A Late Return to a Thesis in Logic*

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My Göttingen thesis for the D.Phil, "Abgekürzte Beweise im Logikkalkul" (Abbreviated Proofs in the Calculus of Logic), was written in the spring and summer of 1933. At that time in Göttingen there was a general rush to finish everything up before the Mathematical Institute collapsed under the pressures of the time and the anti-Semitic decrees of the new Hitler government in Germany. Moreover, I had long intended to finish writing a thesis in Mathematical Logic by that time.

Irving Kaplansky's efforts in preparing this volume of selected papers of mine have now brought me to read through this thesis again. I found it difficult to establish any real contact with my thesis ideas, over a gap of forty-five years and several considerable shifts of mathematical interest in the meantime.

At that time, I was much impressed with the need for mathematical rigor and the importance of making this rigor both clear and intuitively convincing. When I was an undergraduate student at Yale, the impact of the Weierstrassian emphasis on rigor via ϵ-δ was still apparent. I learned rigor from the chapter on Foundations in Edwin B. Wilson's *Advanced Calculus* (despite his typesetter, he favored both rigor and vigor). I also learned rigor from James Pierpont, the senior statesman of the Yale department; he had studied in Germany and had brought rigor to graduate students in this country through his careful books on real and complex variable theory. I also learned rigor from Pierpont's former student, Professor Wallace A. Wilson, who once regretfully told me that he understood rigor better than his master Pierpont, but that he had little mathematical substance, save metric and topological spaces, to which to apply this rigor. Undeterred by this example of form with little content, I went on to learn how rigor looked in logic from F. S. C. Northrop of the Philosophy Department at Yale. Through him, I came to admire *Principia Mathematica* by Whitehead and Russell, finding there a fine symbolic rigor almost untouched by the English language – though at the time I did not understand well why that touch of language was indeed still essential, nor did I press my curiosity beyond the first volume of *Principia*. As a Junior, I did ask Professor Wilson for a reading course in *Principia*, but he advised against it, and had me study Hausdorff's *Mengenlehre* instead – where i learned more rigor, as applied to sets, metric spaces, and ordinal numbers.

My first year of graduate study at Chicago was influenced most deeply by E. H. Moore. He and his disciple R. W. Barnard presented mathematical theorems in a formal and logistic notation (modeled on Peano), but gave proofs in a more informal fashion; I wondered how this could be effectively formalized, and wondered even more on the occasion when E. H. Moore had me give a seminar lecture on a paper by Ernst Zermelo – his proof that the axiom of choice implies that every set can be well-ordered (from Moore's very thorough critique after my lecture I learned a great deal about how to give a seminar lecture). I also listened to G. A. Bliss on the Calculus of Variations, wondering the while about a manner of minor inexactitudes in the construction of fields of extremals. One fine day I challenged Professor Bliss to produce the necessary ϵ's and δ's

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to make the proofs fully rigorous – and he did. I did take another course in (Aristotelian) Logic, this time with Hutchins' protegé Mortimer Adler; there I learned more about argument than about rigor. Finally I wrote a M.A. thesis about algebraic systems with 2, 3, or 4 binary operations; in retrospect, it seems to me now that I was trying to discover Universal Algebra – a search in which I did not succeed.

After a year I left Chicago to go to Göttingen where I hoped that the environment of Hilbert would give more encouragement to my study of logic and rigor. By then I must have vaguely begun to see that a good proof consisted of more than just rigorous detail, because there was also an important element of plan for the proof – the crucial ideas, which, over and above the careful detail, really make the proof function and get to the desired end. I clearly recall sitting in a vast lecture room listening to Edmund Landau lecture on Dirichlet series. As always, Landau's proofs were simply careful lists of one detail after another, but he gave this detail with such exemplary care that I could both copy down in my notebook all the needed detail and enter in the margin some overarching description of the plan of his proof (a plan which he never directly revealed).

Then I came gradually to the insight that proofs in mathematics combined rigorous detail and overall plan – and that overweening attention to the precision of detail could, as in the case of *Principia*, wholly hide the plan. There arose with me the notion that the necessary rigor could be codified and simplified, so as to be made almost automatic. If only the automatic could be properly described and organized, then the essential ideas of the proof would come through. This idea of organizing the *plan* of a proof in a formal way was evidently the germ of my thesis.

I had already started work on an earlier thesis idea, also in logic, early in the academic year 1932–33. I no longer know what was intended as the content of that thesis, but I do clearly recall that it did not find favor with either Professor Bernays or Professor Weyl when I explained it to them in Vienna, where I thought that Rudolph Carnap would be more sympathetic. Instead, I thought very hard in spurts about a thesis. A decisive spurt came on April 18–22, when I finally worked out a plan of the final thesis. In an exuberant letter of April 26th to my mother I wrote:

> "Perhaps I have time to tell you a bit about my new discovery. It's a new symbolic logic for *mathematical proofs.* It applies, as far as I can see now, to all proofs in all branches of mathematics (a rather big order!). It makes it possible to write down the proof of a theorem in a very much shorter space than by the usual methods, and at the same time it makes the proof very much clearer. In essence, it eliminates practically *all* the long mechanical manipulations necessary to prove a theorem. It is only necessary to give *leading ideas* of the *proof.* In fact, once these leading ideas are given – together with a few directions – then it becomes possible to compute *from* the leading ideas just what the *proof* of the theorem will be. In other words, once these leading ideas are given, all the rest is a purely mechanical sort of job. It is possible to define once and for all how the job is to be carried out (the general definition depends essentially upon the abstract methods I have been developing for the past year)."

Of somewhat later date is an exuberant first draft (in English) of the thesis: long-winded, full of rash philosophical assertions, and ending with a long table of things I still intended to develop.

The thesis itself (rewritten later, first in English and then translated into German) is more mathematical and businesslike. It observes that long stretches of formal proofs (written, say, in the style of *Principia*) are indeed trivial, and can be reconstructed by following well-recognized general rules. The thesis develops standard metamathematical terminology to describe formal expressions – as certain strings of symbols, suitably arranged. This is followed by a meticulous description of what it means to substitute y (or something more complex) for x in an expression. This description let me state exactly what it would mean to determine that one expression is a special case of another.

On this basis, I described exactly a number of the routine steps in a proof, giving each a label, as for example:

Inf schrumpf: To prove a theorem $L \supset P$, search for a prior theorem of the form $M \supset N$, where L is a "special case" of M and P the corresponding special case of M.

Sub inf schrumpf: Given a prior theorem $M \supset N$, one can conclude that $L \supset L'$, where L' is obtained from L by replacing every "positive" component of the form M by a new component N.

Sub Def: Substitute the definitions.

Identität: Use one of the standard identities of algebra (or of the propositional calculus).

Sub Theorem #20.43: Use the cited theorem, in the (only) possible way.

$x = C$ *fixieren*: Given a premise $(\exists x)L(x)$, assert $L(C)$ for some suitable "constant" C.

Halbnorm: Move a quantifier $\exists x$ or $\forall x$ to the front of an expression.

All told the thesis gives twenty or twenty-five of such rules (listed at the start of Chapter VII), and then observes that many proofs can be "abbreviated" by listing in order the rules to be applied. In this sense, the thesis gives a formal definition of a routine proof.

Chapter VI finally starts an analysis of *plans of proof* – a *plan* is a sequence of such standard steps. By describing such plans I hoped to define exactly what "similar proofs" would be. There are a number of examples of such plans, chiefly chosen from easier arguments in Algebra and from some early sections of *Principia* (and in those sections, this scheme worked well). For my present taste, the thesis does not give enough hard examples from the rest of mathematics.

In summary, the thesis observed that many proofs in mathematics are essentially *routine* – and that one can carefully write even a complete description of each type of routine step, so that the formal proof of the theorem, written in detail, can be replaced by the much shorter description of these steps. Moreover, since the *steps* are specified one can often summarize the directions of the proof by giving its *plan* (presumably the most crucial of the routine steps).

As a practical means of writing out proof, this scheme didn't (and couldn't) succeed. Mathematicians don't want *formal* proof; my proposed abbreviations were ugly, and analysis of such easy detail is best "left to the reader". Also, the thesis did not carry its ideas far enough into non-trivial proofs. Mathematicians do generally recognize that interesting proofs involve one or two "tricks" or "twists" added to a straightforward procedure. My ideas *might* have been carried to the point of giving a complete and formal description of all the straightforward procedures. They could then have been left aside – formalism of no great use – giving emphasis to the *real* tricks that make the proof work.

In an informal sense, this is what we still do in understanding harder proofs.

There are a few incidental points about the thesis. Initially, some of the English version of the thesis was translated for me by one of my fellow students. However, most of the translation is mine, and it looks "translated". I may have modified the presentation considerably, late in 1933 after I left Göttingen.

In the translation, E. H. Moore's favorite word, "range", has become "Spielraum", while the "scope" of a bound variable has become "Wirkungsbereich". There is an interesting anticipation of the Eilenberg–Mac Lane distinction between covariant and contravariant functors; for example, in the propositional calculus, in $p \supset q$ (for p implies q), p is contravariant and q is covariant (positive). The full description appears in Chapter II §4 and was needed for sub inf schrumpf. It is now an easy and established item in logic, and may perhaps have appeared explicitly first in this thesis – though it is implicitly present in the propositional calculus even since *Principia* or perhaps since Boole.

My thesis was printed, in the requested number of copies, by one of the smaller printers in Göttingen. When I was done, I formally received the degree of D.Phil. (in 1934). For a couple of years, I continued to think about the matter, and wrote one paper (in the *Monist*) to summarize the ideas of my thesis. Some notes for lectures probably given in 1935 indicate that I did then apply the methods of my thesis to get at the structure of the standard proof that the limit of a uniformly convergent series of continuous functions is continuous. In a few years, however, my activities in algebra pushed aside those in logic.

There remains the real question of the actual structure of mathematical proofs and their strategy. It is a topic long given up by mathematical logicians, but one which still – properly handled – might give us some real insight.

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