ERNEST AND ANELLIS UP A TREE

IRVING H. ANELLIS

In [Ernest 1992] Paul Ernest took exception to what he suposed was my claim [Anellis 1989] that my teacher Jean van Heijenoort ought to be given credit for priority for a proof of the soundness and completeness of the falsifiability tree method. At the same time that he defended, in opposition to my alleged claim for van Heijenoort's priority, arguing that his teacher Moshé Machover deserved credit for priority of that proof, he objected that that priority claims were pernicious, and consequently bad history. The purpose of [Anellis 1989] was not to assert any claims for priority, however, but to consider some of the factors which indicate that progress in mathematics is not always, or necessarly and inevitably clear-cut and linearly continuous, to note some of the accidents which can occur even in the history of mathematics and that impact, or even alter our understanding of history when they are ignored. Among the "accidental" factors which I included was the failure, for one reason or another, of a mathematician to publish his results, ensuring that he will not get full credit or recognition for his work. By way of illustration of this point, I considered van Heijenoort's reticence in publishing some of his research results, tracing it back to his having been "scooped" by A. D. Aleksandrov while he was completing his doctoral thesis in differential geometry; just prior to defending his dissertation, van Heijenoort had learned that Aleksandrov had just published a paper in which he had proven the main theorem that had been chief result of his thesis, *On locally convex surfaces* [van Heijenoort 1949],[1] with Aleksandrov's result appeared in *Vnutrennyaya geometriya vypuklykh poverkhnostei* [Interior Geometry of Convex Surfaces] [Aleksandrov 1948]. Consequently, van Heijenoort wrote numerous manuscripts in the mid-1960s through mid-1970s on

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[1]The theorem that van Heijenoort proved says that *if there is a support plane of a set through every boundary point of an open set, or of a closed set having interior points, then that set is convex.* As originally written, the thesis proved this theorem for a two-dimensional manifold mapped into three-dimensional euclidean space $\mathbb{E}^3$.

various aspects of the falsifiability tree method, but never published his results. Thus he never received credit for this work. This was really the point of my example.

In response to Ernest I want to make some general (disconnected) comments on my conception of the nature of the history of mathematics and logic as it impinges upon the issues raised by Ernest, and then to consider in detail the question of claims of van Heijenoort's "priority" in proving the soundness and completeness of the falsifiability tree method.

Historian of mathematics Ivor Grattan-Guinness wrote [Grattan-Guinness 1997, 7]:

> I take the word 'history' to relate to the question 'What happened in the past?'; by contrast, mathematicians (and scientists in general, and even a distressing number of historians) take history to mean 'How did we get here?' The difference between these two questions is worth pondering. Answers to the second one draw *only* on those parts of the past that have led to our present situation; while a perfectly respectable form of research, they can give quite mistaken impressions about the aims and purposes of historical figures, and the priorities they saw in their own work.

As Grattan-Guinness's remarks suggest, this raises an important general historiographical question: *viz.*, What *is* history?, and in particular: What is the relationship between the two questions? I agree with Grattan-Guinness's response, that some part of 'what happened in the past' explains 'how we got here'. But it is after all just a fragment of 'what happened.' The practical problem for the historian in both cases is one of *selection*, of defining criteria that enable the historian to choose what is important and meaningful from the amorphous mass of historical data that is available. The criteria required for responding to the question 'What happened in the past?' will perforce be broader than the criteria required for responding to the question 'How did we get here?' Nevertheless, the criteria in either will naturally be guided by the historian's understanding and perspective of the topic chosen, and even the selection of the topic will in turn be guided by the historian's overall understanding and perspective on the history of mathematics. It is fitting and proper that the historian specify, if not the criteria, then at least the kind of question that is raised and which helps determine the selection of criteria adopted to provide the

account of the developments being discussed. One question to be considered, in connection in particular with Paul Ernest's assertion that it is pernicious or detrimental to our understanding of and appreciation for the history of logic, is whether questions of priority properly belong to history of logic as an aspect of the history of that subject—or to history of mathematics in general. The narrower question of 'How did we get here?', combined with the view that one can find progress in history and the acceptance of the positive assessment of revolutions in history, provided that they are successful, was called the "Whig" interpretation of history by historian of science and historiography Herbert Butterfield (1900–1979), who described it [Butterfield 1931, v], in political terms, as "the tendency in many historians to write on the side of Protestants and Whigs, to praise revolutions provided they have been successful, to emphasise certain principles of progress in the past and to produce a story which is the ratification if not the glorification of the present." It is "associated with certain methods of historical organisation and inference" and elicits, he said [Butterfield 1931, v–vi] "certain fallacies to which history is liable" as well as "problems concerning ... the nature of a historical transition and of what might be called the historical process; and also concerning the limits of history as a study, and particularly the attempt ... to gain from it a finality that it cannot give."

For Kant, logic had essentially no history; it began and ended with its creator, Aristotle, after whom the medievals merely added some minor or trivial refinements (see [Kant 1964, B vii; 18], [Kant 1884, 10], [Kant 1974, 23–24]). It was expressed by George Abram Miller (1863–1951) of the University of Illinois, best known for his contributions to the theory of finite groups and secondarily to the history of mathematics, in his *Historical Introduction to Mathematical Literature* [Miller 1916]. Miller [Miller 1916, 8–12] reminds us that there are differing perceptions of the definition and extent of history of mathematics—whether, for example, it includes everything already known, however recently it may have become known, or if some process of "maturation" and judgment is required. He advocates the view that history of mathematics requires a "chronological element", meaning that a temporal framework is required. History of mathematics is thus perceived to be linear, from great theorem to great theorem, set within a temporal context. Next is the "human element", by which he means an elaboration of the life and work of the great mathematicians. Finally, "external factors", such as the availability and influences of libraries, conferences, journals, professional and academic influences, are to be considered. For Miller, then, history of mathematics (and

by implication history of logic, too) is the elaboration of the survey, in chronological (linear) order, of great men and great theorems. The chief utilitarian benefit of the study of this kind of history is that it develops a better understanding of the field than might otherwise be obained. One might add, although Miller does not say so, that a knowledge of the work of the past masters just might save someone the embarrassment of proving a theorem that has already been proven, thus claiming it as his own original contribution, or worse, of "proving" or attempting to prove a result that has already been shown to be false. If we agree with Ernest, then we might well have to ignore such issues as the quality of procedures and proofs offered in the presentation of a theorem by those who deserve "priority" for formulating the theorem; or as the questions of how influential the role of someone's work proved to be for subsequent development of the field, distinguishing short-range, medium-range, and long-range influences. We know, for example, that Frege obtained his first-order theory in the *Begriffschrift* in 1879, whereas Peirce did not have a first-order theory until 1883; but Peirce's work had an immediate impact upon logical research, the most notable example being Schröder's work in the 1890s, whereas Frege's work had little impact on logic until 1903, when Russell brought it to peoples' attention. In this sense, priority issues do make a difference. Let us say, with Sylvia Nasar in her bioigraphy of John Forbes Nash, Jr., that: "Mathematicis not an intramural sport, and as important as being first is, how one gets to one's destination is often as important as, if not more important than, the actual target" [Nasar 1998, 220]; "Und in die Wie, da liegt der Unterschied." But if we wish to know the 'wie' as well as the 'wer', we ought, as historians, to examine all relevant efforts, not merely the one that was first, nor merely the one that was 'best'.

In the first decades of the twentieth century, the great men and great theorems approach to history of mathematics held sway. This leads to difficulties.   Valentin   Bazhanov   and   Antonino   Drago [Bazhanov & Drago 1999] have shown, for example, that, in the absence of physical documentation for decisions, motivations, and similar accidents, the historian of mathematics is left with insoluble "puzzles", unable to unequivocally and with full certitude account for—to justify—or explain how or why one person's work is neglected, why a certain pivotal article was left unpublished, why one researcher's or group of researcher's contributions were more influential than that of another's whose results are, on the surface and in retrospect, equally significant to the advancement of a field.

It would appear that Ernest, in his disdain for priority questions, wishes to ask 'how did we got here' and to ignore the question 'what happened in the past?'

Let us turn next to a specific case in which an argument on the question of presumed claims of priority for accomplishments on the development of the tree method illustrates a distorted conception of the nature of the task of history and philosophy of logic. The case history being considered demonstrates how a logicist philosophy of the history of logic distorts the relevance of history in development of a logical theory.

In his [Ernest 1992], Ernest set out to disprove claims which were asserted to have made in [Anellis 1989] about the "priority" of Jean van Heijenoort's proofs of the completeness and soundness of the falsifiability tree method for propositional logic and classical first-order functional logic. I would suggest that Ernest has misunderstood both my purpose in raising the question of van Heijenoort's contributions to the tableau method and my specific claim about the originality of van Heijenoort's contributions to the tree method. Moreover, Ernest's view of the history of logic is puzzling insofar as he argues on the one hand that priority claims are wasteful at best and pernicious at worst, while on the other hand making it the main point of his paper to "refute" my alleged "claim" that van Heijenoort was the first to prove the completeness and soundness of the tree method.

Ernest [Ernest 1992] has pointed out that John Lane Bell and Moshé Machover completed the manuscript for their [Bell & Machover 1977] textbook by 1974, noting that, as Machover's student, he received copies of parts of the manuscript before the end of 1973 and had the entire manuscript by the end of March 1974; moreover, that the manuscript, in its final, corrected, form, was ready for the publisher in 1974, but that the book did not appear until 1977 because of printing delays. Ernest also asserts that Bell and Machover were "justifiably pleased" with their work, not because it contained any original results, but because it was an "excellent systematization and presentation" of standard and routine material.

For adding this missing bit of information to our knowledge of the history of the tree method and of the history of logic textbooks, Ernest is to be heartily commended.

Although asserting that priority claims "are not only fruitless, but harmful", Ernest nevertheless makes it his chief point to deny that van Heijenoort deserves any credit for his work on the tableau method,

or in particular the credit for priority for proofs of the completeness and soundness of the tree method that, according to Ernest, I am supposed to have ascribed to van Heijenoort (at [Anellis 1989, 179–180]). There are several issues here, setting aside those that arise from Ernest's assertion in one breath that priority claims are pernicious and in another that to van Heijenoort no priority for his work on tableaux should be ascribed.

Ernest is of course quite correct in asserting that it is unfair to consider on a par unpublished typescripts and a published work that has already gone through and passed the preprint stage, and he is to be thanked for increasing our knowledge of the history of the tree method by informing us about the creation of Bell and Machover's highly regarded textbook. It is also admittedly very clear from the evidence that Ernest adduces that my assertion that van Heijenoort's [van Heijenoort 1973] and [van Heijenoort 1974] proofs of the completeness and soundness of the tree method "were finished first" is wholly incorrect. And had I known, as I do now, about the history of Bell and Machover's textbook, I should not have made the statement without more strongly stressing the qualification that it was van Heijenoort's *simpler* proofs that were contemporaneous with, if not formulated *earlier than*, the proofs given in [Bell & Machover 1977]. (It is entirely appropriate and legitimate, I nonetheless fully concede, for Ernest to inquire whether I really *would* have changed or buttressed the stress in my original statements if I had known, at the time, the full history of the Bell and Machover text!) Moreover, when writing [Anellis 1989], my consideration was limited to only those papers of van Heijenoort's which I had obtained directly from van Heijenoort between 1974 and 1977 (as described in my [Anellis 1987, Anellis 1988] and discussed in my [Anellis 1989a]), and I did not consider the detailed history of the falsifiability tree method or van Heijenoort's earlier writings. If Ernest had examined my [Anellis 1990] paper delving much more deeply and completely into the details of the history of the falsifiability tree method and of van Heijenoort's work, he would have known that van Heijenoort's writings on the tree method go back to at least 1966, when he was on the faculty at New York University and in contact with the group New York City–area logicians that included Smullyan and Jeffrey, at the time that Smullyan was still developing the details of the satisfiability tree method. Let us therefore make a brief excursus to examine more closely the chronology of van Heijenoort's writings and their contents.

I pointed out [Anellis 1994, 222]—before, however seeing [Ernest 1992]—that in his manuscript "Interpretations, Satisfiability,

Validity" [van Heijenoort 1966], van Heijenoort defined the concepts required for presentation of the falsifiability method, and in particular the concepts of *countermodel* or *falsifying interpretation*. In his [van Heijenoort 1968] manuscript "On the Relation between the Falsifiability Tree Method and the Herbrand Method in Quantification Theory", van Heijenoort presented the falsifiability tree method as the "dual" of the truth tree presented by Jeffrey, while acknowledging that the falsifiability tree is studied, "in many variant forms", by Smullyan in *First-order Logic* [Smullyan 1968] and codified in textbook form by Jeffrey's [Jeffrey 1967] *Formal Logic*. If so, then it was van Heijenoort who brought it to the fore. But I also noted, as early as [Anellis 1990, 55], [Anellis 1994, 233] (before [Ernest 1992] appeared, let us note), that van Heijenoort explicitly and fully developed the falsifiability tree method for both propositional calculus and the classical first-order functional calculus in 1972 in his manuscript "Notes on the Tree Method" [van Heijenoort 1971] and "Falsifiability Trees" [van Heijenoort 1972], the latter revised in [van Heijenoort 1974]. These three works ([van Heijenoort 1971], [van Heijenoort 1972], [van Heijenoort 1974]) also present the falsifiability tree method as a technique which the author employs to present his first proof of the completeness and soundness of Jeffrey's version of the tree method in *Formal Logic* [Jeffrey 1967]. (At the same time, van Heijenoort [van Heijenoort 1972a] also studied the falsifiability tree method for simple theory of types with extensionality.) Van Heijenoort's paper "Soundness and Completeness of the Falsifiability Tree Method for Sentential Logic" [van Heijenoort 1973] is a revised and improved proof; likewise, his manuscript paper "Falsifiability Trees" [van Heijenoort 1974] provides an improved presentation of the falsifiability tree for classical first-order logic, and presents a simplified version of a proof of the soundness and completeness of the method for classical first-order logic. I then pointed out (e.g. at [Anellis 1990, 55–56], [Anellis 1994, 233–235]) that van Heijenoort's proofs, unlike those given by [Smullyan 1968] and by [Bell & Machover 1977], do not make explicit use of Hintikka sets. As a result, even though they employ the same concepts and follow the same patterns, they are somewhat longer and require more bookkeeping. (For an example of van Heijenoort's proofs, see the sketch by [Anellis 1989a] of his proof for the soundness and completeness of the falsifiability tree method for propositional logic.) Van Heijenoort's results are therefore closely related to Smullyan's [Smullyan 1963] proofs in "A Unifying Principle in Quantification Theory" that consistency implies satisfiability and that denumerable satisfiability implies sentential satisfiability, and reminded

readers that much of the work on the completeness of the tree method had already been done by Beth, who proved [Beth 1960] the completeness of the semantic tableau method. Smullyan's [Smullyan 1963] is his earliest of the publications in his development of falsifiability trees.

Having sketched the chronology of van Heijenoort's work on the tree method, let us return to a consideration of Ernest's arguments.

Ernest has apparently missed the main point I had been making in [Anellis 1989]—which was NOT that van Heijenoort deserved credit for *priority* in proving the completeness and soundness of the tree method, but that, *because of his hesitancy to publish his writings, van Heijenoort lost the opportunity to receive credit for any contribution* to logic, and, coincidentally, that his proofs were simpler than are given by Bell and Machover. In particular, in quoting me, Ernest [Ernest 1992, 123] places significantly more stress than did I on the question of priority when he italicizes my statement (*un*stressed in the original publication) that "van Heijenoort's proofs of 1973 and 1974 where finished first . . . ." *If* Ernest is right in asserting that the proofs in van Heijenoort's two typescripts [van Heijenoort 1973, van Heijenoort 1974] contain "relatively routine" results which others had already carried out, then certainly my example was, at most, poorly chosen. But that in itself does not obviate my claim that van Heijenoort received no credit for his work because of his hesitancy to publish, since other examples abound, stretching from the mid-1950s to the end of van Heijenoort's life. Examples of the posthumous publications of a small fragment of van Heijenoort's historical and philosophical corpus in the *Selected Essays* [van Heijenoort 1986] give evidence of that. Moreover, since Ernest has, so far as I know, not seen van Heijenoort's [van Heijenoort 1973] and [van Heijenoort 1974], it is hardly fair for him to call them "relatively routine" and "not the first to develop" work on the proofs of the soundness and completeness of the falsifiability tree method, since certainly, even if a theorem has already been stated and proven, a new and better proof is not, in and of itself, necessarily "relatively routine"; nor does the fact that a theorem has already been stated and proven signify that the author of a different proof of the same theorem should be deprived of credit, even the claim to priority, for arriving at a new proof. At the very least, the author of a new and different proof deserves the benefit of being credited for priority in attaining the new proof. To deny this would show a lack of historical sense or sensitivity. But then Ernest's dismissal of the examinations of priority already indicates lack of historical sensitivity or awareness.

We must also note that Ernest [Ernest 1992, 124] makes it a point to stress that Bell and Machover were "justifiably pleased with their text,

not because it presented novel results, but because it was an excellent systematization and presentation of more or less standard material, in a pedagogically effective manner", but he dismisses van Heijenoort's manuscripts precisely with the claim that they presented "relatively routine" results. This would seem to imply that a double standard is being applied to the Bell and Machover text on the one hand and to van Heijenoort's work (although presumably the latter were unseen by Ernest, who would therefore be in no position to judge the pedagogical effectiveness or expository lucidity of van Heijenoort's writings).

Finally, Ernest points to Toledo's [Toledo 1975, 15] reminder that by 1975, the "elementary results," in Ernest's [Ernest 1992, 124] words "were already widely accepted." Yes. But as Freudenthal [Freudenthal 1970–71] has shown in the case of Cauchy's *Cours d'Analyse*, a superficial similarity on some points between Cauchy's *Cours d'Analyse* and Bolzano's "Rein analytischer Beweis ..." is insufficient by itself to prove that Cauchy plagiarized Bolzano. Likewise, the charge by Ernest that van Heijenoort's are "relatively routine" runs up against the similar qualification that a superficial similarity of van Heijenoort's work to "relatively routine" results by others fails to give satisfactory testimony to the lack of originality in van Heijenoort's thinking or the novelty of his proof, if not of the theorem being proved. Lest it be forgotten, I was not claiming that van Heijenoort was the first to prove the completeness and soundness of the tree method in 1973 and 1974, only that he gave a simpler, more detailed proof of the completeness and soundness of the falsifiability tree method than had hitherto been given. Moreover, had Ernest examined my work on the history of the tree method, he would have known that van Heijenoort's work was first undertaken in 1966 and that [van Heijenoort 1973] and [van Heijenoort 1974] were merely the latest incarnations of van Heijenoort's work on the falsifiability tree method and containing proofs of its completeness and soundness.

I raise the issue in these terms because Ernest has made priority an issue in his attack on me even while denouncing priority claims as destructive. From the standpoint of the philosopher, this may well be the case, in particular if the philosopher is an advocate of Popperian hypothetico-deductivist logicism and believes that historically mathematics developed axiomatically and that consequently each theorem proved was derived ahistorically through deduction, and that the chronological order of theorems deduced was the same as the logical order of their derivation from previous results. But the question of priority is not an irrelevant or unimportant issue for history.

I wonder: if, as is generally conceded now, Newton *discovered* the calculus first but Leibniz *published* his version of it first, would Ernest want to argue that Newton's manuscripts are comprised of "relatively routine unpublished material which he was not the first to develop"? I doubt it, and he would be right to refrain from doing so. Even though Newton's writings took into account results of his predecessors such as Barrow, Gregory, and others whose contributions to the development of the calculus all historians of mathematics recognize (let us remember in this context Newton's famous remark that, if he had seen farther that others, it was because he had stood on the shoulders of giants). I also wonder whether Ernest would recognize the possibility of independent simultaneous discovery: some time around 1985, manuscripts of Dedekind were discovered in a desk drawer; among the papers found there was a proof of what we now call the Schröder-Bernstein (or, as some historians prefer to say, the Cantor-Schröder-Bernstein) theorem. Should Dedekind now be give some credit for this result, or not? Rather than speculate on what Ernest's answer would be, let me say as an historian that disentangling the often confused and almost always complex history of specific mathematical developments and assessing priority claims is essential to a correct and comprehensive understanding of the history of mathematics, and that history of mathematics is worthy of study in itself as well as for the sake of understanding and appreciating contemporary mathematics.

Let me reiterate that I agree with Ernest that it is not fair to compare a completed published work with a manuscript preprint in seeking to establish priority. The significance of the delicate question of timing is evidenced in Gana's [Gana 1985] work to clear Richard Dedekind of Charles Peirce's charge that Dedekind in *Was sind und was sollen die Zahlen?* (1888) plagiarized Peirce's 1881 paper "On the Logic of Number" published in the *American Journal of Mathematics*. The main difference between Peirce's 1881 paper and Dedekind's 1888 booklet is that Peirce defined natural numbers in terms of finite set and Dedekind defined them in terms of infinite set. Despite Paul Shields' success in showing that Peirce's system is equivalent to Dedekind's [Shields 1981] (also doing this indirectly [Shields 1997] by comparing Peirce's system with Peano's axioms, which are said [Shields 1997, 46] to be "essentially similar" to Dedekind's), we cannot ignore Gana's claim either that it is unknown whether Dedekind ever had, or had even *seen*, a copy of Peirce's 1881 paper. Another part of the argument made by Gana hinges on the dating of Peirce's MS 47 in which Peirce replaces the definition of natural numbers in terms of finite set as given in the

1881 AJM paper with their definition in terms of infinite set—1900, according to Gana; 1890 by the Robin Catalog of Peirce manuscripts; or Fall 1881–Summer 1882 as more recently dated by the Peirce Edition Project.

We have already seen from the sketch which I provided of the history of the tree method and of van Heijenoort's work on falsifiability trees that there is little evidence that Ernest has paid as strict attention to this history as he intimates. We may respond to Ernest's argument against a claim of priority in favor of van Heijenoort by referring to Ernest's own mention of Smullyan's [Smullyan 1968] work as the basis for Bell and Machover's presentation of the classical tableau method. Of course! But *nowhere* did I *ever* assert that van Heijenoort deserved to be ascribed precedence over Smullyan for work in developing the tree method. If Ernest believes that I claim priority for van Heijenoort over Smullyan for developing the tree method, it must be because Ernest has either grossly misunderstood what I wrote in [Anellis 1989] or because he has failed to read my purely historical writings on the history of the tree method, or both.

I do not have a problem with Ernest's complaint about the incompleteness or supposed incorrectness of my account in [Anellis 1989] of the history of the tree method, and I welcome the new information which he has provided in [Ernest 1992]. My discomfiture with [Ernest 1992] stems from the two problems with his criticism, namely that on the one hand he has himself not examined the full account but makes claims about the history of the tree method nonetheless and has distorted the meaning and purpose of the discussion in [Anellis 1989]; and, more importantly, on the other, that he rejects as pernicious or at least worthless, certain aspects of the study of the history of logic (in this specific case, priority claims), thereby apparently exemplifying the view that history has no rôle in understanding logic and that the truth of the history of logic is unimportant and irrelevant. I cannot agree that the history of logic has no rôle in understanding logic and that the truth of the history of logic is unimportant and irrelevant.

The task of the historian of logic is to present [the] history of logic. What requires work is unpacking what it means to "present [the] history of logic."

If the task of the historian of logic is to present [the] history of logic, then surely questions of priority require examination. But these investigations should be based insofar as possible on as thorough a research as the record allows. In conclusion, I therefore suggest that Ernest is guilty of precisely the kind of mistake that he has claimed that I had

made. Moreover, he has applied a double standard which confuses, if not distorts, the truth of the history of logic, by arguing against the value of investigations on priority while simultaneously arguing in favor of an 'alternative' priority claim. Our questions to Ernest must therefore be: Does logic have a history or not? If logic has a history, what is the task of the historian of logic (and how does that task differ from the task of the philosopher of logic)? Does the truth of the historical account matter in history of logic? And finally, if one does not investigate the question of priority as a part of the task of uncovering the historical development of logic, how complete and true is the historical account?

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629 CENTRAL AVE., APT. 302, FORT DODGE IA 50501-3867, U.S.A.
E-mail address: irvanellis@lycos.com