Conceptualising Epistemological Barriers to the Synthesis of Educational Research: A Challenge to Science and Technology Studies

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A major reason why the use of experimental quantitative research designs is widely assumed to warrant scientific objectivity is the formalisation involved. In quantitative research, the formalisation of procedures of collection and analysis of data is regarded as a key indicator of methodological progress. On the common-sense understanding of science, the reliability of the findings produced hinges on the discipline which formal tools impose on analysts’ interpretations of the data. In a word, methodological rigour is the hallmark of high-quality science. Qualitative researchers are deeply divided, some embracing formalisation, others resisting it. While favourable references to methodological rigour are frequently made in the qualitative literature, too, a common claim is that significant phenomena and processes making up our social world are inaccessible to highly formalised methods. Familiar from the paradigm wars of the 1970s, these contrary positions are no less relevant in current social science.

Evidence-based medicine, as well as the broad movement of evidence-based practice and policy, involves a high degree of formalisation. In the production and application of relevant evidence, decision-making processes are structured by formal tools constraining the scope for exercising judgement. This is evident, for instance, in the development of procedures assumed to vouch for the rigour of systematic reviews. Central to the extension of evidence-based practice beyond medicine is the introduction of systematic reviews in other areas. In policy discourse addressing education, systematic reviews are depicted as a chief means of bridging the presumed gap between research and practice. Initiatives of this kind are strongly resisted by researchers claiming that the formalisation of reviewing practices obstructs interpretative work which, they insist, is crucial to the quality of reviews.

Systematic reviews evolved from meta-analysis, a statistical technique introduced by American social scientists in the 1970s (for the emergence of these methods, see Chalmers, Cooper, & Hedges, 2002; Bohlin, 2012). The term meta-analysis was coined by Gene Glass, who proposed it in a presi-
dential address at the annual meeting of the American Educational Research Association (Glass, 1976). Proponents of meta-analysis stressed its scientific character, referring deprecatingly to the traditional, informal type of review as “narrative” reviews. Though objections were raised at an early stage, by and large, the method pioneered by Glass was favourably received in quantitative psychology and educational research. By the mid 1980s, a handful of textbooks having appeared (Glass, McGaw & Smith, 1981; Hunter, Schmidt & Jackson, 1982; Light & Pillemer, 1984; Cooper, 1984; Rosenthal, 1984; Hedges & Olkin, 1985), meta-analysis was an established method. In 1995, a group of researchers identified almost 1000 meta-analyses published in the social sciences from 1980 to 1994 (Bausell, Li, Gau & Soeken, 1995, 243–44). A few years later, a British commentator referred to the flow of meta-analyses of educational research, most of which were carried out in the US, as “a whole industry” (Davies, 2000, 372).

Yet a controversy over formal methods of synthesising educational research did not erupt until the turn of the 21st century. Up to that point, qualitative educational research and the meta-analytic ‘industry’ coexisted on largely peaceful terms, disregarding one another. It was only on being transferred back to educational research in the form of systematic reviews that formal methods of synthesis generated a substantial dispute in this area. In the course of the 1980s, meta-analysis had gradually been adopted in clinical research, and in a programmatic 1992 article proposing the concept of evidence-based medicine, meta-analysis was identified as one of the methods making way for the new paradigm (Evidence-based medicine working group, 1992). That same year, an Oxford unit was set up from which the Cochrane Collaboration subsequently was to emerge. The term systematic review was introduced by the leaders of this organisation, which was soon to become a cornerstone of evidence-based medicine (EBM). Meta-analysis now refers to a specific statistical technique, while systematic review is the more general format in which research findings are summarised in a range of areas where evidence-based practice and policymaking is held up as an ideal. “Research synthesis” is often used as a general heading for methods combining results from multiple studies.

The idea of evidence-based teaching was first set out in 1996, in a lecture given to the Teacher Training Agency in the UK by David Hargreaves (2000). A few years later, the new British Labour government, advocating the notion of evidence-based policy, decided to fund a unit at the University of London to oversee the provision of systematic reviews of educational research. Taking up its new responsibility in 2000, the unit, which had produced systematic reviews in the area of health promotion since the early 1990s, was renamed the Evidence for Practice and Policy Information and Coordinating Centre – the EPPI-Centre, for short. Teams of reviewers, selected among applicants from various British universities, were supplied with instructions, and before publication their reports were vetted by EPPI-
Centre staff. In the five-year period that the government grant lasted, some 40 systematic reviews were produced. The formalised methodology adopted in the programme was criticised harshly by some researchers from the qualitative tradition, whose presence in British educational research is strong.

An influential critic of evidence-based practice in education, as well as of systematic reviews more specifically, is Martyn Hammersley. Though highly critical of the methodology employed in systematic reviews, Hammersley does not reject formalisation of research methods per se. While the response to formalisation by qualitative researchers, as mentioned above, ranges from embracing it entirely to repudiating it out of hand, Hammersley avoids both extremes. His position with respect to systematic reviewing seems to be that formalisation of the procedures adopted is useful up to a point.

In this chapter, I will employ theoretical concepts drawn from the area of Science and Technology Studies (STS) in order to examine Hammersley’s claim concerning a limit to sensible formalisation of reviewing practice. Time and again, STS analysts have shown that the common-sense understanding alluded to above poorly represents scientific practice. A substantial body of work demonstrates that rigorous following of formal rules is largely an illusion, and that interpretation and judgement are ubiquitous, indeed essential, components of scientific practice. Beyond that, however, STS has had little to say about the exercise of judgement in the production, evaluation and use of research knowledge. Defining the nature of the limit posited by Hammersley is far from a straightforward task, as no established theoretical framework for evaluating such claims is available. The theoretical resources employed in what follows derive from two schools of STS which are involved in a long-standing conflict. My ambition of combining concepts drawn from the opposing camps is unorthodox; as far as I am aware, this has not been attempted before.

The aim of this chapter is to explore theoretical resources by which epistemological boundaries of the kind posited by Hammersley may be understood. The formalisation of procedures for synthesising educational research will be used as a case shedding light on the relation between two major traditions in STS: Actor-Network Theory (ANT) and the Sociology of Scientific Knowledge (SSK). I will demonstrate that, for the purpose of defining the nature of a presumed barrier to the formalisation of knowledge-producing practices, relying on a set of concepts drawn from both sides of the divide separating these traditions may be a productive strategy.

The plan of the chapter is as follows. First, I will explicate Hammersley’s argumentation. The second section is a brief description of the conflict between ANT and SSK. Two sections then set out a series of concepts developed within these traditions, offering examples of how they may be applied to the methodology of systematic reviews. A concluding section iterates major points of the argument.
Hammersley’s critique of systematic reviews

Martyn Hammersley is the most persistent critic of the expansion of the evidence-based practice and policy movement into education (see, for instance, his 1997, 2004, 2005 and 2013a). Though systematic reviews are addressed in several of his subsequent publications (see his 2005 and 2006), in some respects Hammersley’s first article on this topic (Hammersley 2001), remains his most substantial critique of research synthesis. A slightly revised version of that article (Hammersley 2013b) appears in a recent collection of essays of his (Hammersley, 2013a).

According to Hammersley, the methodology adopted in systematic reviews is based on “what, for shorthand purposes, can be referred to as the positivist model of research” (2013b, 100). This, Hammersley maintains, is evident at two levels. On the one hand, systematic reviews rely on the so-called evidence hierarchy of EBM, which gives quantitative, experimental methods, and randomised controlled trials in particular, the status of superior study design. Giving priority to studies “involving explicit and replicable procedures” (2013b, 100) in this manner, Hammersley suggests, may result in valuable findings being disregarded while “misleading evidence” is championed (2013b, 102). On the other hand, the procedures by which information culled from primary studies is processed in systematic reviews themselves rely on the positivist model. More specifically, at both levels, practices are shaped in accordance with procedural objectivity, the notion that “subjectivity is a source of bias” which “can and must be minimised”.

Built into the methodology of systematic reviews, in other words, is the assumption that “both doing research and producing research reviews can be pursued best by following a set of explicit rules” (2013b, 100). That assumption, Hammersley suggests, has been challenged for decades. He cites Michael Polanyi’s work (1958, 1967) as denying “that science can, or should attempt to, operate on the basis of fully explicit procedures” (2013b, 101). Aspiring to procedural objectivity is futile, in the social sciences no less than in natural science, because “even the most simple rule-following involves some judgement” (2013b, 102).

Though Hammersley does not refer to STS, his position on rule-following coincides with an important theme in STS. Harry Collins, in particular, has demonstrated that not even in physics does the practice of scientists conform to the canonical image of rigorous rule-following. As an alternative to the idealised “algorithmical” model of science, Collins has offered an “enculturational” model, based on a Wittgensteinian understanding of rule-following (Collins, 1992, 57, 159–60). His claim is that scientific expertise, like other

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1 Hammersley, like Elliot Eisner (1991, 1992), cites Newell (1986) as the source of this concept. Though Newell certainly discusses issues related to procedural objectivity, I have not been able to identify the term in his text. For a succinct discussion of the procedural and three other senses of objectivity, see Megill (1994).
forms of specialised knowledge, is characterised by professional judgement that can only be acquired through close interaction with competent members of a community.

While the thrust of Hammersley’s critique of systematic reviews is of a piece with this theme in STS, some of his remarks point in another direction. A few sentences before stating that “even the most simple rule-following involves some judgement”, Hammersley contrasts the weighing of evidence which is part of all research practice with the following of specific, explicit rules that “calculating the results of a statistical test” requires (2013b, 102). A few pages on, this notion of a contrast between two types of intellectual operation reappears in a claim of far greater import. It “seems likely,” Hammersley cautions his readers,

that, where a process cannot be proceduralised, seeking to reduce it to procedures will lead to distortion... (2013b, 105)

Phrased in a circumspect manner, this warning hardly forms a crucial part of his strictures on systematic reviews and evidence-based practice. For the purposes of the present chapter, however, the statement is highly significant. Hammersley’s contention seems to be that the methodology of systematic reviews is part of a long-standing project of establishing procedural objectivity in a variety of research practices, and that this project is counterproductive, not across the board, but in certain areas, where the procedures introduced have the effect of distorting the information processed. Hammersley offers little in the way of explaining which types of intellectual operation can and which cannot be proceduralised, and hence the nature of the boundary line remains vague. Clearly, however, this statement posits a limit to reasonable formalisation of the tasks involved in reviewing practices.

The qualifier ‘reasonable’ in the last sentence is important. Taken literally, Hammersley’s reference to a process which “cannot be proceduralised” would be misleading. Formalising the practice of summarising empirical results beyond the limit posited in the statement quoted above is certainly feasible. That such procedures are devised, disseminated and adopted is precisely what causes Hammersley’s concern, leading him to insist that a price is being paid – in terms of research findings being distorted, the quality of reviews being lowered, and misleading ideas concerning the value of different kinds of empirical research being promoted.

Analytical frameworks

The STS literature provides resources for at least three distinct ways of studying cases like the controversy over systematic reviews of educational research. The first approach is a socially grounded form of deconstruction,
representing or at least cognate with SSK. The second approach, based on ANT, focuses on effects produced by heterogeneous networks. Neither perspective, I will argue in this section, can offer an understanding of the nature of constructs such as the epistemological boundary posited by Hammersley.

A pervasive assumption, perpetuated by scientists and philosophers, as well as by accounts of science aimed at the general public, is that the accuracy of findings generated through scientific experiments and observations relies on rigorous adherence to agreed systems of explicit methodological rules. A principal aim of much empirical research undertaken in STS has been to demonstrate that scientific practice, behind the canonical image of strict rule-following, involves key elements of interpretation and discretion. Collins’s distinction between two models of science, the “algorithmical” versus the “enculturational”, is emblematic of this research agenda. Investigating scientific practice in this manner is tantamount to deconstructing claims to objectivity and authority.

Research synthesis is a neglected subject in STS; to the best of my knowledge, Tiago Moreira’s (2007) ethnography is the only one of its kind. Drawing attention to the interpretative work of reviewers, as well as to negotiations over the political expediency of possible results, Moreira’s analysis exemplifies the deconstructivist perspective. Authors outside STS who challenge the scientific status of systematic reviews offer the same kind of arguments. The congruity of Hammersley’s analysis with STS work highlighting the role of judgement in scientific practice was noted above. Critiques of systematic reviews resembling Hammersley’s include MacLure 2005, specifically levelled at the EPPI-Centre programme, and Sandelowski 2008, primarily addressing health research. Clearly, adopting the deconstructivist perspective in an STS analysis of the dispute over systematic reviews of educational research would mean siding with the critics.

Actor-network theory provides the foundation of an alternative theoretical framework. While in the early 1990s ANT was a fairly coherent school of thought, concomitant with its subsequent rise to dominance in STS a degree of fragmentation has occurred, and it now seems more appropriate to recognise a family of “actor-network successor projects” (Law 2009, 142). One of the themes discussed as ANT has come of age concerns alternatives to deconstruction. Bruno Latour (2004), describing his own previous work as sharing the SSK agenda of “debunking”, has proposed a form of critique which, allegedly, is more appropriate for present purposes. Many other authors in the ANT tradition, too, have taken exception to the aim of deconstructing claims to truth and efficiency. Instead, priority is given to investi-

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2 And, for a detailed historical account see Bohlin 2012. Another key resource in EBM, clinical practice guidelines, has been given far more attention by STS analysts. See, for instance, Moreira (2005); Knaapen, Cazeneuve, Cambrosio, Castel, & Fervers (2010); Knaapen (2013); van Loon & Bal (2014), and, for an historical study, Weisz et al. (2007).
gations of effects produced by the introduction of new tools and technologies in networks comprising a variety of human and non-human actors.3

Stefan Timmermans and Marc Berg’s (2003) book-length study of the formalisation of procedures brought about in EBM is a good example. The extensive use of clinical practice guidelines and other formal tools under the banner of EBM has been highly controversial among medical practitioners, drawing extensive commentary from beyond the profession as well. The research agenda of Timmermans and Berg, however, does not include taking sides in this controversy.

Opponents and supporters agree that standards provide order but disagree on the merits and need of such ordering. We ask instead: what is being ordered, who does the ordering, what is the difference, and how does it change medical care? (Timmermans & Berg, 2003, 21)

In another ethnography of healthcare practices, performativity, a key theme in “after-actor-network” studies, is explored in a particularly thought-provoking manner. Annemarie Mol (2002) claims that the medical condition of atherosclerosis is “enacted” in the practices of specific professional groups. On this analysis, “objects come into being – and disappear – with the practices in which they are manipulated” (Mol, 2002, 5). While Timmermans and Berg avoid intervening in the field studied, Mol (1999, 2008) has proposed an interventionist agenda under the heading of “ontological politics”, the basic tenet being that STS analysts may contribute to better ways of enacting realities.

Socially grounded deconstruction and material semiotics based on ANT each has its obvious merits as an analytical framework for studying systematic reviews of educational research. Given the prevalence of the assumption that strict adherence to formal rules marks high-quality research, as well as competently conducted research reviews, empirical studies demonstrating that judgement is, and needs to be, exercised in these processes are certainly justified. In educational research, critics of evidence-based practice and systematic reviews would no doubt welcome STS analyses of this kind. On the other hand, such work tends to be predictable. I agree with Timmermans, Berg and other authors in the ANT tradition that the manner in which specific practices are affected by new technologies and standards is a highly relevant topic for STS, and loyalties with critics or promotors of the formal tools studied constrain the theoretical scope of such work.

3 Megill’s explication of procedural objectivity (see n. 1 above) is partly based on historical work by Ted Porter, who uses the term mechanical objectivity instead. Historical studies reying on that concept (Porter, 1995; Daston & Galison, 2007) offer excellent resources for investigating the effects of formalisation of research practice. Space does not allow a discussion of this line of work in the present chapter; for a discussion of how it is related to ANT and SSK, see Sundqvist, Bohlin, Hermansen, & Yearley (2015).
More important, however, given the purposes of this paper, is the circumstance that none of these frameworks allows an inquiry into the nature of limits to formalisation of the kind postulated by Hammersley. On the deconstructivist perspective, interpretative work is involved wherever socially competent actors are producing or compiling knowledge. Claims concerning limits to formalisation hold little interest, because academic practices are assumed to have the same fundamental character on each side of any such putative boundary. In an article discussing the sociology of climate science, Steven Yearley remarks on what may be regarded as a serious limitation of socially grounded deconstruction:

Though this overall argument has been very persuasive within the history of science and science studies, it has not led to as much detailed analysis of scientific judgement as one might have expected. And, in particular, by presenting all scientific practice as a matter of judgement, it has left philosophers and sociologists of science with rather little to say about cases of explicit judgement, as with the IPCC. (Yearley, 2009, 398; emphais in the original)

From the point of view of actor-network approaches, the effects of whatever formal tools are being put to use in a given form of practice might be worth studying. Insofar as educational research findings take on a new character on being synthesised by formal methods, the differences thus produced may merit investigation. New patterns and regularities identified in the research literature through research synthesis may be assumed to have been enacted by reviewing teams employing specific tools. But again, any presumed epistemological boundary would be irrelevant to such inquiry; the notion that there might be an absolute limit to the forms of technology that can be deployed for a given purpose is foreign to material semiotics.

In the sections to follow, the outline of a theoretical framework appropriate for investigating an alleged limit to formalisation of the practice of synthesising research findings will be sketched. My proposal is eclectic rather than purist. Discussing conceptual resources from which an appropriate framework might be assembled, I will draw on more than one school of thought.

Translation, delegation and scripts

A useful building block with which to begin this assemblage is Latour’s concept of translation, in the form employed with particular lucidity in an essay on a field trip to the edge of the forest of Boa Vista in the Brazilian interior.4 Latour describes the practical issues involved in a series of transla-
tions through which ‘raw’ objects and data are processed into publishable graphs and figures. At each step in this chain, some material is transformed into a new representation or ‘inscription’. Intimately related to translations, the latter concept is defined as referring to:

all the types of transformations through which an entity becomes materialized into a sign, an archive, a document, a piece of paper, a trace.


Keeping certain relations intact through chains of translations is indeed crucial. The value of the procedures making up a given series of translations is a function of the accuracy with which significant properties of the material processed are taken to be represented. Processes of translating objects and data need to be reversible in the sense that, whenever the adequacy of a given claim on some state of affairs is being challenged by critics, authors of academic texts may have to justify each operation performed along the chain of representations and re-representations through which the finding was reached.

The methodology of research synthesis may be regarded as instantiating a Latourian chain of translations. Strategies for searching the literature, the employment of inclusion and exclusion criteria, the use of coding sheets to extract data from the studies selected, as well as the synthesis of the data sets thus produced, are stages in a series of intellectual operations designed to identify and distil the significant contents of a material which, prior to these transformations, may appear amorphous and unmanageable. Meta-analysis and systematic reviews are fairly generally recognised as having this capacity with respect to quantitative research findings in medicine and social science, including educational research. The introduction of systematic reviews in the latter area, however, created an arena where qualitative researchers faced the methodology directly. Hammersley’s objections against formal methods of research synthesis bear witness to the tensions generated by this encounter. Attempts to establish procedural objectivity in this area are taken to distort the essence of what, in Latourian terms, was to be preserved through the process of successive translations.

Equally useful, for the purposes of this paper, is the ANT concept of delegation. Like translation, delegation is a common term, appropriated and invested with a specific meaning by ANT authors (Johnson, 1988; Latour, 1992; Akrich, 1992). Reflecting the material-semiotic nature of actor-network theorising, this concept refers to the substitution of some entity

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ably in this text. I will largely stick to ‘translation’, though the choice of term makes little difference analytically. What is important is distinguishing the ‘Boa Vista’ sense of translation or transformation from other senses in which Latour uses these terms, such as the notion of translation of interests.
charged with performing a certain task by another, and, more specifically, to what is involved in supplanting human actors by artefacts in heterogeneous networks. The ANT concept of delegation has a further implication, however. Door closers, hotel keys and road bumpers are well-known examples of delegation in Latour’s writings (Johnson, 1988; Latour, 1991; Latour 1992). These artefacts are taken to impose certain behaviours on humans interacting with them. Technical objects, thus understood, carry *scripts*; they prescribe human behaviour. Cleverly designed they may be instrumental in disciplining human actors, allowing a hotel manager, for instance, to make it hard for his guests to forget returning their keys. The concept of delegation, then, is closely tied to the central ANT principle of non-human agency.

Obviously involving discipline, the formalisation of research practices may be regarded as instantiating delegation, as conceptualised by actor-network theorists. Developing an elaborate methodology of research synthesis, specifying each stage in the process, is a case in point. Critics as well as advocates of systematic reviews of educational research refer to the formal procedures adopted as a form of technology (MacLure, 2005, 397; Oakley, 2006). This is entirely appropriate. The function of coding instruments, inclusion criteria and other decision support tools deployed in research synthesis is to structure the translations of the material processed, i.e., to limit the range of interpretative options open to reviewers at every point. In this field as in many others, formal tools prescribe proper practices. In the terminology of Bruno Latour and Madeleine Akrich, they carry scripts. This form of prescription is intimately related to methodological rigour; asserting that a study is being conducted in a rigorous manner is equivalent to claiming that its authors heed a script of this kind.

In sum, the concepts of delegation, script and chains of translations are highly useful for an STS understanding of the practices involved in systematic reviewing. Importantly, conceptualising formalisation of reviewing practices in these terms does not imply assuming that the process allows no space for discretion. Though the role of judgement in research practice is a key theme in social accounts of science while avoiding social explanations is fundamental to ANT, the conflict between material semiotics and the socially grounded tradition in STS does not play out at that level. Drawing on Latour’s concepts of delegation, script and translation is fully compatible, I submit, with focusing attention on the exercise of judgement. In a word, discretionary decisions may be essential even within highly scripted practices. Hence nothing prevents us from wedding the set of ANT concepts discussed above to certain concepts developed within the socially grounded

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5 This use of the term technology is analogous with that adopted in Shapin (1984) and Shapin & Schaffer (1985). Apart from addressing ‘material’ technology, Shapin and Schaffer stress the importance of a particular ‘literary’ and ‘social’ technology employed by Robert Boyle in the seventeenth century. All three are understood as “knowledge-producing tools” (Shapin, 1984, 512 (n. 6); Shapin & Schaffer, 1985, 25 (n. 4)).
tradition. This claim is a premise of the analytic strategy pursued in the present chapter.

Polimorphic and mimeomorphic actions

In his numerous publications, Harry Collins has been pursuing three relatively distinct main paths. The first is his core contribution to the development of SSK, theoretically as well as empirically, and hence to establishing the deconstructivist agenda of STS. Whereas the relativism of this kind of analysis is obvious, the essentialist conception of knowledge underlying it is often overlooked. This essentialist bent comes to the fore in the second and third strands of Collins’s writings, in which insights gained in SSK are used for other purposes. One line of argument concerns machine intelligence and types of human action (Collins, 1990; Collins & Kusch, 1998). Collins’s and Robert Evans’s (2002, 2007) project of defining the nature of expertise is a different, if related, way of moving on from SSK. While the SSK tradition was addressed above, the third line of argument will not be considered in this chapter. It is the second main strand of Collins’s writings that provides resources vital to my analytic concerns.

In a 1990 monograph on artificial intelligence, Collins challenged commonly held ideas concerning what is achievable in that area. The basic message of the book is that though computers surpass the capacity of humans in many respects, designing machines which emulate human intelligence is not a realistic goal of engineering. This, Collins claims, drawing on his distinction between an algorithmical and an enculturational model of knowledge, is because human intelligence essentially depends on an ability to share forms of culture which cannot be captured by algorithms. On this perspective, ANT, impervious to what Collins was later to dub socialness (1998), appears no less naïve regarding what properties can be built into technical devices than are artificial intelligence enthusiasts.

Elaborating this analysis, Collins and Martin Kusch (1998) frequently use the concept of delegation to make their points. A typology of human action is offered, the main types being labelled polimorphic and mimeomorphic. The latter, but not the former, Collins and Kusch maintain, may be delegated to machines. This, allegedly, is due to a certain form of indifference; insofar as we are indifferent to the particular way in which a given action is performed, we are inclined to accept machine executions of this action as good enough approximations. The writing of love letters is Collins and Kusch’s chief example of polimorphic actions; washing laundry and writing bibliographic references according to a specific format would be typical mimeomorphic actions.

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6 For a discussion of the relation between SSK and this analytic strand, see Bohlin (2000).
Collins and Kusch divide artefacts into three categories. ‘Tools’ are devices, such as vacuum cleaners or word processors, that we use to facilitate tasks we would be able to handle without them. ‘Proxies’, too, perform tasks we might have handled without them (though often less adroitly), but differ from tools in that their function is to replace us. Dishwashers and computer programmes for formatting and storing bibliographic references are examples. ‘Novelties’, finally, are artefacts with the capacity of doing things that humans are unable to. Windows and telephones fall under this category. Collins and Kusch stress that the divisions are not sharp. The distinction between tools and proxies, in particular, is fuzzy, and depends on the perspective taken. One of the aims of Collins and Kusch’s analysis is to demonstrate that devices which really constitute tools are commonly taken to be proxies. This, they suggest, is because polimorphicity is not properly understood and valued.

Computerised bibliographic databases serve an important function in research synthesis. In the terminology of Collins and Kusch, they may be regarded either as tools or as proxies. Though varying considerably with respect to indexing practices and completeness of coverage, bibliographic databases are the chief resource used whenever a comprehensive literature search is attempted as part of a meta-analysis or systematic review. Due to publication bias and indexing problems, however, even Medline, the world’s largest and most highly developed bibliographic database, is unreliable. Hence reviewing teams are often advised to use supplementary methods, including hand-searching of journals central to the topic area, scanning of the reference lists of relevant reviews and papers, and contacting leading researchers in the area concerned. Several studies suggest that these supplementary search techniques yield a considerable number of potentially relevant publications which conventional database searches would not have retrieved (Helmer, Savoie, Green, & Kazanjian, 2001; Savoie, Helmer, Green, & Kazanjian, 2003; Conn et al. 2003; Hopewell, Clarke, Lefebvre, & Scherer, 2007; Papaioannou, Sutton, Carroll, Booth, & Wong, 2010).

Resources employed for literature searches may be understood in terms of Collins and Kusch’s polimorphicity/mimeomorphicity divide. Evidently, a key role in this context is delegated to bibliographic databases, and most of the operations performed in a standard search, it seems, may be categorised as mimeomorphic. Certain techniques which appear to be indispensable in an extended search, on the other hand, clearly involve polimorphicity. For the present, at least, delegating those tasks to technical devices is not feasible. The apparent limit to what can be delegated to artefacts in literature searches may be taken to exemplify Collins and Kusch’s analysis.

The character of computer programmes used by reviewing teams confirms that conclusion. Software for meta-analysis and systematic reviews has long been available (for a brief overview, see Littell, Corcoran & Pillai, 2008, 143–48). Systematic reviewing is time and resource consuming, and
there are concerns that this enterprise, as currently organised, cannot keep up with growing demands (Bastian, Glasziou & Chalmers, 2010). Hence the prospect of automating as large a part of the process as is feasible is an attractive one. A recent article offers a useful overview of the development of software for a number of tasks involved in systematic reviewing (Tsafnat et al. 2014). This article describes resources that facilitate literature searching by automatically adding or replacing search terms, for instance, or by allowing users to query several databases through a single search engine, and once a list of potentially relevant references has been obtained, systems for automatic retrieval of full-text publications are available. The next step in the process, that of screening publications to determine which ones to include in the review, is one of the tasks for which automation is far harder to achieve. Overall, however, the authors are optimistic. Software for systematic reviewing, they maintain, frees systematic reviewers to shift their focus from the tedious tasks which are automatable, to the creative tasks of developing the review protocol where human intuition, expertise, and common sense are needed and providing intelligent interpretations of the collected studies.

And, they add, “what seemed fantastic only a few decades ago is now a reality” (Tsafnat et al. 2014).

Two points should be noted. First, the distinction drawn here between, on the one hand, technical operations that are ‘automatable’, and, on the other, ‘creative’ ones that are not, roughly matches the distinction between mimeomorphicity and polimorphicity. Second, this review of current developments suggests that rather than being absolute, the boundary between the two types of task is shifting over time.

Conclusion

In research practice across the disciplines, raw data of one kind or another is being translated, in Latour’s sense, through successive steps, into publishable findings. Research synthesis may similarly be understood as involving translation of findings published in the primary literature into knowledge claims of a more general kind. Wherever some material is being translated, the reliability of the procedures adopted and the fidelity of the representations produced are routinely challenged by critics. The relevance to understanding reviewing practice of Latour’s concept of translation, with its concomitant requirement that some types of relation be kept intact through the operations performed, should be obvious.

Given that formalisation of reviewing practice refers to the introduction of a certain form of technology, the concepts of delegation and script are
equally relevant. The technology on which this chapter has focused is formal tools devised to structure decision making, ensuring its uniformity and reliability. The formalisation of procedures involves disciplining practitioners to rely on decision support tools, hence reducing the scope for informal judgement. Hammersley, in his strictures on this way of structuring reviewing practice, posits a limit to sensible formalisation. Trying to proceduralise that which cannot be proceduralised, he claims, leads to distortion of the research findings represented.

While the ANT concepts of translation, delegation and script may be applied to reviewing practice whether or not such a limit exists, taking Hammersley’s claim seriously calls for a theoretical resource of another kind. This is why I have proposed combining the three ANT concepts with Collins and Kusch’s distinction between polimorphic and mimeomorphic actions. Though Collins and Kusch’s theory concerns automation, my analysis suggests that it is equally relevant to understanding the use of decision support tools in reviewing practice. The possibility that a putative limit to sensible formalisation may be historically variable is an interesting complication which space does not allow me to address in this chapter.

Regardless of this and other complications, there are good reasons to apply STS concepts to issues of educational research. Formalisation of reviewing practices is an interesting topic for STS analysis. It is to be hoped that such work may prove useful to educational researchers reflecting on their own practice, too.

References


