



[118] Response to Shimura's Letter

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Letters to the Editor

History of the Woods Hole Fixed-Point Theorem

I recently noticed the following passage in "Interview with Raoul Bott", *Notices* vol. 48, No. 4 (April 2001), p. 379.

"In 1964 Michael and I were together again in Woods Hole, at an algebraic geometry conference.... During that conference we discovered our fixed point theorem, the Lefschetz fixed point theorem in this new context."

I can certainly appreciate that they proved the theorem in the context of an elliptic complex, but I strongly disagree with him in his saying "we discovered," as it suggests that they discovered it completely on their own. What he says contradicts what he and Atiyah said thirty-six years ago.

In fact, in the introduction of "Notes on the Lefschetz fixed point theorem for elliptic complexes", Harvard University, Fall 1964, they wrote: "Our main formula also generalizes a result of Eichler on algebraic curves which was brought to our attention by Shimura during the recent conference at Woods Hole on algebraic geometry. In fact, this work resulted precisely from our attempt to prove Shimura's conjectures in this direction."

Also, their article in *Bull. Amer. Math. Soc.* 72 (1966), 245-250, contains the following sentence: "The first of these [which means Theorem 2 in that article] was conjectured to us by

Shimura and was proved by Eichler for dimension one."

I don't remember whether there is a similar acknowledgment in their paper [42] (*Ann. of Math.* 86 (1967)); probably not in the introduction.

A large number of mathematicians participated in the conference, and I think many of them still remember that the theorem came into existence because of my conjecture. I wonder if they can accept the phrase "we discovered."

The same paragraph ends with the following sentences: "The number theorists at first told us we must be wrong, but then we turned out to be right. So we enjoyed that!"

This is completely wrong. As far as I can remember, no number theorist said they must be wrong. After all, I conjectured it in the holomorphic case, and no number theorist was knowledgeable enough to be against its formulation for an elliptic complex. I may be excused to say that these sentences were added in order to say that they "discovered" it without help from the number theorists, of whom I am one.

—Goro Shimura
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Response to Shimura's letter

Professor Shimura's point is well taken, and I apologize for this gaffe in my interview. Had I the power to replace the two offending sentences, I would gladly replace them by:

At Woods Hole Atiyah and I discovered how to generalize Shimura's conjectured fixed point formula to the elliptic context, and eventually we were able to establish this generalization by pseudo-differential techniques.

There remains the puzzle of how my original account came about. Unfortunately, an answer to this question involves me in precisely what I was trying to avoid at this late stage of the interview, namely, in relating yet another long story. But so be it, and

let that be my punishment for failing to censor my original impulsive account in the final draft.

First, however, this forewarning especially for our younger readers. In his wisdom the Good Lord has endowed all of us with very selective memories, designed to make life bearable even in old age. On the whole we tend to remember even the smallest of triumphs but forget all but our greatest blunders. Please keep this in mind during the following narrative.

For reasons which are now hidden from me, Michael Atiyah and I started our experimentation with a holomorphic fixed point theorem at the very start of the conference. I believe our experiments had to do with the Hecke correspondence in imaginary quadratic extensions. In any case, I have definite memories about my puzzlement that although fixed points were counted with complex numbers, they nevertheless added up to integers in the appropriate circumstances. Our computations dealt with correspondences on curves as well as maps. In any case we finally consulted some of our number-theoretic friends, and it was at this stage of our deliberations that our computations with the conjectured formula were at first declared to be wrong, but after more careful analysis were found to be correct. This is the incident referred to in my second sentence. A minor triumph, no doubt, but one that lifted our spirits and convinced us that we were on to something. This incident is confirmed by Michael, but not remembered by our consultants.

The next part of my account is even more murky, but I would be less than honest if I did not admit to it here. I seem to remember that we did these or similar computations before we interacted with Shimura! According to my memory, it was precisely during our search for the history of such formulas, and after we had been referred to Eichler's work by several other people, that we were delighted to find an expert on these matters in Shimura, who set us straight and informed us that he had, in fact, conjectured the holomorphic fixed point formula in full generality for some time. Here my recollection is that we

were not aware of the general formula before we talked with him. From that time on we of course, and quite properly, referred to the fixed point formula as Shimura's conjecture, but subjectively I always remembered this encounter more as a confirmation than a revelation.

In any case, this interaction now made us all the more determined to find a proof. At this stage, I think, we also discovered how perfectly this Lefschetz formula fitted the Hermann Weyl character formula and found other interesting examples. Simultaneously we mercilessly consulted the large number of algebraic geometers at the conference in this regard, and eventually, in a special seminar devoted to this topic, a proof of the Lefschetz formula in the algebraic context was sketched out. In view of the large number of inputs to this result, it was named the "Woods Hole Fixed Point Theorem". I believe that I served as a sort of master of ceremonies at the event. This proof was sheaf theoretic and used the internal Hom and derived functors but was not considered too difficult by the experts.

These techniques are not directly applicable in the holomorphic category, and so Michael and I, who had mainly been producers rather than actors in the developments so far, turned our attention to this case and eventually to the even more general elliptic version of the theorem. An especially memorable moment for us occurred during a walk in the gardens of the Whitney estate, when we discovered that the Dirac operator fitted into the picture. And, as I remarked earlier, we eventually produced a proof using essentially pseudo-differential techniques.

Finally, a comment on the quotes in Professor Shimura's letter from the contemporary accounts of the Woods Hole story, both of them also written by me, I believe. Alas, here I must plead guilty once again to my penchant for cutting long stories short, for I have a distinct memory of debating with myself whether to include some of the above in those accounts, but at that time and in that context it seemed to me inappropriate.

This then is Bott's long, long story. Is it true or a figment of my imagination? I am afraid that will be difficult to determine, given the universal nature of the "Anosov" evolution of our memory with time, which I alluded to earlier. But, true or not, let me end by expressing my sincere regret to Professor Shimura for having omitted his name altogether in my interview. In view of the foregoing, all I can do now is plead for his indulgence for my having committed this "Freudian" lapse.

—Raoul Bott
Harvard University

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