponding words was, to some extent, even opposite to how we understand them today. In order to understand such terms in their original context, it is necessary to unearth large sections of the corresponding UBK of that time, and part of Bates’s effort in his “Medicine and the Soul of Science” goes precisely in this direction.

According to his account, mechanism, erroneously perceived today as a monolithic block of ideas that arose with the Renaissance, at the latest, could be traced back to two distinct ways of thinking already in place in classical Antiquity: atomic mechanism, which pointed to random motions as the cause of events and organic mechanism, that of man-made mechanisms, for instance, which had purpose although that did not require human agency to operate.

The New Mechanism joins the two components, tracing the origin of both ultimately to a divine psyche, whose agency is however pushed back from the explanatory horizon; instrument gave way to machine, that is, while the fundamental analogy in the first case is that of an object which, although having its creator’s purpose embodied in its own form, still required an external agent to perform its task, in the second it is replaced by a thing that, once started, can function without such constant interaction, that is, it can function automatically, as we would understand today.

The key elements in this discussion are, thus, purpose and agency. Aristotle’s mechanism is “technical,” meaning “goal-directed, result-oriented, professional expertise.” Instead of agency, there is an aim or end, the final cause. Nature has no mind or consciousness. This regularity or automatism did not involve any decision-making, being conceived in terms that today would be called algorithmic. An Aristotelian form is, thus, a natural process working according to a plan—the plan of a form is its logos. And although the Aristotelian view lost its dominant place in Western thought, it remained influential in practice, as evidenced, among other things, by the idea that the human body is an assembly of Aristotelian instruments—organs. In another part of the text, Bates takes these issues even further: “Aristotelian instrumentalism (or technique) combines mechanism with teleology; it occupies that zone where mind and matter meet.”
Modern science’s “materialization” can also be seen as the purging of references to agency—"mind"—from the explanatory mix, being thus related to the Cartesian mind-matter dualism. Bates defines the former as “a tendency to focus on (or maybe it would be more epistemologically correct to say construct) entities, things, objects, as the basic units of explanation, a feature which might be called ‘thinginess,’” a tendency which has its roots in Aristotle’s essentialism and atomism, “by virtue of the very concept of an atom” (again, Bates’ words). Entified objects are defined in terms of themselves, and not their contexts, and entification also extends to processes, thus leading to concepts of linear causation, that is, sequences of events that are also conceived of as encapsulated in themselves, to some extent. Modern mechanism thus developed out of its complex roots in Greek natural philosophy into “a particular type of non-psychic process, a mechanism which focuses on a world of entities which interact with one other in linear chains of cause and effect that at least have the potential to be timeless, universal…and even capable of being expressed mathematically.” Cartesian dualism (or, as Bates calls it, Phase II of Western mechanism) “was not only a radical separation of the domains of mind and matter…but also a redefinition of what is meant by mind and by mechanism.”

As the representation of nature moves toward the non-mental mechanic, the mind which represents it also becomes mechanized, and mathematics played a major role in that regard. In ancient Greece, this meant Euclidean geometry “which, like Aristotelian logic, depended on an axiomatic-deductive procedure that was concerned to secure absolute certainty in the outcome once the input has been given.” The counterpart to that mechanical mind was an epistemology that placed experiencing nature as its foremost validation criterion. “In the 17th century, this ancient sceptical epistemology became a rationale for much more experimentation, now motivated by a pragmatic interest in practical knowledge more than by the quest for philosophical truth.” In rhetoric as well as a metaphysical principle, experience still takes precedence in scientific endeavours, and this both promotes and justifies experimentation. The instruments introduced in scientific practice, from the 17th century onwards, mechanized the actual experiencing of nature. In particular, a key role played by such instruments is that of measuring different aspects of the phenomenon under investigation, again requiring them to have a purpose and at the same time being to some extent a materialization of that purpose; this, however, does not do away with human intervention, since an agent both to use them and interpret their results is still necessary. Measurement and the New Mechanism, thus, reinforce each other. And this, in turn, has further consequences as to
the knowledge being produced: “In order to apply instruments to
to nature, nature, or at least those aspects being investigated, has to be
dealt with as if it were mechanical. But this also mechanizes the knowl-
edge which is acquired.” And this, in turn, also mechanizes the
observer.

In summary, the mechanization of the world is also in a dialectic rela-
tion with the mechanization of inquiry; rationality in this framework is
also peculiarly defined in mechanical terms, as a process void of inten-
tions or values. These features are not only at the very heart of modern
science, they are precisely what distinguishes it from other ways of pro-
ducing knowledge: “it is this metaphysics of, and methodology for, elimi-
inating mind which makes scientific knowledge unique, or at least that
portion of knowledge which genuinely deserves to be called ‘scientific.’
As to why, in addition, it has been so uniquely successful in interacting
with the world, the obvious answer is because it has more to do with
how nature really is than do other kinds of knowledge. And while that
must be true to a significant extent, it is surely also a bit tautological
because, at the same time we are claiming to be in touch with reality, we
are also defining rather narrowly and arbitrarily what part of reality it is
that we are in touch with.”

The Role of Statistics
This is particularly significant to medicine, which has been in recent
years increasingly dependent of statistical tools as a means to validate
knowledge. Bates however doubts the claims for exactitude derived
from the application of statistical methodology: “Talk of ‘evidence-based
medicine,’ ‘clinical trials,’ or, more generally, of ‘social sciences’ which
depend on these kinds of uncertainties of statistics is more a rhetoric of
wishful thinking, an effort to claim reliability, or an expression of hope
for the future, than an example of mechanical science at work.” These
issues will be further analyzed at the end of this paper.

Consequences of Mechanism for the Philosophy of Science
Despite the emphasis on experimentation, science can in the end be
understood as a dialectic between our reasoning and our experience.
Bates anticipates the criticism that such views can elicit, as many other
elements are involved than just those two. He points out, however, that
“that dialectic must go on among a large number of people and over
long periods of time before any of the more complex theories or experi-
mental findings become stable components of scientific knowledge;” it
was the mechanization of knowledge that made it possible for so many
people, for so long, to believe they were dealing with the same “things,
ideas and data.” Bates also agrees with Hacking’s use of the phrase “technology of intersubjectivity.”

This does not do away with empiric data, but undermines the idea of science as a “mirror of nature.” “Experimentation systematically decontextualizes the region of study in order to find chains of cause and effect.” And the “hard data,” the facts thus produced do not exist on their own, but are intertwined with the background knowledge to which it is related. Facts can be deeply embedded in the core of the UBK that makes it meaningful and experientially reliable in the first place, in which case they are more solid than those whose insertion is peripheral. “Hard facts are integrated into the epistemological engine, and help to turn the wheels of mechanical science, while the peripheral ones are like the body of a car; they may give it its present shape, but they are as likely to be merely contributing to the latest fashion as to the ability to move.”

It should be pointed out that the mechanistic view of inquiry also extends to its accompanying philosophy; the idea that science can be defined in terms of the application of inflexible algorithms is in itself yet another algorithm; in other words, the metadiscourse of the mechanized science is a mechanized philosophy.

**EPISTEMOLOGY AND THE DEFINITION OF KNOWLEDGE**

**Knowledge**

Studying knowledge is a task that stands at the core of epistemology. The *Cambridge Dictionary of Philosophy*, for instance, defines epistemology as “the study of the nature of knowledge and justification; specifically: The study of (a) the defining features, (b) the substantive conditions or sources, and (c) the limits of knowledge and justification. The latter three categories are represented by traditional philosophical controversy over the analysis of knowledge and justification, the sources of knowledge and justification (e.g., rationalism versus empiricism), and the viability of scepticism about knowledge and justification.”

More precisely, epistemology is concerned with specific types of knowledge: the currently favoured term for the object of epistemology is propositional knowledge, defined as such in a textbook of epistemology: “There are several different uses of ‘know’ (as in ‘I know John,’ ‘I know how to drive,’ and ‘I know your phone number’), but the sense that epistemologists have focused on is the sense that refers to propositional knowledge, or factual knowledge. This is the sense involved in ‘I know that 2 + 2 = 4′ and ‘Does John know that the game has been delayed?’”

The canonical form of analysis of propositional knowledge is known as the tripartite analysis: knowledge is defined as (a) a belief, (b) which is true, and (c) justifiably so. Each of these terms, however, is a can of
worms on its own, and Hacking has plenty of reason to put knowledge in his list of “elevator words,” that is, words that are made to work at a higher level than those used to describe facts and ideas, and which are usually circularly defined.\textsuperscript{62}

Michael Welbourne dedicated a book to the discussion of the problems entailed by this analysis, and one of the points that he addresses quite extensively is how the tripartite analysis is constantly challenged by counter-examples, to the point that its current state reminds one of Ptolemaic astronomy, “as if the point were to preserve the broad scheme of this style of analysis, at no matter what cost in terms of ad hoc complexifications dreamt up to cover new observations (new counter-examples).”\textsuperscript{63} His proposition is not to analyze the concept of knowledge, but to understand it by examining the role it plays in our lives and its place in the general economy of our concepts.\textsuperscript{64}

He proposes two fundamental departures from the tripartite analysis; first of all, he stresses the social aspect of knowledge: “The point I want to make here, however, is that some knowledge, far from being the prized achievement of a solitary intellect, as conceived according to the Cartesian ideal, is itself the product of an essentially collaborative enterprise; to which we may add that success in such enterprises may often require that there be mutual knowledge among the collaborators.”\textsuperscript{65} Secondly, and as a consequence of that, he adds to the tripartite analysis what he calls the Principle of Communicability (PC): “If a hearer (H) believes that a speaker (S) knows that P, then H believes that H knows that P.”\textsuperscript{66} This would provide a radical difference with regard to belief, since no similar principle can be demonstrated in that case.

Welbourne summarizes his position in these words: “I can describe what we might call the knowledge-game. I can explain roughly how it is played (through social acts of testimony, in which the speaker tells the facts and the hearer accepts them on the speaker’s say-so). I can point to the purposes it serves (the provision of true beliefs to match our practical needs and, it may be, our idle curiosities). I can mention some of the effects of playing the game (the creation of communities of knowledge and structures of mutual knowledge that enable advanced forms of collaborative undertakings). To demand, in addition to this, an analysis, is to miss the point entirely.”\textsuperscript{67}

But although we can acknowledge his contributions, a few problems still remain. Most remarkably, the issues of “truth” and “justifiability” remain untouched. In particular, this still poses a problem for a historical approach. Epistemology, particularly when dealing with scientific knowledge, has to go hand in hand with history. As the French philosopher of science Georges Canguilhem said: “Finally, the specifically philosophical reason [for making a history of the sciences] is that without reference to epistemology a theory of knowledge will be a meditation on the void
and without reference to the history of the sciences, epistemology will be a perfectly superfluous double of the science it intends to discuss.”

Now consider, for example, the following statement: “of course, we could not recognize as knowledge what our forebears were told and accepted as knowledge, half a millennium ago, when they were told that the earth was at the centre of the universe. This is disqualified as knowledge, in our eyes, because we do not accept it; we do not believe it is true.” When it comes to science, it turns out, then, that either we assume that justification and truth criteria are contingent (at least historically), or that it is altogether impossible to know, or at least to know that one knows, since truth is elusive and justification changeable. Although Welbourne seems to favour the first option, it is not clearly stated as such.

Truth and justification are indeed thorny issues, and at the heart of the dissent between essentialist and constructivist accounts of science. Whereas the former would point to “nature” or “reality” as the ultimate instance for validating knowledge, the latter would point out to the human interactions that produce that validation, thus putting higher emphasis on the contingent aspects of such validation processes.

Let us now turn to Bates in search for a way out of this conundrum.

Re(a)dressing the Definition of Knowledge

I will now try to rephrase the previously discussed definition of knowledge in (hopefully) less problematic terms, based on Bates’s ideas. First of all, since we are examining propositional knowledge, instead of “beliefs” (a term itself not void of further problems) we will consider propositions or statements. Furthermore, following Welbourne, these are communicable, shareable and shared statements. Finally, instead of “true” and “justified,” acknowledging the historical contingency and the role played by human agency they would be more adequately termed accepted as valid by specific groups of inquiry, following procedures of validation also accepted by the same groups. Those procedures, in turn, led to the construction of coherences which embed those statements within a network of similar, previously validated statements—the UBK. And in the case of the Western scientific UBK, this means that the legitimate methods of coherencing are those that represent both their object of inquiry and their proceedings as mechanical (more precisely, algorithmic, in the latter case).

But even “statements” can be misleading in this context; although suitable to philosophical exercises, the conception of a validation process that takes isolated statements one at a time, accepting or rejecting them based on some set of rules does not correspond well to the way that communities of inquiry proceed. And there is a fundamental flaw in
that idea, in the sense that the variety of statements at stake here do not have meaning in themselves, but depend on a network of other statements to become meaningful. Fleck exemplifies this idea with a discussion about the concept of syphilis: “The statement ‘Schaudinn discerned *Spirochetta pallida* as the causative agent of syphilis,’ is equivocal as it stands, because ‘syphilis as such’ does not exist. There was only the then-current concept available on the basis of which Schaudinn’s contribution occurred, an event that only developed this concept further. Torn from this context, ‘syphilis’ has no specific meaning, and ‘discerned’ by itself is no more explicit than ‘larger’ and ‘left’ in the examples above [a reference to a previous example on his argument].”72

Another point worth noting is the emphasis on social interactions; this is, indeed, a point of convergence for most of the authors quoted in this paper, including Bates himself. Social instances are both the repository of knowledge and the arenas where new clusters of statements are accepted and woven into the UBK or discarded. But “social instances” is too fuzzy an expression to be of any use here; we can think of Fleck’s thought collectives or Knorr-Cetina’s epistemic communities as a more precise and adequate description of those. And this brings out yet another important feature to consideration: in complex societies there is a large number of such communities, and even in the case of a postulated global-wide “scientific community” that shares large portions of a far-reaching UBK there are heterogeneous zones, as Knorr-Cetina, for example, demonstrated.73 This means that the replacement of a chain of statements in the local UBK of a given group may not have immediate effects in another, even a closely related one. This is in effect what happened, for instance, with most of the scientific developments in human anatomy or physiology, that had no effect in actual medical practice for over two centuries: “medicine only became seriously modern and scientific during the second half of the nineteenth century.”74

Finally, this redefinition is not a mere intellectual exercise, “une méditation sur le vide,” as Canguilhem might have called it. It has very concrete implications for the study of the complex interactions that continuously expand and reshape the UBK, or the collection of UBKs, of contemporary science. At the very least, it enlarges both the scope and the requirements of that task. The quote from Canguilhem in the previous subsection already stressed the importance of a historical perspective. Adding further to that, the analysis of isolated concepts, alone, does not suffice; a more comprehensive approach is clearly required: “I have already suggested that the past of science should be approached as an alien culture, one that the historian strives first to enter and then to make accessible to others.”75 This last remark, finally, suggests the possibility of a non-normative epistemology, one that is focused in understanding and describing how specific groups validate knowledge, rather
than stating up front how it should be done in general. This is essential in particular to historical and anthropological approaches, which otherwise run the risk of becoming whiggish, in the first case, or ethnocentric, in the second.

The final part of this paper will be dedicated to exploring some implications of this discussion to the study of medical knowledge.

MEDICAL KNOWLEDGE

Knowledge in Medicine

Knowledge is a highly prized commodity in the medical profession. A whole publishing industry revolves around it, including prestigious professional journals which have some of the highest calculated impact factors according to the Institute for Scientific Information (ISI), and textbooks that were established as de facto standards for medical practice practically on a global scale (all of them published, at least originally, in English, of course). And no knowledge is more valued than scientific knowledge, assumed as a representation of “things as they really are,” thus bearing considerable normative weight with regard to medical decisions. When we get closer to practice, however, this picture gets somewhat blurred. On one hand, “within the realm of health and the healer the place of science is especially fuzzy and messy.” On the other, as was mentioned earlier, the existence of heterogeneous epistemological communities (or thought collectives) means that validation criteria and processes are not the same across the board. At the very least, practitioners and researchers, albeit in theory bound by the same UBK can have expressive differences in their operationalization. This UBK, nevertheless, defines what counts as reality in the “realm of health and the healer.”

Few objects in medical knowledge better demonstrate the impasses mentioned above than what is arguably its core construct: disease. Much of what is currently done in medical practice can be described as diagnosing and treating diseases. By itself, this should put a special emphasis on the definition of what a disease is. Curiously enough, there is none available as such. Medical textbooks, particularly those dedicated to internal medicine or medical clinics can be described as a catalogue of diseases, which are individually described in their particular details. But no general definition of what constitutes a disease is presented. Even the standard internationally accepted taxonomy of diseases, the International Classification of Diseases (ICD), edited by the World Health Organization and already in its tenth revision, fails to provide a definition of what, after all, a disease is. This poses an important problem for the coherencing processes of medical epistemology, since the absence of a
formal, explicit definition makes it very hard—if not impossible—to examine it critically; that sort of criticism, in turn, is a key feature of such coherencing processes.

This does not mean however that a definition does not exist; it does, but it is not explicitly spelled out. A conceptual framework for the construction of diagnostic categories, summarized in this table, will be presented in the following paragraphs.

Table 1
The Structure of a Disease

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<tr>
<th>ELEMENTS</th>
<th>AXES</th>
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<tr>
<td>Disciplinary domain</td>
<td>Pathophysiology</td>
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<tr>
<td>Core category</td>
<td>Cause</td>
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<tr>
<td>Definition of disease</td>
<td>Process</td>
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<tr>
<td>Characteristic method</td>
<td>Experimental</td>
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<tr>
<td>Historical period</td>
<td>Second half of 19th century</td>
</tr>
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The “axes” presented here are an analogy to geometry; as a three dimensional object can—or, in fact, must—be represented as a series of points along three orthogonal axes, the modern concept of disease includes elements from the three dimensions presented in the table.

The first axis corresponds to the characterization of diseases as processes, possessing one or more causes and a “natural history.” It is in this axis that medical knowledge comes nearer to the hard sciences, in the biological domain.

The second axis relates to the description of characteristic—pathognomonic—lesions, arising from the transition described by Foucault. It must be noted, however, that the concept of lesion mutated through time, migrating from relatively large structures, visible to the naked eye, to ever diminishing portions of the human body; currently many lesions are described at the molecular level. Taking this into account, the laboratory instruments and tests used as part of the examining process are part of this same axis, and it might be said that an altered result may be seen, in this context, as a logical equivalent of a lesion.

Finally, the last axis describes diseases in terms of clusters of signals and symptoms, configuring semiologic gestalts. Characterizing this axis
in the present moment is no easy task, since the semiologic definitions of disease are deeply intermixed with the preceding axes. It must be remarked, however, that the nosological grid—today best represented by the ICD—precedes it by a few hundred years. Medicine is still classificatory, and the taxonomic system that existed in the past became the terrain in which the anatomoclinical descriptions developed. It is clear then that this axis comprises two different dimensions: in one hand, the individualization of single cases, using techniques described by Ginzburg as a conjectural paradigm to sketch a specific semiologic gestalt; on the other hand it also implies a generalization, fitting the studied case into the aforementioned nosological grid. This, in turn, was developed conjointly by medical clinics and epidemiology.

The patchwork described above constitutes the blueprint for the description of a disease in contemporary medicine. It is a result of a collaborative effort that spans temporal and disciplinary boundaries, and a very complex result at that. One of the effects of this complexity is that it is virtually impossible for any individual to master all the different methodological traditions that go into its making, and as such the coherencing criteria can vary broadly depending on the role played by each participant in the process, sometimes with fairly problematic results. An important example is how epidemiological studies are received and reinterpreted by practising physicians. “Risk factors,” for instance, are markers used in the attribution of probabilities; it is not infrequent, however, that they are misunderstood (regardless of what textbooks may say) as causes. Going back to Bates’s remarks on the specificity of statistics (which play a major role in epidemiology), this shows that despite the fact that statistical reasoning is qualitatively different from the linear causality model, knowledge derived from statistics is forced into that mould. On the other hand, and again also as Bates pointed out, one of the main uses of statistics in medicine has been to support the imposition of standardized forms of medical reasoning, such as protocols, that are yet another example of the mechanical conception of rationality.

The inherent complexity of medical knowledge can be made even more evident when we unpack an apparently simple statement to show its connections with other elements of the UBK. Although not referring to that terminology, Paula Treichler provides a compelling example of that interconnectedness; although a bit lengthy, it is worth transcribing it in its entirety:

We can construct a set of statements about HIV, varying the points and the degree of transparency to vary the visibility of fabrication and cultural constructedness:

1. HIV causes AIDS.
2. HIV is the name that scientific culture gives the virus widely believed to cause AIDS.
3. HIV is the compromise name proposed by an international commission to resolve the bitter dispute over the “discovery” of a virus judged by many to be a causative factor in the infection and immune deficiency that can lead to the specific clinical conditions diagnosed as AIDS.

4. HIV is the acronym adopted in 1986 by the international scientific community to name the virus hypothesized to cause immune deficiency in humans and eventually AIDS, another acronym, adopted in 1982 to designate a collection of more than fifty widely diverse clinical conditions believed to be given the opportunity to develop as the result of a severely deficient immune system;

5. HIV is a hypothesized microscopic entity called a virus (from Latin virus, poison) invented by scientists in the nineteenth century as a way to conceptualize the technical cause and consequences of specific types of infectious disease. A virus cannot reproduce outside living cells; it enters into another organism’s host cell and uses that cell’s biochemical machinery to replicate itself (in the case of HIV, often years after initial entry), at which point the cell’s DNA, with which the virus is integrated, is transcribed to RNA, which in turn becomes protein. Our knowledge of this “life story” has been produced by an intense national research effort focused both on HIV and on drugs designed to disrupt its life history at various points; as the major subject of scientific investigation and pharmaceutical research efforts and the major recipient of AIDS research funding, HIV is, therefore, as Joseph Sonnabend puts it, “metaphorically representative of other interests.”

85 It should be noted that Treichler could have gone on and on indefinitely; terms like “protein,” “cell” and “DNA,” for instance, can each be unpacked in the same fashion. Going back to the different epistemic communities sharing this UBK (or parts of it), not all of them would be able to unpack such classes of statements in their entirety; when that happens, items of knowledge tend to be taken at their face value, and the possibility of critical examination of such items is lost.

86 It should also be noted that in the end a disease is just a grouping criterion, a convenient label to aggregate people who are undergoing certain processes that share enough characteristics with each other to make it heuristically and pragmatically convenient to deal with as being subject to the same “thing.” But what is the “thing” that a disease is? Competing medical theories, like Chinese Medicine or Homeopathy, simply do not have a corresponding entity in their body of knowledge. Nevertheless, in the Western medical UBK diseases are things, and have causes which act through linear processes, in accordance with the Western scientific UBK, more specifically with its tendency, as Bates defined, both to entification and to conceive processes in terms of linear causality (see “The Mechanical Mind” section). The entification of disease usually happens at the expense of the patient; and this alone can account for much of the patient-unfriendliness of contemporary Western Medicine.

87 This section illustrated some of the specificities and problems related to the domain of medical knowledge. On the following section we shall
examine how doctors in the present play the “game of truth” to which Bates alluded.

PHYSICIANS AND THEIR TECHNOLOGY OF INTERSUBJECTIVITY

Given the previous outline of some relevant aspects of medical knowledge, how do actual practising physicians interact with it? This question has been a generic guideline for an ongoing research project, whose first results were published in 2002.

Based on in-depth interviews conducted with professors of one of the foremost medical schools in Brazil (supplemented after the paper was sent to print with two other sets of interviews, one from an equally influential Brazilian medical school and the other from a Canadian medical school), that study abstracted some of the strategies employed by them to select, among the constant barrage of new knowledge presented to them, what can be trusted—in Bates’s terms, what are the coherencing criteria and tactics they use in order to integrate new knowledge into their UBK. The current branch of the study is based on a systematic observation of interactions between the chief staff and the residents in charge of providing care in a general clinical ward of a university hospital. The analysis of the data generated by this observation is not finished yet, and only a few preliminary remarks will be made about these.

An important aspect of the medical UBK is the idea that medicine is subject to “frequent, major” changes, an idea that is taken for granted, without further questioning. This puts a permanent pressure on those physicians to keep up with the latest and greatest advances in medicine, a pressure further compounded by what is perceived as an informational overload. This pressure is counteracted by the emphasis that those doctors put on first-hand experience as their primary verification tool, and the scepticism they display with regards to that same flow of new knowledge, motivated by (a) the perception of new knowledge generated by recent research as inherently unstable and (b) the awareness of important economic interests that presides over the presentation of such knowledge, particularly, for instance, with regards to new drugs. This scepticism is, however, undermined by the lack of effective alternatives to the industrial sources of knowledge, on one hand, and by a fundamental trait of that same UBK, already mentioned, which is equating “scientific” to “real” or “true.”

The high value that first hand experience enjoys extends to shared experience, that is, the sort of learning that takes place in doctor-doctor interactions that ultimately shape medical knowledge. Such negotiated learning, when doctors discuss cases, frequently (but not only) at the bedside, is arguably the major strategy of coherencing employed by doctors on a daily basis. The case discussion is the arena where different
segments are interwoven with the present UBK; fragments from recently published papers are thrown in with quotations from established textbooks and observational data generated from several sources (mostly direct physical examinations and results from an array of lab tests), being slowly articulated by the ensemble of doctors to produce a coherent discourse, aiming at solving the problem—ideally, diagnosing a disease and taking care of its ultimate causes. Other discourses are regularly brought into the mix, especially information from other specialists which were consulted on specific medical problems of the patient being discussed.\textsuperscript{91} The clinical round is a node where the threads of a web that extends far beyond the walls of the ward meet.

In these meetings, there are two aspects of the interventions that deserve special attention. First, much of the statements presented are in the packed form, taken for their face value and perceived as self-evident, such as “X is the drug of choice for [the disease] Y;” rarely, if ever, there is an argument about the supporting framework that lends credibility to such statements. Second, and rather paradoxically, there is a continuous back and forth sliding between two different argumentative supports,\textsuperscript{92} experience and literature, evidenced by sentences of the type “I read in [source] that…” and “In my experience, I’ve seen…” The three types of statement (self-evident, backed by literature and backed by experience) are rolled together in the argument, frequently by the same participants, without being perceived as contradictory.

There are, evidently, exceptions to this rule, particularly when things do not work quite as expected, if for instance a patient does not respond to a drug as was assumed (s)he would, or the results of a lab test are particularly unexpected or baffling. In these occasions the participants of the round frequently fall back to the biomedical sciences (particularly pathophysiology) in search of a reasoning that could explain away the anomaly.

Those characteristics are, in my view, a reflex of the inherent pragmatism of a profession that is primarily concerned with interventions and problem-solving, and also of the quirky relationship between the different parts of the medical UBK, and particularly the fuzzy and messy place that science occupies.

CLOSING REMARKS

This text is mainly a proof-of-concept exercise. As such, I believe it fulfilled its goals, in presenting a limited, but focused, view of a particular aspect of Don Bates’s work.

His portrait of modern science’s mechanism exposes its shortcomings, in particular the relative narrowness of that approach. But that is not necessarily a weakness; it may be, if one is unaware of those aspects
and take the scientific discourse not as a possible discourse, but the only possible discourse about the class of problems that it deals with—and this would be also true of medicine. Its relative “unfriendliness” might as well be to some extent unavoidable in order to achieve its goals, as Bates remarks by the end of the paper where he compares contemporary Western medicine with other medical traditions.93

Having said that, I will once again use Bates’s words to close this paper: “Indeed, chains of cause and effect, and the mechanical model of thinking that uses them to hook up entities, could be seen merely as a more simple-minded way in which, over recent centuries, humanity has managed an otherwise too complex world of endless, infinite, context.”94 “Science…is now the instrument by which the collective mind or soul of science strives to manage not only human bodies, but the world at large.”95

NOTES

15 Bates, “Closing the Circle,” Part II. Methods are also mentioned en passant on p. 258 of that paper.
25 I believe those are what Kuhn would call anomalies.
29 Thomas S. Kuhn, The Road since Structure (Chicago: The University of Chicago Press, 2000), chap. 2 “Commensurability, Comparability, Communicability.”
32 “Medicine and the Soul of Science,” §57.
33 “Medicine and the Soul of Science,” §58.
34 “Medicine and the Soul of Science,” §59.
35 “Medicine and the Soul of Science,” §64.
36 “Medicine and the Soul of Science,” §66.
37 “Medicine and the Soul of Science,” §95.
38 “Medicine and the Soul of Science,” §98.
40 “Medicine and the Soul of Science,” §100.
41 “Medicine and the Soul of Science,” §101.
42 “Medicine and the Soul of Science,” §105.
43 “Medicine and the Soul of Science,” §120.
44 “Medicine and the Soul of Science,” §127.
45 “Medicine and the Soul of Science,” §134.
46 “Medicine and the Soul of Science,” §135.
47 “Medicine and the Soul of Science,” §136.
48 “Medicine and the Soul of Science,” §137.
49 “Medicine and the Soul of Science,” §139.
50 “Medicine and the Soul of Science,” §140.
51 “Medicine and the Soul of Science,” §153.
54 Hacking, “The Self-Vindication of the Laboratory Sciences.”
63 Welbourne, Knowledge, p. 67.
64 Welbourne, Knowledge, p. 68.
65 Welbourne, Knowledge, p. 72.
66 Welbourne, Knowledge, p. 110.
67 Welbourne, Knowledge, p. 115.


69 Welbourne, Knowledge, p. 110.

70 It would be impossible to summarize that debate in this paper; for an excellent survey of the field, see Jay A. Labinger and Harry Collins, eds., The One Culture? (Chicago: University of Chicago Press, 2001).

71 The ethnography of laboratories, for instance, has produced a considerable body of literature; a classical example is Bruno Latour and Steve Woolgar, Laboratory Life (Princeton: Princeton University Press, 1986), particularly chap. 2.

72 Fleck, Genesis and Development of a Scientific Fact, p. 39.


79 For an elaborate discussion both of the indetermination of several concepts surrounding the idea of disease and an alternative description based on the concept of normativity see Georges Canguilhem, On the Normal and the Pathological (Dordrecht: D. Reidel, 1978).

80 A detailed account of this model and its application to the study of the emergence of a specific disease in recent times, AIDS, was the object of my doctoral thesis, later published as Kenneth Rochel de Camargo Jr, As ciências da AIDS e a AIDS das ciências (Rio de Janeiro: Relume-Dumará, 1994).


83 While the actual origins of this nosological grid antedate the development of epidemiology as a discipline, rudimentary epidemiological methods were an integral part of the medical clinics being developed in teaching hospitals even in the 18th century, and after epidemiology became a full-fledged discipline, its role became even more conspicuous.


86 See for instance the remark that Fleck makes about syphilis, previously quoted.


89 As expressed, for instance, by Sackett et al., Evidence Based Medicine, p. 5.

90 See for instance Paul Atkinson, Medical Talk and Medical Work (London: Sage, 1995), particularly chap. 3.

91 It is important to note that (a) in the hospital where the observations were made, these meetings are for MDs only; (b) there are other types of systematic discussion
in the hospital, such as weekly clinical meetings, but which are less connected to actual practice and at the extreme can be viewed as a display of medical prowess more than anything else.

92 This does not contradict the previous affirmative. Even when such supports are presented, they are not discussed as such; it’s a rare occasion when, for instance, a paper is presented as the source of authority for an argument and this is contradicted by questioning the methods employed in the study.

