# 1999c Descent for Shimura Varieties

(Michigan Math. J., 46, pp.203–208)

In the article in which he stated his conjugation conjecture, Langlands wrote:

> Together with its supplement, [the conjecture] implies the usual form of Shimura's conjecture [i.e., the existence of canonical models]. To verify this one applies the Weil criterion for descent of the field of definition. Langlands 1979, p. 233.

In Milne 1983, I proved the conjugation conjecture, and hence the existence of a canonical model for a general Shimura variety.

In fact, it is not immediately obvious that the descent data provided by the conjugation conjecture satisfy Weil's criterion. This led to claims (Wildeshaus, Moonen, et al.) that there was a "gap in the literature", and even to claims that the existence of canonical models had not been proved (apparently there were similar claims of a "gap" in Shimura's proofs as well, which I didn't learn of until 2019; see Michael Harris's blog post below). Although it is possible to write down counter-examples to a naive descent criterion, these didn't concern me because they were artificial, and it seemed clear that the descent data of Langlands (and Shimura) did satisfy Weil's criterion. When I finally took up the question, it turned out to be quite easy to prove this.[1] The purpose of the article is to explain this.

**p207.** "Another proof, based on other ideas..." This was based on the referee's report, since I didn't see Moonen's paper until it was published, about the same time as my paper. In fact, the main difference between the two proofs is that mine avoids recourse to Faltings's theorem (Faltings, G. Arithmetic varieties and rigidity. Seminar on number theory, Paris 1982–83 (Paris, 1982/1983), 63–77, Progr. Math., 51, Birkhäuser Boston, Boston, MA, 1984), and hence to toroidal compactifications etc.. Hence it is more elementary (and simpler). In particular, it uses nothing that wasn't available in 1977 when Langlands wrote his article. Also, my result is general, and gives a descent criterion applicable in other situations.

In the published version of the paper, the date of publication of Moonen's article is given as 1999, based on the information on the Cambridge University Press home page. In fact, the copyright page of the book gives 1998 (although it didn't appear until 1999).

**Michael Harris, blog post "*abc* and the foundations"**
`https://mathematicswithoutapologies.wordpress.com/2015/10/09/`

Now that Davide Castelvecchi's lucid Nature article on Mochizuki's "impenetrable" work on the abc Conjecture has been reprinted by Scientific American, many of us can expect our non-expert friends to ask us what's going on. (It has already happened to me.) If your non-expert friends happen to be sociologists, please advise them to review Castelvecchi's text for clues to consensus-formation within our own impenetrable community. Castelvecchi observes that Mochizuki makes the parallel explicit:

> Mochizuki wrote that the status of his theory with respect to arithmetic geometry "constitutes a sort of faithful miniature model of the status of pure mathematics in human society".

[1] I know of no reason to doubt that Langlands checked this. Certainly, this seems at least as plausible as that Faltings checked all the statements used in Moonen 1998.

I don't even have to resist the temptation to offer my own opinion on the correctness of Mochizuki's work, because I decided early on, after spending less than half an hour with the manuscripts, to wait for consensus-formation before thinking about the question. As the Nature article reports, even experts strongly invested in the *abc* Conjecture, as I am not, remain stymied by Mochizuki's approach. I'm happy to let them sort it out, in Oxford in December or at another place and time.

Instead I want to focus on this passage from Castelvecchi's article:

> He is attempting to reform mathematics from the ground up, starting from its foundations in the theory of sets... And most mathematicians have been reluctant to invest the time necessary to understand the work because they see no clear reward...

(Sociologists, please note the word "reward.") Readers of this blog will be aware that Mochizuki's is not the only attempt to "reform mathematics from the ground up" and that a lively and opinionated subculture is hoping to convince mathematicians to "invest the time necessary" not only to understand homotopy type theory but to consider adopting it as a new foundation for mathematics. It's not clear from the lengthy discussion here and elsewhere on this blog whether HoTT has yet unveiled a "killer app" as capable of capturing the community's imagination as the *abc* Conjecture undoubtedly would be, if Mochizuki's proof turned out to be correct. Castelvecchi ... leaves one point ambiguous, and I haven't seen it clarified elsewhere (but I may simply not have been looking carefully enough): would all the rest of number theory still be OK under Mochizuki's proposed "reform"? Can we imagine a situation in which we get to keep either the *abc* Conjecture OR Deligne's purity theorem for $l$-adic cohomology, for example, but not both? (And did we really just imagine that situation, or did we merely imagine that we were imagining it?)

Number theory appeared to be facing an equally stark but much less consequential dilemma nearly 20 years ago, when several mathematicians expressed concern that what appeared to be an established proof that the Langlands conjecture on conjugation of Shimura varieties (completely established by Borovoi and Milne) implied the existence of canonical models in the sense of Shimura and Deligne was based on a flawed interpretation of Weil's theorem on Galois descent. The issue seemed to be that there was no clear way to translate the version of Weil's theorem implicitly used by Langlands, Milne, and others, which could be traced back to some of Shimura's early work and was therefore formulated in the language of Weil's Foundations of Algebraic Geometry, into Grothendieck's theory of faithfully flat descent for schemes, which by that time was the only language anyone cared to use. Some mathematicians went so far as to speculate that Shimura had got it wrong and that the whole theory of Shimura varieties was unreliable; others were ready to entertain the unlikely but not altogether inconceivable possibility that Weil's and Grothendieck's foundations were actually incompatible.[2] In the end Milne found an updated version of what most specialists decided was probably the proof Shimura had in mind, and Yakov Varshavsky gave "a complete scheme-theoretic proof of Weil's descent theorem" in an appendix to a paper on conjugation of Shimura varieties.

Some of us wondered at the time why Shimura had apparently omitted a crucial statement in his proof, although all the preliminary steps had been carefully prepared and were available

[2] I (jsm) was out of the loop, and heard none of this. Although Grothendieck and his school had written copiously on descent, their results did not include those of Weil. The problem may have been that the "several mathematicians" didn't understand Weil.

for use by Milne and Varshavsky. We decided that, in the small world of specialists to which Shimura belonged in the early 1960s, it would have been considered superfluous to mention the missing step. When Shimura was consulted he claimed not to remember what he was thinking at the time. In the (highly unlikely but not altogether inconceivable) event that number theory jettisons its current foundations in favor of Mochizuki's inter-universal Teichmüller theory, collective memory loss may jeopardize preservation of present-day number theory even if it can be proved to be theoretically consistent with the new foundations.