A sociology of formal logic? Essay review of Claude Rosental’s ‘Weaving Self-Evidence: A Sociology of Logic’

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REVIEW ESSAY

A Sociology of Formal Logic?

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Through a variety of case studies, social studies of science – in particular the sociology of scientific knowledge (SSK) – has demonstrated that scientific experiments and theorems can be open for debate. However, can that also be true for logical arguments? Say, someone accepted the assertion (A), ‘things that are equal to the same are equal to each other’, and the assertion (B), ‘the two sides of this triangle are things that are equal to the same’, but then had doubts over whether or not to accept also, (Z) ‘the two sides of this triangle are equal to each other’, would we not be tempted to say to them, as Achilles says to the Tortoise in Lewis Carroll’s famous dialogue, that:

> Then Logic would take you by the throat and force you to [accept] it! … Logic would tell you ‘You can’t help yourself. Now that you have accepted A and B … you must accept Z!’ So you’ve no choice, you see. (Carroll, 1895: 280)

If we really had no choice, then what could the sociologist say about logic? However, if (as Carroll’s dialogue was meant to show) logic ‘in itself’ cannot force us to accept any particular next step, then what other factors could create the impression of the self-evident nature of logical reasoning – and could these be social factors?

For anyone unfamiliar with recent social studies of science, the idea that someone could undertake a project called ‘a sociology of logic’ would certainly...
sound strange. What could such a project look like? They will find one answer in Claude Rosental’s book, which is the first attempt to investigate systematically, and empirically, formal logic with a sociological eye. The book excels in dispelling the idea that logic is intrinsically compelling by showing that logicians have room for manoeuvre (both individually and collectively) when deciding whether or not to accept something as a valid logical argument. Of course, for social studies of science the idea of a sociology of formal logic (or of other conceptual or mathematical sciences such as pure mathematics or theoretical physics) is no longer strange. However, neither is there an abundance of studies that have tried to substantiate the claims about the socially and materially constituted character of the formal sciences. As MacKenzie (2001: 2) observed:

… the sociology of mathematics is sparse by comparison with the sociology of the natural sciences; the sociology of formal logic is almost non-existent. … the single best treatment of proof remains one that is now forty years old, by the philosopher Imre Lakatos in the 1961 Ph.D. thesis that became the book *Proofs and Refutations* [1976] ….  

Over the years, there have been a few important attempts to investigate the mathematical sciences, including Bloor’s (1973, 1976) discussion of the historical and cultural variation of mathematical systems, Livingston’s (1986) ethnomethodological investigation of Gödel’s Incompleteness Theorem, MacKenzie’s studies of the ideological basis of statistics (1981) and the debatable status of mechanized proofs (1999, 2001), Pickering and Stephanides’ (1992) reconstruction of the development of Hamilton’s quaternions, Merz and Knorr-Cetina’s (1997) study of a dislocated research group of theoretical physicists, Warwick’s (2003) history of mathematical physics teaching at Cambridge, and Netz’s (1999) *The Shaping of Deduction in Greek Mathematics*, which Latour (2008) argues did for the origins of mathematical proof what Shapin and Schaffer (1985) did for the origins of scientific experiments. However, Rosental attempts something much more systematic and pioneers in taking the same anthropological approach of the earlier ‘laboratory studies’ of the experimental sciences (e.g., Latour & Woolgar, 1979; Lynch, 1985) to the study of formal logic, by ‘carry[ing] out in situ
observations of the material interactions and practices (writing, among others) that characterized groups of actors in logic’ (p. 47).

Rosental focuses on a controversy over a paper on the status of fuzzy logic to show that proving a mathematical theorem is not a purely logical matter, but one that draws on a set of heterogeneous resources; including those that are neither ‘formal’ nor ‘logical’. Rosental’s study is firmly in the line of ‘controversy studies’ that attempt to show how the content of scientific knowledge is (at least partially) socially determined in so far as controversies are not settled by the logical implications of scientific experiments alone.

The controversy Rosental reviews erupted in the mid 1990s over a paper on the status of fuzzy logic, which is a branch of formal logic developed in connection with the theory of fuzzy sets (Zadeh, 1965). Fuzzy logic’s basic idea is relatively simple: while classical logic recognizes only two truth values, ‘true’ and ‘false’, fuzzy logic allows degrees of truth, varying from ‘absolutely false’ to ‘absolutely true’ (typically expressed by numerical values between 0 and 1). For example, we might give the assertion ‘Peter is tall’ truth degree 0.7, while ‘Mary is tall’ only receives truth degree 0.3. Fuzzy logic received popular attention in the 1980s and 1990s when the media publicised it as both a break with logical tradition and as a new form of control in engineering for household appliances (such as vacuum cleaners, washing machines, or cameras) that were supposedly based on fuzzy logic. While fuzzy logic gained notoriety (and funding), there was nevertheless scepticism over both its claimed innovative status and promised utility. When I recently asked a logician about fuzzy logic, he was sceptical about its usefulness, because for him the definitions of fuzzy logic have some unacceptable consequences. He gave the following example: the typical way in which the truth degree of a conjunction of two propositions is defined is by taking the minimum of each proposition – in symbols: \( t(A \land \neg A) = \min \{t(A), t(\neg A)\} \). However, this means that for a proposition \( A \) with a truth degree strictly between zero and one (e.g., the proposition ‘Peter is tall’ with truth degree 0.7), the ‘classical’ contradiction \( A \land \neg A \) (in our case: ‘Peter is tall and Peter is not tall’) would get a truth degree greater than zero (in our case 0.3) – although for many logicians it should ‘obviously’ get a truth degree of zero.
Charles Elkan’s (1993) conference paper ‘The Paradoxical Success of Fuzzy Logic’ started the controversy, which was initially carried out on the electronic discussion group comp.ai.fuzzy and later in academic journals. The paper started this heated debate by purporting to prove that fuzzy logic is either inconsistent or in the end reduces to classical logic. In that sense, Elkan’s theorem is a ‘meta theorem’ aiming to prove something ‘about’ fuzzy logic (rather than a result ‘within’ fuzzy logic). Various aspects of Elkan’s paper were debated: Did his proof make unwarranted assumptions? Did he define fuzzy logic correctly? Did he fairly assess the applications based on fuzzy logic? In sum: Did he really prove what he aimed to prove? Not only did different people have different views, but Elkan himself revised his paper as a result of these debates. This enables Rosental ‘to bring to light the formation of a plurality of viewpoints on the content of Elkan’s theorem and its proof, as well as on its correctness and its value’ (p. 254). Rosental’s book is thus a canonical ‘controversy study’, albeit one that innovatively applies this approach to the formal rather than the experimental sciences. Thus while, for example, Collins (1985: 2) aimed to show that ‘it does not seem possible to construct a computer-type “algorithm” for ensuring that experimental replication always provides a definitive test for the existence of new and disputed natural phenomena’ (i.e., that experiments do not always settle scientific debates), Rosental could be said to show that it does not seem possible to construct fully explicit mathematical proofs that will be self-evident to every reader (i.e., that proofs do not always settle arguments, even in logic). By following the course of the debate, Rosental is able to show that the ‘stabilization of a logical statement … involve[s] operators as heterogeneous (and perhaps as unexpected) as those that come into play in the sedimentation of experimental facts’ (p. 259).

The questionable idea that logic ‘of itself’ can settle matters is one that, in Rosental’s estimation, presupposes other equally questionable ideas, in particular the image of logic as a purely individual, cognitive, and immaterial activity. His aim is to show that these ideas do not apply in the case of the controversy over Elkan’s paper. Thus, while traditional conceptions of logic reduce logic to debates over immaterial ideas, Rosental observed that logicians at work employed ‘a huge gamut of material resources’ (p. 5), especially writing
(on pieces of paper, on blackboards, on the electronic discussion forum) as a means for making, or in Rosental's words 'de-monstrating', logical arguments. Furthermore, in contrast to the picture of logicians sitting alone in their offices, silently running proofs through their heads, Rosental observed them frequently exchanging ideas with other logicians. He concludes that the 'activity of logical production thus bore little resemblance to the image of actors working in near-total isolation, and it was not exclusively circumscribed by the minds of individuals' (p. 5). In that sense, Rosental's aims resemble those of Hutchins in *Cognition in the Wild* (1995), namely to dilute the cognivist idea of 'mind' as something entirely inner and mental. Rosental's title, *Weaving Self-Evidence*, draws attention to the work that goes into establishing an argument's 'self-evidence' as well as the network of humans, technologies, institutions, disciplines, etc. in which logical discourse is embedded (and which may, in turn, be transformed by it).

**Overview of the Book**

Rosental's argument can be said to contradict three received ideas about logic: first, that logic is self-evidently true; second, that logic is exclusively a mental exercise; and, third, that logic proceeds by pure reason. He counters that logic is not self-evident, that it is a material and public affair, and that it is formed by heterogeneous factors, including social interests. Making these points is divided (roughly) between the three main parts of the book, and each part also presents the analysis of different kinds of data.

Part One, ‘Accessing the World of Producers of Logical Statements’ (Chapters 1 and 2), gives the analytic and methodological background for the study. Rosental argues that most empirical investigations of logic (in, for example, ethnology or cognitive psychology) start with an *a priori* definition of logic, which is used as a yardstick for comparing empirical practices rather than, as he will show, something that is to be decided in and through debate. Rosental argues that although there is much to be learned from previous investigations of mathematics and logic in social studies of science, none of them have responded to Latour's (1987: 246) call for an anthropological study of formalism that captures the numerous and various elements going into formalization in

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order to see how these elements are mobilized by different actors for different purposes.

Before investigating the controversy itself, Rosental introduces readers to the vocabulary of logic through his field study of introductory logic classes at an American university. The instructors aimed to teach “logic” as a separate and autonomous language (p. 60) showing students how to translate ‘ordinary’ expressions into the formal logic’s symbolic notation. What immediately struck Rosental was that the instructors employed a variety of visual resources (symbols, diagrams, colours) at the blackboard to get students to see how to make these translations. Rather than being a purely cognitive and immaterial activity, Rosental concludes, ‘the formalization of statements stemmed, as such, from a material practice of writing’ (p. 61). Furthermore, although instructors made a strong distinction between the formal and the informal (intuitive), this distinction was not assuredly self-evident to beginners. For example, when students were told that the formal translation of the expression ‘the current president’ was ‘[t]here is an x, such that x is president, and if there is a y, such that y is a president, then x equals y’ (in symbols: ($\exists x P x \land \exists y P y \Rightarrow x = y$)), one student wondered why such a cumbersome expression was necessary: ‘Why do you need that? There can’t be more than one president of the United States’ (p. 62). For Rosental, this illustrates that students ‘did not share the instructors’ understanding of the dichotomy between formal and informal knowledge [since] the students found the expression of a unicity clause superfluous’ (p. 63). In turn, this points to the ‘tacit knowledge’ that has to be in place before a formal, symbolic expression can be understood as a ‘correct’ translation of an informal one (cf., Levi, 2000). Rosental concludes from this that doing logic is a skill, echoing Warwick’s (2003) point that learning a theoretical practice involves both material and manual aspects. In other words, mastering logical notation and its application is like ‘learning to play a musical instrument such as the violin or the piano’ (p. 69) and requires much practice.

In Part Two, ‘Practices of De-Monstration: Debating a Theorem in an Electronic Forum’ (Chapters 3 and 4), Rosental reviews a year-long exchange of postings on an electronic forum. From these, he shows that participants gave radically different evaluations of Elkan’s paper, producing a stream of messages that
was not so much ‘polyphonic’ as ‘cacophonous’ (p. 78). However, despite the variety of comments, participants nevertheless shared a common vocabulary and mode of questioning: they did not question every aspect of Elkan’s paper. The origin of this commonality, and here again Rosental follows Warwick, resulted from the fact that the participants had been taught logic in similar ways (p. 86).

In a manner resembling Lakatos’ (1976) *Proofs and Refutations* (albeit more closely tied to a ‘real time’ exchange rather than an artificially reconstructed dialogue), Rosental shows that participants disagreed over what resources Elkan’s proof depended on (in particular, whether there were any hidden assumptions) and debated what would constitute a correct definition of ‘fuzzy logic’ (p. 81). Rosental concludes from this that ‘the formal presentation of a demonstration does not lead automatically to consensus’ (p. 77). In other words, for something to appear as possessing self-evidence (or lacking it) it is not enough for it to be stated in a formal argument; it also may need to be explained and argued for through a set of supporting ‘de-monstrative resources’. Rosental introduces the term *de-monstration* to characterize this specific practice, which consists in producing new inscriptions designed to display something, to make something appear, to show something (a principle at issue in the present case)’ (p. 98). He draws attention to the heterogeneity of ‘de-monstrative modes’ employed: apart from questioning the formal consistency of Elkan’s proof, some critics questioned whether Elkan was fair to engineering applications of fuzzy logic; others saw Elkan’s paper, which won a prize at an Artificial Intelligence conference, as an expression of a general antagonism towards fuzzy logic within AI; and, finally, still others saw Elkan to be expressing a Western ‘dualist’ bias and ignoring alternative Eastern philosophies.

One finding emerging from the careful dissection of the messages on the online discussion forum is that the ‘formal correctness’ of Elkan’s theorem was not the primary topic of debate: ‘Contrary to what we might have expected, the question of the correctness of the demonstrations is thus not central in such a debate’ (p. 95). I was initially puzzled by this remark, since Rosental exhibits participants’ doubts about whether Elkan’s conclusions are ‘correct’, whether Elkan ‘correctly’ defined fuzzy logic, or whether it was ‘correct’ to award a prize to the

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paper. Rosental presumably means that participants in the debate were not so much concerned with any simple missteps in the formal proof sequence, as they were with the assumptions, values, and judgements embodied in the formal statement. Rosental thus extends Lakatos’ observation that, although mathematical arguments are open to refutation, contrary to common perceptions refutations can be no less difficult to construct than the original proofs:

But often the checking of an ordinary (informal) proof is a very delicate enterprise, and to hit on a ‘mistake’ requires as much insight and luck as to hit on a proof: the discovery of ‘mistakes’ in informal proofs may sometimes take decades – if not centuries. (Lakatos, 1976: 4, n. 1)

To put this in more Wittgensteinian terms: what was at issue in the controversy over Elkan’s paper was not so much the truth of the statements, but rather their sense. Everybody seemed to agree that from these formally stated assumptions, you could draw formally derived conclusions, while disagreeing over whether those assumptions were the ones that should be made and whether those conclusions had any significant implications.

Since Rosental aims to show the wide range of factors influencing the outcome of controversies in logic, it would be too restrictive for his purposes to confine his analysis to the online exchanges, with the suggestion that they decided the debate. And so Part Three, ‘Mediations Used to Advance a Logical Theorem’ (Chapters 5, 6, and 7), moves to consider the offline manoeuvring around Elkan’s position. Elkan initially submitted his provocative paper to a conference on fuzzy logic, where it was rejected, before submitting it to the AAAI’93 conference, where it was accepted and furthermore won a prize. In other words, Elkan’s paper was dismissed by one group of reviewers and esteemed by another (so much for the self-evident universality of logical proofs).

Rosental talked to attendees at a fuzzy logic conference about peer review and found that few articles were judged as extremely good or bad, but that the bulk ‘could be either accepted or rejected’ (p. 181). Reviewers did not accept (or reject) an article because it was correct (or incorrect); rather, they had to make judgements on a variety of grounds. In other words, there were no “universal” criteria of “correctness” applicable to the proofs that would have made it
possible to select articles “automatically” (p. 183). The review process in a formal discipline such as fuzzy logic is thus not itself formalised, any more than it is in any other discipline. All of this contributes to Rosental’s conclusion that the ‘decision to publish Elkan’s result was not a de facto “guarantee” of its “correctness”, nor did the decision not to publish confirm its “absence of correctness” in some ideal sense’ (p. 160).

Finally, comparing the 1993 version of Elkan’s text with a longer version from 1994 (Elkan [1994], which was available on the electronic forum from November 1993), Rosental notes significant changes. Elkan originally claimed his result applied to ‘a standard version of fuzzy logic’, but later restricted the claim to ‘an apparently reasonable version of fuzzy logic’; his initial proof assumed the equivalence of two expressions, but was later modified to a conditional claim that would hold only if they were equivalent (which Elkan did not establish). Although Elkan changed his text, he did not accept all the criticisms (in particular, that his result was ‘incorrect’ or ‘irrelevant’). Rosental argues that Elkan assembled a ‘polysemic’ text with strategically different meanings for different readers to ‘satisfy’ at once the variable objections of different critics. The paper was modified so that it no longer seemed to contain ‘gross errors’, while also appearing in the eyes of ‘nonspecialists’ to state a theorem of general scope:

Certain formulations, by virtue of their allusive and polysemic character, seem capable of delivering different messages to different sets of readers. … Displaying formulations that may appear more ‘modest’ and more ‘accurate’ to the first group, the new text is simultaneously positioned to display a ‘strong’ and ‘general’ result to the second group. (p. 230)

Rosental portrays Elkan in a manner akin to Latour’s (1988) account of Pasteur: a strategist who tried to rally supporters and appease opponents through a variety of tactics. Rosental’s main aim is to evidence how the contingencies of controversy play into the acceptance (or otherwise) of logical proofs, though in Elkan’s case, the controversy dies down without any clear outcome.
Discussion

The resuscitation of the sociology of knowledge as the sociology of scientific knowledge (SSK) was rooted in a refusal to exempt the natural and formal sciences from the thesis of social determination. If Mannheim’s sociology of knowledge was a generalisation of Marx’s theory of ideology, then it was an incomplete one, and SSK sought the full generalisation of Mannheim’s thesis of social determination (although Lynch [1993, Chapter 2] argues that SSK exaggerated its differences with the ‘old’ sociology of knowledge).

The exceptionalism attributed to scientific and mathematical knowledge originated in three (nonsociological) suppositions: that knowledge is the product of wholly independent individuals; that their efforts are entirely mental (therefore private) in nature; and that those mental efforts involve pure reasoning that transcends social interests and divisions. Demystifying these suppositions has persistently occupied SSK’s investigations, which have consequently aimed to show that natural scientists’ thinking is affected by contact and collaboration with colleagues; that their work is not a matter of thought alone, but involves practical dimensions (including technological and rhetorical ones); and that scientific research has a crucial public dimension, since research scientists are not cut off from the interests of businessmen, politicians, and others from whom the scientists want things (such as funding, recognition and power) just as much as the latter want something from scientists (as Callon [1995: 5] put it: ‘We want what you want, so ally yourselves with us by endorsing our research and you will have a greater chance of obtaining what you want.’).

Formal logic occupies a somewhat distinctive place in these discussions, since it has been widely presumed to be a special mode of thought, which is purely abstract, has universal application, and is therefore insulated from material interests. Although the aim of SSK was to extend Mannheim’s sociology of knowledge to all forms of knowledge, in practice, formal (mathematical or logical) knowledge has still frequently been seen on the side of what cannot, or can only partially, be opened up for sociological analysis. The few attempts to study formal logic empirically have mainly focussed on denying logic the role of a universal cross-cultural standard of rationality. Arguments to this effect have often centred on Evans-Pritchard’s characterization of the rationality of the
Azande (Evans-Pritchard, 1936; Winch, 1964; Wilson, 1970; Bloor, 1976; for a recent review see Greiffenhagen & Sharrock, 2006). Apart from this, logical formalisation itself has largely been marginalised relative to investigations into experimental scientists’ practice. Up until now, that is. Rosental's study finally manages to demystify logicians’ work in the same way that idealised images of laboratory research methods were demystified by the early ‘laboratory’ and ‘controversy’ studies.

Rosental’s central aim (and achievement) is to attack the picture of formal logic as especially congenial to the formally rational mind, its truths being universal because they should be self-evident to any rational individual. He counters that formalised logical arguments are (at best) evident only to the extensively prepared mind. That is to say, whether a proof is valid or whether a statement is a logical truth are neither immediately clear nor decisively resolvable, even to those who are already steeped in formal logic. That some undergraduate students jib at the most elementary formalisations and that trained professionals can’t resolve to their mutual satisfaction whether something is a logical truth demonstrate that the intrinsic self-evidence of formal logic is only a myth. ‘Self-evidence’ has to be achieved, i.e., ‘weaved’, and will be achieved only relative to the competences, understandings, and affiliations of particular audiences.

One of the strengths of Rosental's study lies in having identified research sites (the classroom and the discussion forum) in which logicians’ activities are readily accessible and intelligible to a sociological audience with restricted familiarity with logical formalisation. In both settings, a variety of actors come together to discuss logic. Due to the heterogeneity of actors (in the classroom novices and experts, in the discussion forum actors from different disciplines and advocates of different approaches to logic), the discussion is often conducted in ‘vernacular’ terms and is thus relatively easy to understand by anyone unfamiliar with formal logic as such. Furthermore, both settings involve discussions ‘about’ logic (its status, usefulness, correct definition, etc.); consequently, actors appeal to a variety of reasons in discussion. This enables Rosental to instruct his audience about the preferences, concerns, and value systems that formal logicians exhibited in these debates. As a result, he is able to show that academics working in formal logic behave in many respects like
those in other disciplines: they are prone to take sides, apt to get into arguments, and interested in getting their work published and in advancing their own ideas and careers.

Another strong point of Rosental’s study is that he does not succumb to the all-too-often-employed sociological supposition that actors are either mystified by their own practices or are mystifying the public with misrepresentations of their work. Neither are Rosental’s actors ‘judgemental dopes’ (Garfinkel, 1967), nor does his description “unveil” elements that the actors would have preferred to conceal’ (p. 260). Rather, his arguments are predominantly addressed to those, particularly in the social sciences, who know little about formal logic and are therefore possibly exposed to the risk of being misled by idealised pictures of logic. As he puts it: ‘It is quite conceivable that the results of this study may appear surprising to readers who have never been directly involved in research in logic or mathematics’ (p. 260).

Clearly the biggest contribution of Rosental’s study is to finally have opened up formal logic for empirical investigations, by conducting the first book-length study of important aspects of the work of professional logicians. Of course, Rosental only looks at one particular case – and a rather special case at that. Not all theorems in formal logic result in such a heated debate as the one sparked by Elkan’s paper. As Rosental (p. 1, citing Stanislaw Ulam [1976]) notes, it is estimated that one million theorems are produced worldwide every five years – far too many to be debated on the scale of the controversy over Elkan’s paper. Consequently, more exciting work remains to be done. For example, it would be interesting to see how Rosental’s sociological approach could be applied to what logicians are doing when they are in their ‘laboratories’, i.e., when they are not conversing with colleagues but working alone in their offices on technical matters. Also, it would be fascinating to extend Rosental’s approach to formal logic from the ‘extraordinary’ case of Elkan’s paper to logic’s equivalent of ‘normal science’. Questions about the normal forms of logicians’ working practices surely now deserve to be asked and answered.
Notes

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References


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