FARZIN BAREKAT INTERVIEWS ROBERT P. LANGLANDS

1. **Please tell us about your experiences at UBC as an undergraduate and as a masters student.** I was very taken by your first two questions because my years at UBC were a decisive period of transformation for me, but to answer them requires a great deal of explanation: what I arrived with from a childhood in British Columbia and what I took from the University. They are certainly the questions of most interest to me. Once I started to answer them there seemed to be no stopping. In an interview, answers are expected to be reasonably brief, but your first question, and the second as well, aroused in me, an aging, almost ancient, man, a desire to evoke my childhood, a childhood passed more than 60 years ago in a land, British Columbia, that certainly had many different qualities then than it has now. Since this evocation is for my own sake I do not expect you to use it. Nor do I expect that you can use all the detail provided in the answers to the first and second questions. On the other hand, my time as a student was the major transitional phase of my life, and it is not possible to appreciate its significance for me without understanding what I brought to it.

I was born in 1936, in New Westminster, then a small, in my memory, delightful city with chestnut-lined streets, the trees planted in what was called the boulevard, a generous lawn between the sidewalk and and the curb. In several of the streets there was a supplementary strip of park, also with chestnuts, separating the two directions of traffic. My first years were not, however, spent there. They were spent on the coast in a hamlet south of Powell River and slightly north of Lang Bay. It was a very small group of houses, to the best of my recollection, three summer cottages. My father had found work at the nearby Stillwater dam. The five or six years spent there must have been broken occasionally, by trips to New Westminster to my parents families, my mother's large, my father's smaller but substantial, so that all in all there were twelve aunts and uncles together with their husbands or wives and children. But I have no recollection of such excursions, simply of my mother, my father, a younger sister, and towards the end a second baby sister. One of the other two cottages, perhaps better described as cabins, was occupied by an elderly woman and a child, who I recall was her granddaughter. That they had a goat has also fixed itself in my mind. I also remember a field surrounding their house, but it could have been small. The other house and its inhabitants I do not recall. It may have been occupied only in the summer. Like ours, it was bounded on one side by the beach and the Straits of Georgia, and on the other by a forest or swamp. The only vegetation that fixed itself in my mind was skunk-cabbage.

When the time came for me to attend school, and perhaps for other reasons, we returned to New Westminster, where I was enrolled by my mother, a Catholic from an Irish-Canadian family with its origins in Halifax, in the parochial school, St. Ann's Academy. It was a school, taught partially by nuns, especially in the first two or three years. They were young, pretty I suppose, and encouraging, so that I enjoyed these first years, taking three years in two, or four in three, but then I was moved for the later years of elementary school to the companion academy, St. Peter's I believe, which was less friendly, with a morose beadle, prompt to

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resort to his baton, and I grew restive. By the time I reached 6th grade just before my 10th birthday, we had moved again, to White Rock, where I learned less, indeed pretty much nothing at all, but which also gave me a great deal.

New Westminster, during the war years was agreeable for a child. There were very few automobiles. They were not yet the lords of the road and there was little if any danger from them. They slowed down, even stopped for us, as we played in the street. The available area for me was from the north at 8th Ave. to the south at Columbia St., and from Queen's Park in the east to 12th St. in the west, although my natural limit to the east was more 6th St., where there was a movie theater for Saturday mornings and one dairy, Drake's Dairy, with two ice-cream parlours, almost side by side. So I had about one-half a square mile in which to freely roam, although I was not very nomadic. I had grandparents and many cousins close to 12th St, or even on the other side of 12th, but it never occurred to me to visit them on my own. The more limited area, about one-half a square mile in all was more than I needed. It contained home, school, the homes of a few school-friends, the public library, where my reading was confined to books like the Dr. Doolittle series, the CKNW studios on Columbia St., where I went once or twice on my own to see the western singers perform—I think the one best known at the time was Evan Kemp—and the Arena, which I may have visited a few times for ice-skating and to which I once accompanied my uncle to a lacrosse match, first to the dressing-room and then to watch the game from the player's box. My uncle, Robert (Bobby) Phelan, was at the time a well-known local athlete, and in particular played lacrosse for the New Westminster Adanacs. Canadian sports had an independent existence then and lacrosse was a popular sport in Vancouver, New Westminster and in the East, or rather in Ontario, and the annual national contest between the leading western and the leading eastern team seemed a major event to me. In fact, I suppose, box-lacrosse had been only recently introduced and its popularity was brief.

Although we were only a short time in New Westminster, it was the period when I made the acquaintance of my many aunts and uncles. My father's parents were not so much stern as pious. In my father there were only unreflected remnants of the piety. In England my grandparents may have been Methodists but in Canada they were members of the United Church. I recently concluded, on the basis of various evidence and with the help of a friend from Hobart, that my grandmother's paternal grandfather had been a private in the British Army, who met a premature death, perhaps in a revolt of convicts, in Tasmania and that his grave with its tombstone lies there on the Isle of the Dead. In an essay on Seventh Day Adventists that I happened to find on the web, it is asserted, on the basis of the inscription on the stone, that he must have been a member of their congregation. On the other hand, my paternal grandfather's two sisters were both married to clerics presumably in the United Church, one a minister, the other a missionary in Kispiox, near Hazelton. My great-grandfather is buried there. As a child I had no interest in such matters and found my father's parents and his sisters and brother, in comparison with my mother's family, a little colourless and distant. My mother's family was, at least for a child, much warmer. Her parents, had drifted west from Halifax, trying their hand at land-grant farming in Saskatoon before reaching New Westminster, where my grandfather had worked at first for the CNR. My grandmother was already a widow when I was born and even before my grandfather's death had had to assume a good deal of responsibility in difficult financial circumstances for her ten surviving children. She had many grandchildren, a few older than me, several of about my age, and a good number younger. Most of the latter I never met. My grandmother had affection and energy enough for all of us, although she cannot have been well. Her heart failed after a bus-ride to White Rock and a walk up a steep, dusty hill on a very warm day as she was coming to visit my mother when I was ten. Since I left British Columbia I have seen my aunts, uncles, and cousins on both sides only infrequently, but it has always been a pleasure. I still correspond occasionally with my mother's only surviving sister.

A good number of her brothers were longshoreman, all I think foremen or supervisors. Two lived on small farms in Surrey that were always a pleasure to visit. Surrey and even White Rock, then a part of it, were still rural. Outhouses were perhaps not the rule but still common. I also had distant relatives on my father's side with a genuine farm at the source of the Campbell River. We did not visit it often, but there were orchards and a herd of cows that we were allowed to try to milk, unsuccessfully of course. There were no children. The wife was the relative; the husband, Len Rowlett, was much younger and survived her. He was, I believe, the owner of the last horse-and-carriage in Langley and drove it, I have heard, until an advanced age in many local parades. He was also a waterdiviner, so far as I know successful. Although there was no close relation between him and my parents, I did, returning to BC on one occasion to visit my mother, who was ill, run across him, now an old man, in the Royal Columbian Hospital in Sapperton. He was still cheerful and lively and described to me less his career as a diviner than his knowledge of the location in the Fraser Valley of underground water and its currents. It was instructive.

I have not visited White Rock for a very long time, several decades. I would suppose that the town of my youth was quite different than the town of today. The beautiful shingled beach had not yet been damaged, perhaps even destroyed, by foolish merchants and bulldozers. On the other hand, the railroad track, the embankment of boulders, the logs escaped from the booms and washed up along the shore were there and for me, for all of us, part of the natural environment, to be enjoyed like the sea itself. Neither did we notice that the forest was gone, the occasional large stump being simply a curiosity, nor were we troubled by the very small number of Indians, the word used then, who had survived the epidemics of the nineteenth century. We were largely unaware what the surroundings may have been one hundred years earlier. Even the more immediate past was already forgotten. I have seen a photo of my father and two sisters, all infants because he was a twin, with their mother before a tent in the forest, probably on Vancouver Island, but he himself never once mentioned those early years.

The pier was, at least after I learned to swim and dive, an attraction. In the summer, it was where many youths who had left high-school early but were not working at the moment as well as boys still in school—no girls—lounged about in the sun. I found my way there when free of responsibility. In the early post-war years there were also not a great many cars, except on weekends, when there was a flood of visitors of both sexes from the tougher areas of Vancouver to the dance hall, the Palladium, on Saturday evening. On Sunday the main street, Washington Road, which ran, to the extent permitted by the topography, along the shore, was devoted entirely to cruising. In my early adolescence, I admired these people, their clothes, above all their freedom, their independence, but I was too young to imitate them. By the time I might have been in a position to do that, I had other goals.

They were a source of income for a time. One could collect the empty beer-bottles, thrown out of the parked cars on Saturday evening, either on Saturday evening itself or on Sunday morning, and then at a rate of either 10 cents or 25 cents a dozen—I forget—bring them to an elderly man, who in turn returned them to the Lucky Lager brewery, also in Sapperton.

There were other sources of income, a newspaper route for the Vancouver Province to which I was faithful for a year before I tired of the inevitable loss of freedom. For six days of every week, one and a half hours of the afternoon had to be given over to collecting and delivering the newspaper. I also remember changing the marquee at the movie theater, next to the dance hall, but visited only by local residents, so that the featured movie ran just for two days, changing three times each week. This meant three visits a week at 10:30 in the evening to the theater in exchange for free access. At first I was delighted with that, but as I was then old enough that the obligatory visits interfered with my social life, I soon abandoned the marquee even though the theatre was only a few hundred yards from our home and business.

Actually my first two or three years in the town had been carefree. I was a little older than in New Westminster, a little more independent than I had been there. The sea and the shore were immediately accessible; the school, now a public school, was mixed. I enjoyed that. Each of us is a child only once, so that it is difficult to distinguish what is particular from the atmosphere in which we grew up from what was particular in us, but reflecting on White Rock later, it seemed to me that many people, the elderly, women with children and no husbands or with children and husbands, but no obvious source of income, were there because, as a resort town with no economic activity except in the summer, its housing, in motels or ramshackle summer cottages, was cheap, or because life elsewhere was for some other reason too difficult for them. It was only very much later that it occurred to me to apply the observation to my own family.

My parents did succeed for some years in establishing and maintaining a business, millwork and builders supplies, so that we were better off than many. After the age of twelve or thirteen I worked afternoons, weekends and summers there, and continued working there in the summer even after I began university. It was typical then, and may still be now, for students at UBC to work in the summer to earn enough for room, board and tuition during the university's relatively short fall and winter terms.

School, except that it was a place frequented by girls and my friends, meant little to me. Although for a short period I was large for my age, I was also younger than almost all my classmates and maladroit, so that I had little success with the limited athletic possibilities. I was probably the despair of the teachers, who, perhaps from the results of IQ tests, were aware that I had considerable untapped academic potential, from which I refused to profit.

In my last year, in grade 12, we had an excellent teacher, Crawford Vogler, with a newly designed textbook, and a newly designed course on English literature. He is one of the people to whom I owe most and for a very specific reason. Unfortunately, by the time I undertook to thank him personally, it was too late. Although still alive after a successful teaching career in the lower mainland, he was, as I learned from his son, no longer in any condition to appreciate expressions of gratitude. In the course itself, he singled me out as one of the students who could usefully present a report on a novel and assigned me Meredith's novel *The Ordeal of Richard Feverel.* He had overestimated me. I read the novel but had no idea what might be said about it, or perhaps I hesitated to express my feelings. It is, after all, a novel of young love. Nevertheless toward the end of the year, he took up an hour of class time to explain to me, in the presence of all the other students, that it would be a betrayal of God-given talents for me not to attend university. I had had no intention of doing that. None of my classmates did; at the time very, very few students, at the best one or two, did so in any year. I was flattered by his comments, my ambition was aroused, and I decided then and there to write the entrance examinations. I worked hard and was successful, even winning a small fellowship from the University.

There was another factor in my changing stance. I have acknowledged elsewhere that I was tainted even at a fairly early age by ambition, but could never satisfy it. I also was not incapable of intellectual or moral passion. I had decided at the age of seven or eight, not long after beginning to attend the parochial school, that I would become a priest, building myself an altar with improvised paraphernalia in my bedroom. It probably delighted my mother to see this early sign of a vocation, but it soon passed. I also decided not long before or not long after to learn French from the lessons in the Books of Knowledge that my mother had bought from some itinerant pedlar, but the little family setting off across the Channel for a vacation, perhaps in Paris, perhaps in Provence—I never reached the end of the lessons—was soon abandoned. I went off to White Rock, where there were Franco-Canadians, but at the time they either kept quietly to themselves or were integrated.

By the time I read Meredith, I had met the girl whom I was later to marry and to whom, although she is no longer a girl, I am still married many years later. In retrospect, I am astounded that by good luck, certainly not with any foresight, I found in a little town at an early age and with no guidance, someone who could give me so much, in so many ways, in so many different circumstances, and for so long, without sacrificing her independence or totally neglecting her talents. Her father, born to a Franco-Canadian, more precisely Acadian, father and Irish-Canadian mother in a village on Prince Edward Island with a substantial number of French-speaking inhabitants, at least at the beginning and even in the middle of the last century, was, to some extent, one of those integrated Franco-Canadians. Having lost his mother as an infant, he grew up in a Gaelic-speaking foster family, but went off to the logging camps at the very early age of twelve. When I met him he had spent the best part of his life in Ontario and the Lower Mainland, some of it during the Depression, unemployed and drifting. We were used to parents, especially fathers, with little schooling. My own father had eight years. His arithmetic was excellent but his reading was on the whole confined to the sport pages. My wife's father had only one year. It was at the age of 30, during the Depression, that he learned how to read, and in principle to write, at the classes that the parties on the left organized for the unemployed. He also acquired a small library, but he could not really read with ease. What he often did, however, was to recite by heart various socialist texts that he had learned more than thirty years earlier. This was of some appeal to me at first but quickly palled. By his books, however, in particular one with biographies of those savants who were heroes to the socialists, Marx, Freud, Hutton, Darwin and several others, I was inspired with the ambition to be a savant. It is odd, but this ambition has never faded. It is the ambition with which I arrived at the University.

2. **What memories (good or bad) do you have from your years at UBC?** Since it is hard to distinguish the first question from the second and since I have spent so much space preparing for the answers to them, I may as well answer them both as one, including at the same time a response to the first part of the next question.

Arrived at the University, I took, as was common at the time and as was perfectly appropriate for anyone with my lack of academic experience, aptitude tests. The results were predictable. In those domains, mathematics and physics, where, at least in the context of such tests, only native talent matters I did extremely well. In the others I also did well, but not so well. So the university counselor, whom one was encouraged to consult after the tests, suggested at first that I might want to become an accountant, even a chartered accountant.

This lacked all glamour. So he then suggested mathematics or physics, cautioning me that this would require a master's degree or even a Ph.D. The latter meant nothing to me, but I did not acknowledge this. I decided on the spot that I would become a mathematician or physicist. The Ph.D., whatever it was, could take care of itself. As soon as I returned to White Rock from the University, I looked up my future father-in-law, found him in bed, and learned from him what a Ph.D. was.

Having set out to become a mathematician and a savant, I was as systematic as possible, even buying a copy of Euclid in the Everyman's edition to remedy, as I thought, my neglect of elementary geometry, but I was also not very imaginative. I took the courses as they came. In the first year at that time, these were standard. French, English, physics, chemistry and mathematics, mathematics being largely trigonometric identities. The English course was the most valuable. I did not know how to write, or rather my orthography was excellent but my notions of grammar inadequate, nor did I yet have, in spite of Meredith, any notion of English literature. The teacher, Prof. Morrison, a small man whose jackets were always too large for him, was excellent. He must have found me a little puzzling. I was eager and attentive to his corrections of run-on sentences and other blunders in the essays that I wrote. I also verified between classes the meaning of every unfamiliar word in every poem assigned. Of course no-one else did, so that I was constantly alone in raising my hand. In retrospect I have found over the years that the habit of consulting a dictionary when reading one's own tongue or foreign tongues is very useful. In the French course, I was also industrious but with no experience with foreign languages went about matters in entirely the wrong manner, so that I did not learn French in a serious way until many years later. The other three courses offered no difficulty, but I did not pursue chemistry beyond the first year. I was focussed.

At some point in the first or second year, because of my intention to choose honours in mathematics, I spoke with Prof. S. Jennings. For me, he became later a somewhat comical figure, dapper but a little soiled, who reminded me of the Penguin in Batman comics, although I cannot say for certain that he always carried an umbrella with a crookhandle. I shall return to him as a teacher, but in this interview he gave me a piece of advice for which I am grateful to this day. The year would have been 1954, thus not long after the war, when the atmosphere that reigned in mathematics before the war had not dissipated. He declared that to be a mathematician one had to learn French, German and Russian. He did not mention Italian. I thought I knew French; I bought a second-hand copy of some basic German grammar from an acquaintance who had taken the course, read it over the summer, thereby in my view at the time successfully learning German, and as one of my courses for the second year chose Russian. German, like French, I only learned in a serious way many years later. I speak and write them both with considerable ease. Russian I finally learned to read tolerably well and can write it with considerable effort and frequent consultation of a text with lists of the declinations and the forms of various participles, active, passive, present and past, but have never, to my very great regret, learned to speak it. It is, or rather can be, a very beautiful language. Russia, in my lifetime, has not been a country that it is possible to visit professionally with any ease. Readings of the classical Russian texts and of modern texts have been, fortunately, readily available since 1989. The instructor, Irena Carlsen, from whom I also took a second year of Russian—in my fourth year at the university—I remember with great affection. I would like to think that she is still alive. From, I would guess, a family of White Russian emigrants, she was not a great deal older than the students and a passionate fan of Russian opera, so that we spent most of our time listening to it, not such a bad introduction to any language. I am not, however, very musical, so that my attachment to Russian was formed more by the written word than by opera.

Although I came late to languages, my experience as a student being brief and superficial, and although it was not until I was more than 30 years old that I had any experience in speaking them, the first being not a European language but Turkish, and it was only gradually, during visits to, even long stays in, Germany, Quebec, and France that I acquired any proficiency, the seed, the romantic desire to penetrate the present and past of the enchanting, mysterious or seductive tones of a tongue not sung at my cradle, was planted in me as a student. This desire came to me simultaneously with mathematics and could still in the seventies when I finally reached Germany be satisfied within the mathematical community. That is no longer so. Even aside from the distance the universal use of English, even when unnecessary or inappropriate, creates between mathematicians and the past of mathematics, I am troubled by the general loss of intellectual opportunities in a mathematical career that it entails, and not alone for those for whom it is the mother tongue. Through the advice of S. Jennings, thus through mathematics, and through opportunities given me as a mathematician, I was eventually led to the very words and the very sounds not only of Gauss, Galois or Hilbert but of Thomas Mann, Proust, Pasternak, or even Giuseppe Tomasi di Lampedusa and Ahmet Hamdi Tanpinar, not to speak of Robert de Roquebrune or Michel Tremblay, or Mommsen and Michelet. I am afraid such possibilities are no longer available. Unfortunately, I myself have not yet reached the classical languages or any truly exotic language, except in an inadequate way. I like to think there is time left to remedy this.

For English in the second year, an introduction to classical English literature, from Beowulf to the Romantic Poets, we had Earle Birney. That should have been appealing and I was at first very pleased, for Birney was at the time the best known of the local poets and I had read some of his poetry. I had also wanted, from a kind of silly, naive curiosity, to ask him a question. In one of his radio scripts, *The Damnation of Vancouver,* the medieval English poet William Langland appears, storming down from Little Mountain to warn the city of its sins. Since my grandfather had had a brother William Langlands who lived, I believe, in the vicinity of Little Mountain, I wanted to ask Birney if, perhaps noticing this in a telephone book, he had been inspired to include him in the script. But Birney, although a poet still, I find, worth reading, was, apparently, a snob for whom teaching ignorant second-year students was a burden and he left the room at the end of each class just as quickly as he could. So my question was never asked. The answer is probably “no”.

The other courses in the second year would have been basic calculus, physics beyond the basic mechanics of the first year, including some thermodynamics, and perhaps, to fill in, as there were not many options, logic. The logic course, whether taken in the second or third year was not much use at all. It was given by someone who had come to Canada to escape the loyalty oath in the McCarthy era, itself a laudable step, but he was unfortunately also an indifferent teacher. As we shall see there were at the time in mathematics at UBC a number of teachers who did their very best, but there was also a substantial fraction of mediocre ones.

In algebra there was a basic course in the theory of equations, with an excellent textbook by L. E. Dickson and an excellent and enthusiastic teacher, Robert Christian. We must have covered the section on solutions of the equations of third and fourth degree by radicals, but I do not remember having been struck by it or aware of its importance. A curious insensitivity! Determinants, especially the Vandermonde determinant and its symmetry properties did strike me, because I had trouble at first understanding the reason for them. The course must also have been given in the second-year, but as a half-year course. I am not sure. Then the other half of the year would have been a course in linear algebra from Murnaghan's book, perhaps again taught by Christian. Certainly Christian recommended Halmos's book on vector spaces to the class as extra-curricular reading. I bought it and read it over the summer, without, however, fully understanding it. Sometime after that, I acquired, ordering it by mail as there were certainly no mathematics books to be had in Vancouver, the Chelsea edition of Schreier and Sperner's *Modern Algebra and Matrix Theory.* That was perhaps my real introduction to linear algebra, especially to the theory of elementary divisors. Halmos's book was nevertheless my introduction to abstract mathematics and I fully appreciated its beauty, perhaps even allowing it to distract me from the substance of other topics. Christian had just arrived at UBC. So had Marvin Marcus, who was also full of enthusiasm. I do not clearly recall having had him as an instructor at any time, but reflection suggests that he must have been the instructor for basic calculus in the section for those who hoped to continue in mathematics. I certainly recall no-one else and do recall that Marcus recommended on some occasion the use of Courant's two volumes as a supplementary text. I acquired it and, although I did not study it thoroughly, certainly studied parts with care, profiting greatly from it. At some point, perhaps in my third year, Marcus began a seminar on the Laplace transform that started out very well. I was anticipating its continuation with pleasure but, so far as I can tell, it just stopped. Perhaps most of the students who took part in it were having trouble. There were in any case very few. A year or two later, I found D. V. Widder's book on the Laplace transform in the University of Washington book store on an excursion to Seattle and read it with pleasure.

I had met Roy Westwick already in my first year or at least early in the second, through his brother Henry, who was in the same year as I and had also come to UBC with the intention, I believe, of taking an honours degree, but whose career finally took another turn. He became a very happy, very successful dairy farmer on Vancouver Island. Roy, a year ahead of me, was the mathematics student to whom I was closest. His background was not so different from mine, but his ambitions were different and he spent, with only brief and occasional absences, his entire career at UBC. The only other student from his year whom I remember or have since met is Ian Hacking, now a distinguished philosopher in Toronto and Paris. He once visited for a year the School of Social Sciences at the Institute for Advanced Study, where I have spent almost forty years, and where, in my first years, UBC was the single university represented on the faculty by two alumni, myself and Homer Thompson, an archeologist from Chilliwack, whose background was different from mine although we were both from the Fraser Valley. His father, although a farmer, had had a sound education and continued to read Greek. Homer had graduated about 30 years before me, but was delighted by my arrival and I always appreciated his presence. He was a gentle man responsible, I believe, for the excavations in the Agora in Athens. The year of Hacking's visit there were several temporary members who were alumni of UBC. Homer and I organized a joint luncheon, but Hacking, presumably embarrassed by such fits of sentimentality, excused himself.

In my year there were only two other honours students, Roger Purves and Klaus Hoechsmann. They seemed to be constant companions and spent a lot of time in the Green Room, a centre not only for the students with a passion for acting but also for some of the campus poets. The relation of both to mathematics was curious. Roger, whose parents dwelt in a good-sized house in Shaughnessy, had a curious languishing air and, in my recollection, was constantly complaining of the difficulties and the tedium of German grammar. To my astonishment not only did he attend graduate school in Berkeley, but is now a professor of statistics there. Klaus had entered university in a cloud of glory, performing brilliantly on the entrance examinations, so that he appeared in both the Vancouver dailies, an immigrant lad succeeding in spite of all impediments in the new land. Oddly enough I never learned anything about his childhood. I may have heard later in Germany that he was a Transylvanian Saxon, but this would have meant nothing to me in 1954. When it turned out that we were both aspiring mathematicians, I was prepared to admire him greatly and to remain in his shadow. However, or so it seemed to me, his interest in mathematics waned during his undergraduate years. It was a genuine surprise, when 10 or 15 years later, I discovered that not only had he continued his studies with Zassenhaus at McGill and Notre Dame but that, later, he had also spent several years in Germany where he made a name for himself in algebraic number theory. I have several German friends, also distinguished number theorists, who knew him well there and who described to me his conversational brilliance and expressed their appreciation of his mathematical talent. I not long ago learned from one of them his characterisation of me as a young man as *unbedarft.* This is amusing, fair, and perhaps even today just. It will be evident from my responses to your questions that I must have been naive, ignorant and self-absorbed. As, in my own eyes, a potential savant, I nevertheless did not see myself as a bumpkin! I have since met Klaus on several occasions, in Bielefeld, Vancouver and, not so long ago, Quebec City. Like me, he is now considerably older. He may also be more subdued than he once was, but in some respects he has not changed over the years. He still prefers, at least with me and perhaps unwittingly, to keep the upper hand in conversation.

In the third year, there were three honours courses in mathematics, differential calculus, integral calculus, linear algebra and algebra. The first two were intended to be taught in tandem, problem sets alternating between them. Eugene Leimanis taught the first; Frederick Goodspeed the second. There is an obituary of E. Leimanis in the Bulletin of the American Astronomical Society. His field of research was celestial mechanics and he was certainly one of the most serious mathematicians, perhaps the most serious, at UBC at the time. He was, at some point, a member of the executive of the Latvian Evangelical Lutheran Church in America. He had only received a doctorate after the war, already in his forties, from the Baltic University in Hamburg, a short-lived university with a small number of graduates that had been created in the refugee camps for displaced persons, and he had not been in Canada long, was not yet very comfortable with English, and, I suppose, in any case somewhat reserved. He lectured invariably in a suit and tie. Later, in the year I spent on the master's degree, I took a graduate course from him with material from the Annals of Mathematics Study *Introduction to Non-linear Mechanics* by Kryloff and Bogoliuboff, but I have no clear recollection of the content. His style had not changed in the intervening years. Nevertheless he was conscientious in both courses. The first problem set in differential calculus was well-prepared, at the appropriate level, and the problems a pleasure to solve. Then we waited for the second from Goodspeed. It never came. Goodspeed, as I recall a South African with a doctor's degree from somewhere in England, perhaps Oxford or Cambridge, was a jovial fellow but completely irresponsible, not only with the problem sets but with the course itself. I learned nothing from it. Fortunately I already had Courant in hand, so that whatever I know about multiple integrals, Stokes theorem and so on, I could learn from it. Linear algebra, taught by Stephen Jennings was no better. As I observed earlier, I have a genuine debt to him and as a specialist in group theory and the advisor to Rimhak Ree he deserves respect, but in regard to his responsibility to beginning honours students he was no better than Goodspeed. Both improvised, regaling us often with stories with little or no relation to the material at hand. Luckily, for linear algebra I had both Halmos and Schreier and Sperner.

The fourth-year courses were much better. There was a variety, most competently taught. Galois theory was the responsibility of Rimhak Ree. He did his best, but, in retrospect, I do not think he communicated the essence of the subject, nor did he find any of the students very promising. He was still young then and his work on Ree groups was still in the future. I believe the course on function theory was taught by Christian. The text was the three small Dover volumes by Konrad Knopp. Function theory I fully appreciated from the beginning and, on my own, went on to the last volume, especially the chapter on the Weierstrass elliptic functions. That too was a pleasure. Douglas Derry gave a course on convexity from the book Внутренняя Геометрия Выпуклых Поверхностей by A. D. Alexandrov. Although we did not move very quickly, it was a treat to use the Russian I had already learned. Derry was the most cosmopolitan of our teachers. He had, I discovered while writing this text, taken his doctor's degree under Hasse at Göttingen in 1938 and may have spent some time, either before or afterward, in Italy. He was a tall, quiet man with an air of distinction. There was also a course on methods of applied mathematics, basically special functions, separation of variables, and second-order boundary value problems, all topics that appealed to me then and have appealed to me over the years, although I have had little time to cultivate them. The teacher was Thomas Hull, a melancholy but helpful fellow, who went on to develop computing at the University of Toronto.

There were two seminars that year in which I eagerly participated. One by Harry Davis was on modern functional analysis, as practiced in MIT where he had taken his degree. As a graduate student, I also, during my year as a candidate for the master's degree, took a reading course with him from Dixmier's newly published volume, *Les algèbres d'opérateurs dans l'espace hilbertien : algèbres de von Neumann,* but I did not acquire any real understanding of the subject. It was certainly not the only time that in my eagerness to move ahead rapidly, I plunged in over my head—fortunately not far from shore. On another occasion, but as an undergraduate, certainly inspired by a course on classical mechanics in the physics department, I undertook to deliver a lecture from Bliss's *Lectures on the Calculus of Variations* to the small Mathematics Club. I understood absolutely nothing, but as no member of the audience, not even the faculty sponsor, understood anything about the subject either, it is possible that they did not appreciate just how confused the lecturer was. I enjoyed the conversation of Harry Davis, but he was not happy at UBC. I believe that circumstances, perhaps difficulties with colleagues, ultimately forced him to leave Vancouver for the University of Waterloo, where he, I believe, found domestic contentment, but where, I suspect, happiness continued to elude him. The second seminar was directed by David Murdoch, whose distinctive personal trait was that his nose constantly ran, adding to an already melancholy air. His handkerchief never left his hand. He was not, however, particularly melancholy. The topic was Noetherian rings, from the pamphlet by Northcott. It is an elegant little book that I enjoyed reading, but later experience suggests that a more concrete introduction, through algebraic geometry, is a better way to acquaint oneself with modern algebra.

Although it may appear that I quickly abandoned the desire to become a physicist or even to acquire some understanding of physics, the desire persisted and I took a large number of courses that I enjoyed, both as an undergraduate and during my year as a graduate student at UBC. My experience suggests nevertheless that it is easier to learn mathematics on one's own than physics. It also suggests that my natural aptitude for mathematics was greater than my natural aptitude for physics. Physics appealed to me, especially mathematical explanations as in the treatise of Maxwell on electromagnetic theory or that of Raleigh on the theory of sound and I would read such books or those of more modern figures, such as Neils Bohr, with pleasure. I fear, however, that I was more attracted to the mathematical explanations than to the physical phenomena themselves. The department of physics had in my time some good experimentalists, but the theoreticians were perhaps weak. F. A Kaempfer, from whom I took a course in the third-year, had some brilliance, but I am not sure that he could have corrected my weaknesses or that I would have made a favourable impression on him. Besides, by that point it was too late. The coming divergence manifested itself in my second-year. The second-year physics course contained some thermodynamics which fascinated, puzzled and troubled me. For some reason or other that I no longer remember I had occasion to submit my theoretical reflections to the instructor, James Daniels, a low-temperature experimental physicist from England, probably better than competent, but he was somehow offended by my attempts to deduce the laws of thermodynamics from the perfect gas laws or conversely. I have no real recollection of what I wrote, but he, apparently having little respect for such philosophical lucubrations, chose to mock it in class. I did not take his mockery very seriously, but I did not appreciate it either. Now, of course, I know that such a reaction to the genuine attempts of a youth, just turned 18 and with previous experience typical for that time and that place, to grasp very difficult material is entirely inappropriate, even reprehensible. Certainly, it was one factor in my decision at the end of the year not to continue with the double honours course, but to concentrate on mathematics.

I nevertheless continued in the third year to take some courses in physics: optics; thermodynamics; and classical mechanics, the course of Kaempfer. These I enjoyed very much, especially the optics course, which included experiments. Clumsy in the laboratory, I was fortunate to have an experimentally gifted partner to whom my theoretical explanations were useful. I believe he worked for many years in a government research institute in Ottawa. His name was Alan Goodacre, and although a search on the web yielded more than one Alan Goodacre I think I have found him. I have sent him a letter, hoping that he is still alive and will reply.

As a graduate student I again took some courses, one in electromagnetic theory, with an instructor of somewhat doubtful competence, another in group theory and quantum mechanics, given by Opechowski, a professor in the department, whose competence I could test myself. The course was basically a matter of the use of orthogonality relations for characters to decompose representations of finite groups. The final examination, a takehome exam, was to decompose the representation of the tetrahedral group in the space given by the coordinates of four particles moving independently at the vertices of a regular tetrahedron. His idea was that we should use the same orthogonal coordinates at each vertex and character formulas. It was not brilliant. It is clearly much better to take as the coordinate axes at each vertex the sides passing through that vertex. Then the representation can be decomposed by inspection. This I did. He was utterly confused by all the zeros and ones, but persuaded that it was I who was confused and who, in the hopelessness of my confusion, had introduced the regular representation into the problem. There was no convincing him otherwise. I expected to fail the course. This did not happen, but I did receive a very low grade. It was the final evidence of an incompatibility, if not between me and physics, certainly between me and the physics department at UBC.

It was not possible at the time I was an undergraduate to take books from the library at UBC—there was only the main library—or to visit the stacks. It was nevertheless my first encounter with a library after that on Carnarvon St. in New Westminster and I profited from it as much as whatever knowledge I had would allow. It was a pleasure to handle the books.

3. **Why did you study math, and what led you to work in the fields of automorphic forms and representation theory?** There are two questions here. The first was answered in my response to the first two questions. The second is best answered when responding to the next question.

4. **Why did you choose to go to Yale University as a PhD student?** I was eager to finish the Master's degree as soon as possible and to continue with what seemed to me genuine graduate work. I applied to three universities all American, why only American I do not know. It would not have occurred to me then to go abroad, thus to Europe or Great Britain, and no-one suggested a Canadian university. That was probably just as well. I applied to three institutions: Harvard, Wisconsin and Yale. Yale because Robert Christian had obtained his doctorate there and spoke to me often of the many functional analysts who were active there, why Harvard I cannot say. Perhaps because Benjamin Moyls was formally the supervisor of my Master's thesis and that is where he had taken his Ph.D. Wisconsin was probably simply in case I succeeded at neither of the other two. I was accepted by all three, but Wisconsin was without aid. I would have had to teach. I had discovered in my year as a candidate for a master's degree at UBC that teaching interfered with learning mathematics. So I did not hesitate to decline Wisconsin. Yale offered a fellowship that would, with almost no help from my family, support both me and my wife, who would not be allowed to work in the USA. Besides I had some familiarity with the mathematics of the faculty at Yale, above all of Hille and Dunford, thus functional analysis, but informed by classical analysis. I was accepted at Harvard but with no support. So the choice was evident.

In retrospect, it was by far the best choice. I finished at Yale in two pleasant years, indeed the thesis was written in one, so that I had a great deal of time after it was finished in which to think about various problems and to learn various techniques. We had little money and by the end of the two years we had two children. So it was just as well that I was then already employable. Employment was not difficult to find in that period, but I was particularly fortunate. As far as I can recall, I took three courses in the first year. One offered by Nelson Dunford, consisted of exercises from the first volume of Dunford-Schwartz, thus basic functional analysis. One exercise in their collection had to be corrected, but it took some effort to convince Dunford of this and some time before he was willing to listen to my explanations and, I suppose, examine my counter-example. He was generous enough to include it in the second volume of the work and to attribute it to me. Hille's course consisted of excerpts from the book *Functional analysis and semigroups.* Many of the lectures were given by Cassius Ionescu Tulcea, who was formally the director of my thesis. Its first half was a solution of a problem left open in the book, in the somewhat obscure domain of Lie semi-groups and their representations. It became my first paper, appearing in the Canadian Journal of Mathematics. I had also taken a course from Felix Browder on partial differential equations, the subject on which I had hoped and expected to write my thesis. The course was largely on a priori estimates, which were quite popular at the time and one of his specialties. It, combined with some of what I had learned about semi-groups, became the second half of my thesis, never properly published, but available, because Derek Robinson incorporated it into his book *Elliptic Operators and Lie Groups.* It had in another way an important effect, because it drew me to the attention of Edward Nelson, then an assistant professor at Princeton, and on his recommendation alone, with no application, with no information whatsoever about me, the department appointed me as an instructor.

But there is more to say about Yale. In retrospect, it was extremely fortunate for me that Harvard did not offer a fellowship. I would have gone there and missed in one way and another a great deal. At Yale I was on my own and allowed to follow my own inclinations. At Harvard, I would have had to deal with fields that were both popular and extremely difficult and with fellow students who were already initiated into them. That would have taken an incalculable toll.

After all these years, I forget what I read and when. I did not prepare for the oral examinations to take place at the end of the first year. In some sense I took them cold, but I had certainly read with some care the first edition of Zygmund's book on Fourier series, available at the time in a Dover edition. I had also read in Burnside's book on finite groups, with dreams of solving the famous conjecture that all simple groups were of even order. Of course, for that conjecture it would have been better to be studying at Harvard. I think I also read then Stone's book on the spectral theory of unbounded hermitian operators. I certainly did not read it before then, and do not see how I could have found time to read it after I went to Princeton. I used techniques from it in the long paper on Eisenstein series written in 1964, so that I had to have read it either at Yale or earlier at UBC. My guess is Yale. During my year as a candidate for a master's degree at UBC I had probably tried to read both Weyl's book on algebraic number theory and Lefschetz's *Introduction to topology.* The first is, of course, a difficult book and God only knows what I understood, but I did come away with a clear notion that the law of quadratic reciprocity, which never appealed to me as an undergraduate, was in the context of the theory of cyclotomic equations genuinely a thing of beauty. I have learned in the meantime to appreciate the elementary inductive argument of the *Disquisitiones* of Gauss as a youthful tour de force, but as an undergraduate—and even today—there is much about number theory and number theorists that I felt—or find—lacked—or lacks—imagination and elegance. I confess that as a very young man the law of quadratic reciprocity represented to me the quintessence of the stiff-necked, sometimes thickheaded, obstinacy frequently found among them. Weyl opened my eyes. Lefschetz's book would have been my introduction to topology. It contained exercises, among them problems open at the time and difficult. I innocently tried to solve them as exercises and was discouraged. I never did take to topology. Whether Lefschetz is to blame or my intellectual limitations I cannot say.

What I did not do during my first year at Yale was review Northcott's book on ideal theory, which, as I mentioned, I had begun to study at UBC in the seminar of David Murdoch. That seminar influenced my master's thesis, not a very successful undertaking because I discovered just as I was submitting it that I was trying to prove a false theorem. I could not recover much useful from what I had written. There was, I believe, some question about whether it could be accepted. My guess is that the committee was generous, gave me credit for independence and enterprise, and let me go on to Yale and the next step, for which I am still grateful. There, after a year, my oral examination began badly because I could not prove the simplest things about noetherian rings. I had obviously read Northcott's book too quickly and too superficially. Fortunately for me, when Shizuo Kakutani, one of the examiners, discovered that I knew something about Fourier series he began to question me closely about interpolation theorems, which take up a certain amount of space in Zygmund's book and are probably still popular among Fourier analysts, but otherwise little known. Having recently spent considerable time reading the book, and with considerable pleasure, I could respond quickly and correctly to his examination. Thanks to this—so far as I know—I was saved and could go on and write my thesis, presumably already started and then have a year completely free, with no courses and no thesis to prepare. Since Kakutani had, for reasons I never completely understood, taken a dislike to me, and on other occasions had not hesitated to do me a disfavour, I am particularly grateful to him for discovering that, even if I did not know what I should have known, I did know something.

Actually, one of his disfavours was a great favour. What I really hoped to do when I completed my Ph.D. was to stay at Yale. I had fallen in love with the atmosphere there: I had a freedom to study and think that I had never had elsewhere. Several of the faculty encouraged me to stay, but my appointment was blocked, probably by Kakutani. So I accepted the offer from Princeton, where I had the great good fortune to meet Salomon Bochner, whose encouragement had decisive, concrete consequences. I am not sure that Bochner ever understood how much he had done for me. I was a timid young man and he was a genuinely timid old man, so that there were some feelings that were never expressed.

But I have anticipated Princeton. There was still a year to spend in New Haven. During this year the major event for me, personal life, the birth of our second child, a daughter, aside, was a course of Stephen Gaal on analytic number theory, more precisely, on Hecke theory. His intent was to prepare himself, and incidentally us, for the study of the work of Atle Selberg on the spectral theory of automorphic forms, a theory introduced by Hans Maass but given a surprising turn by Selberg. I listened to Gaal's lectures with enthusiasm and studied the laconic writings of Selberg himself. I had also at some point learned a little bit about functions of several complex variables, especially about convexity. It was preparation for a seminar on a paper entitled *Zum Begriff der analytischen Fortsetzung in algebraischen Funktionenkörpern einer Veränderlichen* that Felix Browder and Kakutani were to conduct jointly, but their quite different personalities made a joint seminar impossible. Anyhow this knowledge enabled me to establish the analytic continuation for some of the functions later known as Eisenstein series. I did not attach any importance to this initial work on Eisenstein series. It was more in the nature of an exercise. I came, however, to Princeton, where there was an analysis seminar conducted by Robert Gunning, but which Bochner, an aficionado of Dirichlet series frequently attended. He not only encouraged me to continue with the topic, in particular, urging me to read Hecke's work on the Dedekind zeta-function and related Euler products, but also had me moved one step, and later more steps, up the academic ladder. Above all, he suggested two or three years later that I offer a course on class field theory, a suggestion that he accompanied with considerable moral pressure. I was scared stiff. First of all, I had only some position—I forget the title—intermediate between instructor and assistant professor. Secondly I knew almost nothing about class field theory. I had attended during my first year at Princeton a seminar on the subject conducted by graduate students, in which the principal speaker was Armand Brumer, the single informed member of the audience of three or four Michael Rosen, and where I did not hesitate to reveal my ignorance by asking a question whenever I did not understand. Brumer treated these questions with the contempt they undoubtedly deserved, but Rosen was kinder. In spite of this background, I gave the course, in which there were five students or auditors, two who were visitors at the Institute, two, Roy Fuller and Dan Reich, who not discouraged by the experience went on to write a thesis with better equipped colleagues, Shimura and Dwork perhaps. Only one, Dennis Sullivan, was discouraged, as he told me later by my gothic letters, and abandoned the lectures. He does not know it, but a few years later he had his revenge in spades. When in preparation for my lectures in Antwerp, I tried to learn something about etale cohomology, a topic that had been anathema in Princeton while I was at the University and still had not reached Yale, I used his MIT lecture notes, but that explains only in part the obscurity of my Antwerp lectures. It is not uninteresting, however, to ask why Grothendieck was systematically ignored at Princeton for so long. What the course on class field theory meant to me is best explained during the answer to the next question.

Before I pass to it, there is a question still to be answered. It is now, I hope, clear how I came to automorphic forms. What about representation theory or, more precisely, the work of Harish-Chandra? This has a brief, simple answer. I continued in Princeton to reflect on the ideas of Selberg, especially the trace formula and one of the simplest problems it can be used to solve, that of calculating the dimension of the space of automorphic forms. This was a matter of evaluating explicitly some multi-dimensional definite integrals. I had occasion to mention my reflections to David Lowdenslager, a young mathematician at Princeton, although a few years older than I was, who died prematurely and unexpectedly not long after. He suggested that it was generally expected that the papers of Harish-Chandra would be relevant. I began to study them. Not so very long after I saw that the integral I was trying to evaluate was the same as the orbital integral of the matrix coefficients of the holomorphic discrete series that appears in Harish-Chandra, thus its value was a character. It was an insight that came to me suddenly on the Princeton University campus as I was returning home in the evening. That it was a sudden glimpse into the relation between the trace formula and characters I know but its exact nature has been lost because Harish-Chandra explained to me later, presumably not much later, how to formulate the calculations leading to the trace formula group theoretically, thus directly in terms of the representation on the functions on a homogeneous space, so that orbital integrals appear of themselves. With time the principles of the theory have fused in my thoughts with the reciprocity law of Frobenius, so that I can no longer recover precisely whatever insight was intimated to me somewhere between Brown and Witherspoon Halls.

5. **Which of your accomplishments are most meaningful to you?** The ideas formulated in the paper Problems in the theory of automorphic forms and earlier in a letter to Weil, a letter now available on the web and fairly widely read, have certainly been decisive for my career and, I suppose, my reputation. I had finished in the spring of 1964 a very long paper on Eisenstein series. This was certainly the technically most difficult of my papers, very long and very demanding, especially the little read final chapter. There is in it a delicate induction at which I had to take several runs, weakening each time the inductive assumption, because I found that each time I had hoped for too simple a formulation. Each time I had to find an example to convince myself that the weakening was unavoidable. Appendix III to the SLM volume *On the functional equation satisfied by Eisenstein series* that finally appeared is the most striking example. Certainly, without understanding why, I was exhausted at the end and, during the next year, which I spent at Berkeley with my wife and three children, the last child being born just after our return to Princeton, an event which allows me to fix the dates, I was unable to initiate any new project. I had hoped, having established the analytic continuation of the Eisenstein series, to turn immediately to the trace formula, but it was too daunting. There are, I now understand, several useful things left over from the year: the

proof of the Weil conjecture on Tamagawa numbers for Chevalley groups and implicitly for quasi-split groups, although the latter had to await the thesis of K.-F. Lai; a formula for the inner product of truncated Eisenstein series; and a conjecture inspired by calculations of P. Griffiths and proved by W. Schmid on the cohomological realisation of the discrete series representations constructed by Harish-Chandra not long before. Nevertheless I did not attach much importance to them and was discouraged. The next year was perhaps worse. I reflected on a number of matters, above all, the problem of finding a general analogue of the Euler products attached to automorphic forms on GL(2) by Hecke and Maass and that of finding a non-abelian form of class field theory. The first was popular at the time and many unsuitable possibilities were discovered. My interest in the second was certainly a consequence of the course I gave at Bochner's insistence.

Even during the first year my discouragement led me to consider abandoning mathematics. The year before coming to Berkeley I had met in Princeton and formed a friendship with a Turkish economist, Orhan Türkay, who then, coincidentally, also spent the next year in Berkeley, where we met frequently, as we had children of the same age. In Berkeley, I spoke to him of my impulse to try something different, perhaps foreign travel, as I had never been out of North America. He suggested Turkey. In the months after my return to Princeton, these plans matured, and by Christmas my decision was made and I began to make the necessary applications. Bochner was puzzled and distressed, but made no comment.

I was, no doubt, attempting to liberate myself, but the results appeared sooner than expected. I did not need to go anywhere. The decision itself freed me and I began to amuse myself with mathematics without any grand hopes or serious intentions. I began, for example, to calculate the constant terms of the Eisenstein series associated to Chevalley groups. Suddenly, without any effort on my part, the results suggested the form to be taken by the Euler products for which I had been looking in vain. The constant terms were quotients of Euler products and these Euler products could be continued to the entire plane as meromorphic functions, already a convincing beginning. The ideas must have matured over the course of the summer and fall of 1966, my thoughts quickening towards the year's end. When and how I recognized the role of what is now called the L-group I do not know. The structure of the algebra of spherical functions on a general p-adic group had been established by Satake, as an extension of the known structure theorem of Harish-Chandra for K-biinvariant differential operators, but without any hint of the particular nature of the algebra for quasi-split groups or any sign of the L-group. Without the desire to find an adequate notion of automorphic L-function, there was no need for it. The last, culminating insight came on reflecting how the analytic continuation might be proved in general, using the analytic continuation for $G = \mathrm{GL}(n)$ and the defining representation of ${}^LG = \mathrm{GL}(n, \mathbb{C})$ as the standard L-functions to which all others are to be compared, just as one uses the L-functions of Dirichlet type together with the Artin reciprocity law to establish the analytic continuation of the Artin L-functions. The Artin reciprocity law has to be replaced by what I now refer to as functoriality.

This last insight was decisive and came certainly during the Christmas break of 1966. I was in the old Fine Hall, at the time a magnificent building, also on the Princeton campus, across from what was then the President's residence. I had a small office to the right, on entering, of the main entrance. There was a seminar room on the left, where I usually worked because there was a large blackboard and room for pacing. The building was quiet, which is why I think it must have been during a vacation, and I was distractedly looking through the leaded windows—the building had some gothic elements—towards the President's garden.

I suppose I was convinced immediately that I had found what I had been searching for, but I do not remember being especially eager to communicate this to anybody. Who was there? Weil, although he might seem in retrospect to be a natural possibility, turned up by accident.

I had met him before. Indeed I have a vivid recollection of our first meeting. It was during my first year in Princeton, in 1960–61, or perhaps in 1961–62. I shared then an office, again in Fine Hall, with Donald Ludwig, who went not long thereafter to UBC, and perhaps one or two others. It was also a period when Weil's current literature seminar, for me the event of the week in Princeton, was still active, so that Weil came every Wednesday afternoon just before 4:30 to the University. One day, to my astonishment, for world-famous mathematicians did not normally come knocking unannounced on the doors of young mathematicians of no distinction and only a few months from the Ph.D., he presented himself, sat down in a chair, threw one leg over its arm, and began to chat. Weil was not a modest man, and was also often a difficult man, but he had at times a simple, unpretentious side that inspired affection. It also inspired some unguardedness in my conversation, for I remember—apparently I had already begun to think about modular forms, having read Gunning's book, and this suggests 1961–62—expressing some conjectures that I recognized after a few weeks, to my great embarrassment, to be utterly naive.

At all events, on January 6, 1967, we found ourselves pretty much alone and together in a corridor of the Institute for Defense Analysis, having both arrived too early for a lecture of Chern. Not knowing quite how to begin a conversation, I began to describe my reflections of the preceding weeks. He, with a stratagem that I now recognize, having used it myself to escape politely from importunate, perhaps addled individuals, suggested that I send him a letter in which I described my thoughts. Ordinarily the letter never arrives. Mine did, but with Harish-Chandra, who was then a colleague of Weil and to whom I was closer, as an intermediary. Harish-Chandra perceived its import, but Weil, so far as I have since understood, did not.

In spite of his extremely maudlin review of Eisenstein's collected works, Weil was not known as a benefactor of young mathematicians. Very strong ones may have made him uneasy. My own experience was mixed. He did have the hand-written letter typed, so that it was easier to read. We did subsequently have a conversation in which he drew my attention to two papers of his own that were, for different reasons, very useful to me. The first was a paper in which he introduced what is called in the Western world the Weil group, although Shafarevitch had also introduced it. Whatever the appropriate designation is, it is certainly a group that I have often used. Secondly, he gave me a copy of his reworking of the Hecke theory. Moreover, he invited me shortly thereafter to attend an informal seminar at the Institute for Advanced Study at which he was attempting to extend this theory to fields with complex places.

I had, in my earlier unsuccessful attempts to find the correct generalisation of the L-functions introduced by Hecke and Maass, given considerable thought to the Hecke theory. So I knew immediately what was to be done. I tried to explain this to him, or perhaps I promised him a letter. This letter was also written. It was a useful exercise and more, because it led to a year of useful work, not necessarily very difficult, during my stay in Turkey, exploring, at least in the context of GL(2), the consequences of the suggestions in the original letter, especially the local correspondence between representations of the Weil group and representations of GL(2). Considerable precision was added. The extra difficulties arising from residual characteristic two were recognized but not solved. They required different techniques and were obtained considerably later by P. Kutzko. I recognized the function of the special representation, which had been a puzzle to me, on discovering its Galois counterpart in a paper of Serre on elliptic curves with non-integral j-invariants that he sent me in the course of the year. This special case clear, there was little mystery as to where similar correspondences might appear for other groups.

As a consequence of the local-global form of the Hecke theory for GL(2), not only the L-functions but also the ϵ-factors had a product formula. The correspondence then predicts a product formula for the ϵ-factors in the Artin L-functions. This was a puzzle for me, or rather a challenge, and it became an issue to establish this not just for two-dimensional representations of the Weil group but in general. It was a test of the ideas in the original Weil letter and those ideas were so novel that all significant tests were welcome.

My source for the detailed local study of representations of the Galois group or of the Weil group was Serre's book, although I do not remember having had a copy in my hands in Turkey. The proof of the product formula was never published, although a good deal of it is available on the web. It was itself complex and relied, in addition, on some difficult results of Dwork whose proof required a very elaborate study of Artin-Schreier equations and was itself never published, and not even available to me in full. The proof was not, however, as suggested by Deligne in his Antwerp talk incomplete, although recovering or even verifying the proofs of Dwork would have demanded a tremendous effort. So we can be grateful to Deligne for a brief proof that, even if philosophically unsatisfactory, allows us to postpone the search for a proof both accessible and satisfactory to a time when we have more leisure. Nor was it as suggested by Serre in a general appreciation of Deligne's work "peu de chose". Serre is a good enough mathematician to recognize how important it is to accumulate genuine evidence, especially at the beginning, for conjectures and hypotheses that are clearly important if true but whose truth is by no means assured. He also, undoubtedly, knows the difference between proving a serious theorem whose truth is in doubt and upon which one may have stumbled in an unlikely setting, an effort that is sufficiently exhausting that there is no desire or strength to renew it immediately, and finding a simpler proof for a theorem about which there is no doubt.

I add that Dwork on one occasion reproached me for referring to his contributions as lemmas. Although I certainly had no intention to depreciate his work, for Dwork's thesis is an impressive and powerful analysis of ramification for forms of the Artin-Schreier equation that I hope will be made generally accessible, I am afraid I am rather indifferent to the distinction between a theorem and a lemma, so that I did not respond to his reproaches. It would have been more gracious to accept them.

What Weil learned from my first letter is not clear. I had occasion, when he began to write about the relation between two-dimensional Galois representations and automorphic forms on GL(2) and when a few prominent number theorists began to refer to this relation as a conjecture of Weil, to recall my letter to him. His response was evasive. He knew little about semi-simple groups and the wrong things about infinite-dimensional representations. Since both were central to the letter, it is possible that he found it simply incomprehensible. That would be difficult for him to acknowledge.

6. **In the 1980s, why did you turn your attention to physics? Do you still work in physics?** The differential calculus, the integral calculus, and the theory of differential

equations, ordinary and partial, were a mathematical response to the needs of physical scientists. It is, I believe, clear that in contemporary science, in, for example, quantum field theory or statistical mechanics, or in fluid mechanics, there are many questions that arise to which the current state of mathematical analysis is inadequate. The key word is "renormalisation". My attention was not caught by the physics implicit in these problems but by the lack of effective mathematical tools. It would be fatuous to suggest that I had anything to offer to the physics. I did, however, hope to contribute to the mathematics. The mathematical phenomenon that appears in renormalisation is extremely rapid convergence to the fixed points of an infinite-dimensional dynamical system in which the manifold of fixed points is of very low dimension.

The mathematical problem is to understand how this behaviour is to be established and, especially, to find effective methods of determining the manifold of fixed points, thus to find revealing choices of coordinates. I, or rather we, since I usually worked with Yvan Saint-Aubin at the University of Montreal, introduced the crossing probabilities in percolation to this purpose. They did become popular and this is to our credit. They did turn out to be conformally invariant, an appealing property suggested to us by Michael Aizenman, but proved not by us but, later, by others. Moreover they were incorporated into the theory of the Schramm-Loewner-Equation, an extremely attractive and extremely popular theory due to the late Oded Schramm. This theory was an achievement for which I have great admiration, feeling at the same time that its charm distracts attention from the central mathematical issues, with which it does not deal. In other words, there is something to be said for our coordinates, but they did not help with the central mathematical problem.

I hope very much to return to these questions some day. A good place to begin might very well be be the recent papers and lectures of David Brydges on the renormalization group. What I have heard in a lecture by one of his collaborators intrigued me.

7. **What role did your work play in the solution of Fermat's last theorem?** I do not think you want me to review the proof of Fermat's theorem, which can be found in various forms in various places, as can the relation of Fermat's last theorem to the Shimura-Taniyama conjecture. Fermat's theorem is, of course, extremely beautiful in itself and of great historical importance for the development of mathematics, although for mathematicians perhaps of more interest unproved than proved. The reduction to the Shimura-Taniyama conjecture, too, has a great deal of intrinsic charm and will be, one can hope, a model, in some sense or other, for future efforts.

Nevertheless, the abiding issue in Wiles proof is perhaps how to establish the expected relations between automorphic forms and Galois representations. This is, in spite of his marvelous proof, not yet clear. We know too little. I had recognized how to use ideas of Saito and Shintani to establish various special cases of functoriality and how to use this bit of functoriality to establish some special cases of the correspondence from Galois representations to automorphic forms, more specifically for two-dimensional representations of tetrahedral type. Tunnell observed that a similar conclusion could be established for octahedral representations. With this much—and it is very little—from functoriality, Wiles was able with deformation arguments to reach the Galois representations appearing in the Shimura-Taniyama conjecture.

There seem now to be two questions to be faced when attempting to deal with the general form of the correspondence: (i) how much can we prove about functoriality? (ii) how much functoriality do we need before the deformation arguments do the rest? Not everyone understands the issue, but even among those who do, there are differing views, none I suppose with very secure foundations.

8. **What do you think are the exciting fields in mathematics at this time?** I have seen in the course of 50 years as a mathematician many "exciting" fields come and go. So I have no answer to this question. My continuing interest in mathematics is, by and large, reserved, on the one hand, for mathematics to which I was introduced in the course of my career but about which my curiosity was never fully satisfied and, on the other, for mathematics related to the topics with which I have been seriously concerned during my career: first of all, automorphic forms and representation theory with a little arithmetic and geometry; secondly, and to a considerably lesser degree, indeed in a very amateurish way, mathematical aspects of renormalisation, thus a little statistical mechanics, field theory, even fluid mechanics. At one point, when fairly young, I read the book of Coddington and Levinson on ordinary differential equations and was taken with the topic of differential equations with irregular singular points. There are traces of this topic in the long letter to Weil about the Hecke theory. The same topic reappears in the "geometric Langlands programme" over $\mathbb{C}$, where it has quite a different flavour than in Coddington-Levinson, whose references will have been Poincaré and G. D. Birkhoff. In its current guise it is little explored. I hope to find time to learn more about this before I am done.

9. **What recent result in mathematics interests you most?** If the sense of the question is, "To what mathematics do you currently give the most time and the most effort", the answer is to "functoriality" as a part of the Langlands programme. What I have called functoriality is not the whole matter, but it is, for reasons I have explained in my reflections on receiving the Shaw prize indispensable. I believe, although with less solid evidence than I would like to have, that the clue to it is the trace formula. Thanks to the proof by Waldspurger, Ngô Bao Châu and others of the fundamental lemma, the trace formula, in what is called its stable form, is now available. So we can examine its possible use. We could have done this before, in say the special case of GL(2) or SL(2), but for lack of the assurance that the fundamental lemma provides, the courage to do so was not there. Now it is. In collaboration with Ngô Bảo Châu and Edward Frenkel, and on my own, I have reflected a great deal on the trace formula in the current context. This is not the place to discuss these reflections. There will be an analytic part; there will also be an arithmetic part. My guess is that the arithmetic part will be closer to the class field theory of Takagi and his predecessors than to that of his successors! Even in the context of the trace formula, there are several ideas currently being tested and by several people. So the situation is fluid.

I add that the outstanding contributor to the development of the trace formula, in all its forms, and, in my view, the outstanding mathematician in Canada is James Arthur at the University of Toronto.

10. **What advice would you give to aspiring mathematics students?** None. In so far as I accomplished anything of value, I did it by following my own inclinations. I hope they do so too.

11. **Is there any question that you would like to be asked?** I have already exhausted both myself and you by answering your very few questions at great length. You will be grateful that you asked no more. So am I. I observe nevertheless that you asked nothing about my relations with Canada, a country in which I continue to have a residence, and

in which the majority of my four children and five of my six, or better, six of my seven grandchildren live, and nothing about my relations with Turkey, in which I spent a year and with a few of whose mathematicians I have continuing connections. Perhaps these questions can be for another occasion.

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