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The theory and practice of evidence-based information work – one world?

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Abstract

Purpose: To investigate the validity of the assumption that evidence-based library and information practice (EBLIP) – as conceived, defined, and implemented up to now – is indeed adequate and appropriate to the full spectrum of library and information activities as historically constituted.

Methodology/approach: The author's study and conclusions are based on: 1) a survey and critical appraisal of quantitative as well as qualitative research as reported in our own professional literature – particularly with respect to intermediated information retrieval, reference services, and collection management; 2) a survey of relevant literature from other fields where the introduction of evidence-based practice (EBP) has been discussed; 3) a wide-ranging examination and appraisal of major scholarly publications in epistemology, in the philosophy and sociology of science and scholarship, and in cognitive psychology; 4) long personal experience as researcher and university teacher, in scientific publishing, and (since 1989) as academic librarian.

Findings: This reflective investigation suggests that it is much too early to assert with confidence (as is often nonetheless done) that EBLIP enjoys either universal or holistic applicability in the library and information services arena. Available evidence and insights point, rather, to at least these six alternative conclusions:
1) The only reliable approach to ultimately legitimating investments in library and information services (LIS) is to broaden the terms of reference within and through

which we not only conceptualize our objectives and functions but also select and gather, validate, and apply the relevant evidence. This broadening may naturally entail, for example: participatory research; less disregard for long-existing but un(der)utilized evidence; a less presentistic and vainglorious mindset.

2) There exist many varying conceptions of the phenomenon "evidence", conceptions that have proven fruitful in diverse areas of intellectual endeavor. EBLIP would do well to take note of and to profit from, more than is presently the case, such conceptions.

3) The EBLIP model as presently developed and delimited is by its nature poorly suited to use in supporting truly user-oriented LIS. It is far too unreflectively taken over from the early, orthodox version of evidence-based (clinical-epidemiological) medicine. EBLIP still has much to learn from the enormous amount of criticism that EBP has encountered in the health-care world. I offer a selection of that criticism in eleven categories here.

4) It is prudent, as in other disciplines, to base practice no less on (sound) theory and theory-building than on "empirical" findings. We should look for a better balance between inductive and deductive processes.

5) The closer the contact between LIS and the lived world of their users, the more difficult (if not impossible) it is to measure or even to identify outcomes – and of course to correlate these, if isolable, to LIS in- and outputs. Ethnographic, discourse-analytic and other qualitative methods of investigation are here of particular value. The up to now orthodox vision of EBLIP is, in turn, more appropriate to optimizing the less user-centered, more infrastructural and "backoffice" activities. These lend themselves more naturally to quantitatively construed evidence.

6) The disciplines to which one should normally look, beyond library and information studies, for useful evidence and theories, ought to be expanded to include at least philosophy, sociology, and the cognitive sciences – and perhaps even to philology (discourse, rhetoric, narrative).

Research implications: The basic assumption behind EBLIP is valid, and in fact essential to further assuring the effectiveness and relevance of our work. We must however, both as researchers and as appliers of research, develop a broader and more

nuanced vision of the EBLIP approach. Evidence – how it is found, construed and used – should function more clearly in the service of theory.

Practical implications: Practitioners must be prepared to follow where the best evidence (in the broader and more varied sense suggested above) leads. That implies an even more deeply reflective (but also reflexive!) orientation than EBLIP already recommends. It means the readiness to give up – or to adapt – even the vested interests and articles of faith themselves on which we are accustomed to found our professional legitimation and identity. If, that is (but only if), the evidence testifies that such is in the best interests of the clientele we are there to serve.

Value/originality: Encourages a more inclusive, heterogeneous viewpoint regarding – and therefore a more workable approach to – EBLIP.

1. INTRODUCTION

Exceptional indeed must be the librarian who has never encountered, probably taken some measure of offence at, and perhaps even further reflected upon, the less than flattering stereotypical image of his or her occupational group that the outside world still today seems remarkably reluctant to relinquish. Quite apart from the question whether this image may not imply a kernal of truth, it does in any event fail to take account of certain indisputably common if not in fact universal traits among persons who look upon themselves as library or information professionals. In this case I mean their tremendous conviction that the competencies and talents they possess hold great potential for benefiting others; their sense of mission in putting that point across (particularly to the immediate communities of potential beneficiaries, of decision-makers and of actual or potential funders and sponsors, but in truth especially to each other); and their endearing readiness to demonstrate in practice just how their competencies can be of real help and significance to others. The boon of library service. The blessings of information literacy. There for the plucking.

The rhetorical import of this posture is easy to grasp. It is an old recital. Perhaps even a century old. And because it is an old recital, and furthermore a comforting one, it has become more than a recital, it has become a credo.

There's nothing *wrong* with credos in themselves, of course. They certainly have their place. Most of us can't get along without them. Within a community of believers – which need not be a religious community, and can also be a 'community of practice' – they are strong, if not essential, forces for cohesion, for self-assurance, self-legitimation and self-esteem. It is nevertheless in the nature of a credo that this internal force and validity, however robust it may be, enjoys no automatic external equivalent. This need not be a problem where the community in question is autonomous and self-supporting, as in the case of most religious and many ideological communities. The community of practice which we shall here, in accord with current convention, call that of "library and information services" ("LIS"), does not fall within such a category.

If we leave aside their functions as custodian of cultural heritage, we can properly claim that LIS exist only by virtue of their capacity to facilitate other undertakings – or rather by virtue of the extent to which they *exercise* that capacity in the interests (now and for the future) of those undertakings. What this means is among other things the following. The LIS credo need not be founded upon an illusion. Few of us within the field would be disposed to entertain that possibility (though there are notable exceptions). But that *we* may believe it is by definition insufficient. The legitimation of what we do implies two further requirements, one immediate and the other more fundamental. First of all, what we believe must also be believed by others, namely (the majority of) those engaged in what I above referred to as "other undertakings" – who directly or indirectly bear the costs generated by LIS. This we can call the "credibility condition". Secondly, it must be more or less true. This we can call the "objective warrant condition". (Here I use the formulation "more or less" advisedly, and the term "objective" relativistically.)

We in LIS have long been fortunate that fulfilment of the credibility condition was sufficient to guarantee very considerable investments in ourselves and our activities – in

public as well as private sectors. This success was once possible with only moderate express effort to that end; in the last couple of decades, though, that effort has had to be concertedly increased ("advocacy"). Societal but especially technological developments have however since the later nineties led to a situation in which one has more and more felt it either advantageous or imperative to take the logical step backward and switch one's emphasis to the "objective warrant condition". That is a backward and logical step because this latter condition brings with it not only a more basic level of discourse but also, deductively, an "entailment" of the former condition. That which is true (or, more realistically, that which has been convincingly argued to be true) is necessarily less vulnerable to credibility problems.

We might therefore rightfully surmise that, whatever the practical details of its history, either it is no accident that the evidence-based library and information practice (EBLIP) movement developed when it did, or it was a particularly fortunate accident for our profession. I suspect however that it was in fact something of both. But let us not be too sanguine. Neither the evidence-based practice (EBP) mentality, nor EBP implementation, has yet taken our field by storm, far from it. The circle of EBLIP enthusiasts is still small (and outside the health sciences environment, perhaps even minuscule), and that of genuine EBLIP practitioners of course smaller. But more importantly: what has to date been said and done on the subject – while for the most part well-considered and useful – has not been able to inspire full confidence that one has now with a sufficiently critical eye and reflective insight been able to crystallize a sophisticated yet sturdy enough conceptualization of the approach in order to move vigorously forward toward widespread implementation.

We still need quietly to contemplate, I suggest, how we arrived where we now are on the way to evidence-based practice and what the implications so far are of this EBLIP "journey" (Brice, Booth, Crumley, Koufogiannakis, & Eldredge, 2004), and honestly to search out where adjustments may now be desirable. We need still to think carefully about, and broadly to discuss, what we actually mean by evidence and its use. We need to identify, as well, the inherent problems and challenges yet to be addressed. We need to decide which of those problems we are now in a position to resolve, and which we

shall only later or perhaps never be able to overcome; how we can best confront the former and how we can best live with the latter. It can not be the ambition of this paper even provisionally to outline in its entirety such a way forward. Others have already begun work on such a task, and made considerable progress since the beginnings some ten years ago. My own impression is nonetheless that up to this point the EBLIP advocates, in their initial enthusiasm, may have taken a somewhat too simplistically optimistic perspective, and adopted a perhaps too positivistic discourse.What I would therefore like to offer below is one concerned professional's critical exploration of factors he would suggest now deserve a more attentive reflection than they have perhaps heretofore been given.

Booth & Brice (2004a, p. X), two EBLIP luminaries, have made clear their conviction that precisely what we *don't* need is "some uneasy and artificial consensus", and have called upon their fellow LIS-practitioners "to challenge, to criticize and even to combat!", possibly even to irritate, in the heat of the ongoing EBLIP discussion. Combatting is something I prefer to leave to others, however it *is* possible that some of my colleagues would claim that I'm not averse to irritating them now and then. Yet what they may call irritation, I would prefer to see as stimulation (at any rate, such is my intent), and if I really get lucky, perhaps on occasion inspiration. I couldn't agree more with Booth and Brice, therefore: artificial consensus isn't going to get us very far. But that doesn't mean – I hasten to add – that genuine consensus should be our real objective either. (Consensus is not only relatively uninteresting, it is also an infertile ground for creativity and innovation.) If LIS are to have a vital future, what we in my view have to strive for is not consensus at all – except to the extent that we all manage to agree that evidence in principle *matters* – but rather a continuous informed dialectic involving not only our own community of practice but also the concerned outside parties, who alone can ultimately provide the real legitimation for what we do.

2. THE EVIDENCE-BASED-PRACTICE ARROACH

I have above suggested that the viability of the EBLIP-idea, and the appeal which it from the beginning will have had to many reflective practitioners, can best be seen in the context of a growing crisis of legitmation in their environment. But that is of course by no means the whole story. EBLIP was not a fully organic, nor a spontaneous, LIS development. The EBP conceptualization, discourse and program as initially introduced to the library profession were directly and deliberately transferred, with a minimum of necessary adaptation, from the health care sector, more particularly from the realm of clinical epidemiology. That conceptualization, that discourse, and that program have remained, for EBLIP activists, largely the same even to this day. The compelling idea seems to have been: if such an enlightened innovation can catch on in medicine, why not in LIS?

This history explains in large measure why the movement in 2007 looks the way it does. This is simply a fact of life, and must be accepted as such. Some of us may have problems with it. I do myself, as will soon be obvious. I do not, however, find that a good reason to try totally to reinvent the wheel. I don't think the wheel we have is perfectly round, the mounting and suspension leave something to be desired. And it not only needs a lot better lubrication, the design itself requires significant adjustment. But we would all at least agree, I hope, that the wheel itself is a good idea – even if we have to go back to the drawingboard. And indeed, that's what I think is going to have to happen.

I have for many years been personally convinced of the vital importance of basing LIS on the best possible evidence, of the importance of good critical appraisal, of never-ending evaluation of resulting practice. (Ask anyone who knows me.) In my own field of academic librarianship it is clear to me that many of the things we presently do – or at least how we do them – follow from assumptions apparently in conflict with the realities of the scientific and scholarly enterprise. If I had started a crusade for adopting EBP in our sector, I would have done it quite differently. But I *didn't* start one – so I've got no right to complain. I do however have a right, we all have that right and perhaps that duty, as Booth & Brice (2004a) suggest, to be critical, to be constructively critical, and to make constructive suggestions. The more the better. When I look at that EBLIP

wheel, I see that some of the materials should perhaps be different (what kinds of evidence should we admit, what sort of questions should we try to answer?), that the structure could be improved (evidence hierarchies, domain categories), but what I see above all is that the whole wheel isn't big enough, not big enough for the job to be done. The reason has partly to do with the history of this EBLIP project, but more fundamentally it's a question of the reigning mentality within this profession (as in others, to be honest), in which the credo I mentioned above is an essential element: we take as a kind of departure point, or "given", the LIS operation as we have collectively constituted it over the last 130 years – with its well defined and demarcated functions and categories, its identified and sanctioned objectives and criteria – and then seek the best evidence for most efficiently/effectively (and *persuasively*) perpetuating that framework. The framework itself we have then placed a priori outside the purview of evidence-seeking and evidence-application. But that framework and all its components are *also* part and parcel of our practice. They may have come to be taken for granted (by us, that is); and opening them up for examination, change, replacement or abandonment may be painful. A genuine commitment to EBP nonetheless mandates that we do so – precisely because, as indicated already, LIS are in their essence a phenomenon of facilitation within broader contexts. It must be a *prerequisite* of adopting EBLIP that our terms of reference for it are both more broadly defined and more broadly maintained than has so far been generally the case. We must consciously take one remove (I called it above a "logical step backward") before we even try to decide what qualifies as evidence – and, a fortiori, what questions to ask. Even the continued existence of LIS as we know them should naturally not be taken as a premise, given that our mission is clearly not survival per se, but utility for other undertakings.

This is not only a matter of honesty and consistency, it is (or will become) bitter necessity. For in no other way can we satisfy the objective warrant condition, as I have called it. I'm not saying that we've got to start tomorrow, but as the adage goes, if we don't do it ourselves, somebody else will eventually do it for us (even if it's only *de facto*, and that somebody else is as amorphous as technology's grip on society).

3. CONSIDERING EVIDENCE

In here professing myself as avid supporter of EBLIP, I have quite consciously been tacitly working with the broadest possible utilitarian conception of "evidence", but not so broad as to render it *pragmatically* meaningless. Something like this: a robust (and, ideally, recurring) indication that something in the outside world is or is not the case and can be reliably assumed provisionally to remain so, an indication ideally (repeatedly) both perceived by oneself and independently reported by competent and trustworthy others, and on the basis of which one takes a decision within LIS practice – without regard to (and often as opposed to) what one or others would prefer or wish or guess to be (or not to be) the case, what once may have been (or have been not) the case, what the clichés would have to be (not) the case, or what would be convenient or easy or gratifying or rewarding should it (not) be the case. I am under no illusion that such a definition is water-tight, much less unambiguous in various details. It is in any case firm enough to distinguish how I think LIS decisions should be taken from how I know them in fact often to have been – and still now not infrequently to be – taken. To me this means that a successful EBLIP implementation on *this* basis would already be a gigantic step forward. I'm not even sure I wouldn't settle for it at the end of the day. Of course I am aware that there are many evidence-enthusiasts who wouldn't.

One problem is that those who wouldn't, will have many different and even conflicting reasons for not doing so, and will in some cases fundamentally disagree with each other on essential points. There are many takes on what evidence is and does, how to get it and to use it, how you classify it into types, how to judge its relevance or its force, how you should accept or reject or ignore it. The early EBLIP enthusiasts and their followers have characteristically given the impression that the proper meaning of the "E" in EBLIP can be considered fairly clearcut. For better or worse, that is far from the case. I shall have more to say about that standard rendition of evidence in the next section. What I think it is useful to do here is to offer a selection of alternative views. If we are serious about a critical discussion of what EBLIP should be and do, we should take also such perspectives as these reflectively into consideration. David Schum (2005, p. 14), a

professor of "evidence science", has rightly observed that "most issues arising in the study of evidence have roots in epistemology". I would go further and claim that many are also fodder for ontologists and logicians. (It is interesting to observe, given the EBLIP background mentioned above, that an entire book [Jenicek & Hitchcock, 2005] has been dedicated to the principle that a firm grounding in philosophy is essential for the successful evidence-based practitioner in the medical sector.) Evidence has in fact become something of a separate specialism for some philosophers. Getting down to such philosophical brass tacks could certainly be enlightening, and probably helpful as well for refining our feeling for what EBLIP can or should become. But some other time; that's not (as one referee of the proposal for this paper was fearful) what I wish to do here. I want, rather, simply to highlight certain sorts of other well considered positions regarding, or doubts concerning, evidence in relation to *practice* – in our field or in related ones – which seem to me to have been underrepresented or even unrepresented in the EBLIP discussion so far. Perhaps we can in the meantime keep in mind what Jenicek (2006, p. RA245) wrote about evidence-based medicine: "The concept of EBM is only as valid as the definition and concept of evidence itself." Not hoping to be exhaustive, I have chosen twelve categories which I think to be especially interesting:

▪ **The negativist position**. Should evidence not in fact be explicitly rejected as basis for decision- and policy-making for LIS? At least one university library director has taken this position (Smith, 2003), claiming that depending on evidence endangers creativity and innovation. Not evidence, but *principles*, should be the guiding light. Do we have irrefutable evidence to show that he is wrong?

▪ **The cautionary position**. Rejecting the systematic use of best evidence goes too far – but so does insisting upon it. Here too, the argument from creativity and innovation is pertinent. Numerous professionals have wondered whether our goal should be not evidence-*based* practice, but rather evidence-*informed* practice (EIP?).

▪ **The normative-scientific view**. The best, or only real, candidate for the function of guiding practice (in LIS as elsewhere) is not evidence, but theory. Evidence, then,

becomes demoted to secondary status: its destiny is to be sought out and used as a support for (or, more strictly speaking, and shown to be unable to falsify) the theory of choice. This is held up as the classical picture of how the exact and natural sciences, at least, actually function. The most stringent version of this view maintains, or silently assumes, that evidence – like science itself – is value-free.

▪ **Non-essentialist and non-representationalist conceptions - 1**. What qualifies as evidence in the context of discovery is an objective matter, but what qualifies as evidence in the context of use is a subjective decision or judgement.

▪ **Non-essentialist and non-representationalist conceptions - 2**. What qualifies as evidence both in the context of discovery and in that of use is subjectively determined (see, e.g., Berkwits, 1998; Gonzales, Ringeisen, & Chambers, 2002). This means, among other things, that evidence can never be considered value-free.

▪ **Non-essentialist and non-representationalist conceptions - 3**. Whether or not (best) evidence should be understood internally (i.e., to the LIS-community) as objective, it is necessarily subjective (and potentially value-laden) unless it has been judged also by external stakeholders – and in particular the community of LIS users – to qualify as (best) evidence for LIS practice. This stipulation could be seen as implicit in what we termed the "objective warrant condition".

▪ **Non-essentialist and non-representationalist conceptions - 4**. Evidence is not in fact an entity at all. Evidence is *argumentation* (see Amrine et al., 1996; Twining, 2003). The essence of evidence is therefore rhetorical (in the non-pejorative sense of that word), and therefore non-ontological, processual, and situated. Scallen (2003, p. 111) speaks of "the inherently rhetorical quality of Evidence". Lambert states: "Clearly however, evidence is about making certain claims as much as it is about representations" (2006, p. 2642). Jenicek (2006, p. RA246) speaks of the "critical appraisal of the argumentation process in which evidence is used", and holds that "critical appraisal of uses of evidence relies on the modern methodology of argumentation" and is "at least as important as the critical appraisal of evidence itself".

▪ **Non-essentialist and non-representationalist conceptions - 5**. A common claim of philosophical evidentialists is that evidence in the last analysis boils down to a "mental state" (Conee & Feldman, 2006), whether conscious or otherwise, and is therefore always *personal*. (See also Schum et al., 2006, p. 68.)

▪ **Relativistic reservations**. What one seeks and accepts as evidence depends on the paradigm (in the Kuhnian sense) within which one operates. This point has also been acknowledged by researchers in our own field: Ford et al. (1999, p. 395) write: "Conceptions of what constitutes 'evidence' in the generation of new knowledge may differ fundamentally according to different research paradigms." – and draw furthermore a distinction between *analytical* and *logical* inference. A very interesting question for us in LIS, at least for those in academic environments, is the extent to which what we consider best evidence for our practice should vary with the paradigms – and more broadly, disciplines – within which our *users* operate (see e.g. Amrine et al., 1996). Is it irrelevant whether we are serving physicists, historians, lawyers, econometricians, engineers, classsicists, or clinicians? Another dimension is differences across countries, language areas, cultures. Are univocal standards of EBP for information services achievable at all? Lewontin (1991, p. 153) reminds us that even in the natural sciences "the quality of evidence itself is tailored to fit ideological demands".

▪ **Diachronic reservations**. What yesterday was best evidence may not tomorrow be best evidence (or evidence at all). How does one go about managing this aspect? Kuhn (1977) believed that science (as opposed to art) naturally "destroys its own past" (including its evidence?). How much of LIS is art, and how much science?

▪ **Intellectual reservations**. A reflective practitioner is thought essential to the success of EBP (Abbott, 2006; Partridge & Hallam, 2005; Todd, 2002). But is it not the case that implemented EBP subtly yet inevitably inhibits and silently discourages further "reflection-in-action" (Schön, 1983)? How might we avoid such a danger? Is EBP not in fact too deterministic? Ford et al. (1999), following Hudson's *Contrary imaginations*,

have emphasized the importance for innovation of what they term “divergent thinking”, which “often entails the questioning of current knowledge and current approaches, and the development of new concepts often via creative analogy” (p. 395), and even proposes “radical new alternatives” (p. 397). How might this fit in (or not) with the following of evidence-based practice? Even “convergent thinking”, we might note, is according to Ford et al. not immune, even within disciplines, to disagreements “in relation to the nature of ‘evidence’, to criteria determining different levels of evidential quality, and to the notion of ‘authority’” (p. 395).

▪ **Open questions**. Diligent investigation can perhaps lead to the conclusion that for certain activities no (reliable) evidence, or insufficient evidence for guiding the practitioner, can under present circumstances be derived. Nonetheless, those activities must be pursued. Oakley (2002) speaks of areas which “escape the evidence net”. Can the very lack of evidence in some way be treated as a kind of evidence? Booth (2006c) has remarked that doing the EBP-process well can perhaps itself count as a legitimate outcome for the person involved, even if the exercise does not result in positive change to the organization’s practice. Exactly what value he wished to attach to “failure stories” (2002b) remains however unclear to me.

Another question concerns what one might call degree of evidentiary outspokenness. Must evidence be explicit, or may it be implicit? If it may be both, is explicit per se better than implicit evidence? This can be important for considering the status of narrative and anecdotal evidence – the usefulness of which has according to some been insufficiently acknowledged. Relevant here is likewise the notion of a “conjectural paradigm”, which Carlo Ginzburg developed as a fundamental epistemological alternative to the standard of quantitative evidential rigor often held out as definitive by the natural sciences (Ginzburg, 1980). Ginzburg considered this conjectural kind of evidence a natural one in the medical sciences and elsewhere. Schum (Schum et al., 2006) claims that evidence is usually inconclusive, often ambiguous, commonly dissonant. He also recognizes a category of “ancillary evidence”, or “evidence about other evidence” (2005, p. 28).

Yet another possibility is that, in certain contexts, *coherence* may be able to function as valid evidence. *Analogy* may also be thought to have evidentiary value. And finally, what is the importance of "eliminative evidence" – and where and how should we use it, for example, in combination with affirmative evidence?

Lambert (2006) has suggested that differing legitimate ideas of evidence, even in the health-care world, may ultimately be irreconcilable, that consensus will remain elusive. Can LIS then fare any better? Scott (1991) describes the possibility of simultaneously maintaining two essentially different working notions of evidence (her instance is historians).

4. THE PROBLEMS AND CHALLENGES FOR EBLIP TODAY

In the Introduction I posited the necesssity "to identify, as well, the inherent problems and challenges yet to be addressed" at this point along what others have termed the "journey" to EBLIP. It may be convenient to arrange a selection of these – as I see them – into three categories: the ideological/philosophical, the formal/procedural, and the incidental.

4a. The ideological/philosophical problems and challenges

Scope. The greatest and most fundamental challenge which I perceive for EBLIP today is that of broadening its terms of reference. No small paradox lies in the fact that while this is the greatest challenge, it is also the easiest one to meet. *Doing* it is no problem; we can all start tomorrow. The problem is to become (unless you are already – but I would guess that very few of us are) *willing* to do it. The "P" in EBLIP means not only *how* to practice but also *what* to practice, and *why* – in some cases perhaps even *whether*. Our entire profession – with all its functions and categories and criteria – is a *demonstrandum*. No sacred cows, no vested interests, no protectionism. Inspiration for change must *always* come from outside, it must *never* come from inside – just as must

(as already suggested) the legitimation of change. Susan Lee, Associate Librarian at Harvard, has stated (in Lee, Juergens, & Werking, 1996) that we shall "need to change values, behavior, and thinking"; Shirley Echelman, former executive director of the Association of Research Libraries, recommended (1988, p. 41) "zero-based evaluations of every function currently performed in libraries". Now that's exactly what I mean when I say that everything's a *demonstrandum.* And *demonstranda* mean: evidence. Abbott (2006, p. 65) is quite right that EBLIP "requires librarians to develop a culture of questioning and reflecting on what we do" – if by that she means questioning everything and reflecting quintessentially.

'*Thirdspace*'. That was the most fundamental challenge; now the most difficult one: what Learmonth & Harding (2006) have referred to as that of "evidence-based imagination". Living within our world of practitioners (and researchers for practice), we are aware that our mission is to serve a lived world of information/library users. What actually happens, however, is that we, whether we are aware of it or not, in our dedication to optimal performance aim our plans and efforts (and, too often, even our reactions) not squarely at that second lived world, but rather at a third "space" – in Soja's terminology, "geography" – that is, a users'-world-as-imagined (or, mentally constructed) by us. Learmonth & Harding developed their insight in the context of evidence-based management, but it applies in no lesser degree to LIS. The *thirdspace* problem applies to LIS with or without EBP, but EBLIP makes it more critical. It seems to lie in our very nature: we strive to serve users not as they actually are and function, but as we think that they do or ought to, as our systems tell us they should behave. Learmonth & Harding (2006, p. 260) quote Soja's (*Thirdspace*, following Lefebvre's *The production of space*) argument that "this imagined geography tends to become seen as the 'real' geography in the sense that 'the image or representation come[s] to define and order the reality'". They go on to observe of research for EBP that "When such research fails it becomes plausible to blame the managers for the failure rather than the model used to abstract space, for the model has achieved the status of the taken-for-granted. The possibility that it is the model to blame does not enter thought.", and further: "Our position, then, is that evidence-based management, as it is currently constituted, claims to describe a world which does not exist in lived space: its world

‘exists’ only as a series of concepts. And, unfortunately, these conceptual spaces have little relationship to the lived, material spaces its proponents claim they represent.” (p. 261). We would do well, I propose, to think long and hard, indeed to reflect earnestly, on these words and on how they might well apply also to us.

Empiricism. EBLIP as originally instigated and until now propagated has been an uncompromising gospel of empiricism. The role of *theory* has not only been largely ignored, it has occasionally been explicitly played down or even belittled (Booth, 2006c; Eldredge, 2000; Crumley & Koufogiannakis, 2002). Eldredge’s tendency in this direction is particularly remarkable in that he in the same essay does his best to convince us that EBLIP is making librarianship more “scientific” – while in fact theory is the very soul of the scientific enterprise (see above).

Questions of theory-choice, theory-building and theory-testing need much more careful attention in LIS, and therefore in EBLIP. Is best practice possible in a theory-impoverished framework? Is the very concept of evidence not weaker, or even meaningless, without solid theoretical orientation? One wonders whether many EBLIP enthusiasts have not taken note that inattention to, or undervaluation of, theory has been one of the most recurrent points of criticism in the masssive reaction against EBP in various fields, from medicine (Buetow, 2006; Cohen, Zoë Stavri, & Hersh, 2004; Sehon & Stanley, 2003; Tonelli, 2006; Tonelli & Callahan, 2001; Upshur & Tracy, 2004) through nursing (Ingersoll, 2000; Whall, Sinclair, & Parahoo, 2006) and psychotherapy / clinical psychology (Messer, 2004; Ratner, 2006) and speech-language pathology (Beecham, 2004; Justice & Fey, 2004; Ratner, 2006) to management and education (Ratner, 2006). Harvard geneticist R. C. Lewontin reminds us that “facts make a theory, but it takes a theory to make facts” (1991, p. 147), and writes with gusto about observations which are “pushed to the back of the collective scientific consciousness” because they don’t fit with a current theoretical structure (p. 148). Cohen et al. (2004, p. 38) remind us of the long-recognized fact “that making theory-free, objective observation is impossible”, and furthermore claim that evidence-based medicine “ignores this essential interplay between observation and theory”. Partridge & Hallam (2005) recognize, in a general way, the importance of theoretical grounding for

successful EBLIP professionals. I can not, myself, really understand how the "five classic stages of evidence-based practice" (Booth, 2006c) can be executed *without* a theoretical framework – and that goes perhaps even more clearly for the added sixth stage. The great irony is of course that the effectiveness (and therefore appropriateness) of EBP is itself just a theory, for which empirical evidence is lacking and which "must fall back on rhetoric" as its only support (Norman, 1999, p. 143).

Smith (2003) has rejected empiricism for LIS practice in favor not of theory, but of principles. Principles have a long history in shaping LIS practice. What happens when there is no evidence to support them – or even evidence against them?

Lack of depth. This is a problem which has been explicitly acknowledged by some colleagues. Much LIS research deals with what one might term superficial matters, with perhaps overly specific questions, with currently fashionable technology. Too little, on the other hand, tries to shed light on more fundamental questions. Plutchak (2005, p. 193) has gone so far as to wonder whether "the questions that are most important to librarianship are the kinds of questions that are amenable to the sort of rigorous investigation that EBL, it has seemed to me, calls for". EBLIP doctrine often emphasizes (Koufogiannakis & Crumley, 2002; Dickinson, 2005) that questions should be specific, but evidence for broader and deeper questions is at this point at least as important. Instead of the nth superficial empirical study on document retrieval, I'd like to see some real rigorously assembled and valid qualitative evidence on how we can better deal with the problem of *relevance* for the user – to name one possibility. Instead of another article on how we (allegedly) did it good in our information literacy program or on some possibility for improving reference service, how about some real evidence at last to guide us responsibly in much more fundamental questions around the classic dilemma of *information vs. instruction* (e.g. Rettig, 1993a,b)?

Measurement. The idea behind EBLIP is to take consciously evidence-informed decisions leading either to changes which represent an improvement for the user or to no change because the current procedure can be cogently argued to be the best feasible. The fifth step in the EBP-process is evaluation, and that implies *measurement* –

measurement of "outcomes". The only problem is that in our business, the outcomes are not directly accessible to us. Such is certainly the case in my sector, that of academic libraries – and arguably for all LIS organizations. Simply put, the foremost outcome of our performance, as facilitators, is the construction of meaning by the user. Our slogans claim that information is important, but information is *not* so important – meaning is important. (The Canadian "HEALNet evidence project" concluded also, with respect to measurement, "that evidence-based approaches must be equally concerned with meaning" [Upshur, VanDenKerkhof, & Goel, 2001].) One determinant of meaning is the information that the user *makes* out of the intellectual or affective content of those documents which we collect, preserve, offer and/or retrieve for her. We don't ourselves acquire or store or select or find *information*; we do facilitate communication over space and time. We can (and should do all we can to) make it easier and faster for the user to inform himself, and as a consequence further to construct meaning in his life and work.

How do we go about measuring this outcome? How do we conceptualize that fifth step in the EBLIP-process – much less operationalize it? *Indirect* measurement is by definition the best we can do. And the last thing we should do is to engage in a discourse which suggests that we can do more. What are the most robust forms of indirect measurement under what circumstances in (ultimately perhaps infinitely) different kinds of cases? To what extent can you quantify something that is by nature qualitative? What degrees of (probable) validity, reliability and applicability are you willing to accept? This is not going to be easy. After more than a century, we as a profession have almost no systematic and employable notion of how the contents that we preserve and provide are actually used. (Gabbay & le May [2004, p. 1016A], we should probably note, make a remarkably similar observation concerning "the ways in which clinicians derive and use their knowledge in practice, either collectively or individually". As do others, they also observe that clinicians seem in general to prefer informal sources and channels to formal ones, even to the ones prescribed by standard EBP.) Really effective evidence-based practice requires that we get our act together, or otherwise that we accept, admit and learn to live with our helplessness, and hope for the best.

Helplessness doesn't seem to me to be a realistic option, if we want (while resisting the tender trap of reductionism) even to make a decent start with EBLIP. After all, some grip on the nature of desirable outcomes is necessary already at the question-formulation stage. And let us remember what Urquhart (2004, p. 211) has written, namely that outcomes "are, essentially, how the users *use* library service outputs" [my emphasis], and that "performance of the service should be measured in terms of what matters to the users …, not what library staff think users should do". Knowing *that* our clients need the recorded knowledge we make available or organize for access is *one* thing, and relatively easy. But we're going to have to know *how* they need it. At this point, we hardly do – and we hardly *can*, given the limitations of our current ways of working and thinking. But fortunately there are at least some specific steps (e.g.: participatory research and ethnography) we can take to improve the situation, and to these I shall briefly return below.

Particularism. LIS approaches have traditionally failed to reckon with the *social* nature of the activities they claim to support. In the case of academic LIS, we can justifiably claim that the social dimensions of science and scholarship, and the degree to which knowledge is socially determined and validated (and meaning constructed), have played a far too limited role in the choice and adaptation of systems and services. EBLIP, with an orientation toward the individual taken over from the doctrine of "evidence-based health care" (EBHC), runs the same risk. This does not mean that we shouldn't consider the possibilities offered by single-subject research designs: if properly done, they can open windows on exactly the social contexts which are significant for LIS users.

Discourse-free. EBLIP has so far given little indication of sensitivity to the existence or ramifications of varying discourses and discourse communities in the world in which it operates. Even the variety of knowledge domains and domains of expertise which LIS necessarily serve seems not to represent a factor of significance in the standard EBLIP discourse or in most EBLIP-inspired research.

The beaten path. One of the most telling indictments of traditional library/information systems and services is the one impugning their ossified approach to knowledge

organization and intellectual activity. They are tied to simplistic notions of "aboutness" (Swift, Winn, & Bramer, 1978; Weinberg, 1988); they insufficiently recognize and support factors such as perspective, insight and inspiration as basis upon which persons seek and use documents (Intner, 2003); they are insufficiently supportive of creativity (Bawden, 1986). They are supply- and technology-driven, while they should be need- and use-driven. EBLIP seems so far not to have found this a problem.

Reflectiveness. That real, everyday evidence-determined practice may tend by nature to work against reflection-in-action is a possibility already mentioned above. The implicit legitimation in EBLIP of addressing the less profound questions, and the actual inclination so far to pursue EBLIP for such questions, seem to make it all the more advisable that habitual reflectiveness continue to be systematically and expressly encouraged in all practitioners. How can we best go about doing that? Booth (2003b) seems confident that this is what our future holds, and Partridge & Hallam (2006) seem convinced that it can be taught – but should we really be so sanguine? Booth later (2006b) pointed out the threat to reflectiveness of "technical-rationality", a characteristic which seems even now all too common. It appears to me also that the problem may sometimes lie a bit deeper: is our profession one that attracts naturally reflective persons in the first place? If not, possibly we should do something about that problem, along with an attempt to instill (if we can) a reflective disposition in those who already work in LIS. Upshur (2005) interestingly suggests that commitment to a strict evidence hierarchy in itself discourages reflectiveness.

4b. The formal/procedural problems and challenges

The EBHC inheritance. Koufogiannakis & Crumley (2006, p. 329) mention "health sciences librarians' collaboration with medical researchers in conducting biomedical systematic reviews", and state then that "A natural extension of these librarians' involvement in such research projects is that they began applying the same principles to their own profession." Then, as we are all aware, they took the next step of maintaining that these principles are, with the necessary minor adjustments, applicable to all areas of

LIS. And so we have, ten years and three international conferences later, the EBLIP movement as we know it today. The enthusiasm for the transferability of the EBHC (or, as it is often termed, EBM; I prefer to use here the broader term) "model" to LIS (Eldredge, 2000; Brice & Booth, 2004) was considerable, and is still alive. Eldredge (2000, p. 298) even wrote: "Finally, librarianship may now have a plausible strategic framework through EBL to catch up quickly to the rigorous levels of EBM." Others have questioned the transferability to LIS in general (Roddham, 2004; West, 2003), and some have even suggested that the EBHC connection should be played down in order not to prejudice acceptance of EBP in the LIS world at large (anonymous, in Brice et al., 2004; West, 2003). Interestingly, critics in other fields (e.g. education, management, psychotherapy) have likewise seriously questioned whether the EBHC model is an appropriate one for them. Oakley (2002, p. 281) suggests that all the social sciences should "revisit critically" this question. Most of the prominent EBLIP advocates are indeed firmly entrenched in the health-care environment, and give an impression not only of preferring to preach to the choir, but also of having an underdeveloped appreciation of librarianship as a distinct community of practice whose culture has little affinity with that of clinical medicine (see especially Eldredge, 2000). We note therefore with gratification the position taken by Partridge & Hallam (2005) that EBLIP has to coexist with the traditional "conceptual structures", "thinking processes" and "cultural dimensions" of our profession.

Most remarkable, however, is that these advocates seem unaware of – or at any rate choose not to refer to – the fact that there has been, almost from the beginning some fifteen years ago, an enormous and unremitting storm of (published) criticism and resistance to EBP within clinical medicine and the allied health services themselves. In the words of Greenhalgh (2002, p. 396), it "has drawn both passionate criticism and undisguised mirth". What points have been made by these critics? A selection:

1. There is still no good evidence that EBHC in fact leads to better health care (Cohen et al., 2004; Gupta, 2003; Porta, 2006; Tonelli, 2006; Upshur & Tracy, 2004; Weisz, 2005). Even its original advocates have conceded that such is the case (Norman, 1999; Weisz, 2005). For Jenicek (2006, p. RA249) it is "another more or less ... new

unsubstantiated belief". Gupta writes: "It cannot demonstrate empirically that it leads to improved patient health, therefore it has relied on an implicit epistemological and ethical basis for its justification. However, this justification has serious weaknesses." (2003, p. 119), and even asserts that it "may lead to worse rather than better health outcomes" (2003, p. 113). On that last point he is supported by Koch, Otarola, & Kirschbaum (2005) as well as McGuire (2005) and Walker & Jacobs (2002). (That EBP can lead to *worse* management is one of the points made by Learmonth & Harding [2006].)

2. EBHC is conceptually underdeveloped. Lipman (2006, p. 271) writes that its view of evidence "encompasses a very limited concept of research and science which cannot address complex or social phenomena .... This problem has long been recognized by proponents of EBM". Goldenberg (2006, p. 2622) speaks of "an antiquated understanding of evidence". Simplistic and naive are characterizations one also encounters; "reductionism" is a frequently recurring accusation (Branchereau, 2006; De Simone, 2006b; Maier, 2006; Upshur et al., 2001). Sehon & Stanley (2003) claim that it can't even define itself adequately. For Couto (1998, p. 274), it is "no more than rhetoric", and "ignores history and common sense"; for Miles, Bentley, Polychronis, Grey, & Price (1999, p. 97) it is "unscientific and anti-scientific".

3. EBHC is philosophically suspect. It is inductive rather than deductive, and therefore in fact irrational (Shahar, 1997; Tonelli, 2006; cf. Couto, 1998). Indeed, its rejection of rationalism is explicit (Tonelli & Callahan, 2001; Upshur, 2006), and the "purely research-derived conception of evidence that privileges randomized designs" leads to absurdity (Upshur, 2006). Its prioritization of empirical evidence is logically indefensible (Tonelli, 2006). The standard categorization of evidence it propagates is philosophically untenable (Gupta, 2006; Tonelli, 2006; Upshur, 2005).

4. EBHC's conception of evidence is fatally flawed because it systematically excludes "social structural influences and social, cultural, political and economic dimensions, despite their critically important role in determining health status and outcomes" (Lambert, 2006, p. 2642). The "individualist bias" and disinterest in social dimensions –

on the evidence-producing as well as on the evidence-application side – is a commonly heard point of objection (Berkwits, 1998; Lambert, 2006; Malterud, 2006). ("Individualist" must not be taken here to imply emphasis on, or a particular respect for, the patient's own perspective or preferences. Indeed, the movement has, Lambert [2006] tells us, been denounced as "dehumanising".) The unduly political aspects have been emphasized not only by Lambert but also by Holm (2005), Goldenberg (2006), and Barry (2006). May (2006, p. 529) writes "that what counts as evidence and its modes of production are always socially constructed in ways that are deeply embedded not only in general political contexts, but also within the strategic imperatives that drive the interactions of that politics with the local organisation of practice itself".

5. Even rigorous research producing EBHC's favored forms of evidence not only sometimes gives results which are of no use in clinical practice, but is often contaminated not only with numerous forms of "structured" bias but also with cultural bias (Gupta, 2003; De Vries & Lemmens, 2006). Such structurally biased evidence – particularly from randomized controlled trials (RCTs) – has been especially well described by De Vries & Lemmens. They also write (2006, p. 2704) of "biases built into the way researchers perceive the world" and claim "that the cultural assumptions of researchers, visible in existing in [sic] clinical practice, shape the gathering and interpretation of evidence". They are certainly not alone in pointing up the "over-emphasis" on RCTs, a matter to which I return below. Lambert, Gordon, & Bogdan-Lovis (2006) write of the possibility that evidence-based practice tends to promote established approaches over more novel ones, and Grossman (2004) of "controversies over statistical techniques which threaten the validity of the methods recommended by EBM".

6. A fundamental problem is the (very limited) extent to which evidence pertaining to a (non-naturalistic) trial group can appropriately be applied for the single "patient-at-hand" (Barry, 2006; Goldenberg, 2006; Lambert et al., 2006; Shahar, 1997; Tonelli & Callahan, 2001). Schum (2006, p. 7) reminds us of an old truism when he writes "that a statistical account of members of a group actually describe[s] the behavior of no person in the group of persons being studied". Often one encounters the observation, advanced

by both advocates and critics of EBHC, that an article of faith in the EBHC-campaign was that *variation* in clinical practice was a bad thing, and had to be reduced. Though variation *can* be an indication that something's amiss, and that available resources are being inefficiently used, it can just as well mean that the individual patient is receiving the best treatment for her particular circumstances. (Would we in LIS want to throw out the baby with the bathwater in this way, in deciding how we should serve our users?)

7. The EBHC-movement is inherently disingenuous: whatever the rhetoric of evidence and the discrediting of previous medical practice, even EBHC ultimately boils down to opinion (Gupta, 2003; Kulkarni, 2005; Shahar, 1997; Weisz, 2005): "a medical problem within the EBM research tradition still relies on majority, expert opinion to be considered, finally, 'solved'"; "in order for something (e.g. data) to be construed as 'evidence' it must be judged to be relevant and weighty with respect to a conclusion"; "evidence is a status conferred upon a fact, reflecting, at least in part, a subjective and social judgement that the fact increases the likelihood of a given conclusion being true"; "evidence is not ... simply research data or facts but series of interpretations that serve a variety of social and philosophical agendas". Greenhalgh & Hurwitz (1998, 1999) have pointed out that medical practice has always been largely a matter of ad hoc interpretation.

8. EBHC unjustifiably devalues and neglects forms of qualitative evidence (e.g. narrative, ethnographic) which can tell us things which quantitative evidence never can (Cohen et al., 2004; Greenhalgh, 2006; Greenhalgh & Hurwitz, 1999; Ingersoll, 2000; Lambert, 2006; Upshur & Tracy, 2004). Even narrative as therapy has many adherents, and the "narrative-based medicine" movement has derived its strength partly from reactions against EBHC. Greenhalgh (2002, p. 398) writes of "growing evidence that clinical knowledge is stored in our memory as stories rather than as structured collections of abstracted facts". The HEALNet evidence project (Upshur et al., 2001, p. 95) found that "approaches from the social sciences and humanities have equal standing as those of clinical epidemiology" and that "all forms of discourse" have evidentiary standing in health-care decision making.

9. EBHC is not so much about rigorous evidence-gathering or a sounder basis for clinical practice – it's about *authority* (albeit a different kind of authority than what it expressly set out to circumvent) (Couto, 1998; Denny, 1999; Lambert, 2006; Shahar, 1997; Traynor, 2000). Traynor (like some others) has emphasized its *disciplinary* force. He has also characterized it as evangelical. Buetow (2006) refers to its "unacceptably evangelical" style. Barry (2006) writes of its symbolic, but especially its rhetorical, character.

10. Starting already in its early years, the EBHC-movement has more and more come (or been compelled by the critiques) to acknowledge that rigorously obtained evidence is and can be only one determinant of high-quality health care practice (Buetow, 2006; Lambert, 2006; Norman, 1999; Upshur, 2006; Weisz, 2005). This concession has however led to what is at this moment held to be perhaps the chief challenge confronting EBHC proponents, the "integration problem" (Cohen et al., 2004; Greenhalgh, 2002; Gupta, 2006; Tonelli, 2006): if evidence is one thing, and the rest (experience, expertise, pathophysiologic rationale, intuition, setting, patient perspective and so on) is, while useful or necessary, of a different order, how can "evidence" and "non-evidence" preferably be combined into the best basis for practice? Just how, too, is the rigorous evidence to be integrated with those forms lower down in the hierarchy? EBHC seems not able, or willing, to take up this challenge.

11. Even the *gathering* of pertinent evidence is a problem that has been underestimated. Norman (1999) reminds us that, in the medical field, overlap rates even among expert literature searchers lie in the 10%-15% range. He also reports that the teaching of critical appraisal skills has little effect in the long run (see also Dobbie, Schneider, Anderson, & Littlefield, 2000; Upshur, 2005).

These are some of the major criticisms that have been leveled in the last ten to fifteen years against the original bastion of evidence-based practice, mostly by health-care professionals themselves. I have left other, more general, ones out of account – such as those addressing the ethical problems, and accusations such as that in essence EBHC is just a (deceptively and unobjectionably named) instrument for cost-containment or for

giving an impression of accountability. In any event, it seems that (even apart from the utopian, almost totally uncritical, representations in the EBLIP-literature) the actual impact of EBP in the medical world has been overestimated (Porta, 2004; Timmermans & Angell, 2001; Traynor, 2000), and that many clinicians simply prefer to ignore it or at least if possible to avoid actually implementing it (De Simone, 2006a; Gabbay & le May, 2004; Thorp, 2007). I have listed the above criticisms of EBHC here, at some length, because it seems to me that it would be irresponsible for any program that holds up EBHC as a role model, or considers itself to be operating in the same conceptual tradition, not to take note of them, and indeed to come up with explicit, well-thought-out responses. EBLIP has yet to begin this exercise. Indeed, it is still largely basing its ideal of EBP on what Miles et al. (1999, p. 97) have called the "absurd posture" of EBHC in its early years. Various commentators have declared evidence-based medicine to be now bankrupt. Miles, Polychronis, & Grey (2006, pp. 243-244) stated last year that "it is surely now ostentatiously clear that evidence-based clinical practice as defined by Sackettism ..., and good clinical medicine, cannot possibly be equated and remain in our view fundamentally irreconcilable", that EBM shows "no evidence whatsoever for a superior clinical effectiveness or patient satisfaction profile", and "an absolute lack of evidentiary basis"; EBMers have witnessed "the devastation of their creed". If we are serious about basing LIS practice on good evidence, we have a lot to learn from EBP implementation and its proponents in the health sector – but perhaps even more so from its critics.

Interdisciplinary awareness. Though EBLIP has grown out of EBHC, its advocates have long emphasized that the LIS "evidence base" was less well developed than the medical one, pleaded for strong efforts to improve it, and recognized the potential value, if not necessity, of looking also to other disciplines for this purpose. The disciplines most often named are education, business and management, and computer science.

In this regard, I would like to make only two observations. First, the research findings of both education and management may indeed often be of evidentiary use for LIS practice (more obviously, one might add, than those of the health-care sector). We must however at the same time be aware that also in these two fields there has been much

criticism of, and resistance to, the introduction of EBP. Reservations among management specialists have been particularly deep-seated. It behooves us not only to realize this, but furthermore to take the trouble of examining the issues and discussions and proposed solutions in those fields – not only now, but also as they develop in the years to come. Our own understanding, and our own professional practice, can only stand to profit from such an effort.

Secondly, extending one's gaze yet further afield will bring with it the potential for encountering new conceptions of evidence, of combined forms of evidence, and of their use, which can be of value also for LIS practice improvement. Psychology has been mentioned, and philosophy is also, in light of what I have stated above, an obvious possibility. But anthropology must decidedly also not be ignored. I would even go so far as to propose that we might well have something to learn from two perhaps unsuspected quarters, where one's very professional survival is largely dependent on one's success in the very strenuous search for, as well as the sophisticated combination and application of, various kinds of evidentiary warrant for claims made, and perhaps not less so in the mustering of elegant arguments for and from the evidence. I am referring here to archaeology and (classical) philology.

Valorizations of evidence. One of the standard indictments of EBHC is that it overvalues some kinds of evidence, especially evidence emanating from RCTs and meta-analyses, without duly acknowledging their inherent limitations and susceptibility to bias, while concurrently undervaluing, even denigrating, other kinds which in fact can be of great and sometimes of unique value. The former, preferred kinds of evidence are, the critics maintain, more subject to reliability en generalizability problems than usually admitted (Barry, 2006; Cohen et al., 2004; Koch et al., 2005; Shahar, 1997; Tonelli & Callahan, 2001; Upshur, 2005). Different RCTs, systematic reviews and meta-analyses addressing the same question can and do give (diametrically) conflicting results (Shahar, 1997; Upshur, 2005; Vineis, 2004; Weisz, 2005). Given results may be and have been interpreted quite differently by different experts (Goldenberg, 2006; Shahar, 1997). Miettinen (1998), in particular, lays out the serious reliability problems of systematic reviews and meta-analyses. Oakley (2002) points to striking variations in

review coverage. The traditional EBHC evidence hierarchy, it is asserted, can not stand up to rational analysis or normal standards of logic (see above, and Upshur & Tracy, who state [2004, p. 200] that "the entire edifice of evidence hierarchies is not based on systematic research at all, but on expert judgment or consensus. In other words, the warrant or justification for viewing evidence in such a hierarchical structure rests on the lowest form of evidence, that is, the beliefs of a few."). It is in any case difficult even in clinical terms to define the concept "effectiveness" (Barry, 2006; Goldenberg, 2006; Tonelli & Callahan, 2001; Vineis, 2004; Walker & Jacobs, 2002). The promotion of quantitative evidence may have less to do with a commitment to rigor than with ideology or with factors political, cultural or social in nature (see above).

It would be difficult to argue, it seems to me, that EBLIP – to the extent to which it has adopted the preferences, the evidence hierarchy, and the supporting rhetoric of mainstream EBHC – is not considerably even *more* vulnerable to this line of critique.

Add to this the point, already sufficiently made in the case of medical evidence, that even the most rigorously obtained quantitative research evidence *always* has to be interpreted – already in the discovery and development context, and even more obviously in the application context. Clyde (2006) provides an example of how drastically even experts can disagree in their evaluation of research reports. To this I would, myself, add that we must be extremely cautious in dealing with the factors *explanation* and *causation*. Our tendency in general is to be much too cavalier when thinking about and discussing such things – and LIS are an area where intellectual sloppiness in this respect is especially dangerous. We would de well to consider attentively what, for example, Latour and Callon (e.g.: their contributions in Woolgar, 1988, and in Pickering, 1992) have to say about this, as well as writers in our own field who work in the tradition of the actor-network theorists.

The major observation to be made here is perhaps however a very general one: namely, that various forms of qualitative evidence are probably inherently more productive for increasing our understanding of the significance of library and information services to the actual users of these, and *as a consequence* of the quality of our practice, than

quantitative evidence can ever be. This seems indeed more than obvious when we consider what I have posited above regarding the nature and measurability of the actual outcomes which what we do is ultimately all about. To the extent that *measurement* is difficult or inappropriate to service assessment and performance evaluation, to that extent is *hermeneutics* – and not infrequently even a "double hermeneutic" (Stauch, 1992, p. 347) – of inestimable worth. I see no reason whatsoever to believe that RCTs or meta-analyses yield more valid, reliable or applicable evidence – also for prediction and probably even for intervention – than for example well-conceived, -constructed, -executed and -reported ethnographic research: quite the contrary. It is precisely this last kind of research which I would prefer to see as a "gold standard". (It is interesting that also the well known reference specialist James Rettig (2003) has not so long ago pleaded for a more anthropological approach to be taken by librarians.) It has two other advantages which must not be underestimated: *doing it* not only gives us a much better opportunity to gain an appreciation of the tacit knowledge, discourse characteristics and cultural/social factors at work in the real world of our clients, it has the secondary virtue, I am personally convinced, of arousing in those clients more sympathy and respect for us and what we are trying to do.

While I tend myself to think that ethnography is the royal road to best evidence for better practice, I would be the last to deny that narrative (which can itself play a role in ethnography) is another possible candidate for that honor. Let us not forget what Greenhalgh & Hurwitz (1999) have written about narrative in the health-care world: namely, that "the lost tradition of narrative should be revived in the teaching and practice of medicine" (p. 48); and "the study of narrative offers a possibility of developing an understanding that cannot be arrived at by any other means" (p. 48); and further that narratives "allow for the construction of meaning" and "encourage reflection" (p. 49). If this is important in the clinical context, how much the more so in our own? Twining, reporting on discussions on evidence among scholars from various fields, tells us that "all agreed that problems of evidence and inference could not be kept separate from questions about interpretation and narrative" (2003, p. 8) and that "there appears to be very wide agreement among scholars and practitioners that narrative is of central importance in fact-determination" (2003, p. 12). (This comes interestingly close

to Rorty's [2000] notion of the fundamental significance of "conversation".) And with narrative comes of course also rhetoric. (See the reference above to Scallen's rhetorical view of evidence, and her quotation [2003, p. 112], from *The rhetoric of the human sciences*, "how reason is rhetorical and how recognizing that fact should alter research".) Here one may begin to understand better why I have above suggested that one might be well-advised to look to philology for possible inspiration and enlightenment on dealing with evidence.

And what we can say of narrative, we can also say of its nephew the anecdote. But anecdote has, too, its unique qualities. It has served as a kind of EBHC whipping-boy, and been held up as one of the villains of the bad old days. But Rodrigues (2000, p. 1346) has (in addition to speaking with favor of anthropological evidence in health care) stated that "anecdotal evidence may be found to be the most decisive factor in the selection of a course of action". Gabbay & le May (2004) have pointed out how much even EBP-aware clinicians value "anecdotes with a purpose": not surprisingly, if Greenhalgh & Hurwitz (1999, p. 49, citing Schmidt and others) are right that "anecdotes … may be the underlying form in which we accumulate our medical knowledge". Amrine et al. (1996, pp. 25-26) discuss the special place of anecdote as evidence, including importantly its "transgressive" and subversive nature. Orgel states there that "the anecdote is the most circular and *self-reflexive* kind of evidence" (my italics). There furthermore exists, at least in Europe, direct practical experience with the systematic use of anecdote as an effective form of evidence in evaluation of the services of special libraries and in gaining institutional support for them (!) (B. Hendriks, personal communication, 21 February 2007).

One wonders then quite sincerely how it could be the case that qualitative evidence of the above-mentioned and other sorts can have gotten such a bad rap from EBLIP. Ford et al. (1999) tell us that some disciplines tend "to employ notions of 'evidence' that entail the analysis of subjective interpretations and experiences". They are referring to the whole of the humanities – but the same assessment can be applied almost as easily, it would seem, to the social sciences. Barry (2006) makes a very similar observation about evidence in anthropology. (To "interpretations and experiences" above I would

want to add "perceptions".) Lambert (2006) points out that both critics and performers of qualitative research have imposed totally inappropriate methodological criteria on the appraisal of qualitative research. Just how difficult it may be to arrive at consensus on *appropriate* criteria we can suspect from data offered by Oakley (2002).

Given (2006, p. 384) writes concerning LIS that "it would be ideal to put the controversies to rest regarding the value of qualitative evidence", but it all seems slightly more complicated than that. We need not just an armistice, we need rather a new mentality and a new rhetoric, preferably a non-scientistic and more non-positivist rhetoric which better fits the time we live in. We need this not in the last place because the quantitative methods (still) cherished by many and touted as supreme, have after several decennia shown themselves unable to bring us to the point where how we do things, and verily *what things* we do, correspond sufficiently and gratifyingly (for us *or* for the users) to how intellectual work and communication are carried on in the real world. How long shall we, like medicine, remain unable to produce good evidence that EBP in truth results in better practice as evaluated against outcomes? And we, like medicine, have a real "integration problem" yet to be attacked. Quantitative evidence certainly has its virtues and its place (I would tend to think a secondary and supporting role, rather than the other way around) – but how do we make qualitative and quantitative evidence work well together for LIS?

Question choice. Booth (2002b, p. 57) has referred to the widely endorsed opinion "that research in our profession tends to be more opportunistic than strategic". I could not agree more with this assessment, and have no difficulty in qualifying it here as a significant problem and challenge for EBLIP. Indeed, "opportunism" would seem almost by definition to have little place in an evidence-based culture, certainly not in one of the sort I would most like to see. Opportunistic questions are usually surface questions, questions about technical and logistic matters which presently attract attention (and about doing things that are "influential in a specific situation", as Clayton, quoted by Genoni, Haddow, & Ritchie [2004, p. 53], nicely puts it), but in another five or ten years may be (in their formulation or even in their substance) no longer relevant. But perhaps more importantly, answering logistic and technical

questions evidentiarily well in most cases means – or should mean – answering them not on their own isolated grounds but on grounds of the service category to which they contribute or which they support – if not of one's practice as a whole. The choice and validity of the evidence serving to mandate a certain answer to the technical or logistic question can then stand or fall with those of the evidence warranting the current constitution of the service category as a whole.

In this way we can see therefore that in an evidence-based environment, paradoxically perhaps, the less opportune a research question is, the more urgent it may be that it be addressed. If, that is, we want to avoid the effort involved in finding the best evidence to support selected aspects of our practice which may not deserve being, and may not long remain, aspects of our practice. There is much to be said for giving concerted attention first to the larger questions (so that we can then better know just what smaller-scale questions ought to be addressed, how and in what order). These larger questions – in contrast to the opportunistic ones – are ones whose answers affect the longer-term future of LIS, and often ones that were already being asked forty or more years ago. That they have yet to find good answers, and answers that have resulted in implementing changes, indeed suggests that the research they require has never been opportunistic. But in my view EBLIP will now have to come up with some good answers and some robust implementations if it wants to make a mark, and win the respect of the outside world.

Question type. The two preceding paragraphs suggest that the terminology "foreground" as opposed to "background" in typifying LIS questions, taken by Booth (2001) from the medical context and used to refer to another, what is in fact a universally recognized, kind of distinction (that between what elsewhere might be termed "closed" as opposed to "open" questions), could possibly lead to some confusion. Booth quite rightly accords "foreground" and "background" questions equal priority and status – and that seems all the more reason to keep this contrast clearly separate from that between what on the one hand is of immediate, local and often passing concern, and what on the other hand is more inherent, more fundamental and of broader professional concern (and not to lose sight of the fact that the former often ought to be contingent on

the latter). Booth himself elsewhere gave expression to just this second kind of contrast, writing (2006b, p. 365) of the danger that lists of recommended EBLIP research questions "are dominated by questions around new technologies and interventions", i.e. "what we know we don't know", to the detriment of questions more likely to reveal "what we don't know we don't know", or, as he even more powerfully puts it, research on "things that we do every day without questioning our procedures and practice". He had earlier (Booth, 2004b, p. 67) referred to questions "that are more longstanding and fundamental to our practice" as opposed to "more 'glamorous'" ones. Research questions in either case may appropriately be either of the background or of the foreground variety. Perhaps we could properly speak of question *depth* and of question *breadth* – with all four combinations being legitimate.

But this potential ambiguity in classifying research questions for evidence-based practice leads us to ask, and to answer affirmatively, whether other typological suggestions for EBLIP designs might not have been too hastily propagated. Such suggestions include that (Crumley & Koufogiannakis, 2002; Booth, 2003a) for "major areas under which questions can be grouped", also called "librarianship domains" (too artificial, inconsistent, incomplete, overlapping, system- and supply-oriented instead of user-, discipline- or demand-oriented?), that (Eldredge, 2006) for the "Prediction"/ "Intervention"/ "Exploration" categorization with accompanying "levels of evidence" (too oversimplified, over-positivist, EBHC-oriented?), and that (Eldredge, 2002) for the "what" "how" and "why" categorization (too contrived, simplistic, incomplete?). I would not want to deny that *a priori* distinctions among types of EBLIP questions, or of EBLIP research, might to some extent be useful, but these will have to be better thought out and less precoordinate, with realistic contexts and *users'* perspectives in mind. Simply setting oneself the goal of "constructing a comprehensive typology of question types" (Booth, 2004b) or, even more so, helping librarians to "quickly identify the best research designs for answering discrete classes of questions" (Eldredge, 2002) has something patronizing about it, and may ultimately do more to undermine than to stimulate effective evidence-based LIS practice.

Dealing with uncertainty. Applying evidence ideally results in minimalization of uncertainty. Such a goal is not yet anywhere near achievement. But in an environment in which trusted evidence is building up and being put to use, how is one pragmatically to deal with the service aspects where it is and may long remain insufficient? Upshur (2006, p. 287) terms uncertainty "the most pressing issue not addressed by EBM". In the management field, Learmonth & Harding (2006, p. 252) go much further and even surmise that "a search for 'evidence'" in some areas "would introduce more ambiguity and uncertainty, thereby seriously reducing (rather than promoting) the possibilities for rationality enhancing decision-making". What about EBLIP, then? Booth is to be commended for explicitly recognizing this kind of challenge facing us, and for emphasizing the importance of "acquiring strategies for handling uncertainty", of "different ways of acknowledging our ignorance and techniques for making decisions when faced with limited information" (2006e, p. 52). For the LIS professional, we can predict that this will be especially difficult. At any rate, we should always keep in mind what Weick (2001, p. S73) has observed: "Given all the hype suggesting that information is good and more is better, one can well imagine that many will reject this so that they can preserve the simpler picture they need to remain in action."

4c. The incidental problems

Linguistic and cultural limitations. It has often enough been claimed that EBLIP will have to grow beyond its birthground in the developed English-speaking world. Brice & Booth (2004) speak of "a global initiative". After ten years, that still doesn't seem to want to happen. How can we lend an effective helping hand? It is obvious that much LIS research evidence is published in other languages, and that such evidence is not necessarily less valid because it is reported in a language other than English. It virtually never appears also in translated versions. Systematic reviews limited to one language are almost by definition invalid. Other disciplines routinely operate in that realization, and would find such a monolingual orientation an intolerable shortcoming.

Publication overload. The literature of LIS is already huge. Yet, the usable published "evidence base" is underdeveloped. The good EBLIP practitioner is at the same time a researcher and writer. There is much for her still to do. Can we handle all those added publications? Do we really want them? Can we somehow effectively filter them for the various purposes at hand? How many of us are sufficiently accomplished (re)searchers and writers?

Biases. EBHC has made clear that eliminating structural and other biases even in the most rigorous research is surely an idle dream. It is hardly likely that LIS research can do any better – probably it can not do nearly as well. The extent of existing 'technical bias' should be apparent from the discussion under *Question choice* and *Question type* above. 'Publication bias' is likewise a significant problem (Eldredge, 2000). The 'reporting bias' emphasized by Nicholson (2006), together with his critique of employing aggregated surrogates, seems little short of devastating for our confidence in traditional quantitative LIS research (and indeed a good additional argument, in disguise, for preferring qualitative evidence, given the practical problems attaching to his quantitative alternative of "bibliomining"). He is also almost unique in his readiness to broach the subject of 'affective' biases (possessiveness, insecurity, fear, neglectfulness, predilection) in LIS research. 'Language bias' and 'cultural bias' we have also touched upon; related to these is 'availability bias'. 'Source-of-funding bias' may be less prominent than in EBHC, but does exist. 'Familiarity bias' has been adequately presented by other EBLIP commentators.

We have to find a realistic way of living with bias as a fact of life – of recognizing what kinds of bias exist, and taking them soberly into account. (Qualitative researchers already have a better track record, partly perhaps because they've learned to be less attracted to the siren-song of "objectivity". Cayley [2003] even claims that they can play a positive role "in the application of evidence".) They are not going to go away, and pretending they don't exist or can be disabled does more harm than good.

Non-users. How valid is the evidence we gather and apply for deciding in what way to modify (improve!) services, if it is based on research which has not taken sufficient

account of non-users? This is of course a form of ‘selection bias’ – but important enough to deserve separate mention. Non-users can be disappointed or frustrated former users, they can be potential users, they may be non-aware users, they may even be satisfied and determined non-users (the classic example being the eminent sociologist Charles B. Perrow [Perrow, 1989]). But research designed to take them honestly into account will almost always yield evidence of unique value.

5. WHAT CAN WE DO?

Anyone venturing to draw attention to such a variety of options, difficulties, problems and challenges which in his opinion still confront our field in making the transition to a viable evidence-based practice, can hardly be taken aback if then asked how we might be able successfully to confront them. My response would in part be that an appropriate course of action is often to a greater or lesser degree implicit, I would at least hope, in the way the options and problems have been formulated. In part I would have to concede that I have no ready proposals on offer. An effective way forward can in many cases only be worked out, as already suggested, via a continuing dialectic among, and in coordinated initiatives by, the various parties involved – and with sufficient sensitivity to the points I have raised. The space remaining here does perhaps nonetheless allow a few suggestions of a rather general nature.

Stuart McCook (1996, p. 197) concludes his essay on nineteenth-century biological field sciences with the sentence: “Anybody could discover truth, but as far as scientists were concerned, only scientifically trained people could turn that truth into evidence.” Heather Dubrow (1996, p. 16) refers to “how often performance functions as an alternative to the presentation of evidence”. She was referring to questions of peer assessment in literary scholarship, but her observation, taken together with that of McCook, seems to me to say a good deal about the situation in which LIS professionals have long found themselves – whether by choice or not. We’ve got our act pretty well together, and have been able to convince our audience most of the time. Our

accustomed performance would for most of us seem to be quite sufficient, for many of us it may amount to a source of pride, and indeed such an accomplished continuing performance could easily be felt to constitute an alternative to the tedious marshalling and presentation of evidence in support of what we precisely do. Yet the world around us goes as always its own way, makes its choices, adjusts and validates them with no regard for what we might think or prefer. That world – whose doings it is often our purpose to facilitate – thus finds or makes its own truths. But as far as what we do is concerned, that those truths may exist is not enough. It is up to us ourselves to discover and to acknowledge them, and then, like McCook's authorized biologists, to transform them from truth into evidence. Nobody else can do it for us. And of course: we have to *want* to do it.

With such considerations in mind, I would like to suggest that LIS professionals, certainly in a context of evidence-based practice, can come closer to dealing judiciously with the options, to handling the difficulties, to solving some of the problems and to meeting many of the challenges as I have tried to sketch them, by:

Looking genealogically at our profession. How did we get to where we now are, what were the determinants of such a process, and what are the implications? Genealogy is meant here in the specialist sense deriving from Nietzsche and Foucault, as "a patient tracing of the descent of authoritative discursive practices that structure the application of power" (Pryor, 2006, p. 700). As Malterud (2006, p. 293), also referring to Foucault, writes of medicine, "Understanding how medicine is institutionalized can help us recognize issues on evidence validation which are not obviously apparent." We find a similar observation on evidence-based management in Learmonth & Harding (2006, p. 246). A conscientious "genealogy" opens to view and allows one to examine, as well, the illusions and pretensions upon which a practice thrives and which have played a role in determining the rhetoric it employs; systematic *déformations professionnelles* become easily comprehensible. It is the only route to solving the stubborn 'thirdspace' problem described above, aside from its paving the way to eliminating various of the other obstacles on the EBLIP journey.

Not ignoring the evidence already available. A corollary is: not considering as evidence only what does not conflict with our own basic assumptions. As Marshall (2003, p. 41) puts it: genuinely "making our professional decisions and basing our actions on the strongest evidence available as to what would work best for our clients". Booth (2002a, p. 119) has suggested that the term "evidence-based librarianship" can better be abandoned as soon as possible, and in any case once the contrast with "*non*-evidence-based librarianship" is no longer useful. That day appears still a long way off (Eldredge [2002] puts it in 2020). In the same publication he refers to the realization "that the emperor has few, if any, clothes" (2002a, p. 116), elsewhere (Booth, 2004b, p. 67) to his judgement that "as librarians are poor at utilising and exploiting their own professional literature they may prioritize questions already answered", and he and Brice (Brice & Booth, 2004, p. 33) write of "existing, under-utilised research that highlights ineffective practices that consume large amounts of information professsionals' time and resources". How committed are we, then, to turning Marshall's fine words into reality? Are we just not aware of what evidence there is, or are we not inclined to bother? Partridge & Hallam (2005) regret that we don't have a sufficiently "clear understanding of the skills, knowledge and attitudes required for evidence based practice within the profession". But experiences in other fields have suggested that evidence may indeed remain deliberately unselected or ignored, if it doesn't suit our purposes or convenience or disposition. So it is in biology and physics (Lewontin, 1991, p. 145): "totally impervious to evidence", "*evidence* ceases to exist" (his italics) in the face of dogma. So too in clinical medicine (De Simone, 2006a; Gabbay & le May, 2004; Pope, 2003; Thorp, 2007; Upshur et al., 2001; Walker & Jacobs, 2002), in speech-language pathology (Ratner, 2006), and even in literary studies (Amrine et al., 1996). How about LIS practitioners? According to Booth (2002b, p. 56), librarians "are very unlikely" to search for evidence to support their own practice. The "prevailing culture of librarianship" perpetuates the "research-practice gap" (Booth, 2003a, p. 4). Librarians "find it more challenging than they might expect to formulate and refine their own questions" (Eldredge, 2006, p. 344).

One might be excused for suggesting that the problem lies deeper than difficulty in formulating questions or inclination to search. Many LIS professionals *do* follow the

literature and/or communicate with others who do so. There has accumulated over the decades in that literature, and in the literature of cognate fields, strong evidence that many of the assumptions and principles upon which we continue to base our systems and services and procedures are simply mistaken. The thrust of that evidence is consistent and clear – but it has had little or no effect on our practice. A few examples:

▪ The skills required for, and the methods actually used in, doing high-quality and successful research have little in common with what librarians consider to be "research skills", or what textbooks or manuals prescribe. This has been known for at least a hundred years, and been repeatedly emphasized in the LIS and other literatures, from physics through the social sciences to the humanities (Bouty, 1908; Gaddis, 2002; Kaplan, 1964; MacGrath, Martin, & Kulka, 1982; Stoan, 1984). Rationality is not a predominant factor in creative intellectual work. Such appears, incidentally, even in clinical medicine to be the case: as Greenhalgh (2002, p. 398) puts it, "the more experienced a clinician gets, the less logical their decision-making processes are shown to be". Furthermore, Barry (2006, p. 2653) points out that "what people say about their behaviour" (an idealization) can be quite at variance with "what they do".

▪ When researchers are asked to prioritize the information sources they value and use most, it is the unsystematic and informal ones that score highest, the ones we promote score much lower (these tend to be used by specialists mostly for questions outside, or on the periphery, of their own fields), and "consulting a librarian" comes almost invariably at the bottom of the list, in one case as the thirteenth-ranked option (Frost, 1987; Kotter, 1999; Martyn, 1987; Stieg, 1981; Talja, 2002; Tibbo, 1993; Uytterschaut, 1966; Voorbij, 1999).

▪ The notion of relevance built into our systems and our manner of assisting and instructing users – based on simplistic matching and ranking routines – is only a primitive and partial approximation of what determines relevance, much less pertinence and utility, for an actual user. (Bookstein, 1979; Budd, 2004; Harter, 1992; Park, 1993; Saracevic, 1976; Wilson, 1973). It has even been pointed out that our outlook and way of doing things amounts more to a hindrance than to a benefit to the work of our users (Swift et al., 1978; Swift, Winn, & Bramer, 1979; cf. Logsdon, 1970 and Stielow & Tibbo, 1988).

▪ It was explained very clearly more than forty years ago (Swanson, 1964) how our online catalogues – in fact, our document retrieval systems in general – could best be conceived and constructed to serve well our clients (instead of ourselves) on their own terms, and in fact to take continuing advantage of those clients' input in enriching the capabilities of such instruments. The LIS community did virtually nothing with these insights, though they were technically realizable to an important extent. A few attempts to revive them some thirty years later (e.g., Kantor, 1993; King et al., 1994) seem totally to have fizzled out. They are since that time being graduately implemented (and warmly received), but thanks to initiatives from outside our profession (in computer science, particularly) and often in ways that circumvent LIS practitioners.

These are not novel observations – far from it. But the measure wherein they have a familiar ring is precisely the measure wherein we must consider it all the more noteworthy that practitioners seem to have been unprepared to take them seriously. Can EBLIP change this "prevailing culture"? Høivik (2003, p. 1), who states that "librarians know the evidence, but they do not act on it", finds that "a change within the professional identity itself" is required.

How badly wrong it can go was demonstrated in the case of personalization (MyLibrary-type systems). We thought we had good evidence – and indeed our users when asked said they would welcome a personalized approach. The only trouble was that what *they* had in mind was apparently much different to what *we* had in mind, and what we were in fact in a position to offer (based largely on our systems-oriented mentality and on misunderstandings like those listed above). The result was a costly failure, and disappointment all round (Hunsucker, 2005a). The future of personalization for information-seekers and -users *does* appear to have a bright future, but presumably not one in which many LIS professionals will be involved (Hunsucker, 2005b).

Tempering presentism. Busy practitioners in particular may understandably be inclined to perceive that the practical difficulties with which they today have to contend, problems attaching to the current state of their systems and technologies, the inadequacies of current services and possibilities for new services, are the principal, if

not only, matters justifying the effort and resources required for accumulating, appraising, interpreting and applying evidence. They may furthermore – perhaps again understandably – tend to regard both such difficulties as these and the more encompassing, fundamental or even existential questions that they may see themselves confronting, as ones characteristic of, maybe even peculiar to, this day and age. And the evidence relevant to these difficulties and questions must therefore also necessarily be roughly contemporary – if in fact it even yet exists. Research questions, and the potential evidence on which we can base our decisions concerning them, if they are to be seen as relevant, must be conceptually coextensive with our working context as currently constituted.

But this fallacy to which the LIS practitioner may readily fall prey, what we can call "presentism", not seldom afflicts the LIS researcher as well. To a very considerable degree, the essence of what our profession does and stands for has changed little since the days long before most of our own careers began, and will persevere indefinitely into the days of our successors – whatever tools or approaches we may at present happen to be employing. In preparing this paper, I went out of my way to find and to read as much as I could lay my hands on of what has been written about EBLIP and in the EBLIP spirit. Serious, thoughtful and penetrating published research on, and visions of, library and information practice have been appearing in a fulsome and growing stream certainly since the later 1950s. Some of the most insightful, stimulating and useful contributions to our literature of which I am aware are from the sixties through the earlier eighties. The EBLIP literature known to me appears blissfully unconcerned with all of this, if not indeed unaware of it. It is as though some of the greatest thinkers and writers our field has known in fact never existed: Jesse Shera, Patrick Wilson, Maurice Line, B.C. Brookes, Robert Taylor, Don Swanson and others. (One of the rare examples I have noted is in Booth, 2004a, where Brookes is indirectly cited, but then interestingly in a manner explicitly pointing up not the advantages but one serious difficulty of EBLIP implementation.) Even the relevant work of more recent leading researchers and theorists – Tefko Saracevic, for instance, Marcia Bates, Michael Buckland, Brenda Dervin, Gernot Wersig, John Budd, Christine Borgman, Ross Atkinson, Donald Case, Sanna Talja, David Ellis, to name just a few whose work I have found quite useful – is

rarely encountered. I found only one, extremely passing, textual allusion to such an important instrument as the *Annual review of information science and technology*. Major research journals such as *JASIS(T)*, *IP&M*, *L&ISR*, *JDoc*, *JIS*, *LQ*, are cited far less than one might expect.

An evidence-based practice not steeped in the long tradition of LIS reflection and scholarship, not seriously engaged with the cumulated research experience, and not attending to questions which fifteen, twenty years from now will also be relevant to our work (some of which have been long ago formulated and not yet satisfactorily investigated) – such a practice can hardly be expected to make a significant, not to say a durable, contribution to our profession's improvement. And it is not likely to earn for us the respect of those in other professional and academic areas.

Selecting theoretical frameworks within which to identify, order and evaluate evidence. The work which evidence does within scientifically organized activities is that of offering support for hypotheses with respect to theories (Glymour, 1980). In this way theories are either confirmed (or if you prefer, shown again to resist falsification), indicated to be in need of adjustment, or in the extreme case demonstrated to be untenable and therefore in need of replacement. Evidence is not something "out there" that can be simply encountered or discovered; it *is* evidence by virtue of its function. That function is one of persuading me and others either to persist in or to adapt the way we view (a circumscribed part of) the world of our perception and experience. Theories are ways of viewing; they make it possible for us to agree upon a *provisional* manner of understanding processes and situations. They allow us, *consequently*, to determine what counts as evidence and what does not. They permit us to delineate advancement and refinement in the way we conceive of processes and situations, as well as in how we deal with (anticipate, explain, control, react to) them.

It is probably no exaggeration to say that without theory there can be no evidence (only signals, observations etc.): the word becomes vacuous. It is entirely common that even within the natural sciences the same observations, the same data, are in differing ways made into evidence by the adherents of differing theories (Glymour, 1980). This

possibility makes it all the more advisable, it appears to me, that implementers of EBLIP always make clear – to themselves and to anyone else concerned – within what theoretical framework they are operating. It has in the last ten years or so become fashionable in LIS research to speak of "metatheories", and indeed the judicious application of this concept could well enrich our approach to EBLIP. (Marcia Bates gave recently a handy list of possible metatheories for LIS [Bates, 2005, pp. 10-14].) The predominant metatheoretical choice even in this day and age, and one certainly not absent in EBLIP environments, is what Bates (2005) terms the "engineering approach". My own tendency is to a (by the way, reflexive) "constructionist approach" – and within that to a theoretical framework drawing on the "paradox of the active user" (Carroll & Rosson, 1987), as well as on "optimal foraging theory" (Sandstrom, 1994) and on the principle of "satisficing" (Simon, 1997). Others will surely have different preferences, but as Couto (1998, p. 268) very simply puts it, "Practice, in any discipline, cannot be isolated from theory."

Involving users in identifying, appraising and applying evidence, and in evaluating results. EBLIP-enthusiasts have for some eight or more years now been trying to agree with each other, or to persuade each other, what kind of a thing "best evidence" is – as well as how best to get hold of it, to appraise it and to apply it. Some found it fairly easy: orthodox EBHC had already figured it out (conveniently ignoring the multifarious and widely supported arguments against normative EBP within the world of clinical practice itself). Others were not so sure. A few expressly disagreed. It seems to have occurred to almost no one to inquire: "What do our users think about this?" Some believers in EBLIP would probably agree that yes, solliciting user input might be interesting or even useful. I can not entirely agree with that attitude. My own feeling is that without the participation of LIS users, but also of LIS non-users, in the discussion and determination of what constitutes best evidence for making decisions about LIS practice, and what kind of research is good research, and how such evidence ought to be put to work – *without* such participation, it is probably not actually worth the effort of pursuing something called evidence-based practice for LIS. And if Jenicek (2006, p. RA246) is right that "the best evidence is only meaningful if used in proper argumentation", and that (p. RA247) "'Evidence-based argumentation' … may be the

way to go.", can we be optimistic that we can under our *own* power alone make a meaningful success of EBP at all?

I would in fact want to go a step further, and propose that we would be even better off if we were, as a matter of course, to involve our users on an equal footing in designing and carrying out the EBLIP-research that we do. "Participatory research" has found only limited, and I suspect reluctant, application in our field (Penzhorn, 2002). This is terribly unfortunate. The EBLIP "journey" seems to me a golden opportunity to rectify this fundamental shortcoming. We can even take some encouragement and inspiration from the health-care sector, where diverse voices have been raised in pleading for a more meaningful involvement of the patient in the whole evidence-based exercise (Grypdonck, 2006; Holm, 2005; Upshur, 2005; Walker & Jacobs, 2002; Widder, 2004). There are even added advantages to be reaped: participatory research can increase the overall available research capacity, enhance the possibilities for appropriate funding, potentially mitigate the much-lamented "research/practice gap", and – not to forget – virtually ensure that one is not violating Schutz's (1982, p. 44) "postulate of adequacy" for research procedure and models (simply put: the users should be able to understand what we're saying about them).

Taking qualitative evidence more seriously. I would think that it requires no well-wrought argument to bring us to the realization that, since the outcomes at which our performance is aimed are only indirectly accessible to, and only vaguely measurable by, us, full user participation in the whole EBLIP process is no less than natural. To me, the notion is hardly less than intuitive. (I grant that this may be less surprising in the case of a former non-LIS university teacher and researcher.) Nonetheless, even enlisting LIS users (*and* present non-users) as full EBLIP participants is not going to be enough.

Constructing evidence is a propositional matter. The EBLIP exercise depends on developing and employing *propositional* knowledge. But what our users as professionals and as experts do – what *makes* them experts or scholars or scientists – is to a considerable degree *nonpropositional* knowledge (you might call it *know-whether*, *know-when* and *know-how* instead of *know-that*). And they can not themselves tell you,

even if they want to, how it works (Bouty, 1908; Greenhalgh, 2002; Kaplan, 1964). But how it works is vitally important, and how it works in its native social, cultural and intersubjective environment (where meaning and knowledge are constructed and/or validated) is surely more important than anything else – certainly for those whose professional aim (yes, *raison d'être*) it is to facilitate it. How-it-works research is the most important research we can do. Fortunately, we do have instruments at our disposal for this kind of research. Such research, it almost goes without saying, if it is to give us the best possible evidence and therefore to lead to best practice, should be as naturalistic as possible. The instruments that can do the best job would seem to be ethnography, discourse analysis and narrative studies. That they are "qualitative" methods in no way pleads against them. It would be strange if it should be taken to do so, when we consider that we as LIS professionals are dealing most importantly with qualitative processes and qualitative goals.

Seeking out robust and realistic approaches to orchestrating the introduction and maintenance of the EBLIP processes. There has been much talk in the EBLIP literature of creating "a culture of evidence-based practice". Less attention has been devoted to the question of implementing, and thereafter maintaining, a consistent (and, one hopes, unrelenting) *practice* of evidence-based practice. We need good, pragmatic answers to questions such as: How do you gain commitment in your LIS organization, and keep that commitment alive? Where do you *start* the EBLIP process? How do you apportion responsibility? Should you have an overall EBLIP coordinator? How much can you reasonably do simultaneously, and otherwise what's the right sequence? How do you organize the quality control? The answers to these questions would have to precede the launching of any serious EBLIP implementation trajectory, it seems to me.

This is all on the organizational level. You could perhaps say that that's the easy part. No organization is an island, and none can become EBLIP-compliant using exclusively its own resources. The problem lies especially with evidence-gathering, but even more with evidence-generation, as others have quite properly pointed out. Let us not pretend that these are not, or can be made other than, contingent, situated, subjective, values-influenced activities. There's no way around that – except to recognize it and to try to

allow for it. But they are nonetheless collectively far too demanding for one organization or even a consortium to take on. The trick is of course to organize all of this on a grander (international?) scale, a very neat trick indeed if you can pull it off. Not so much a neat trick because it's hard to do, but because it's – apparently – hard to *decide* to do. That there's so little actually happening seems to be leading to frustration on the part of some dedicated EBLIPers. That's what I sense, for example, in a recent piece by Booth (2006d); if so, frustration has, as sometimes happens, engendered an excellent practical idea. His "ReSolutions" proposal is very attractive, precisely because of the "living organic" and "dynamic" quality and the mixed character of contributions allowed. Perhaps some coordination with the "evidence summaries" project of the electronic journal *Evidence based library and information practice* would be workable (see also West, 2003). Other ideas for cooperative activity that have been advanced, for instance in the health-care context, should possibly also be taken into account: e.g., those of Gabbay & le May (2004) for collectively validated "mindlines" and networking, and that reported by Mykhalovskiy (2003) for recontextualized narrative messages.

I am myself optimistic that, given sufficient collective will within the profession (and that is the real problem), such successful supra-organizational cooperation can yield quite interesting results within a reasonable term. There is already a lot of evidence out there which isn't yet being used (see above on not ignoring evidence and on presentism). We certainly don't, in many LIS areas, have to start from scratch – at least where evidence is concerned, certainly if we free ourselves of the constricted original notions of evidence rigor which EBLIP took over from the medical sector (where they have since been largely abandonned in the wake of widespread criticism). In one respect, though, we would be well advised to start from scratch, in the spirit of Echelman's "zero-based evaluations" mentioned above – if we want to increase the possibility of a successful transition from the current positivistic rhetoric to the actual *practice* of evidence-based practice. That is the message of the following section.

Swearing off the illusion that users have more to learn from us than we have from them. At the beginning of this essay, I wrote about a "credo" of our profession as I see it. I

tried there to use language that was as non-confrontational as possible. Yet that "conviction" which I mentioned regarding our self-conceived potential as benefactors is perhaps not quite so innocuous as it might seem. If it goes to our heads, then we may lose contact with what's really going on around us. Has it gone to our heads?

In the LIS literature in general, and more surprisingly in that of EBLIP, we come with some frequency across observations like "librarians are experts in refining reference questions for their users and matching these questions to the appropriate sources for finding needed answers" (Eldredge, 2002, p. 74), "library and information professionals are uniquely placed 'to model the principles of evidence based practice, not only as they apply to other disciplines which we serve, ... '" (Partridge & Hallam, 2006, p. 405, quoting Ritchie), "the skills of the information worker had become recognized as pivotal to the conduct of practical and useful research" (Booth & Brice, 2004b, p. 6), "given domain expertise in understanding user information needs, in the generation and dissemination of scholarly communications, and in the management of information and the extraction of knowledge from data, librarians are well suited to partner in the integration of advanced information management technologies into the clinical enterprise", (Perry & Kronenfeld, 2005, p. 12), "librarians … frequently work with researchers and their publications and hence understand the nature of research and how it is used" (Marshall, 2003, p. 41). This kind of utopian representation is normally presented without any supporting evidence, rigorous or otherwise. Indeed, all the evidence of which I am aware points in the other direction. But however that may be, even if it's only remotely true, it can't mean much if others – namely our clients – not only recognize that it is, but, more importantly, act accordingly. That would mean a fairly rosy prognosis for our profession, and imply the conclusion that EBLIP's future is pretty well assured. Is that the case?

6. OUTLOOK AND CONCLUSION

For determining whether that is indeed the case, we do have at least some preliminary evidence. I have above already adduced a small selection of the quantitative results which almost unanimously indicate that it is *not*. (Useful to note is that the more advanced the level of the user, the more pronounced that indication tends to be.) I shall for simplicity's sake call the working hypothesis that this indication in fact possesses truth-value "the disintermediation hypothesis".

But some have claimed that the advent of EBP has meant, certainly in the health-care environment, a new appreciation for the unique capabilities of LIS professionals and the role they can play. On this count I am aware of less evidence, yet I *have* come across a good many pertinent indications, of a naturalistic sort. It occurs not infrequently in the EBP literature that emphasis is placed on the great importance of good literature searches and results appraisal. The conclusion that is drawn, however, is that this is properly a task for the domain practitioners themselves, and they simply must be well trained to perform it – initially or if need be through continuing education programs. That there is a role in all of this for librarians or information specialists, or that they might sometimes be of some use, is not mentioned (Lambert, 2006; Pravikoff, 2006; Ratner, 2006; Rodrigues, 2000; Slawson & Shaughnessy, 2005; Timmermans & Angell, 2001; cf. Koufogiannakis & Crumley, 2006 and Winning & Beverley, 2003). Porta (2004) suggests that EBP was a conspiracy by, among others, librarians, to gain power for themselves in clinical affairs. Timmermans & Angell (2001) published an entire article on clinical literature searching by "librarians" as contrasted with "researchers". The only problem is that these two terms are employed symbolically to designate two differing types of EBHC-aware residents in terms of their literature-searching and literature-use characteristics. The "librarians" are less thorough and less critical. Real-life librarians are never mentioned in the article.

Thus we are left, it would seem to me, with a number of perhaps uncomfortable questions. I shall restrict myself to three. In the first place: should we consider this to be "evidence" for our purposes, perhaps even usable evidence? (Here I would think also of Roush's (2005, pp. 31-32) "tracking view of what evidence is", with "the ideas that evidence *indicates* the truth of the hypothesis, and that evidence *discriminates* between

the truth and falsity of a hypothesis" [her emphasis].) Or are we only prepared to see as evidence that which supports the kind of assertions quoted from our colleagues at the end of the previous section? The actually *available* evidence (as I see it) points forcefully in quite the other direction. The essence of the phenomenon which we call "evidence" is that it mandates particular inferences for use in testing a hypothesis (with regard to a theory). I am confident that a thorough systematic review would unequivocably compel us to infer from all evidence already at hand that the disintermediation hypothesis is indeed correct, and that no amount of new evidence is likely to overturn it (a confidence strongly reinforced, furthermore, by more than forty years' international and multidisciplinary experience as student, teacher, researcher, publisher and librarian).

The second question is: once we have at least provisionally accepted such evidence as valid, reliable, generalizable and applicable, how would we then proceed in operationalizing it? To do so would mean adapting the approaches we currently take to much of what we do, and a need to assemble and employ a great deal of further specific evidence in support of that process. Such seems to me exactly the task that lies before us, sooner or later. And only if we take it up can we in my opinion permit ourselves an affirmative response to the question chosen as a title by Brice et al. (2004): "A future for evidence-based information practice?". Jonathan Eldredge (2004) brought John Cotton Dana back onto the stage as a forerunner of EBLIP as originally conceived – but it appears to me that he should be seen, even as Eldredge describes him, more as a forerunner of EBLIP as it ought to become.

The last of my three closing questions mentioned above is then: What can we now say about the "objective warrant condition" described at the beginning of this essay? The euphoric assertions quoted at the end of the preceding section and many others like them abound in our professioanl literature; they are for internal consumption and serve the purpose of bolstering our collective self-esteem. There exists little or no evidence either to support them or to suggest that they represent a factor in the behavior or thinking of our actual or potential clients. They can therefore only conceivably play a positive long-term role in satisfying the objective warrant condition (i.e., in reliably

legitimating LIS) if we can manage to bring about a radical change in the way research is carried out and in the way scholars, professionals and other domain experts think and behave. This is of course not going to happen – nor should it. If we nonetheless take such gratuitous assertions seriously and build our approach to evidence-based practice around this kind of self-assessment – then we ultimately run the risk not only of failing to come up to the objective warrant condition, but even of failing to satisfy any longer the credibility condition.

James Rettig (1992, p. 162) put it well when he wrote: "every information seeker should be free of the librarian's expectations". If the LIS profession can take this recommendation to heart (it might even make a good, alternative credo for us), and act accordingly – if it can furthermore come to realize that its highest possible achievement is, as Lanham (1997, pp. 164-165) put it, "orchestrating human attention structures" – then I for one see a bright future for EBLIP. Finding, appraising and using a broad array of heterogeneous evidence to best effect for adapting LIS faultlessly to the lived world of their users requires strong reflective, but also *reflexive*, capacities. There's a very great deal still to be done. But if we look to the users to measure up to our expectations, then I truly wonder just what kind of future awaits not only EBLIP, but LIS in general.

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