CERTIFYING KNOWLEDGE: THE SOCIOLOGY OF A LOGICAL THEOREM IN ARTIFICIAL INTELLIGENCE

CLAUDE ROSENTAL
Institute for Advanced Study, Princeton

This study examines the concrete modalities of the production and recognition of a specific logical theorem in the field of artificial intelligence in the 1990s. Ethnographic *observations, interviews, and textual analysis, reveal the impact of a heterogeneity of practices of evaluation and other forms of interaction between the author of the theorem and a varied community of interlocutors, especially during the draft stage of the theorem. Individual and collective representations of the theorem were structured by the proliferation and polysemy of its reformulations, by the imperfect access to proofs and counterproofs, and by the coordination of action within opposed groups. The stabilization of debates over, and the certification of, the theorem were not based on the simple victory of one side over another but on relatively unified responses by critics and by the author's responses to critiques—responses that tended to allow for multiple interpretations. This ethnography of logic in development illustrates why sociologists should not consider logic to be just a methodological tool; it is also a privileged object that enables exploration of the material and social forms of intellectual work, including the building of credibility.*

WHILE DURKHEIM ([1912] 1990: 616–17, 625–26) held science, and especially logic, as an object of sociological investigation par excellence, his observation concerning the rarity of relevant empirical research on logic,[1] designed to lay the foundations of sociological analysis as such, is still relevant almost one century later. My goal is to contribute to the exploration of a world of practices widely ignored by social scientists: I analyze the concrete modalities of the production and certification[2] of a particular logical theorem in the field of artificial intelligence in the 1990s. I investigate, in particular, how this theorem was collectively accredited[3] in practice—what "recog-

Direct correspondence to Claude Rosental, Institute for Advanced Study, School of Social Science, Einstein Drive, Princeton, NJ 08540 (rosental@ias.edu or rosental@ehess.cnrs-mrs.fr). I am indebted to Joan Scott and Debra Keates for their suggestions on early drafts of this paper, and I thank the ASR Editors and anonymous ASR reviewers for their suggestions and criticisms. I also thank Liz Libbrecht for her help with English, and Charles Elkan for making this research possible.

[1] If "sociology" should not be equated with "philosophy," neither should "logic" be equated with "mathematics." The overlap between these sciences is limited: They have in particular different research objects, histories, and institutional settings (also see Mackenzie 1993:49–50).

[2] Even though the notion of "certified knowledge" has been long employed by sociologists of science (e.g., Merton 1942), the various and complex realities to which this term might refer deserve more study. Shapin and Schaffer (1985) have shown, for example, how certification had a privileged link with "witnessing" in a specific sociohistorical context. I hope to contribute to the investigation of the possible meanings of "certified knowledge" and therefore will not start from a closed definition.

[3] Latour and Woolgar's (1986:208) notion of accreditation originally applied to persons and offered an account of how some individuals are accepted in the economic cycle of facts' production. I extend this notion here to statements: An

nition" it and its author received.[4] I draw on the results of empirical work combining ethnographic observations, textual analyses, and interviews. I successively consider the different stages in the emergence of the theorem, from its earliest drafts, to its first publication, and then to the author's writing of several new versions in response to the critiques it triggered.

I begin by examining representations of "logic" in the social sciences to show that logic should not be considered to be a static methodological tool. I argue that logic itself is also a proper object of sociological inquiry and ethnographic observation, and a privileged source of data for exploring the material and social forms of intellectual work, such as the building of credibility.[5]

The theorem studied here was first presented in a context of strong competition between proponents of various approaches to artificial intelligence, and it was the starting point of a critical article concerning the "paradoxical success" of one of the branches of artificial intelligence (fuzzy logic). As such, it generated a major controversy in this field. From the first reactions to the theorem and the strong critiques that followed emerged a few dominant points of view. How the debates stabilized and how the theorem was eventually accredited, are key aspects of my investigation.

Let me begin by demonstrating just how much logic—the object of this research—has been disregarded by the social sciences.

LOGIC AS SEEN BY SOCIAL SCIENTISTS

Apart from some circles of sociologists who specialize in the study of science, logic is often viewed by the social sciences as a method likely to guarantee the coherence of analyses (Passeron 1991:154–60), or to reveal their structure (Berthelot 1996), or to determine the rationality of observed behaviors. Apart from some noteworthy exceptions (e.g., Naville 1982), the representations of logic that one encounters in social scientific literature commonly date either to the nineteenth century or back to Ancient Greece. They are comparable to logicist approaches first denounced then largely abandoned by logicians in the early twentieth century. Logic is presented here as a stable rather than a dynamic instrument, structured by the existence of a small set of unchanging and ideal "principles" or "rules" easily invoked and seldom questioned. Reference is most often made to syllogisms (Rosental 2002a). This static view of logic is one reason why it is rarely perceived as a possible object of sociological inquiry and empirical investigation. But other reasons exist.

First, like intellectual work in general, logic is often perceived as an activity in itself essentially immaterial. As several authors have pointed out, social scientists find it difficult to analyze processes of abstraction in their material dimensions (e.g., Brian 1995; Hutchins 1995; Latour 1996). Also, the production of logical statements is commonly apprehended as an essentially solitary process, the result of purely individual cerebral activity, and therefore unobservable by the sociologist.

Second, various forms of idealization and stylization of logical activity have been developed in philosophy. These forms have probably managed to seal a genre such that the question of logic is conceived in the form of a philosophical dissertation, of the study of texts by authors as mediums for ideas, or of analysis of the relationship between a *typical knowing subject* (not a multiplicity of *social actors*) and the objects of logic (e.g., Husserl [1901] 2001).

Third, as Lynch and Bogen (1997) have pointed out, "core" sociology textbooks de-

"accredited statement" is a statement that may be accepted in this economic cycle.

[4] The term "recognition" is taken here as a black box (Latour 1987:131). Its meaning has to be provided a posteriori, instead of limiting it a priori to issues of rewards, for example (e.g., Hagstrom 1965).

[5] As Latour and Woolgar (1986:194–98) have pointed out, some sociologists of science have used the term "credit" to denote reward, instead of linking it to an economy of facts' production that implies various forms of investment. Insisting on the fact that the receipt of personal rewards is not the ultimate objective of scientific activity, Latour and Woolgar have introduced the term "credibility" to substitute for "credit." I adhere to this distinction.

vote no attention to the methodological implications of recent sociology of science. Little or no space is devoted to the sociology of scientific knowledge in general texts on theory and method. This is unfortunate as there is work conducted by sociologists of science and mathematicians that depicts mathematical work, and also, more rarely, logical work, as a collective and "social" activity.

Since the end of the nineteenth century (e.g., Bloor 1976; Erdmann 1923; Sigwart 1889), some authors have been devoted to *theoretical discussions* of an anthropology or sociology of mathematics and logic.[6] Several historical case studies on *mathematics* also have been conducted from a sociological prospective, although they were based on textual analysis. These works are certainly not cumulative: In them, the term "social" takes on differing meanings, and the corresponding programs of research go in different or opposite directions without generating real debate (Rosental 2003:33–79). Here, I briefly evoke several of these lines of research to position the present study within a set of disparate works.

Some authors, like Bloor, have been particularly interested in exhibiting the lack of consensus on mathematical statements and conceptions and pointing out their variations over time and between societies or other institutional settings. Pushing Lakatos's (1976) critique of "formalist philosophy" further, Bloor (1976, 1981, 1982) argues that formal reasoning holds only in relation to the particular institutional configurations in which it develops. Mackenzie (1993, 1999) has studied historical cases where mathematical notions were locally negotiated or debated; his case studies relating to the relatively recent rise of formal computer methods have led him to conduct both text analyses *and* interviews.[7]

Other authors have not focused on controversies and have defended other views. Some, like Pickering and Stephanides (1992:139–67) on the case of the development of Hamilton's theory of quaternions, have argued that symbolic manipulations are in part objectively constrained and in part determined by the social game in which the mathematician participates.[8] In his study in eighteenth-century France of the crossing point between mathematical calculus and administrative counting that a century later would become the science of statistics, Brian (1994) argues that mathematicians at the Royal Academy of Science were able to configure their objects *and* institutions at the same time through the use of decomposition practices.

Another form of research that does not look at institutional causality focuses on textual resources of mathematical practice. In his careful analysis of Gödel's proof, Livingston (1985) adopts an ethnomethodological stance that takes the actors' competent practice as the ideal metalanguage to describe their activity (also see Livingston 1999). Semiotic approaches offer a still different view of mathematical practice. For example, Rotman (1993) argues that mathematical texts define a triple body of the mathematician in a transhistorical and ideal way.[9]

If most of the previous studies essentially rely on textual analysis rather than on ethnographic observations and apply to mathematics rather than to logic (also see Collins and Restivo 1983; Gingras 2001; Restivo 1992; Restivo, Bendegem, and Fischer 1993; Warwick 1992), I argue that ethnographic *observations* of *logic in action* are indeed possible.[10] Through the very performance of

[6] Such is also the case of Latour's (1987) program of anthropology of "formalism." For Latour, formalism does not simply refer to logic but to material inscriptions generally, *including* logical inscriptions. Hence, based on observations, he has conducted empirical studies on formalism, if not on logic itself.

[7] From a cognitive point of view, the evolution of the material conditions of (a limited part of) mathematical production linked to the development of computers and related techniques of theorem-proving has indeed introduced considerable opportunities for social scientists in the past years to *conceive* inquiries that would not be exclusively based on textual analysis (in particular, see Mackenzie 1993, 1995, 1996, 1999; Rosental 1993, 2002b).

[8] According to Gingras (1999), Pickering's posture resembles a Piagetian approach (also see Pickering 1995, 1999).

[9] For another semiotic approach of mathematics, see Coleman 1988.

[10] Some writings of sociological inspiration by

this study, I challenge the view that logical activity is necessarily unobservable by the sociologist because of its immateriality or cerebral embeddedness. I also show that scholars working in the field of logic should not be treated as homogeneous subjects defined only in relation to their texts but as fully fledged social actors. Nor should their views be seen as simply determined by stable institutional configurations but rather as contributing to the (re)shaping of the social, as in the case studied by Brian (1994). Furthermore, I bring into light the fact that logical activity is not solely a textual practice, contrary to the view of mathematics in previously cited semiotic and ethnomethodological approaches.

Finally, I argue that logical debates should not be treated as simple exchanges of *arguments*. This point is generally not made clear in studies of mathematical controversies based on textual analysis because textual points of view are opposed to one another by the analysts in a relatively homogenous way. Lakatos's (1976) presentation of the controversy around Euler's theorem, in the form of imaginary dialogues between mathematicians, provides an extreme example of such a treatment. I challenge this possible representation by exploring in detail the material economy of scholarly exchanges and the very mediations[11] of such debates. I also take up a number of related issues such as: What does the expression of a point of view in the public and private spheres mean and require for the actors? Who has the resources to express him or herself, and in which cases? What individual and collective representations emerge from such interventions in the debate and how? Who reads what, how, and why? How are symbolic languages appropriated, and do they generate univocal readings of proofs? As a result, do the actors necessarily reach a consensus at a certain point through a simple victory of one side over the other (e.g., Latour 1987:1–100)? Or are misunderstandings possible, and can these misunderstandings contribute to the building of specific forms of agreement?

I now turn to the heart of the matter: an analysis of the dynamic of certification of a logical theorem.

ETHNOGRAPHY OF A THEOREM

The theorem analyzed here was formulated in the early 1990s by an assistant professor at the University of California San Diego, Charles Elkan. It was published for the first time in July 1993 in the proceedings of a large annual conference in the United States on artificial intelligence, the AAAI 1993 (Elkan 1993). Elkan's paper denounced the "paradoxical success" of electronic and computer applications of a logical theory called "fuzzy logic." His denunciation was based on the proof of a theorem stating that fuzzy logic, characterized by a system of four specific axioms, is in fact nothing but classical binary logic.[12] The author pre-

mathematicians might be considered as already offering promising results regarding this issue, even if they addressed *mathematical* (and not logical) production. Indeed, these texts have offered sources of nonstylized descriptive elements on concrete modalities of mathematical activity based on personal experience or on the systematic observations of insiders (Davis and Hersh 1987; DeMillo, Lipton, and Perlis 1979; Fisher 1966, 1973).

[11] I take the term "mediation" from the sociology and social history of art, and more particularly from the sociology of music that studies medias (Hennion 1993). The history of music seems to encounter the same difficulties with so-called immaterial objects as does the history of logic and mathematics. I use the term "mediation," as opposed to "intermediary," to account for all resources that might be considered as in-betweens (texts or instruments, for example) in the production of knowledge or art, and to reveal them as proper beings.

[12] The expression "classical binary logic" refers to the fact that all propositions are considered either true or false. In other words, the propositions may be assigned one out of only *two* truth values (true or false). Here, "$\wedge$" represents the logical connector "and," "v" represents the connector "or," "~" represents the connector "no," and "t( )" is the truth value of the assertion in parentheses. The four axioms mobilized in Elkan's proof are the following: (1) $t(A \wedge B) = \min\{t(A), t(B)\}$, (2) $t(A \vee B) = \max\{t(A), t(B)\}$, (3) $t(\sim A) = 1 - t(A)$, and (4) $t(A) = t(B)$ if A and B are logically equivalent. The theorem states that within the formal system defined by these four axioms: "For any two assertions A and B, either $t(B) = t(A)$ or $t(B) = 1 - t(A)$" Elkan 1993:698). Elkan immediately inferred that fuzzy

sented this result as a direct challenge to one of the founding ideas of fuzzy logic, which is supposed to allow for the expression of an infinite number of degrees of truth along a continuum with poles "true" and "false" (like "half true" for example).

During the 1991–1992 academic year, like Elkan I was working at the University of California–San Diego. While setting out to find relevant empirical material to contribute to a sociology of logic, I observed that the activity of logical production was neither immaterial,[13] nor individual. I confirmed this observation by further ethnographic investigations conducted until 1994 at sites such as Harvard University, MIT, Stanford University, and UC–Berkeley, in the departments of mathematics, philosophy, computer science, and cognitive science, as well as at conferences' sites. Logical activity was carried out in conference rooms, in laboratories, and in front of computer screens. It involved the creation of working groups, the organization of seminars, engagement in multiple interactions, and an intense practice of writing.

It was during the first phases of this research that I met Elkan. He was a member of the computer science department—a discipline that brings together a large number, if not the largest number, of producers and manipulators of logical formalisms in his generation. He told me about his intention to publish a theorem showing the contradictory nature of fuzzy logic, a project already supported by an initial draft proof. In parallel with other field work I had already begun on the dynamics of the production of certified knowledge,[14] I started to study Elkan's work, the development of his oral theses, and his writings at a time when his article was still in the draft stage. This research view enabled me to relate the writing of his text and its amendments to his accounts, his talks (given either in private or in research seminars), and to the study of his daily work—especially to the interactions in which he was involved.

For the next phase I investigated the modalities and consequences of the rejection and acceptance of his paper at major conferences on artificial intelligence. I studied the methods for selecting articles for these conferences. I relied again on ethnographic observations and interviews conducted inside and outside the sites of the conferences. I also examined texts produced for the conferences (such as brochures and programs).

As soon as it was published, Elkan's article attracted a great deal of attention in the research community. Initially, points of view were exchanged in an Internet forum devoted to fuzzy logic called comp.ai.fuzzy. Its electronic archives allowed me to reconstruct the "discussions." I also had access to the content of private mail and held interviews with the actors involved. The debate subsequently shifted to specialized journals. One year after the theorem was first published the "discussions" stopped. My investigations of this "last" period therefore rely more heavily on analyses of texts and interviews.

Between autumn 1994 and winter 2001, a systematic bibliographical search led me to traces of comments on Elkan's article that supplemented citations in comp.ai.fuzzy and in specialized journals. A few articles also were devoted to continuing the debate

logic, characterized by these four axioms, is "in fact" a logic with two truth values (true and false). For an introduction to the "technical" notions introduced here, see Rosental (2003:86–105).

[13] For ethnomethodological arguments converging with this view, see Livingston (1985).

[14] The documentation of this process thus relies on relatively large-scale investigations. Although not all collected data were needed to grasp this specific process, much of them were directly or indirectly useful, in particular to "know" the "cultures" and fractures of the research communities involved. The investigations included ethnographic observations and interviews, especially within the communities of fuzzy logic researchers, probabilists, logic, and artificial intelligence researchers at large (on the West Coast and in Cambridge, Massachusetts, as previously mentioned). Around 200 interviews were conducted (face-to-face or by e-mail); seminars, meetings, and some interviews were recorded, totaling 150 hours of audio and video tapes; around 1 000 pages of field notes were taken; I also relied on the analysis of numerous books, articles, reports, drafts, brochures, and (private and public) electronic archives of correspondences, whose sheer volume is hard to evaluate all together.

(Elkan 2001; Trillas and Alsina 2001a, 2001b), but on the whole the controversy was clearly running out of steam. The arguments put forward were not really new and triggered no new "outbursts" of reactions. An analysis of the context of citations of Elkan's article provides some evidence that the representations of Elkan's theorem have hardly evolved since summer 1994.[15] To date, the outcome of the process, as described here, appears to have "lasted."

How did Elkan's theorem emerge and gain access to a possible status of certified knowledge? Before addressing these questions, I should clarify that the process I study is obviously not universal—it is not identical to that of any other emerging logical theorem. Yet a study of the characteristics of this process is an excellent laboratory for a whole range of probative mechanisms encountered in other fields. Moreover, studying this *particular* case highlights the limits of a number of *general* representations of the modalities of knowledge production in the domain of logic—representations that, as I will show, are highly reductionist.[16]

THE ROLE OF INTERACTIONS AND COMMON ATTITUDES

Elkan's theorem was not the fruit of solitary work by an isolated author. Its formulation stemmed from Elkan's circulation in an academic community that denounced—most often orally—the limited value of fuzzy logic. The author's interactions with academics in that community appear to have played a major part in the progressive elaboration of his article and in the solidification of his conviction about the accuracy of the first version of his theorem, even before he considered himself ready to submit it for publication.

Elkan was, as I said, doing research in artificial intelligence. Pro-logician and anti-logician schools constitute this domain (Graubard 1988); in this context, he could be said to have "adhered" to a pro-logician culture whose favorite research objects and tools were so-called "first-order logic," also called "classical logic," and "nonmonotonic logic" (Elkan 1991, 1992a, 1992b). Elkan could thus be described as an advocate of what was commonly called classical AI, as opposed to those theorists interested in the upsurge of rival theories such as fuzzy logic, neural networks, or genetic algorithms that offered alternatives to the methods developed by the "classical" AI schools for elaborating electronic and computer products (Bouchon-Meunier 1993; Cowan and Sharp 1988; Dubois and Prade 1980; Mitchell 1996).

Elkan worked in a research milieu whose most famous representatives taught at Stanford University and at MIT. These academics denounced the limited value of alternative theories and methods that were also an increasing source of competition for available research resources (industrial and military grants, university posts, etc.). Univocal denunciations by the members of this milieu were usually expressed orally during chance meetings at the university or in seminars; they frequently took the form of jibes or witty remarks intended to attract attention in public. "Classical" AI researchers generally mobilized similar types of argument: For example, they would assert in some way or other that texts on fuzzy logic were "incomprehensible," or that it was a waste of time studying them or trying to talk to their authors. The following passage from an interview with a famous researcher in "classical" AI illustrates this attitude :

> Now with regard to fuzzy logic, I have to admit a reaction to it which is similar to the composer Rossini's reaction to Wagner's operas. Somebody asked him what it was, and he said this is a work one can't make a decision about for the first year, and I have no intention of listening to it for more than

[15] The analysis could be conducted thanks to Nec Research Institute's Citeseer Search Engine (http://citeseer.nj.nec.com) and the Science Citation Index.

[16] The concrete modalities of emergence of logical theorems have not been documented enough to allow me to say more about the issue of the "representativeness" of the case. Although debates around a few major *mathematical* theorems have been studied (e.g., Goldstein 1995; Lakatos 1976; Mackenzie 1999), it appears that comparing their similarities and differences would not be much more useful for that matter, especially if one relies on a recent estimate that evaluates the world production of mathematical theorems at around 1 million units every five years (Ulam 1976).

a year. So I worked a little bit in fuzzy logic and this discouraged me from looking at it anymore.

Moreover, as soon as the opportunity arose, the scholars of this opinion expressed their refusal, on other grounds, to finance projects on fuzzy logic, or to publish writings, to sit on a Ph.D. jury, or to recruit a researcher in this area. Given such practices, interactions with researchers on fuzzy logic were reduced to a minimum, and their writings appear to have been largely ignored, although they constitute an abundant literature (on production in the 1970s, see, e.g., Gaines and Kohout 1977).

Elkan, deeply involved both "intellectually" and "professionally" in the so-called classical AI community, was imbued with these attitudes toward fuzzy logic when he considered writing a first version of his article. He also had a convinced readership. After having read some articles in journals devoted to fuzzy logic—one that was an article by Kosko (1990), a researcher in fuzzy logic, and one that praised a critique of fuzzy logic (Elkan said he "already knew" at that stage which of the two points of view was "the right one")—and having participated in a conference attended by several researchers in this field (Elkan qualified some of the papers delivered as "incomprehensible"), he was convinced there were intrinsic limits to the theory of fuzzy logic. He noted during an interview that fuzzy logic "applications" might function well but that functionality should not be ascribed to fuzzy logic as such.

Elkan gathered his reflections on these interactions and readings in a written draft, which he amended as the months went by. This paper consisted primarily of a proof of a theorem that, as it was written in the last quarter of 1991, stated that fuzzy logic, characterized by a system of four specific axioms, was a logic with only one truth value (not two as in the first, and later, published version of this text). According to Elkan, this showed the contradictory nature of fuzzy logic.

Elkan's "negative" representation of fuzzy logic and the absence of elements enabling him, at least initially, to identify a flaw in his proof when rereading it (what Davis and Hersh [1987] have called "the lack of counter-evidence") led him to conclude that his theorem was accurate. Elkan then submitted his paper to an "eminent" colleague working in the field of probability theory, and his colleague found no errors. According to Elkan, this colleague even added that he had suspected that such a result could be demonstrated and that Elkan's proof had confirmed his doubts about fuzzy logic. This reaction could only reinforce Elkan's conviction that his theorem was correct.

It was only later, in the late stages of a publication process, that he submitted his theses to other interlocutors (see his acknowledgments in Elkan [1993:702]) and that he became convinced that the very last step of his proof was wrong. This prompted him, in the process of amending the proof, to formulate a substantially different result—namely that fuzzy logic, characterized by a system of four specific axioms, has a total of *two* truth values—not just one.

His peers' attitude of patent and avowed disregard for the abundant literature on fuzzy logic visibly contributed toward the stabilization of this version of Elkan's theorem. It allowed him to be confident of his assumptions and hardly encouraged him to question himself on the representativeness of his readings in regard to the characterization of fuzzy logic that he had adopted or to seek contradictory dialogue with researchers in fuzzy logic by submitting a draft of his proof to them.

Thus, the actual experience of the "correctness" of Elkan's theorem, for both its author and his interlocutors, appears to have been a matter of degree of conviction influenced by mutual reactions and upheld by attitudes amply shared by the group of actors within which Elkan was interacting. DeMillo, Lipton, and Perlis's (1979) thesis appears to apply in Elkan's case. According to the three mathematicians, the interactions in which the author of a proof is engaged play an essential role in the conviction that she or he can obtain of its validity. The author's conviction would be strengthened by third-parties' convictions. Elkan's case further illustrates and extends (to the case of a logical theorem) Lakatos's (1976) claim that the formal presentation of a result, despite the impression of everlasting stability it may provide, often hides a long process whereby the proof, the final and intermediate results, and the concepts involved are negotiated.[17] In Lakatos's view, informal thought always circumvents formal presentations, which in fact represent only "minutes" of those more detailed negotiations.

What, then, was the significance of the submission of Elkan's article for publication for the collective accreditation of his theorem?

HETEROGENEOUS EVALUATIONS

Elkan's (1993) article was the object of contradictory decisions (refusal then acceptance for publication) by two separate selection committees: first the organizing committee of a major conference on fuzzy logic in the United States; second, the committee of the AAAI 1993 conference. This result stems primarily from the fact that the selection processes were not based exclusively on the direct application of straightforward universal criteria for the validity of proofs;[18] had this been the case, Elkan's theorem would have been evaluated in only one way. The modalities and significance of the outcome of these processes were far more complex and were indeed completely opaque to anyone who had not followed them in detail.

The selection of contributions to conferences involved a large number of evaluators—with a ratio of as many as 170 reviewers for the 270 texts submitted in the case of the organization of a preceding major conference on fuzzy logic. Selection then could be described as the product of a specific *sequence* of decisions by groups of actors. For a given article, the outcome of the selection process essentially depended, as in other areas of scientific research, on reviewers' choices and know-how, the topics chosen for the different sessions (which may not have corresponded clearly to the text), quotas chosen by program committees for each session, representations of the type of publication to select (which were often heterogeneous), and elements of assessment that were constantly the object of creative work, tradeoffs, hesitations, and uncertainties among the reviewers.

A closed and final list of several factors cannot account for the complex, uncertain, and creative character of this evaluative work. For the reviewers, it consisted in *seeking*, often on a case-by-case basis, several *decisive* criteria on which to base and to justify their decisions. Often the idea was to avoid inexpressibility, the vocabulary of taste, or an observation of irrationality or relative arbitrariness of choices, and to replace these types of responses with registers of justification likely to win the acceptance of their potential interlocutors (members of the program committee and sometimes authors, in cases of objections to decisions). Some members of the program committees responsible for assigning articles to the various evaluators said that they chose reviewers for their "capacity to say something," even if they were not always considered to be experts in understanding and evaluating papers in detail, considering the extreme specialization of the knowledge concerned.

But apart from the diversity characterizing the evaluation of texts in general, a number of other specific elements at least partly explain why Elkan's article was refused for the first conference and accepted for the second. These conferences, which brought together academics, researchers working for universities or high-tech firms, engineers, industrialists, and scientific journalists, were organized for various purposes. One major consideration was to allow actors working throughout the world in the same subspecialty, whether related to essentially "theoretical" work (e.g., mathematical theory of fuzzy sets) or to the development of a particular type of device via different methods (e.g., the construction of pattern-recognition software), to meet one another and to interact. But this gathering simultaneously afforded the possibility for the founders of a research domain to demonstrate the fertility and "universal" nature of one or more theo-

[17] For a comparison with the field of biology, see in particular Myers (1985).

[18] The term "validity" has several meanings in logic, as well as in the history and philosophy of logic, depending on the authors. But it commonly refers to the issue of knowing whether a result "follows" by "correct rules of inference from the premises."

ries considered to be representative of the approach they had helped to forge (e.g., fuzzy logic or nonmonotonic logic in "classical" AI) through the entire range of its "applications."

Thus, such meetings were at once places for the *gathering* of actors sharing an interest in a research area, in tools to facilitate their interactions and exchanges, and in resources for the organization of *demonstrations of strength* (Mukerji 1997) celebrating the virtues of particular theoretical approaches. For this reason, as far as the conference organizers were concerned, some conferences had to be relatively "unselective" (that is, with a refusal quota about one out of two papers) and "representative" of the extreme diversity of research and achievements in the field(s) in question. Elkan's paper, which was neither a theoretical nor a practical *contribution* to fuzzy logic, was unlikely to be selected for a major conference on that topic. Moreover, the chairman of the program committee understood Elkan's theorem as conveying a basic error: It mobilized an axiom equivalent to the principle of the excluded middle[19] in order to describe fuzzy logic. For the chairman, this error was comparable to an attempt to demonstrate the impossibility of non-Euclidean geometry by inadvertently introducing into the proof an axiom of Euclidean geometry. As a result, Elkan's paper was categorically rejected.

By contrast, AAAI's organizers were essentially proponents of "classical" AI methods. This factor is important to understanding why Elkan's paper was accepted at AAAI 1993. Indeed, this conference took place in a context of very stiff competition on various fronts between proponents of a number of so-called classical AI approaches and those of fuzzy logic. There are few explicit written accounts of this particular competition. Some messages posted in the electronic forum devoted to fuzzy logic (comp.ai.fuzzy) addressed this issue. The following extract offers a good example of such accounts (page numbering corresponds to that adopted on the forum):

[19] The law or principle of the excluded middle is generally defined as, "every statement is either true or false."

> Of course there is anti-fuzzy, anti-Neuralnet, anti-GeneticAlg, anti-HotNewMethod feeling in the AI community, but it is neither a bigoted nor a hidden bias. Underanalyzed, it may look like a "Not Invented Here" syndrome. (J. Pollack, article 856).

As a result of the competition between AI subfields, very few papers on fuzzy logic were delivered at the AAAI. Indeed, it is easy to imagine that the proponents of "classical" AI methods in charge of the organization of the AAAI were not inclined to promote their competitors' approaches by publishing their articles. Basing their behavior on previous bad experiences, fuzzy logicians did not even send their articles to the AAAI as a general rule—their infrequent attempts to submit papers rarely led to publication. This situation was in part pre-orchestrated by a powerful device: When submitting a paper at the AAAI, authors had to indicate the session in which their text might be presented; in so doing, they had to choose a session category within a list which did not even contain "fuzzy logic."

In the year that Elkan submitted his paper, in the heated context of subfield competition, conference organizers who did not perceive a flaw in the theorem not only published it, but promoted it by awarding it a prize. The paper was listed as one of the four best articles at AAAI 1993.

The acceptance by AAAI of Elkan's theorem and its rejection by the fuzzy logic conference, thus brought into play a set of mechanisms, competencies, considerations, and (partly contextual) decisions that differed from one forum to the next. The selection of articles did not amount to a mechanism for identifying theorems that any reader could apprehend as being true (or, if not selected, false).

It seems that these phenomena were not specific to the two conferences in question. Indeed, in the field of mathematics, according to Davis and Hersh (1987), *any* article submitted to a journal can be refused or accepted. Its fate depends on the nature of the reviewers concerned. Decisions on the satisfactory nature of a proof, on that which is problematic, and on that which is interesting, are based on "corporatist" expertise that is often inaccessible to individuals outside a subdiscipline. An author's reputation, her or his reliability and originality, and the reviewing committee's representation of the research field covered by the journal, also are key elements in the acceptance or rejection of an article, especially since reviewers generally do only partial checks and trust the author regarding the rest of the proof.

In the case of Elkan's theorem, the actors working in the research fields concerned (and especially those who had participated in the selection of papers) were fully aware of the complicated meanings of a paper's rejection or acceptance. Thus, depending on how much they knew about the dynamics of evaluation for either the conference on fuzzy logic or for the AAAI 1993, and depending on their methodological options (which could prompt them to state that they could "guess that such a theorem could be proved" or, in contrast, lead them to think that such a theorem "could only be false"), they could interpret publication of Elkan's theorem and the prize awarded to him very differently. For some these events were signs, considered more or less certain, of the theorem's value and relevance; for others the events were seen as overt antagonism toward fuzzy logic. The following message from comp.ai.fuzzy illustrates the later point:

> It is clear from papers such as Elkan's that not enough is being done in certain forums to assure that papers dealing with fuzzy logic, either pro or con, are subjected to a fair and competent review process. Many technical conferences and meetings are, in the views of many in the fuzzy-logic community, unfairly hostile to fuzzy logic, while being, on the other hand, ready to accept the work of skeptics with nary an effort to determine its value. (E. Ruspini, article 816)

In any case, the prize awarded to Elkan's article triggered a series of strong reactions from researchers in fuzzy logic and generated debate in several quarters. As soon as the article was published, comp.ai.fuzzy became the main locus of an exchange of opinions on Elkan's theorem for a period of six months, until these first reactions were consolidated in the form of articles published by specialized journals. What were the first representations of Elkan's theorem that emerged from debates on comp.ai.fuzzy during that period?

FROM THE ORIGINAL PROOF TO ITS SUBSTITUTES

Authors and readers of messages posted on comp.ai.fuzzy were, for the most part, academics, researchers, and engineers working for universities and industrial organizations in the fields of fuzzy logic and "classical" AI. Precisely due to its absence of selective or editorial constraints, the forum helped to open relatively broad debate within this specific gathering, allowing the public expression of many points of view. This openness was also reflected by the fact that e-mail messages were essentially rough texts written, for example, during a "break" of a few minutes.[20]

In this framework, what was the nature of the interaction around Elkan's theorem? First, the author was accused of errors: The exact significance and scope of his theorem, as well as the nature of the hypotheses mobilized in his proof, were criticized. As time went by, the messages revealed that Elkan's article was difficult to access and/or seldom read, as the following examples indicate:

> Elkan's argument is a straw horse, from what you have said of it. . . . I would like to see what he has to say. . . . So where can I get this document? (J. Wiegand, message 767)

> Is Elkan's paper on the reducibility of Fuzzy Logic available on the net? Thank you for your help. (Allen, message 1778)

There was then a proliferation of "abstracts" and "presentations" of the theorem and its proof designed to support critiques. Although they were presented as faithful substitutes for Elkan's productions, in reality they were all reformulations of the theorem, constructions of *new* demonstrative mechanisms, often elaborated from messages displayed previously in the forum—in other words, from secondary sources. Saying what the theorem and its proof consisted of, or judging them by writing a review on the subject, were therefore basically the same exercise and amounted to the production of new proofs.

[20] On this point, but also for further reading on the social uses of the Internet in research, see Rosental (1998a).

Points of view were often drafted by using passages from previous messages and by inserting comments in the course of a step, in a proof, or in a counterproof. This mode of writing spawned collective texts consisting of several levels of citation and proof sequences. It also led to explicit disagreements, for some participants wrote that they failed to see at such-and-such a step what others had shown or claimed to see.[21] The extract of the following exchange clearly illustrates this point:[22]

>>Zadeh's logic is distributive and, there-
>>fore, the roots of Elkan's mistake do not
>>lie on distributivity but on failure of the
>>law of the excluded middle (as correctly
>>pointed out by Kroger).

Well *let's look at this again.* We can simplify Elkan's proof to the following: Starting from Gaines axioms t(true) = 1, t(not A) = 1 – t(A), t(A or B) = max(t(A),t(B)), t(A) = t(B) if A and B are logically equivalent. We may consider t(A or not A) = t(true) by the last axiom => max(t(A), 1–t(A)) = 1 => t(A) = 1 or t(A) = 0. I think the problematic step is the first one, the use of the last axiom. *I don't see the excluded middle coming in here.* (H. Lucke, message 827, my italics)

What representations of Elkan's theorem resulted from this set of proofs?[23] As mentioned above, considering the material difficulties of obtaining access to Elkan's original article and the small number of individuals who actually read it attentively—a "difficulty" that resulted primarily from the fact that many participants spent very little time on these debates—it appears that representations of the theorem were often shaped by the "substitutes" proposed in the Internet forum. Some participants thus either strengthened or completely reversed their point of view on this theorem after reading earlier messages. The following extract of a message constitutes a good example:

>t(not(A and not(B))) = t(B or (not(A) and
>not(B)))

This statement was worrying me too. I was wondering if it really should hold in fuzzy logic. There are other statements in classical logic that don't work in fuzzy logic. It would seem the answer is no, it wouldn't. I wasn't sure if I was missing something though, but I think Loren's post illustrates the mistake in it. (S. Kroger, message 1823)

On the other hand, most of the participants, after reading those same messages, maintained positions based on antagonistic normative approaches as to what logic, fuzzy logic, a logical proof[24] or ways of discussing it were *supposed* to be.

Hence, certain participants expressed contradictory opinions on the Internet as to the usefulness or the need to maintain the law of excluded middle:

This doesn't make sense to me. I cannot imagine a scenario in which I was uncertain about the truth of (B or not(B)). (H. Lucke, message 813)

You're still thinking Boolean. . . . With fuzzy logic (if B or not B) has much more meaning than in Boolean (it's meaningless in Boolean). (Y. Tanaka, message 814)

Others highlighted norms for holding a debate, as in the following passage:

I did not reply to his [Elkan's] post since it was directed at an emotional level. (J. Wiegand, message 832)

In particular, sometimes highly stabilized approaches regarding priorities in the writing of proofs such as Elkan's—that is, re-

[21] On the sometimes major role of "those who have not seen" in science, see Ashmore (1993) and Schaffer (1992). On the fecundity of a sociological approach of apparitions for insight into other human practices, see Claverie (1991).

[22] The symbol " > ", generated by e-mail software in the formulation of answers to former messages, corresponds to quotation marks; a sequence of several symbols of this type corresponds to the same number of levels of citation.

[23] "Magma" would be in fact more appropriate than "set," taken into account its importance in Greek rhetoric. But developing this notion here would go beyond the scope of this article (see Cassin 1995).

[24] The fact that the notion of proof itself might be debated here is certainly not a singular fact: Mackenzie (1995, 1999) offers studies that document the evolution, variations, and debates around the conceptions of mathematical proofs. For a contribution to the sociology of proof in logic and in mathematics, also see Rosental (1993, 2003).

garding the most essential elements of proof—were then at play.[25] Although formal proof provides some instructions for its own reading, the heterogeneity of conceptions at work in the practice of logic, which corresponded to the heterogeneity of know-how with which the participants in the debate were endowed (apart from a minimum of shared competencies), worked against the replication of readings desired by the authors of these messages.

This interpretation might serve to provide a resource for understanding why logic (like mathematics) is even debated, a state of affairs hard to explain if one considers symbolic writings as such to allow for immediate consensus. It might also give meaning to DeMillo, Lipton, and Perlis's (1979) claims, by extending to the field of logic their observation that the vast majority of mathematical theorems are contradicted or disqualified—when they are not rejected due to doubts or simply ignored by mathematicians.

In any case, highly fragmented and contradictory points of view about Elkan's theorem and its proof resulted, especially with regard to questions of their validity, scope, and exact meaning. While certain authors affirmed that Elkan's theorem was correct, others stated that it was not. Various reasons were invoked, including his introduction of axioms inadequately characterizing fuzzy logic, the improper introduction of a principle of distributivity (of and/or) in the proof, the implicit and erroneous introduction of the law of the excluded middle, the inappropriate nature of the notion of logical equivalence mobilized, the introduction of erroneous numerical equalities, or the incorrect "injection" of binary values into statements about fuzzy logic.

After a few months, some few points of view began to acquire more visibility than others: those of Elkan and those of fuzzy logic personalities working in computer science, such as Didier Dubois and Henri Prade from the CNRS (France) and Enrique Ruspini from SRI (United States). Before they could express themselves in journals, Dubois, Prade, and Ruspini had put in token appearances in the forum to counterbalance the proliferation of representations of fuzzy logic, and of the theorem, that were too distant from what they considered to be reasonable or desirable. Several mechanisms were adopted by some researchers to make certain messages more visible than others: citations and frequent reappearance, with placement of some texts in a data base accessible via Internet, the electronic address of which was often displayed. The following extracts illustrate this point:

> Please read especially the enlightening and very well written evaluation by Enrique Ruspini (sent in the next mail). . . . Again, please read especially the enlightening and very well written evaluation by Enrique Ruspini (first message in this mail). (W. Slany, article 1823)

> Two responses to Elkan's paper, one by Enrique Ruspini and the other by Didier Dubois and Henri Prade, may be found as ftp.cs.cmu.edu:/user/ai/areas/fuzzy/doc/elkan/response.txt. (FAQ, article 2072)

Mechanisms such as these promoted differentiated visibility of messages; the texts displayed in the forum did not have the same impact on the shaping of representations of Elkan's theorem. The capacity for conviction that could be granted to these new proofs and counterproofs of the theorem was therefore not only *limited*; in the very frame of this material economy of access to texts, it was also *variable*.

Hence, the shaping of representations of Elkan's theorem in the context of the forum did not derive from a sum of individual homogeneous examinations of its proof obtained from attentive readings of an easily accessible text. Nor was this process reducible to an exchange of *arguments*, or even *discussions*, given the scriptural dimension of these interactions and the aforementioned material economy of access to texts. Finally, the process of dialectical evaluation to which Elkan's theorem had been subjected by no means resulted in a clear and uniform collective view on its correctness. Even the apparently simple question of the exact scope and significance of the theorem was the object of a multiplicity of representations.

[25] These debates are evidently not unrelated to the radical dissention that has marked the history of logic up to the present (e.g., Carnap [1934] 1937:51–52; Largeault 1993:21–23, 110–19).

Taking this situation as a point of departure, what were the consequences, from the point of view of representations of Elkan's theorem, of the continuation of debate in other forums?

POLYSEMY, DIFFERENTIATION, OR COORDINATION OF POINTS OF VIEW

In the eyes of many leaders in fuzzy logic, the publication of Elkan's theorem could have resulted in a substantial loss of credibility for their research domain. The following passage provides a good example of this concern:

> The impact of the paper should absolutely not be underestimated. The AAAI proceedings are very well read in the AI community, and I fear that this article will destroy some of the trust that has so far been given to fuzzy logic in the AI community. This is a very dangerous situation as it might entail academic hostilities between the AI and FL communities. (W. Slany, article 1823)

Several of these leaders rallied together to organize an "effective" counterattack on several fronts. In conjunction with their intervention in the electronic forum, a protest letter was addressed to the organizers of the AAAI 1993, and plans were made for the publication of several responses to Elkan's article.

Thus, several months after the conference, the center of debate shifted from the electronic forum to journals that specialized in artificial intelligence. This shift of *exhibited interaction* to other arenas was accompanied by a radical transformation in the time-scale of debates and a substantial rise in the barriers to be surmounted to "stay in the game." Making a point of view public now required authors to produce polished texts and to subject them to editorial constraints. It also required full investment in a milieu in which interindividual relations and reputations were essential in the processes of selecting (and often commissioning) articles, as Davis and Hersh (1987) have noted for the field of mathematics.

Some researchers in fuzzy logic already had extensive experience in facing criticisms. For many years they had endured a wide range of criticisms leveled at their research, to the point that they were able to compile what they called a "big collection of foolish quotations." They had thus become accustomed to defending fuzzy logic and to acting as its spokespersons, gradually building up real repertoires of counterproofs for both written and oral use. In the latter case, the repertoires were often supplemented with a store of witticisms and amusing anecdotes, thus echoing the jibes of fuzzy logic's critics. The reactions of this group to Elkan's article were based on already well-established know-how and wide experience with methods of counterattack.

Endowed with these specific competencies, several of the defenders of fuzzy logic united to formulate a response that would stand in deliberate contrast to the cacophony of messages in the electronic forum. The repetition, the constant attack on the same elements of counterproof, was a source of *discredit* to Elkan's theses but was at the same time likely to give the impression that all researchers in fuzzy logic stood against Elkan in a knee-jerk fashion. No fewer than eight major researchers in fuzzy logic, including Dubois and Prade, cosigned an article whose content was close to that of a message in the electronic forum, thus subjecting Elkan's point of view to the test of numbers in case their approach was not enough to marginalize it (Berenji et al. 1994).

Yet this unity among the stars of fuzzy logic was only apparent. The computer scientist Bart Kosko from the University of Southern California, author of the article that had served as a starting point for Elkan's formulation of his theorem, had not participated in the debates (at least not to my knowledge). He defended a definition of fuzzy logic that, although increasingly visible and supported by a growing number of proponents, was nevertheless challenged by other fuzzy logic researchers (Kosko 1993; McNeill and Freiberger 1993a, 1993b; Ruspini 1993a, 1993b). At the time of publication of Elkan's theorem, two coalitions of actors were thus converging, each defending rival views of fuzzy logic; this conflict became increasingly open (Rosental 1998b).

Publication of Elkan's theorem had helped to mobilize certain "big names" in fuzzy logic against Koskoian views of this theory, which could appear to be the fundamental source of actions similar to those undertaken by Elkan. Thus, faced with the spokespersons of fuzzy logic who had countless reasons not to encourage his publication in journals, and with the risk of being caught in a crossfire if he publicly expressed his point of view, Kosko notably, if not paradoxically, kept silent. In so doing he helped enhance the image of unity among fuzzy logic researchers regarding Elkan's theorem.

The multiplicity of counterproofs formulated in the electronic forum were thus succeeded by only a small number of interventions by recognized spokespersons for fuzzy logic organized around united points of view. "The" theorem of Elkan was presented, in particular, as a result that was "in fact" very simple (when reformulated "appropriately"), "known for a long time" and "without any effect" on the foundations of fuzzy logic. The image of two-sided confrontation between Elkan's viewpoint and "that" of researchers in fuzzy logic was progressively built up through additional approaches such as a co-signed article, for example, or publication in journals (and shortly afterwards in the electronic forum) of abstracts of papers, in which the standpoints of participants in the debate were simplified and frozen in a conflict reduced to its simplest expression.

In particular, once the debate had shifted to journals, a FAQ (Frequently Asked Questions) section was created in comp.ai.fuzzy that presented interaction around Elkan's theorem by contrasting two single points of view.

> The presentation of Elkan's AAAI-93 paper . . . has generated much controversy. The fuzzy logic community claims that the paper is based on some common misunderstandings about fuzzy logic, but Elkan still maintains the correctness of his proof.

AI Magazine highlighted the following opposition in oversized letters :

> The theorem in my paper is correct. . . . (Elkan 1994b)

> The author fails to acknowledge that by definition, classical equivalence does not apply to fuzzy assertion. (Berenji et al. 1994)

As mentioned above, considering editorial constraints, which required a heavier investment in contributions to the debate, the possibilities for publicly expressing a point of view were substantially reduced for some.

The journal *IEEE Expert*, by eventually devoting a special issue in August 1994 to the debate on Elkan's article, had nevertheless opened an exceptional forum for the question. But even this apparent opening remained relatively limited: The fuzzy logic authors it included were "stars" of the field, such as Didier Dubois, Henri Prade, Enrique Ruspini, and the computer scientist Lotfi Zadeh from UC–Berkeley, who was generally reputed to have "invented" fuzzy logic.

The number of participants in the debate was very small, and most of those who earlier had participated in the interaction in the forum disappeared. This did not mean, however, that they had been convinced by the very few emerging points of view and had adhered to them. The sudden unity stemmed from the fact that some had managed to remain in the debate and to enhance its resonance, while others had kept silent because they were unable or unwilling to overcome the obstacles to asserting their opinions. This singular dynamic worked powerfully toward stabilizing the debate. What about Elkan's approach and that of his partisans? What were their reactions to this counterattack?

The series of viewpoints expressed in the forum and in private letters sent to Elkan by researchers in fuzzy logic, as well as in personal conversations with them, had allowed him an opportunity to clearly perceive the divisions that existed between researchers, especially regarding the definition of their own research objects and their representations of his theorem and its proof. The existence of these evolving rifts gave Elkan the opportunity to multiply reformulations and personalized approaches. This, in turn, afforded him the chance to quiet antagonistic reactions and to "solidify" his results—or, more precisely, what he could re-present in each case as "the nature" of his results. This was a considerable resource for Elkan, for it emerged in circumstances wherein little support for his theses had been publicly expressed during the months immediately following publication of his article.

Elkan thus had the opportunity to elaborate and test several different reformulations on diverse interlocutors. He adapted his talks to suit his interlocutors and the forums at which he presented, adjusting his presentations in a differentiated, evolving, and sometimes personalized way. To a certain extent,

Elkan was more involved in "dramatic" than in "communicative" acting (Goffman 1959; Habermas 1984). His talks could possibly help to curb the virulence of the counterattack by fuzzy logic researchers without, for all that, deceiving researchers in "classical" AI.

For example, Elkan formulated the following private answer to criticism of his "implicit" introduction into his proof of an axiom ("equivalent" to the law of excluded middle) rejected by fuzzy logic, in order to obtain the theorem: "The average user" of fuzzy logic could fail to be aware of the impossibility of mobilizing this type of axiom, despite it being "so usual." Although, according to one of Elkan's interlocutors, the author affirmed on the day he delivered his paper at the AAAI 1993 that he did not see why he could not introduce the law of the excluded middle, Elkan subsequently claimed, in another context, that he had used an "equivalent" axiom in his proof by attributing an essentially educational value to his theorem.

Such nuances underline the primacy of public expression for grasping acts of enunciation (Quéré 1990). Because his article, a singular material device launched in the world, eventually proved somewhat ineffective in countering criticism, Elkan *added to it* by producing new texts and new speeches. He thus provided new instructions for his theorem's interpretation and general comprehension, thereby forging new tools for changing readers' relationship to his original text.[26] Such adjustments also helped to stabilize debate because they limited disagreements by making them appear, retrospectively, and at least partly, as misunderstandings (which differed, of course, depending on the interlocutors and the publics).

Elkan thus undertook a fundamentally different exercise than did his opponents. While their idea was to organize their debate as a unified and coordinated counterattack, Elkan proved to be a highly mobile target and was a talented mediator capable of producing different and evolving responses. He had to be more skilled at improvisation than orchestration. For him this exercise appeared to be no routine exercise but rather a new, ad hoc approach.

Yet after a few months Elkan had forged tools that enabled him to limit his production of differentiated answers that had become consuming, both in time and energy. In a new version of his article, published in the journal *IEEE Expert* (Elkan 1994a), a draft of which was available on the Internet in November 1993, he perfected reformulations that he could present to widely diverse publics. These reformulations did not concern the core of the proof itself, but the actual formulation of the theorem, the comments on the result, and the footnotes. Considering their polysemic nature, their effect was twofold. After the different readers or listeners had read or heard these reformulations, they could adopt radically divergent points of view on the nature and meaning of his theorem. But at the same time they could agree to grant it a form of correctness that was obviously variable.

For example, in the new version of his text, Elkan (in the November 1993 draft noted above) stated that the four axioms he had used to prove his theorem offered an "apparently reasonable" description of fuzzy logic. This expression could be interpreted in different ways by its readers. For some researchers in fuzzy logic who were relatively well informed as to current debate on the subject, the use of this expression might have signified that Elkan had taken into account criticism on the "limited" scope of his theorem, and that the "misunderstanding" was over. However, a reader who had little knowledge of the debates and fuzzy logic literature could attribute a far more general scope to his theorem.

This diversity of possible readings was also maintained, throughout Elkan's text, by expressions alternately supporting either one interpretation or the other of his theorem. The following extract offers a good illustration of this phenomenon:

[26] For an illustration over a longer period of the fully historical nature of readings in the field of mathematics, see Goldstein (1995) on the case of Fermat's theorem.

> The equivalence used in [my] Theorem ... is rather complicated, but it is *plausible intuitively*, and it is *natural* to apply in reasoning about a set of fuzzy rules, since $\sim(A \wedge \sim B)$ and $B \vee (\sim A \wedge \sim B)$ are both re-expressions of the classical implication $A \rightarrow B$. It was chosen for this reason, but *the same result can also be proved using many other ostensibly reasonable logical equivalences*. (Elkan, November 1993 draft, my italics)

In other words, in the new version of his article, Elkan mobilized many ambiguous expressions to describe the ins and outs of the theorem ("plausible intuitively," "natural," "ostensibly reasonable"). As a result, readers could perceive the scope of the theorem in ways that suited them.

Elkan could easily use this type of text in discussions with proponents of "classical" AI to assert that he had stood his ground and proved a theorem showing "the limits" of fuzzy logic. But he could also affirm to fuzzy logic researchers, "without any contradiction," that he had essentially formulated constructive criticism and had delivered an educational message by proving his theorem, to the point of being able to consider collaboration with them in the future. Indeed, after a long discussion with one of the leading figures in fuzzy logic, Elkan and his interlocutor had considered coauthoring a paper.

The polysemic nature of Elkan's text thus enabled him to assert "the" correctness of his theorem in various modes in his private interaction. He could also rely on a single reformulation of his article for all his answers, thus avoiding the need to multiply adjustments to suit the forums in which he had to express himself.

The existence and impact of such polysemic text actually illustrate the limits of those semiotic approaches of formalisms that establish the univocity of signs and overlook the possible failure of the effects that seem to be programmed in them. Another example of this kind of analysis can be found in Proust's ([1919] 1989:258–59) work. Proust demonstrates, through his characters, how individuals use formulations that are intended to deliver different messages to the different people around them; for this purpose, they play on the slightest material mediations of their intervention. But Proust's work also clearly illustrates the regular failure of communicative aims by insisting on the divergent understanding of even those signs that may seem to convey the most unambiguous meaning.

Yet it is the case that the stabilization of the formulation of Elkan's theorem caused the debate to run out of steam, because different forms of agreement became possible. After *IEEE Expert* had devoted a special issue in August 1994 to reactions to the theorem, in which a few papers by important representatives of both fuzzy logic and "classical" AI were published side-by-side, there seemed to be no fundamentally new elements left to advance. Although these debates finally appeared to be drawing to a close, however, no real consensus had been reached. Agreement that seemed to have formed around the statement "the correctness of Elkan's theorem" was simply apparent and must be understood in relation to the *distribution* of the distinct, and often antagonistic, points of view to which its expression corresponded. It was a matter not of a univocal statement representing a sublimation of temporary divergences, but of a *collective statement*.

The latter term, "collective statement," was coined by medievalist Boureau (Boureau 1989, 1992) and appears indeed appropriate to describe the situation under study. It could be described as a cousin notion of "boundary object," with which sociologists of science are more familiar.[27] "Collective statement" refers to a "verbal or iconic fragment that creates around itself a certain convergence of languages and thoughts, through the play of a structural fuzziness allowing to capture a still implicit thematic and to welcome the most diverse projections and appropriations" (Boureau 1992:1072). Such was for example the case of the statement "Vox Dei, vox populi" ("Voice of the people is voice of

[27] A boundary object is defined in Star and Griesemer (1989:387) as an object that is "both adaptable to different viewpoints and robust enough to maintain identity across them." The authors have developed this notion to give an account of how the work of amateurs, professionals, administrators, and others were connected to the Museum of Vertebrate Zoology at the University of California–Berkeley during its early years.

God"), which played an important role in the building of the English nation between the eighth and the twelfth centuries.

Similarly, the statement "the correctness of Elkan's theorem" was appropriated by each participant in the debate into its own specific mode of agreement, while at the same time the statement managed to serve as a point of coordination for various points of views regarding fuzzy logic. Thus, although it became necessary to talk about the "recognition" of Elkan's theorem and its certification by specialists, such "recognition" was possible only as a distribution of partly united representations in a void produced by the increasing scarcity of publicly expressed points of view.

The changing shape and general evolution of representations of Elkan's theorem—like the reduction of a set of points of view expressed publicly in a small number of contributions to journals—did not occur as a simple formulation of more or less convincing univocal *arguments*. Other factors also played a decisive role in this dynamic—especially polysemy, certain actors' capacities, depending on the case, to coordinate with, to consult, and to co-opt one another, to materially manage the visibility of their texts, to show flexibility by producing evolving and differentiated discourses, or to accompany their claims by reformulating them as much as necessary. Such competencies were all instrumental in stabilizing debate and were indispensable in the constitution of shared "evidence" of a statement lastingly endowed with multiple significations:[28] "the correctness of a theorem" as a collective statement.

CONCLUSION

In the course of this analysis I have shown how "recognition" of a logical theorem in artificial intelligence depended on the combination of a multitude of conditions and on the author's ability to overcome a set of distinctly different trials.[29] While the fact of *effectively* having empirically studied the steps in the emergence of a statement of this kind (in particular through observation) is in itself the first fundamental result of this research, the fact of that irreducibility is the second result. In a case such as the evaluation of Elkan's theorem, only an examination of the entire sequence of diverse mediations at play allows one to grasp the reality to which notions of "recognition," "approval," and "certification" of a statement correspond.

Note that although such mediations are often disregarded in stylized descriptions and in certain epistemological analyses, one should not conclude that this approach is an exercise in the denunciation of "secret" phenomena. The researchers did not try in any way to hide these mediations. In fact they often saw them as subjects of debate and objects of more or less elaborate or allusive reflexive formulations.

In keeping with the results of this study, I therefore have no more intention now than previously to *stylize* the case under consideration. We thus end up with a description of the shaping of representations of a theorem that defies a whole range of reductionist approaches.

First, this dynamic does not simply derive from a production of abstract reasoning, exchange of ideas, or verbal arguments. Representations of Elkan's theorem were primarily constituted during interactions between readers and texts. They were partly determined by the existence of a general economy of access to the original texts and by the management, by certain actors, of the visibility of their printed production. Moreover, the expression of a particular point of view was, as such, a problematic act implying various degrees of investment, as well as conditions that were not always met for a particular actor at a particular point in time.

Elkan's theorem was not evaluated collectively through the aggregation of attentive readings of the proof presented in his article. The readings it did receive were often rapid

[28] I refer to "the correctness of Elkan's theorem" as a "statement lastingly endowed with multiple significations" because at the time of this writing, and according to my investigations, the situation has hardly evolved since the summer of 1994.

[29] The term "trial" refers here to the various types of tests which lead to perceive a statement as "objective" and which *constitute* the ontology of the latter (Latour 1987:74–79).

and partial, when they existed at all. In the context of what could roughly be called the imperfect reading market (considering the limited resources of the actors for practicing reading, especially in terms of time, know-how, and access to the texts), points of view on Elkan's theorem were largely shaped on the basis of texts presented as faithful "substitutes" of the author's original and in the framework of a series of representations. If only for this reason, certification of the theorem could not be reduced to the systematic application of universally shared criteria for evaluating a proof. This type of view would, moreover, have been particularly simplistic in so far as both Elkan's proof and its substitutes were confronted with a wide diversity of modes of reading and ways of practicing logic, especially in regard to the definition of priorities in the drafting of proof.

In fact, simple agreement on the question of the correctness of Elkan's proof was particularly difficult to obtain because his texts were not read in the same way, despite the use of shared symbolic language. Even before the problem of validity of the theorem arose, the protagonists faced the question of the theorem's exact signification and scope—a question closely related to that of the nature of the hypotheses mobilized. Answers to these questions were by no means unanimous. Admittedly, the proliferation of proofs and counterproofs, versions of articles, and polysemic messages by Elkan also contributed to a large extent to that state of affairs.

Moreover, the formation of representations of Elkan's theorem was not a sum of strictly individual approaches. It was very much a question of collective actions involving a substantial amount of coordination and was set in the struggles between coalitions of actors, the configuration of which evolved partly in line with viewpoints expressed during the debate. This coordination of viewpoints and the management of their visibility by means of various mechanisms was one of the elements clearly showing that the practice of logic did not simply amount to the production of reasoning, whether oral or written.

Participants in the debates over Elkan's theorem could not finally be compared to homogeneous or typical subjects, revealed or formatted solely in a relationship to the texts or logical ideas. They were social actors, part of long-term and large-scale conflicts, sometimes engaged in rival activities and projects, endowed with variable resources and competencies, and therefore neither "typical" nor substitutable to one another. Although entry into the debate was marked by the adoption of a process approach to phenomena, it appeared clearly that a better grasp of the actors' positions required a far more comprehensive and in-depth inquiry, which led in turn to the latter result.

Another point characterizing this study is that it reveals an absence of "signs," especially institutional ones, that could clearly be interpreted by the actors or the social scientist as guarantees of an unquestionable certification of the theorem. In particular, various significations could be associated with the theorem's publication; understanding of this significance varied with the interlocutor's familiarity with the modalities of selection that the paper had overcome.

Even apparent agreement with the statement "the correctness of the theorem" was not easy to interpret. It could correspond to very different representations of the theorem's significance and scope. It stemmed mainly from the emergence of a collective statement, appropriated through variety of means. This type of result shows the extent to which it would have been simplistic to pose a priori the problem of the certification of the theorem in terms of univocal rejection or acceptance, or to believe that the use of symbolic languages made ambiguity impossible.

Furthermore, in so far as this apparent agreement also emerged because not all participants managed to remain in the debate, we see that the evolution of public representations of Elkan's theorem did not operate only in the mode of conviction. Remember that those who disappeared from the debate had not always been convinced by other emergent points of view. Nor had they been prompted to renounce antagonistic normative approaches of logic to which they may have adhered to previously.

Studies carried out in the sociology of science often present the emergence of scientific facts as total victories of one side against another. They have hardly accustomed scholars to observing agreements with statements based on divergent points of view on which broad and straightforward consensus is not eventually reached.[30] Yet my research on other dynamics of the production of certified knowledge in logic suggests that the case studied here is a good laboratory for everyday situations. At the cost of a highly detailed analysis, I saw that apparent agreements with other statements stemmed from relative misunderstandings, from points of view that were more or less expressed and visible, even if the number of specialists and differences of representation at play were often much fewer.

The fact that some of the debates took place in an electronic forum certainly contributed to this interplay, which is more difficult to grasp in other contexts of logical production. The increase in use of electronic fora in the 1990's not only constitutes a tool for objectifying certain scientific practices; it also changes the ways arguments are framed and scientific debates are conducted (Lewenstein 1995; Rosental 2000).

Finally, consider the representation of the certification process for a theorem that defies various forms of reductionism. In particular, note that, even if a detailed analysis of the authors' texts would have been useful to enhance observation, the formation of points of view about Elkan's theorem was not simply determined by stable institutional configurations (e.g., the fractures in the field of AI) but also individually by the details of the proofs themselves. To this extent, I escaped from a form of social reductionism, in which debates would have consisted in a series of arguments of authority, and institutional considerations would have entirely determined the reception of statements. On the contrary, the production of new statements were able to somewhat transform coalitions. This result will not surprise readers of Goody (1977), who clearly showed how the setting up of scriptural devices could have consequences for the transformation of the social relations of those who write or are put into writing.

[30] This situation is different from another interesting case in which views perceived by the actors as opposite coexist (Bloor 1982).

We have not, for all that, been forced to adopt an idealistic position consisting in highlighting the ideal truth (or falseness) of "the" theorem of Elkan, and to posit that it is enough *in practice* to explain "its recognition" (or "its rejection"). In fact, not only was it possible not to use such an approach, but the approach I used enabled me, in this case, to reveal the erroneous nature of this hypothesis and to show the complex reality to which the above terms ("the" theorem, "its recognition," "its rejection") could correspond.

A vast field of empirical investigation into the concrete modalities of the collective accreditation of logical statements is open for investigation. Although the circles in which the scholars of logic work are soundly entrenched behind their abstruseness, complex probative mechanisms, and tricks of the trade, it is nevertheless possible to provide elements of an answer to questions about the practices of the certification of logical theorems, and to construct a number of tools (especially cognitive) to further the inquiry. Since the page has been turned on excessively long discussions of principles, one can only hope that the social sciences will be quick to further the exploration and analysis such that we may better grasp, if not radically change, our representations of models for rationality and intellectual work.

Claude Rosental *is a member of the School of Social Science at the Institute for Advanced Study in Princeton, New Jersey, and a researcher at SHADYC (CNRS/Ecole des Hautes Etudes en Sciences Sociales) and the Groupe de Recherche Innovations et Sociétés, France. He is author of* La trame de l'évidence. Sociologie de la démonstration en logique *(Producing self-evidence: Sociology of proof in logic, Presses Universitaires de France, Paris, 2003).*

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