

# Recollections of some connections of Bourbaki with logicians

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The comments below are excerpted from summer 2013 letters William Howard sent John Baldwin when he inquired if Howard knew anything of relations between Bourbaki and logicians in the early 50's. In particular Howard responds to Mathias' mention [1] of the role of Rosser.

## **Note of June 2, 2013**

I did not know that Weil knew Rosser. It's news to me.

Here is a funny story about Myhill. In the fall of 1953, when he had a visiting appointment at the Univ. of Chicago, he was walking around with a manuscript that Weil had asked him to evaluate. It was a presentation of mathematical logic by Bourbaki. He kept saying to Tennenbaum and me:

"Either it is incredibly naive or it is incredibly deep."

Eventually he told Weil that it was not publishable. Weil's response was:

"Well, we are going to publish it."

I don't know what became of it. Maybe it was meant to be part of Bourbaki's foundational volume, or maybe it was something separate.

I never had occasion to look at Bourbaki's foundational volume. It is shocking that Bourbaki took ZC, rather than ZFC, as a foundation. Presumably this had something to do with Mac Lane's hobby horse about the relation between set theory and category theory.

Sometime around 1980 (but could be +or- 10 years, I cannot seem to be able to remember the date), Tennenbaum gave a talk to the Univ. of Chicago Math. Dept. entitled: "On current attempts to destroy set theory". After giving a nice presentation of set theory, he turned to Mac Lane, who was in the front row, and said:

"And \*you\* and your category-theoretic friends want to do away with this," (or, at least, words to that effect).

Mac Lane really enjoyed it. Afterwards, Mac Lane said to Stan, "It's just like old times." They talked for awhile; mainly Stan bringing Mac Lane up to date on current set theory.

## **Note of June 7, 2013**

You are welcome to quote me on the Myhill story. I was careful to stick to the facts.

Actually, my impression at the time was that Myhill was 99% sure, as soon as he had read it, that the manuscript was incredibly naive. But there was that 1% uncertainty. After all, how could a group of world-class mathematicians come up with something that was incredibly naive? I think that he was genuinely puzzled. Remember, Myhill was a genuinely humble person. On the other hand, Tennenbaum was 100% certain; but, of course, Stan was not so humble; also he had an excellent sense of what mathematicians—even world-class ones—were like. As for myself, I had a brief look at the manuscript during one of the conversations between Myhill, Stan and me, and my impression was pretty negative, but I did not have much interest in the situation. Also, I did not know much about the state of mathematical logic at the time (my world at the time was the world of professional mathematicians; and, by the fall of 1953, I knew a lot about that world); so I wasn't in a position to give Myhill a professional opinion, in any case. Admittedly, I knew Weil very well by then, so I had no difficulty with the idea that a world-class mathematician could be naive about mathematical logic.

After your mention of Rosser, I realized that I had a paperback copy of Rosser's 1953 book, "Logic for mathematicians". I had acquired it many years ago, out of curiosity (probably saw it while browsing in the math section of the Seminary Bookstore). From the book, one would get the impression that the principle task of mathematical logic is to show that current mathematics is formalizable—essentially the project of Whitehead and Russell; in fact, quite a few pages consist almost entirely of formulas: strikingly similar to Whitehead and Russell's "Principia".

Reply by Howard to a slightly later note.

Your final paragraph on Rosser is fine. I like your point that the idea of what mathematical logic is all about had already evolved by, say, 1953 (and I already had some sense of this, as is shown by my response to Rosser's book), and, of course, there has been more evolution since 1953. In particular, there has been an evolution of the conception of the relation between mathematical logic and ordinary mathematics (or, if you will, between the two professions). In my first year as a graduate student, I had a significant encounter with, first, transfinite induction (I encountered it in Banach's book and had no idea what he was talking about), then Zorn's lemma (used in a paper about real closed fields by Artin and Schreier, and also in a paper by Weil giving an elegant derivation of Haar measure). When I arrived at U. of C., I expressed my concerns to Norman Hamilton, and he told me about Gödel's results for ZFC. I felt that this gave real insight into the status of the axiom of choice (or Zorn's lemma) in ordinary mathematics. So this was a real accomplishment of mathematical logic. No indication of this in Rosser's book. Note also that Norman was in no way a mathematical logician; he was just a mathematician with unusually broad interests.

As you say, it appears that Rosser, by 1953, was something of a throwback. The contrast between Rosser's book and the book of Kleene (1952) is enormous. Kleene does not give the whole story, but one can indeed see there a big evolution of mathematical logic, not indicated in Rosser's book (or certain other books, but let's not dwell on this).

## References

- [1] A.R.D. Mathias. Hilbert, Bourbaki, and the scorning of logic. preprint <https://www.dpmms.cam.ac.uk/~ardm/logbanfinalmk.pdf>, 2012.