MY UNFORGETTABLE EARLY YEARS AT THE INSTITUTE
Enstitüde Unutulmaz Erken Yıllarım

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`And what was it like,' I asked him, `meeting Eliot?'
`When he looked at you,' he said, `it was like
standing on a quay, watching the prow of the Queen Mary
come towards you, very slowly.'
– from `Stern' by Seamus Heaney
in memory of Ted Hughes, about the time he met T.S.Eliot

It was a fortunate stroke of serendipity for me to have been at the Institute for Advanced Study in Princeton, twice during the nineteen eighties, first as a Post-doctoral member in 1982-83, and later as a Sloan Fellow in the Fall of 1986. I had the privilege of getting to know Robert Langlands at that time, and, needless to say, he has had a larger than life influence on me. It wasn't like two ships passing in the night, but more like a rowboat feeling the waves of an oncoming ship.

Langlands and I did not have many conversations, but each time we did, he would make a Zen like remark which took me a long time, at times months (or even years), to comprehend. Once or twice it even looked like he was commenting not on the question I posed, but on a tangential one; however, after much reflection, it became apparent that what he had said had an interesting bearing on what I had been wondering about, and it always provided a new take, at least to me, on the matter. Most importantly, to a beginner in the field like I was then, he was generous to a fault, always willing, whenever asked, to explain the subtle aspects of his own work.

I first heard of Langlands when I was trying to think about what I should try to work on for my doctoral thesis. It was during 1976-77, and I was in the second year of my graduate studies at Columbia, learning about Number theory (using the books of Lang, Davenport, and Artin and Tate) and Algebraic Groups (using Borel) as my two special topics in which I was going to have an oral exam in, later in May. In the Spring of 1977, I went to visit my friend Sheldon Kamienny, who had just begun his graduate work at Harvard; I had moved to mathematics after getting a Master's in electrical engineering, when Sheldon was an undergraduate mathematics major. During that visit to Harvard, I was fortunate to meet John Tate, who exuded genial wisdom. I mentioned to him that I was trying to come to a decision on what to work on, when he said, "Isn't Hervé Jacquet at Columbia? Lots of people in Number theory want to understand what Langlands is proposing, and if as a graduate student you can learn the basics of infinite-dimensional representations from Jacquet, it will give you an advantage and propel you forward," or something to that effect! Tate was very gracious and quite open to new directions, which might not have been the prevailing view of all the Number theorists then. The revolutionary idea that non-commutative harmonic analysis on Lie groups should have a say on the decomposition of primes in non-abelian extensions of the rational field was too foreign, and was not so easily accepted! Spurred by Tate's comments, I tried to learn a bit about the sweeping ideas of Langlands, and took a look at the forbiddingly long masterpiece *Automorphic Forms on GL(2)* by Jacquet and Langlands, which resisted my initial forlorn attempts to comprehend. After my orals I asked Jacquet if he would agree to guide me, and he said yes, a bit after seeing how I progressed; it was helpful to first read portions of Godement's *Notes on Jacquet-Langlands* and Gelbart`s *Automorphic forms on adele groups.* I slowly got drawn into this large area of mathematics which has come to be known as the Langlands program, and was also helped by Diana Shelstad, who was at Columbia then. I learned from Jacquet, a wonderful advisor, a lot about Whittaker functions, global and local, and I worked on the spectral decomposition of the L^2 Gelfand-Graev representation of GL(n) over any local field, archimedean or not, with a sliver of it applying to general reductive groups G. While doing that (starting with some notes of Jacquet in the GL(2) case), I tried to understand in my own way, for inspiration and analogy, a little bit of the monumental work of Langlands on the spectral decomposition of $L^2(\Gamma\backslash G)$. I learned about his philosophy of cusp forms being the fundamental building blocks for automorphic forms; I did notice the astonishing difficulty of Langlands' formidable chapter 7, which I could not wrap my head around, let alone decipher or grasp in any real sense. In my (easier) situation, the `cusp forms' were just the generic discrete series representations, and a description of the continuous spectrum involved making the measure explicit in terms of ratios of gamma factors.

I had been to the Institute before that post-doctoral year. Already in the Fall of 1975, when I had just started at Columbia, I took a train from New York to

Princeton and attended a lecture of André Weil there, a follow up to his lectures the previous year on Eisenstein and Kronecker, which was being turned into a lovely monograph. I was tongue tied in front of so many luminaries, the `*who's who'* of Number theory, even all of mathematics, who were there. Nevertheless, I learned a lot from the (accessible) lecture as well as from talking to C.S. Seshadri and Madhav Nori, whom I met for the first time. I think Langlands was already there, but I didn't meet him then, nor any other permanent member. At least I got to know the Institute grounds by walking in the woods and around the beautiful pond, thinking about Weil's 1949 paper, `Number of solutions of equations in finite fields', which I was carrying with me, dealing with diagonal varieties, due to my interest then on Gauss and Jacobi sums. I was to learn later that more importantly, that paper of Weil had concluded with an amazing set of conjectures for all varieties over finite fields, the most difficult of which, the Riemann hypothesis in that setting, had in 1972/73 been resolved in a monumental work of Deligne. Equally, I had no idea then that in 1974, Langlands had posed a far reaching conjecture of his own in De Kalb, IL, on the points modulo p of modular varieties bearing the name of Shimura in `Some contemporary problems with origins in the Jugendtraum, Mathematical Developments arising from Hilbert Problems,' relating them to automorphic forms and their L-functions. I am mentioning all this to give an idea of how exciting, and equally forbidding, the Institute was in those days.

Before arriving at the Institute, I was a Dickson Instructor at the University of Chicago during 1980-82, due to some happy happenstance. In my last year as a student at Columbia I took a break from writing and finalizing my thesis, and managed to derive a new result on the ubiquitous Dilogarithm function and its connections to the Heisenberg group. It got Spencer Bloch interested, due to the connection to the tame symbol for algebraic curves, leading to a job at Chicago. I was also partly helped by the fact that the chair there, Paul Sally, was one of only two people in the world (besides me and my advisor) who found my thesis interesting! Paul was a wonderfully hilarious personality, with a pirate's eye patch, who gave me very good comments on a paper I was writing based on my thesis, and introduced me to his students Allen Moy, Dan Bump and Sol Friedberg. However, he was so busy being chair that we had very little time to discuss some new mathematics projects. So I became involved much more with Spencer's group, working on questions arising from algebraic cycles and K-theory, and in the process got to know many including Richard Swan, V. Srinivas and Chad Schoen.

Coming back to representation theory, the second person who showed an interest in my thesis topic was Harish Chandra, thanks to Paul's prodding me to send him a copy of the dissertation. Surprisingly, a few months before I arrived at the IAS, I received a handwritten letter from Harish saying that he had completely solved my thesis problem for all reductive groups G, using his powerful theory of wave packets and the Schwartz space. I was filled with admiration since my *ad hoc* methods for GL(n) would never do the trick in general. I was also relieved in a way to have to find other problems. It is sad that Harish never got around to writing out the details of his work on this topic, which would have been a major contribution. However, there is a sketch of his method in Volume IV of his Collected Papers, edited by V.S. Varadarajan.

When I arrived at the Institute in September 1982, I went up to Langlands' office in the main building, which used to be Einstein's office in the old days, I think, and said hello. Then I moved my books, etc., to my office in Building C (which has now changed and is no longer with mathematics anymore). I found that my office (on the second floor) was just two doors down from Harish Chandra's, and downstairs near the entrance was Armand Borel's office. I don't think there was any post-doctoral member who worked anywhere as hard as Harish or Borel. I was quite embarrassed, but I was human and had other interests like English literature and music. I was attending four seminars a week; one of them was the Algebra seminar at the University, to which I rode my bicycle. (I saw Langlands riding his bike too everywhere.) But I think Borel was attending six seminars, while giving talks at two of them! I went to his seminar as well and learned a bit about the L^2 cohomology of arithmetic groups Γ, which I had been introduced to earlier by Avner Ash, while he was a Ritt Assistant Professor at Columbia during my graduate student days there. At Princeton University, I got to know Goro Shimura , Nick Katz and Andrew Wiles, and I went to a few of their lectures; they were inspiring. I had met Andrew before, during one of my visits to Harvard in 1977, when he had just arrived as a Benjamin Pierce Lecturer; he was very friendly and told me something *clairvoyant* in retrospect, namely that the key to understanding the arithmetic of elliptic curves and their generalizations lay in the Selmer group! He is my contemporary, but I consider him among my teachers, along with Jacquet, Bloch, Langlands, and Shalika – in chronological order.

Before discussing the mathematical insights I received from Langlands those two years, let me briefly mention some occurrences, partly of a personal nature. In 1982-83, I was shocked one day to find André Weil walk up to me in the reading room and ask me if I knew Sanskrit. He seemed pleased when I said `Yes, I have learned some.' He asked for my favorite work in it, and I said *Shakuntala*, to which Weil nodded in approval and said Kalidasa, the author of the play, was gifted. I never discussed mathematics with Weil, but attended his historical lectures, which I loved. At the Institute there were many other post-doctoral fellows, and I became close to them, like the analytic number theorists Brian Conrey, Amit Ghosh and Dan Goldston. One day Amit introduced me to Atle Selberg, who was extremely nice to me and answered my question as to what made him think of the powerful technique now known as the Rankin-Selberg method in Automorphic Forms. It was illuminating as I had asked earlier Robert Rankin the same question while at a conference in Britain, and I now realized that they had completely different interests and motivations in coming up with that method – independently. There was some ongoing activity in the field of automorphic forms at the Institute, which led to my meeting Jim Cogdell, Laurent Clozel, Gerard Laumon and Jean-Luc Brylinski. That year I got married to Pat Carlton, and the reception at my IAS flat was attended by Charlotte Langlands, as well as by Lily and Harish Chandra. That year I also got to know Don Blasius very well, whom I had met during the previous Fall during a brief stay at Princeton, but who was now a Ritt Assistant Professor at Columbia.

Let me now recount two specific things I learned from Langlands during those two years in the eighties, which shaped my research. The first was his explanation of the notion of *distinction* for cuspidal automorphic representations π of GL(2) over a quadratic field F (over the rational field Q), relative to the subgroup GL(2)/Q, and the relationship to the Asai L-function, which played a key role in his work with Harder and Rapoport on the Tate conjecture for Hilbert modular surfaces X over Q. I used this indirectly in my work on the Beilinson conjecture for periods of integrals on X arising from K_1 of X, in modern terms from a suitable motivic cohomology group of X. I needed to tackle a problem in the non-distinguished case, and the integral representation arising from their work was crucial, as well as its geometric meaning, relating the residue to the non-triviality of the π-component of the diagonal cycle. It led to a *Comptes Rendus* Note and a preprint. Later, in the summer and early Fall of 1986, I proved with Kumar Murty (whom I met that year), the full Tate conjecture for Hilbert modular surfaces, which came down to establishing it over non-abelian extensions. The Tate classes over abelian fields had been shown by Harder, Langlands and Rapoport to be accounted for by the Hirzebruch-Zagier cycles, which are Hecke translates of embedded modular curves, and they are all defined over cyclotomic fields. So Kumar and I did not have any algebraic cycles to play with, which were defined over non-abelian fields, so we decided to show that every Tate class gave rise to a Hodge class, which came down to exhibiting a period relation (in our perspective), allowing us to appeal to the Hodge conjecture for divisors, known by Lefschetz. Coincidentally, almost to the day when we finished the project, or perhaps a day earlier, there appeared another proof, by Christophe Klingenberg, a student of Harder, and his method was to restrict to SL(2) and make use of endoscopy. In a sense his approach was more in line with the Langlands philosophy. Anyhow, it motivated me to learn more about the stable trace formula, especially for SL(2), and this is the second insight of Langlands which impacted my work, albeit some years later. I learnt of the problem of multiplicity one for SL(2), and came to know his approach to prove it using the adjoint transfer of automorphic forms from SL(2) to PGL(3), which was being carried out by Y.Z. Flicker. Somehow, I wanted to find an alternate way to establish this multiplicity one result for SL(2), and later I found such a path, by making use of a different functoriality. I needed the conjectured automorphy of the Rankin-Selberg product, sometimes called the automorphic tensor product, of two cusp forms π, π’ on GL(2), again predicted by Langlands to exist. I had to show that if they had the same adjoint squares, whose automorphy in GL(3) was known by Gelbart and Jacquet, then π and π’ are twist equivalent, as required by the seminal work of Labesse and Langlands, which I read carefully in 1986. So I needed to prove the automorphy of π×π’, which I managed to do after some years, and one of the tools was the strengthened converse theorem for GL(4) due to Cogdell and Piatetskii-Shapiro, along with a lemma coming out of earlier work with Blasius, and some local identities for triple product L-functions, as well as some bounds for the growth of Eisenstein series.

One thing Langlands impressed upon me in 1986 was that while instances of his general conjecture are useful to isolate and prove, such as the reciprocity conjecture relating the (appropriate) representations of a connected reductive group G to certain morphisms from the Weil (or the conjectural Langlands) group to the dual group ${}^{L}G$, the entire picture of functoriality must be viewed all together, like a complex pattern, to understand better the interconnections between different aspects of functoriality. Many years later, I found an example of what he was talking about in a joint work of mine with Dipendra Prasad, which I would now like to explain. The local Langlands conjecture LLC, a theorem for GL(n) with different proofs (due independently to Harris and Taylor, Henniart and recently Scholze) asserts over a p-adic field F that irreducible n-dimensional representations σ of the Weil group correspond to supercuspidal representations π' of GL(n,F), which in turn correspond, by the (extended) Jacquet-Langlands correspondence, to certain irreducible representations π of D^*, with D being the central division algebra over F of dimension n^2 and invariant 1/n. One sees easily that σ is selfdual if and only if π is selfdual. However, if we ask whether σ and π are simultaneously orthogonal, i.e., leaves invariant a symmetric bilinear form, then the answer is not predicted by LLC. Prasad and I could show that surprisingly, when n is even, σ is symplectic if and only if π is orthogonal! The way we did it was to use the functoriality from the relevant classical groups to GL(n). So in effect, it is still as predicted by Langlands, but one has to use LLC in conjunction with this functoriality.

I also learned from Langlands, either then or later, that one way to understand the conjectural automorphic Galois group, called the Langlands group by Jim Arthur, is to think of its essential consequence predicting the existence, for each cuspidal automorphic representation π of GL(n), a reductive subgroup H(π) of GL(n,C) not contained in any Levi subgroup; this is what happens when π is cohomological and corresponds to an n-dimensional Galois representation ρ with the Zariski closure of its image being (the p-adic avatar of) H(π). The beauty is that this reductive subgroup is supposed to exist even for non-algebraic cusp forms!

In the Fall of 1986 I got to meet Gerd Faltings at the University, and also managed to attend a course taught by Andrew Wiles at the University on Hilbert modular forms, and there I met three exceptional graduate students, namely Michael Larsen, Richard Taylor and Fred Diamond. Even then it was clear Richard was going to be a major player.

One day that year Langlands made an offhand remark which I filed away in my mind and stored for later rumination. There had been some casual discussion of the Riemann zeta function, and Langlands wondered if something could be made of it occurring as a factor of L-functions of appropriate Eisensteinian forms on GL(n). I said nothing, but remembered then that Selberg had conjectured that every Dirichlet series in his class admitting a pole must be divisible by the Riemann Zeta function. Many years later I came to think about Siegel zeros, which are spurious real zeros just to the left of s=1, and wished I could say something about such zeros of automorphic L-functions of GL(n). Spurred by a comment of Goldfeld at a conference, Jeff Hoffstein and I managed to show that the L-functions of cusp forms on GL(2) had no such zeros. More importantly, we showed that for any n>1, if we assumed Langlands' functoriality, say just for the Rankin-Selberg product GL(k) x GL(m) → GL(km) (for suitable k, m), then no cuspidal L-function L(s) of GL(n) could have a Siegel zero; it was deduced by exhibiting an automorphic L-function D(s) of positive type which was divisible by a higher power of L(s) than the order of pole (of D(s)). Thus functoriality does in an indirect way have something to do with zeros inside the critical strip, albeit just inside. Curiously this line of argument fails for n=1, the key (in fact only) unresolved case being that of a quadratic Dirichlet character. It certainly seems like GL(1) is harder than GL(n) in this regard, because there is not enough room, perhaps in the same way low dimensional topology is harder than in higher dimensions.

Something which impressed me about Langlands was his desire and ability to master several foreign languages, like the fact that he wrote part of his paper with Harder and Rapoport in German; my own German improved studying that paper minutely, which incidentally exposed me to that beautiful word \`*ausgezeichnet*' (distinguished)! When I got to know that he was fluent in Turkish, I became more than a bit curious, as his visitor Cihan Saçlıoğlu told me he thought many names of dishes in Indian restaurants seemed to be cognates of Turkish words. This led me to study a bit of the etymology of words in Hindustani, which is really a continuous spectrum of languages between Hindi at one end and Urdu at the other, with people in different regions of India speaking at a different point in the \`moduli space.' Indeed many words have Turkish origins; even the name of the language Urdu means \`army' in Turkish, but it also means \`camp' in Farsi, so we can't be sure where it came from; but we do know that Urdu evolved when the Mughal army recruits from Tajik and Uzbek lands to the north (in Central Asia) began mixing expressions of Turkish, Farsi and Sanskritic origins; some Arabic words got thrown in as well. Like children do in many of those lands, I also got exposed to the stories of Nasruddin Hodja in India, especially the comics made in Bombay (now Mumbai). The word Hod (or Khod) is of Farsi origin meaning `God,' and Hodja is a cleric. The Albanian name Hoxha, pronounced exactly as Hodja (though routinely mispronounced in Europe), means the same, apparently, and it presumably came from Turkey. Anyhow all this led me to learn a little bit of some Turkish; the subtitle of this *recollection article* is in it and means the same as the English title.

Langlands also shared with me some of his unpublished notes, one on the representations of abelian algebraic groups, another giving his thoughts on the theta correspondence, and yet another on his factorization of the Artin root number. The very first one, which extended class field theory from the multiplicative group to arbitrary tori, was later published in the Olga Taussky memorial volume, in a special issue of the Pacific Journal of Mathematics in 1989. The last one never got in print, perhaps because, soon after his result was known, Deligne found a short global argument. It is still very valuable to understand Langlands' approach, which could be helpful in other contexts.

I quickly learned that Langlands' vision was so broad, and all encompassing, that I could find problems which probed and combined my three areas of interest, namely Representation theory, Analytic theory of L-functions and Arithmetic Geometry. He also said once: `Think conceptually in the most general framework, but then prove something non-trivial in some case!' He also disliked routine extensions and incremental advances, and approved of taking bold and courageous steps. I have always treasured his advice and have passed on to my students some of the wisdom he imparted.

One of the works of Langlands I am enamored of is his old paper on the volumes of fundamental domains of arithmetic groups Γ. Expressing the volume as the integral over the fundamental domain Ω of the constant function, which is orthogonal to the cuspidal spectrum, and using his spectral decomposition of this orthogonal part in $L^2(\Gamma\backslash G)$ via Eisenstein series, he expressed the volume of Ω as ratios of products of suitable L-values, generalizing old formulae of Siegel. I think this method of his is a tour de force. I was originally thinking of writing an exposition of it for the second part of this volume, but then I found out (through

Clozel) that Michael Rapoport had already written such an exposition, which can be found on his website, and I have nothing more to contribute to it.

I would be remiss if I do not acknowledge the hospitality shown by both Bob and Charlotte Langlands over the years. Charlotte, among her many talents, is a gifted sculptor, and it is always a pleasure to view her astounding works. The bust she made of Weil is at the Institute, and the one she made of Harish Chandra is at the HRI (Harish Chandra Research Institute) in Allahabad, India.

It is very rare indeed that a perspicacious person such as Langlands enthralls us with his insights, making fundamental contributions in an ancient and challenging subject. Therefore we are grateful to be so fortunate. Many future generations will continue to be influenced by his thinking and predictions. In one sense, a lot has been proven and clarified due to the works of several people, in particular in the field of Shimura varieties and its relationship to Galois representations, and also for geometric analogues where Q is replaced by C. However, from another perspective, only the surface has been scratched and many basic questions still remain unanswered - even for GL(2) over Q, which bodes well for the future.

It is not in the stars to hold our destiny but in ourselves.

William Shakespeare

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