Reflections on Corvallis

Letter to Akshay Venkatesh prior to The 2022 Summer School on the Langlands Program at IHES, July 11-29.

Hi Akshay,

Here is an account of my thoughts on the Corvallis conference. My memories of it still seem quite fresh, and I was surprised not to recognize any participants from Corvallis in the list of speakers for the IHES Summer School next week. It was only then I recalled that Corvallis took place forty-five years ago, quite a long time!

Corvallis was itself intended as an update of the earlier Boulder Conference from 1965. Boulder was devoted to the geometry and analysis of quotients of real algebraic groups by discrete subgroups. The early work of Robert Langlands, and in particular his recently completed theory of Eisenstein series, had a prominent role there, and certainly impressed the other participants. However, it was before the time of functoriality.

There was no explicit mention of the Langlands program in the description of the Corvallis conference, nor in its title, "Automorphic Forms, Representations and L-functions". Actually, I don't think the term had been introduced at that time. But the goal was clear in the choice of lectures. It was to bring Langlands' Principle of Functoriality, with all of the representation theory and number theory that it entails, to a larger mathematical audience.

Corvallis participants began to arrive at the Oregon State University campus in Corvallis the weekend of July 9-11, 1977. We were all billeted in a vacant student dormitory, with adjacent rooms each sharing a small common bathroom. We were somewhat surprised to find that the neighbouring dormitory would be taken by the participants of another summer school, a football camp for high school kids run by Craig Fertig, the coach of the Oregon State Football Beavers! We soon got used to seeing these participants in addition to our own. Not surprisingly, they all looked really young. It was an interesting contrast between the two groups of summer "students", which was most obvious in the cafeteria-style dining room we all shared.

The Corvallis program itself was tightly crafted by the chief organizers, Armand Borel and Bill Casselman. Each lecture was intended to be a component in a sequence that would lead to a broader understanding of Langlands' theory. This can be seen in the two-volume set of proceedings, in which the Parts I-IV were designed to coincide with the four weeks of the conference.

Part I begins with the excellent group theoretic articles of Springer and Tits. It passes then to local representation theory of Harish-Chandra and others. We should note that the work of Harish-Chandra was a major influence on Langlands, and in particular, a foundation for his theory of Eisenstein series. Eisenstein series (through their constant terms/S-matrices) in turn served as initial motivation for Langlands' later introduction of general L-functions and his Principle of Functoriality.

Part II moves on to the global theory, with lectures on automorphic forms and representations. It includes lectures of Flath and Piatetski-Shapiro on the basic properties of these objects. It also includes two lectures, one by Borel and Jacquet, the other by Langlands himself, on the difference between automorphic forms and automorphic representations. It was well worth emphasizing this difference, since while the two types of objects are closely related, they also represent two quite different modern aspects of the subject. In addition, there were lectures on the Howe/theta correspondence between automorphic representations on different groups. While this topic seemed to be quite distinct from the Langlands program at the time, it appears now to be gradually becoming a part of it. Finally, there were lectures on the trace formula. The first applied to $GL(2)$, and came with an article by Gelbart and Jacquet. In the second set, which included Langlands' theory of Eisenstein series, I described how the trace formula was developing for higher groups.

I must confess to a clumsy (beginner's ?) error I made in the first of my three lectures on Eisenstein series and the trace formula. I devoted the lecture entirely to notational preliminaries! I was later rescued by a suggestion from John Millson, who said that I should begin the second lecture by announcing that there had been requests to review the notation! The resulting good-natured(!) laughter seemed thankfully to dispel some of the tension that had arisen between audience and speaker.

Part III begins with Tate's article expanding on a lecture he gave on Class Field Theory. This lecture was *not* part of the original program, having instead been scheduled by popular demand at the conference. I remember Tate excoriating the audience members- which seemed to include everyone with the possible exception of Borel- for not having done their homework! (Class Field Theory was listed prominently as a prerequisite for the conference.) However it was a beautiful lecture, and Tate's article has become a classic.

After Tate's article, Part III contains lectures by Jacquet and Borel on Langlands' construction of general automorphic L-functions, objects that are believed to include all the L-functions that originate in arithmetic and algebraic geometry. It is this idea that leads naturally to the Principle of Functoriality. The lectures then proceed to the Saito-Shintani-Langlands proof of base change for $GL(2)$, using a new kind of trace formula obtained by twisting the underlying group by an outer automorphism. This is what led to Langlands' proof of functoriality for general (two-dimensional) tetrahedral representations of the Galois group over a number field, a theorem that includes the Artin conjecture for the original L-functions attached by Artin to these Galois representations. It was this result, or rather its extension by Tunnell to octahedral representations, that was in turn the critical initial step for Wiles in the work that ultimately led to a proof of the Shimura-Taniyama-Weil (S-T-W) conjecture, and most famously, a proof of Fermat's last theorem. Finally, there was a lecture by Tunnell on the local Langlands conjecture for the group $GL(2)$.

After three weeks, everyone was exhausted. The football students must have been tired too, for there was increasing friction between us. It wasn't so much a problem in the cafeteria. At breakfast, for example, there was minimal interaction, as they tended to congregate around the oversized peanut butter and toast table. Phones however were another story. I don't have to recall that cell phones did not exist at the time. The best one could manage was one of the three pay phone booths on site. And at these, it was not uncommon to be interrupted in the middle of a call home by two or three young fellows banging on the outside of the booth shouting, "Coach says you guys are not allowed to use these phones!" Hah! What mathematicians would be intimidated by Coach Fertig when they were already being supervised by Armand Borel!

Part IV of the conference nevertheless began on schedule, bringing with it the most intensive week yet. It was devoted to the theory of Shimura varieties. This is a topic that in its modern development by Deligne and Langlands has become a central part of the Langlands program. After much work by Shimura in the years preceding 1970, Deligne formulated a common set of principles on which Shimura's ideas were based, and which made the theory accessible to a broader audience. Deligne expanded his original 1971 Bourbaki report on them in the first of his two articles for Part IV. This followed articles from lectures by Casselman and Milne on progress in the subject before 1977. Deligne's second article explored a remarkable new direction. It gave a conjectural generalization, undoubtedly the broadest possible, of the Euler's famous formula for $\zeta(2n)$ as an explicit rational multiple of π^{2n}. Langlands' main article, which appears between the two articles of Deligne, contains perhaps the most revolutionary new ideas from the conference. Like Deligne's second article, it is formulated in terms of motives.

The idea of a motive is one of Grothendieck's greatest insights. In general terms, motives are supposed to have two simultaneous roles. On the one hand, they are to be regarded as fundamental building blocks of algebraic varieties. On the other, they also represent a universal cohomology functor for (nonsingular) algebraic varieties, of which Betti cohomology, (algebraic) de Rham cohomology, l-adic etale cohomology and crystalline cohomology become concrete realizations. Their existence was predicated by Grothendieck on a number of conjectures, which are still largely unproven today. Perhaps for this reason, it seems to have been considered impolite to discuss motives in formal mathematical circles before 1977. However, this unwritten understanding, if it existed, came to a decisive end with the articles of Tate, Langlands and Deligne in the Corvallis proceedings.

Langlands' primary contribution was to extend the S-T-W conjecture, which applies to the motives of elliptic curves, to arbitrary motives. He formulated it in terms of Grothendieck's conjectural motivic Galois group, a universal reductive pro-algebraic group whose finite dimensional representations classify motives. Langlands' idea was to postulate a parallel universal automorphic Galois group, whose finite dimensional representations classify a natural class of automorphic representations of general linear groups. His conjecture was then the existence of a natural homomorphism from the automorphic Galois group to the motivic Galois group. The S-T-W conjecture is the very special case that would apply to a map between two natural factors of the two universal groups, each attached to the group $GL(2)$. Langlands used the conjecture in his article to formulate interesting new properties of quite general Shimura varieties. I will not discuss these here. The conjecture itself is now known as Reciprocity. It is so named because it would include all reciprocity laws, between Frobenius eigenvalues on etale cohomology groups of nonsingular varieties on the one hand, and Hecke eigenvalues for automorphic representations of general linear groups on the other. The search for such laws was a constant preoccupation of Shimura, and more recently, Ihara, Langlands himself, and others.

The fourth week, and with that, the conference itself, came to an end on Friday August 5, 1977. After the usual dinner in the cafeteria, there was a farewell reception in which, as memory serves, the wine flowed freely. Spirits were high, even euphoric. People seemed to have a sense of excitement about the future of the subject they had chosen to follow. It is true that many of the lectures had not been well understood. However, I believe that there was a general feeling of confidence that the many ideas could be mastered, a promise that even now is still being realized!

My strongest impressions of that last night were of one of the participants, Takuro Shintani. I knew him in connection with his earlier work on cubic forms, but on this occasion, he appeared in front of me smiling broadly, and started to sing: songs from my childhood that he must have learned from his early schooling in Japan! He then moved about the room among mathematicians from the many different backgrounds there, singing *their* songs. He was utterly charming.

It is worth adding some further mathematical notes at this point. Although Saito seems to have been the first to have used the twisted trace formula, it was Shintani who really developed it adelically, and who brought it to the attention of Langlands. Shintani could only treat special test functions (those that were "cuspidal at two places"), and he wondered whether Langlands could do anything with the complicated "parabolic terms" that would otherwise arise. Langlands thought very hard about the problem, and was eventually able to complete the full base change comparison. It was only in this generality, and only after considerably more hard thought, that he saw its spectacular applications to Artin's conjecture/functoriality/reciprocity for certain two- dimensional Galois representations. Shintani must have been disappointed not to have gone further himself, and had expressed himself forcefully at the end of the lectures on base change. But it is possible that if Shintani had not worked on the twisted comparison for base change, and then brought the deeper problems to the attention of Langlands, there would have been no application to two dimensional Galois representations, and perhaps no proof of the S-T-W conjecture and no proof of Fermat. We can think of Shintani in these terms, even if it is more likely that Langlands had already thought of the arithmetic implications, and needed only general base change for $GL(2)$ (of prime order), together with a charactization of its image, to complete the theorems. But for me, thoughts always return to the solitary figure exuding warmth and friendship, circulating about the room that night in Corvallis.

Where does the Langlands program go from here? If we agree that the fundamental question is functoriality, how should we best attack its proof? Opinions vary on this, but it is my view that Beyond Endoscopy, the stategy proposed by Langlands in 2000 for combining the trace formula with the general theory of automorphic L-functions, offers the best prospects. I have actually not described the assertion of functoriality in this letter, and I believe that the name itself came only after the Corvallis conference. One can find the relevant assertion in Borel's Corvallis article on automorphic L-functions, or if one prefers, any of several modern articles with elementary descriptions of functoriality. This is also not the place for me to describe Langlands' theory of endoscopy, let alone Beyond Endoscopy. Suffice it to say that Beyond Endoscopy again prescribes a comparison of trace formulas, like the original one in Jacquet-Langlands, the more advanced base change comparison and the general endoscopic comparison, but one that is much deeper still.

We now understand that there are really two fundamental pillars of the Langlands program, Functoriality and Reciprocity. If I had to guess, I would say these might ultimately need to be proved together. The most difficult part of Functoriality is thought to be its restriction to Galois representations, which is to say, the part of it that applies to the original Artin conjecture for the L-functions he defined.

But this is also part of Reciprocity, insofar as it applies to the basic structure of the motivic Galois group. One might imagine a proof of Reciprocity that combines whatever methods are eventually used to prove Functoriality for Galois representations, with extensions of the methods applied by Wiles to elliptic motives. If this is the case, Shimura varieties will assume an even greater importance in the subject than they presently hold.

The implication here is that the methods of Beyond Endoscopy might be what is needed to construct the motivic Galois group, simply because their role in Functoriality should ultimately lead to a construction of the parallel automorphic Galois group. As something vaguely reminiscent of Deligne's proof of the last Weil conjecture, it is not a prospect that would likely appeal to Grothendieck. Instead of having his standard conjectures lead to the motivic Galois group, it would work in the opposite direction.

Enough idle speculation! Like everyone else, I use it for inspiration, but we all have our own ideas. I hope that the participants in the upcoming IHES Summer School will be inspired over the next few weeks, and that they will return with renewed wonder and excitement for our subject and for the Langlands program.

Regards,
Jim