From Clinical Counting
to Evidence-Based Medicine

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Several years ago, I gave a talk in Gérard Jorland’s seminar on the quantification debates that stirred the Parisian medical world during the 1830s. During question period, one of my listeners asked whether the “numerical method” advocated by P.C.A. Louis was in fact evidence-based medicine (EBM). The query struck me as historically naïve; but both advocates and opponents of EBM have since made this very same connection; the latter use it to emphasize the historical legitimacy of their enterprise, while the former argue that there is nothing particularly original about EBM – old French wine with a new Canadian label. The more interesting question is whether parallels and differences between nineteenth-century debates and today’s can tell us something about long-standing medical efforts to quantify.

To that end, this essay looks at quantification in the nineteenth and early twentieth centuries and then considers the rise of objectification as a social movement as manifest in the recent intense interest in evidence-based medicine.

QUANTIFICATION IN THE NINETEENTH
AND EARLY TWENTIETH CENTURIES

Quantification in medicine is part of the growing trust in numbers that has gradually affected all aspects of social life during the past centuries. More narrowly, it is part of a process of objectification in clinical medicine that has been going on since at least the eighteenth century. It has been most evident in diagnosis, which has come to depend
less and less on patients' accounts or physicians' subjective judgment and more and more on objective signs that, in theory at least, transcend subjectivity and compel agreement among qualified observers. Among the first and most compelling of these signs are the anatomic lesions that defined the scientific medicine of the Paris School during the first half of the nineteenth century. These are so forceful precisely because we can see them (and, as we know, “seeing is believing”). Once observable only in post-mortem dissections, lesions can now appear as images in living patients produced by CAT-scans, MRIs, or X-rays or, at the cellular level, by microscopes of various sorts. Frequently when we cannot see lesions directly, laboratory tests—counting something produced by the body—serve as a proxy. Or mechanical means measuring specific functions serve similar ends, defining variations outside specific numeric parameters as disease. When no external sign of this nature manifests itself, doctors frequently dismiss illness as psychological in nature. Quantification in this context seems to be one of many techniques of objectification; it is, however, particularly central and ubiquitous because numbers seem especially “objective” and are applicable to so many domains.

The diagnosis of illness had by the early twentieth century become highly objectified; however, the same was not true of therapeutics. Doctors and patients—as always—defined success subjectively. If a new therapy came along, doctors used it and decided, on the basis of patients’ experiences and colleagues’ reports, whether it was effective or not. Quantification, we know, had surfaced in the eighteenth century—to evaluate smallpox vaccination and as the basis of Lind’s famous scurvy experiment. It was the subject of philosophical controversy in France, and medical reformers in Britain made it the cornerstone for a new program of clinical medicine that was quite visible by the beginning of the nineteenth century. Louis presented it as a methodological innovation and research program in 1830s’ Paris, provoking much controversy. Scholars have written much about these debates, and Ann La Berge discusses them in this volume. I therefore do not explore this debate except to observe that nineteenth-century criticisms of clinical quantification were essentially of two kinds: it was not effective, and it limited the freedom of doctors.

First, quantification did not work. Objections were both practical and theoretical. Among the latter was Risueño d’Amador’s philosophical critique of quantification based on his vitalistic convictions. Many critics argued that it was impossible to transfer data about groups or populations to individual cases. Isolated individuals such as Jules Gavarret in France emphasized the mathematical limitations involved in counting via averages and means. Many defended clinical experi-
ence, informed by pathological and physiological knowledge, as a more valid form of knowledge than mere counting. Decades later, Claude Bernard presented his philosophical objections to "observational" knowledge, which he contrasted to the certain results of laboratory experimentation.

But even without such theoretical underpinnings, scepticism was widespread because results of quantification were frequently unconvincing. Disease categories being counted were too imprecise – if they in fact existed at all – to take account of individual variability among people and cases of a given disease. Therapies moreover were never applied in exactly the same way. Different observers thus quite regularly came up with very different if not contradictory results. There was no response to such objections except to say that correctly made observation under proper conditions could yield consistent results and that individual variability did not affect efficacy in the aggregate.

Second, decisions based on counting threatened both the therapeutic freedom and the judgment of the doctor. At a time when physicians took pride in their ability to make subtle distinctions among patients, diseases, and therapies on the basis of their understanding of pathology and physiology, and also to weigh and judge large numbers of factors and the consequences of various choices, it was easy to imagine that quantifiers were looking for the one best way that would promote profession-wide conformity and destroy traditional medical intelligence.

This threat of course did not materialize, largely because the critics were right about the limitations of counting. Simple counting of therapeutic results by individual doctors was inadequate in many and perhaps most situations for producing conclusive results. The standard historiographical argument is that this technical inadequacy prompted doctors largely to abandon quantification. According to this view, first popularized by Major Greenwood, clinicians rejected quantification, which the new public-health movement of the mid-nineteenth century took over and that became eventually the basis of epidemiology. Clinical rejection of statistics, according to this view, did not dissipate until the twentieth-century development of new techniques made possible a more effective program of quantified therapeutic evaluation, based on advanced mathematical statistics. However, I have argued elsewhere that this view is at best partially true. Far from disappearing, counting came to be standard for many doctors after 1850. They could easily incorporate it in practice routines, and it came to serve as an extension of practical medical judgment. Counting alone might bring about consensus in a few cases where results were particularly striking – such as averting certain death; but often in
such cases, quantifying results merely formalized direct experience and perception. Most often, clinical statistics were one element among many in medical discussions. The weight that they received depended on who was presenting them and what other kinds of evidence were available.9

As it became routine, quantification lost its normative and programmatic character. Few individuals followed Louis in arguing that it would transform medical practice. Counting in fact had rather low epistemological status in the latter half of the nineteenth century. A doctor did not achieve a reputation for being scientific by presenting clinical results in quantified form. The mark of science in the late nineteenth and early twentieth centuries was the laboratory, which had produced astonishing successes and demanded skills that most doctors lacked. To the extent that doctors worried about threats to traditional clinical judgment, they feared laboratory tests or new forms of imaging such as the x-ray when these received a definitive role in diagnosing diseases, superseding clinical judgment.10 Even in the case of a highly technological procedure such as electrotherapy, some observers insisted that the doctor’s senses should determine the amount of electricity to be applied to the patient rather than relying on precision instruments of measurement.11 Eventually clinicians accommodated themselves to those innovations that proved more consistently reliable than clinical observation and experience; doctors dealt with the resulting tensions by adopting some variant of the cliché that medicine is both science and art.

By the end of the nineteenth century, counting results no longer seemed a threat to clinical judgment. Any physician who kept records could do it. It was highly individualized and did not require the enormous infrastructures that were necessary for compiling public-health statistics. If enough people reached the same conclusion, a collective change in practice might occur. Clinical counting could take various forms. One strategy was the retrospective audit of results,12 comparing, for instance, two hospitals or wards with different practices. (This was a very old technique, used in French debates on tracheotomy of the 1840s.)13 A variation was to compare results in the same institution before and after a particular innovation. (Lister’s results on amputation before and after antiseptic method, or Semmelweis, discussed by Jorland in this volume.) The work of Karl Pearson transformed clinical counting but only slowly and gradually. Quantification took many forms. The famous debate between Pearson and Sir Almroth Wright was not really about the role of statistics in clinical medicine. Rather, it concerned two competing scientific models for quantification in clinical medical research – one based on statistics, and the other on
quantified laboratory procedures that left some room for the researcher’s subjective judgment.\textsuperscript{14}

The new statistical procedures developed by Pearson and pupils such as Major Greenwood gained in influence because, during the first half of the twentieth century, distrust of subjective experience continued to grow.\textsuperscript{15} Part of this followed expansion of the pharmaceutical industry and the glut of therapeutic products on the market. The old reliance on practitioners’ experience was visibly unequal to the task of evaluating and choosing from among so many products. And it did not help that many people thought producers of pharmaceuticals or apparatuses to be venal. Medical researchers gradually came to understand that effective counting requires sophisticated techniques, large infrastructures, and rigorous regulation of conditions. The development of new statistical techniques such as randomization and the chi-square test was certainly necessary to the development of randomized clinical trials (\textsc{rcts}). But equally vital were new forms of social organization and regulation. We know that these forms of regulated testing emerged in Britain during the interwar years and in the United States, on an even larger scale, after 1945.\textsuperscript{16}

Since the 1960s, \textsc{rcts} have assumed canonical significance everywhere in the Western world – they are the gold standard. They do not always resolve controversies or convince everyone, as sociologists of science have repeatedly documented,\textsuperscript{17} but unsuccessful test results usually keep new drugs off the market. The acceptance of \textsc{rcts} has reawakened fears that mechanistic formulae are displacing individualized medical judgment. More seriously, to the extent that \textsc{rcts} appear to provide reliable information about efficacy, it seems logical to seek to eliminate wide variations in medical practice that seem to deviate from correct clinical procedure. This pressure comes not only from third-party payers who have an interest in cutting costs by eliminating ineffective treatments, but even more from within the medical profession; it is a logical consequence of the quest for objectification that has energized and transformed medical research during the past two centuries.

Marc Berg has provided an interesting analysis of how the barriers to “objective knowledge” were understood in the years following 1945. His work suggests that perceptions have changed continually. Starting in the postwar years with the notions that external, structural forces impeded the correct application of science to practice, professional rhetoric increasingly came to blame misunderstanding by individual physicians, making the problem essentially a “cognitive” deficiency.\textsuperscript{18} It begins to make sense then to seek to displace judgment from individual physicians and to argue that algorithms or expert systems can
do a better job than individual doctors in diagnosing and choosing therapies. In response, doctors argue that physical examination and judgment will always be indispensable, no matter how smart the computer or the algorithm.\textsuperscript{19}

RCTs are not a panacea and have thus become the starting point for an entire infrastructure of practices that aim to clarify frequently confusing and contradictory test results – Cochrane collaborations, consensus conferences, meta-analyses, etc. – and that try to monitor or shape doctors’ actual behavior – medical audits, practice guidelines, courses in medical schools in evaluating clinical research. In the context of efforts by governments and private insurance providers to slow rising health costs, these emerging practices have again raised the old fear that reliance on numbers threatens individual clinical judgment. This brings me back to the ubiquitous notion of evidence-based medicine that somehow exemplifies all these different concerns and pressures.

**EVIDENCE-BASED MEDICINE:**

**OBJECTIFICATION AS A SOCIAL MOVEMENT**

In preparation for the conference on which we based this volume, I did a search on PubMed web site for articles that contained the term “evidence-based medicine” in their title. Since this is a book about quantification, it seems appropriate to inundate the reader with some numbers. I found 1,255 articles published before 1 October 2002. Using just “evidence-based” resulted in over 3,400 listings; a broader search including keywords and NLM categories produced about 9,000. This torrent of publications started in 1992 with an article in the *Journal of the American Medical Association* (*JAMA*) by a group based at McMaster University in Hamilton, Ontario.\textsuperscript{20} In 1995 there were 55 articles with “evidence-based medicine” in the title; in 1997, 110; and in 2001, 256. The vast majority of papers published before October 2002 are in English (958), with German a distant second (89) and French third (45). The three journals with the most titles are British, the *British Medical Journal* (*BMJ*), *Lancet*, and what is now called the *Emergency Medicine Journal*; these make up 16 per cent of the entire sample and over 20 per cent of the papers published since 1998. *JAMA*, dominant in the early years, is now in fourth place.

This of course raises interesting questions about the apparent taste in Britain for evidence-based medicine (EBM) that may shed light on the emergence of clinical trials there early in the twentieth century. (I would suggest as possible reasons the long history of statisticians on staff in British public health institutions and the managerial, indeed
accounting, ethos in the National Health Service where “audit commissions” seek to standardize all aspects of medical training and practice.) In recent years, medical journals in almost every country, and representing every specialty, have published some articles on this topic. The New York Times Magazine “Year in Review” included EBM among the most influential ideas of 2001. Articles are of various sorts. A large group has to do with educating doctors to use on-line evidence. A variation attempts, in true evidence-based fashion, to test the effectiveness of such educational programs. Another category, exemplified by reports from the Cochrane collaboration but extending far beyond it, evaluates the literature on various medical problems in order to generate recommendations for practice. A large group of articles makes rhetorical statements in favour of EBM in one field or another. Early papers in this genre were highly polemical. The very first, published in JAMA in 1992, was a call to medical arms robed in the language of Kuhnian philosophy of science. EBM was replacing an old paradigm based on a variety of bad things – intuition, unsystematic, and pathophysiologic rationale as sufficient grounds for clinical decision making; the New Paradigm emphasized examination of evidence from clinical research ... and the application of formal rules of evidence evaluating the clinical literature. One was the Way of the Past; the other, The Way of the Future. (Which would you choose?)

Since then this language has substantially moderated. The most prolific advocate of EBM, David Sackett, together with a number of co-authors, published an editorial in the BMJ in 1996 that exemplified this growing moderation: EBM “means integrating clinical expertise with the best available external evidence ... neither alone is enough.” This changing tone, with its constant insistence that EBM complements rather than replaces traditional clinical virtues, is a result of EBM’s critical reception from many sectors of the profession.

The literature certainly includes many critiques of the concepts behind EBM; frequently the authors are Europeans, but there are considerable numbers of North Americans. Unlike the situation in 1830s’ Paris, these critics seem to be swimming against an immensely powerful tide. They have political allies that were not available to their predecessors – a public-health left, for instance, which sees EBM as part of the reductionist trend that obfuscates social causes of disease, and a popular movement that supports alternative medicine. None the less, the concept’s power is such that each of these two domains has generated its own EBM claims – evidence-based public health or evidence-based alternative medicine. The overall thrust of the critics’ arguments is not unfamiliar to the historian of nineteenth-century
medicine and testifies to the enduring tensions inherent in all efforts to “objectify” medicine. First, critics argue that EBM does not work in many cases—where good evidence is lacking; where variations in skill are very marked, as in surgery or psychodynamic therapy; where the numbers of cases are small and not susceptible to RTCs or, on the contrary, where very large numbers produce great variability in the nature of the condition being tested. RTCs are “bedeviled by low inclusion rates and potentially important recruitment biases. ‘Real world’ trials often do not give the same results as these highly artificial controlled clinical studies … There is a bias in the hypotheses tested in large clinical trials, as the costs involved are usually covered by commercially interested companies.” The contradictory results yielded by different trials are striking reminders that they do not “work” very well. That is why consensus conferences and meta-analyses are necessary to adjudicate such discrepancies. One recent critic has argued that evidence-based medicine is in fact opinion-based and suggests that there are parallels here with the emperor’s new clothes. Most of the time, results are inconclusive and reflect the opinions of experts on how to interpret them. Meta-analysis is particularly untrustworthy and gives the impression of trying to wring statistical significance out of a morass of small effects. Not only does it occult hidden variations in procedure, but it underrepresents trials with negative results, which are frequently never published.

The original popularizers of EBM cast this method, rather naively, as a new “paradigm.” They have thus been fair game for epistemologically sophisticated critics who have pointed out the EBM is at best a set of practices with almost no features of a classical Kuhnian paradigm. It is suggested that the insistence that information from the medical literature should have precedence even when it contradicts the dominant pathophysiological paradigm goes directly against Kuhn’s understanding of paradigms and is in reality profoundly anti-scientific. If the clinical trial always takes precedence over medical theory, “why not conduct double-blind, controlled, randomized clinical trials of the effectiveness of Voodoo, followed by a meta-analysis?” It is, moreover, self-contradictory to demand a change in practices when there is not the slightest evidence of the kind that EBM regularly demands to demonstrate that such practices provide better outcomes. The only possible justification is one of theoretical plausibility, a form of argument that EBM advocates regularly label as “unsubstantiated claims.”

Other critics have used philosophical theories of science in an even more scathing way. One of these examines the claims of EBM in term of Popperian standards of verifiability and points out that what it calls evidence consists of many levels of subjective interpretation of empir-
ical data. “Unfortunately there are no rules of logic that can guarantee a truthful interpretation. There is no evidence in the sense of proofs; there are assertions which are held to be true by some people, by many people, or by practically everyone and which might be false, regardless of how many believers line up to support them.”

The nineteenth-century argument that individual clinical expertise retains its legitimacy continues to be advanced. Repeated even more frequently is the old proposition that quantitative knowledge applies to collectivities and thus cannot apply to individuals. It is not that such knowledge is totally inapplicable – we now can speak in terms of probabilities – but specific results do not apply to real individual people because trials tend to enrol patients who are younger and healthier and in general of lower risk than real-world patients. (A fascinating political dimension to this critique involves identity groups of various sorts apparently excluded from testing, and that has led to radically transformed regulations for recruiting test subjects.) But we return to conclusions remarkably similar to those of Louis’s opponents in the 1830s. Patients are individuals, and need individual treatment. As in the previous century, proponents of pathophysiological reasoning and laboratory experimentation in medicine are among those most threatened by the new trust in numbers.

Second, the old fear resurfaces that clinical authority will shift from doctors. For many people, particularly policy types, this is not a threat but an occasion for beneficial change. EBM provides managers with information that allows them to question the judgment and autonomy of physicians. The techniques that produce data typically require many sorts of non-medical expertise (notably statistical); this breaks the lockhold that the medical profession traditionally has had over judging medicine ... Now armed with more and better information about medical practices, payers and purchasers can deny payment for medical services that they deem medically unnecessary or ineffective.

This of course revives an old nightmare for doctors – that others will make medical decisions on their behalf. Critics consistently use the term “orthodoxy” in speaking about EBM; words such as “rigid,” “politically correct,” and “knee jerk–like” are frequent qualifiers of EBM in this literature. The fear is that EBM will dictate practices, not just in the obvious sense that those who pay health costs will impose practice guidelines to save money, but more subtly by defining out of existence what cannot be measured by RCTs (environmental or psychosocial factors) or what is uninteresting to those who pay for tests – usually pharmaceutical companies – (conditions and therapies from which no one is going to make much money and, more generally, prevention as opposed to cure). From his Popperian perspective, Eyal
Shahar sees EBM as using logically meaningless terms such as “systematic” and “evidence” in order to impose “a new type of authoritarianism” that makes some interpreters of the evidence dominant. “What is behind the title if not other doctors who claim to know better? Who claim that what they call evidence is more valid than another doctor’s interpretation of empirical experience?” As is often the case, sociologists follow the protagonists in emphasizing that political and social factors underlie EBM. Some seek to analyse it a “social movement”; similarly, the rise of a new stratum of professional “experts” producing guidelines fits with Freidson’s “stratification theory.”

Other critics worry less about power than about the “dumbing down” of the medical profession, as doctors lose their ability to judge and discriminate and understand disease mechanisms. In response, spokesmen for EBM such as Sackett insist that it is not “cookbook” medicine but depends on individual clinical expertise. “External clinical evidence can inform, but can never replace, individual clinical expertise.” To fears that the hidden agenda is cutting costs, Sackett replies, “this may raise rather than lower the cost of their care.”

Not all the arguments against EBM are traditional. The medical field, for one thing, now includes a large number of occupational groups, many of which do not share the values of doctors. A variety of other forms of expertise are struggling to make themselves heard. Research in health administration, for instance, emphasizes that clinical effectiveness is only one criterion of decision-making, others include “cost-effectiveness” and “patient and public preferences.” Some observers argue for a much more complex form of administrative decision-making to take account of these different imperatives. Allan Maynard sees EBM as concerned just with what is effective for the patient, whereas the economist or public-health physician looks at the interests of society as a whole – “the population-health ethic.” Ours is a far more cynical, relativistic culture than was 1830s’ Paris; the idea that evidence alone drives practice has become unconvincing to some. Some critics maintain, for instance, that EBM reflects specific ideological investments — in curative medicine rather than prevention. EBM is bound to fail in shaping practice, others say, because it does not recognize the complex nature of medical decision-making.

One such article coming out of the health-management domain applies Aristotle’s analysis of rhetoric to argue that logos, in this case evidence, is only one element in the arts of persuasion, which also include pathos, the power to stir the emotions, and ethos, the authority of the speaker. “Perhaps the most obvious lesson we can take away from the rhetorical triangle is that scientific evidence is but one component of persuasion. Health care innovation adoption is not only a
function of the argument, but also of the credibility of the proponents and the values, experience, and interests of potential adopters. Innovations are more like to be adopted when a convincing argument (logos) is presented by credible research proponents (ethos) who stir the interests, needs, and emotions of adopting practitioners (pathos).”

The authors add: “in this pluralistic situation there is no such thing as ‘the evidence.’ Instead, there are several competing bodies of evidence that are subject to multiple interpretations by different stakeholders.”

In the face of this onslaught, prominent leaders of EBM have not just moderated their rhetoric but conceded to the critics on many points. R. Brian Haynes of McMaster University, a major figure in the movement, has admitted that no direct evidence exists to show that practice based on the principles of EBM is in any way superior to that based on clinicians’ experience. He agrees that “the research methods of medical science are pluralistic and expanding, driven by attempts to address a broader range of questions” and that “evidence from research can be no more than one component of any clinical decision.” He admits that it is difficult to argue for EBM’s superiority when results of methodologically similar trials frequently disagree with one another and when findings of observational studies frequently agree with supposedly more potent RCTs. Among EBM’s greatest failings is its inability to distinguish between doing the greatest good for the individual patient and doing the greatest good for all patients, collectively.

Stripped of its preachy tone and moral fervour, and presented in this new and distinctly ecumenical mode, EBM does not mean very much. As critics have noted, admitting the validity of many different forms of knowledge without prescribing a strategy for evaluating them and resolving conflicting conclusions hardly amounts to a new paradigm and may not be distinguishable from traditional practice. (In any case, the majority of more hardline EBMers do not shrink from claiming a strict evidentiary hierarchy.) None the less, pronouncements to the effect that EBM has been a failure seem wildly premature. Not only do they ignore the time required for even the most successful innovations to take root, they understand success as the unrealistic expectation that most physicians will suddenly change their ways and practice the “best” medicine. The reality is likely to be much more modest. I suspect that EBM will continue to gain in popularity, at least as a slogan and as a claim and, not incidentally, as an ever-expanding multitude of procedures, instruments, and guidelines. Certainly my own distinctly non-evidence-based impression of the literature is that
criticism is becoming less frequent as the movement gradually loses its threatening edge (even as it becomes more powerful). It may even someday become—like simple clinical counting in the nineteenth century—a routine part of medical practice. It may or may not lead to better clinical decisions, as more and more medical students receive indoctrination in its principles. (This is certainly the case at McGill University, where I teach.) Certainly the widespread availability of practice guidelines has reduced the economic implications of spending hours at a computer instead of seeing billable patients. There is no reason to assume that the underdetermination of medical practice by solid evidence or expert opinion will not continue, given the complexity of so much “real-life” medical practice. Whatever form EBM comes to assume, it will not go away. It is perhaps a slogan as much as a concrete program (let alone a paradigm), but it serves a wide variety of functions.

First, it is clearly plays a rhetorical role in defending medical authority from a variety of contemporary threats. This is of course the function that sociologists of science and of the professions tend to emphasize. EBM identifies medicine as scientific and distinguishes it from various forms of alternative healing, the heartless cost-cutting of insurance providers, and the irrational whims of patients. This collective authority may limit somewhat the clinical autonomy of individual clinicians (and concentrate authority in the hands of clinical elites), but this may be unavoidable. And one of its side-effects is to make doctors feel that they are coping with an ever-expanding volume of medical literature that they would otherwise never master.

Second, within the hierarchy of clinical research, EBM particularly valorizes patient-centred clinical research—a form of science that has never achieved the success or status of the laboratory sciences. It thus places clinical research on a more nearly equal footing. If it does not have the epistemological status of the laboratory experiment, it at least has the immediate relevance of rigorous applied research that defines effective practice, not to mention statistical techniques that few people can fully comprehend. It joins to medicine’s existing status the new status and power of the computer while allowing doctors—and especially the men among them—to play with computers as a routine part of their work.

Third, EBM is such a protean concept that anyone can appropriate it. Politicians and health administrators can invoke it to argue for cost-cutting and for limiting the autonomy of doctors; simultaneously, doctors can use it to defend clinical practices and judgment against economic pressures to cut costs. Everyone from public-health professionals, through nurses, to alternative practitioners can associate himself or herself with both the term and the scientific rigour that it supposedly embodies.
It is thus possible that EBM will lose its ideological and programmatic edge and become just one routine part of practice, among many others, in much the way that clinical counting did in the nineteenth century. However, several important variables have changed since the nineteenth century. Chief of these is the sheer size of the medical enterprise. Medicine has become so institutionalized, bureaucratized, and central to social concerns that one cannot dismiss the possibility that the increasing numbers of practice guidelines will eventually become rigidly enforced criteria of practice. Courts may come to use practice guidelines as standards in litigation judgments. It is equally possible that insurers, both public and private, will use them to determine which procedures to reimburse and which not. There is little evidence that this process has begun, in North America at least, but this does not mean that it will not occur in the future.

A less likely alternative to EBM receives frequent mention in leading medical journals. Even those clinicians who most fervently support EBM understand that there is a major gap between evidence about populations and evidence about individuals. The former may provide us with the best existing “objective” data about the latter, but “average responses” cannot replace knowledge of the individual. This latter form of knowledge has traditionally been subjective and thus unreliable. If knowledge of the individual could become truly “objective,” there would be no need to rely on “averages.” This is precisely the promise that genotyping seems to offer: “objective” genetic data, some people think or hope, may allow clinicians to individualize therapy in a way now possible only through trial and error. Such claims or hopes reveal a great deal about the tensions within the EBM movement and clinical medicine in general. There is, I would argue, fundamental incommensurability between forms of collective objectification and the aims of the clinician at the bedside, however loudly he or she clamours for EBM. Therapy based on genotyping – “objectified” individuality in therapy – would allow clinicians to have their cake and eat it too. However, this is at present a distant hope rather than an immediate prospect. In the interim we have EBM. Available to everyone, and meaning relatively little, EBM will probably remain a popular catchphrase, at least until something better comes along.

NOTES

1 P.K. Rangachari, “Evidence-Based Medicine: Old French Wine with a New Canadian Label,” *Journal of the Royal Society of Medicine* 90 (1997), 280–4. A more positive spin on this connection appears in Han P. Van-


5 On these matters, see the essays in this volume by Marks, Rusnock, and Tröhler.

6 Ann La Berge in this volume.


9 I describe a number of examples in Weisz, *The Medical Mandarins*, 159–88.


12 The term is in John R. Hampton, “Evidence-Based Medicine, Opinion-Based Medicine, and Real-World Medicine,” *Perspectives in Biology and Medicine* **45** (2002), 554.

13 Weisz, *The Medical Mandarins*.

From Clinical Counting to Evidence-Based Medicine


16 This perception may be based on simple ignorance. We do not really know what was going on in other countries, because research is lacking.


Hampton, “Evidence-Based Medicine,” 563.


Hampton, “Evidence-Based Medicine,” 565.


Sackett et al., “Editorial: Evidence Based Medicine.”


R. Brian Haynes, “What Kind of Evidence Is It That Evidence-Based Medicine Advocates Want Health Care Providers and Consumers to Pay
Attention to?" *BMC Health Services Research* 2 (2002). The copy that I consulted on the internet is paginated 1 to 7 but warns on the bottom not to use this numbering for citation purposes. I have been unable to find any other pagination. The quotes in the text are from pages 2, 4, 5, and 6, respectively, of the internet version.


