MATHEMATICAL PERSPECTIVES

ON THE GENESIS
OF ROBERT P. LANGLANDS' CONJECTURES
AND HIS LETTER TO ANDRÉ WEIL

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ABSTRACT. This article is an introduction to the early life and work of Robert
P. Langlands, creator and founder of the Langlands program. The story is, to a
large extent, told by Langlands himself, in his own words. Our focus is on two
of Langlands' major discoveries: automorphic L-functions and the principle of
functoriality. It was Langlands' desire to communicate his excitement about
his newly discovered objects that resulted in his famous letter to André Weil.
This article is aimed at a general mathematical audience and we have purposely
not included the more technical aspects of Langlands' work.

CONTENTS

1. INTRODUCTION

This article is about the life and work of Robert Langlands, covering the first 30
years of his life from 1936 to 1966 and his work from 1960 to 1966. His letter to
André Weil dates to January 1967 and the *Langlands program* was launched soon
afterward.

Section 2 of this article focuses on Langlands' early years, from 1936 to 1960, and the story is told by Langlands himself. The material is taken from an interview given by Langlands to a student, Farzin Barekat, at the University of British Columbia (UBC), Langlands' alma mater, in the early 2000s. A copy of this interview is available on Langlands' website at `http://publications.ias.edu/rpl/`.

1.1. **Overview of Langlands' early life (1936–1960).**

Let us list a few important events in Langlands' early life which, in our view, shaped his professional life later on.

(1) Toward the end of his twelfth grade (see section 2.4), an English teacher, Crawford Vogler, took up an hour of class time to explain to him, in the presence of all the other students, that it would be a betrayal of God-given talents for him not to attend university. Langlands had no intention of attending college because none of his classmates did. At the time very, very few students, at the best one or two, did so in any year. Also school meant little to him. However, Langlands was flattered by his teacher's comments and his ambition was aroused.

(2) Taking the aptitude tests at UBC in the fall of 1953 (see section 2.5) was a decisive event in his early life. From the results of his tests, the university counselor suggested at first that he might want to become an accountant, but Langlands rejected that right away. The counselor then suggested mathematics or physics, but cautioned him that this would require a master's degree or even a PhD. Langlands did not know what a PhD was, but he decided on the spot that he would become a mathematician or a physicist.

(3) Another important event was the discovery of his natural talent for languages (see section 2.6). Sometime during the year of 1954, Langlands spoke to Professor S. Jennings about his intention to choose honors in mathematics. Professor Jennings declared that to be a mathematician, one had to learn French, German, and Russian. Taking Professor Jennings' advice, Langlands became fluent not only in French, German, and Russian, but also in several other languages, including Italian and Turkish.

(4) Langlands' unique and profound talent in mathematics was first revealed during his first year as a graduate student at Yale University (see section 2.8). He not only completed all the required courses and examinations for the degree, but also submitted his PhD thesis at the end of his first year. Consequently, he was completely free to pursue his own research during his second year as a graduate student at Yale.

While all things seemed to point toward an illustrious career in mathematics, it is important to note that Langlands, as a young man, harbored ambitions of a much more general nature. He did not envision a career as a mathematician. Here he is, in his own words:

> I began my mathematical, or better my university, studies more
> than sixty years ago just before my seventeenth birthday, after a
> childhood and adolescence that were in no sense a preparation for
> an academic career, or a career in any sense. Various influences
> came together that suggested that I attend university not so much
> to prepare myself for a profession but to become what one might
> think of as a savant. A major influence was perhaps Ernst Tratt-

ner's *The story of the world's great thinkers*, a book that was very popular in the late thirties, so that there are still many copies available on the used-book market at very modest prices. What I envisioned, after enrolling in the University of British Columbia in 1953, was myself, in a jacket with leather elbow-patches and perhaps a pipe in my mouth, gazing over the lawns and the trees while reflecting on a still undetermined, but certainly abstruse, topic. That topic became mathematics, not out of a strong preference for that subject, but because it pretty much required no preparation, only native ability.

1.2. **Overview of Langlands' early professional life (1960–1966).** The second part of this article (section 3) is about Langlands' professional life from 1960 to 1966. Here we focus on what led him to his discoveries of automorphic L-functions and the principle of functoriality. Our source material in this second part is taken largely from our correspondence and interviews with Langlands himself, over the past few years. That correspondence can also be found on Langlands' website.

During six years in the early 1960s, Langlands' research output was prodigious. Let us highlight some of his important contributions.

(1) Langlands' spectral theory (see section 3.8) was certainly one of his first major contributions, and his construction of the most general Eisenstein series later played an important role in his 1966 discoveries. His success in generalizing Selberg's result on spectral theory (see section 3.7) from $\mathrm{SL}_2(R)$ to arbitrary reductive groups was helped, in part, by his knowledge in harmonic analysis and representation theory, which he acquired while working on his PhD thesis (see section 2.9).

(2) Langlands' calculation of the constant term of his generalized adelic Eisenstein series in the fall of 1966 (see section 3.14) was a decisive factor in his discovery of automorphic L-functions, which were sensational new objects to number theorists. It also led him to create the L-group, as well as extending the notion of an automorphic representation, in particular, the Satake isomorphism (see section 3.15.2).

(3) We learn from Langlands' own words that one of the major moments in his mathematical career, the first hint of the genesis of functoriality, was during the Christmas vacation of 1966. While looking through the leaded windows of his office distractedly, he was suddenly struck by the epiphany: *The Artin reciprocity law has to be replaced by what I now refer to as functoriality* (see section 3.16.2).

There are 17 subsections in section 3. A large part of the last section, section 3.17, is a presentation of an interesting and illuminating example of Langlands, which gives a rough sketch on how the functoriality conjecture was discovered. This part is for readers with slightly more technical background, and a general reader can skip it and still get the whole story. In fact, the story of Langlands' discoveries ends with the end of section 3.16.

We hope that by prefacing the story of the evolution of Langlands' mathematical landscape with a summary of crucial life events, we provide the reader with greater insight into both the man and his work.

The idea of writing this article originated from my book project *The genesis of Langlands' program* (Cambridge University Press, to be published).

2. Langlands' early years (1936–1960)

My time as a student at UBC (University of British Columbia)
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.

2.1. Preschool years

I was born in October 1936, in New Westminster, British Columbia,
Canada. 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.

One of the other two cottages, perhaps better described as cab-
ins, 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.

The five or six years there must have been broken occasionally,
by trips to New Westminster to my parents' families. But I have
no recollection of such excursions, simply of my mother, my father,
a younger sister, and toward the end a second baby sister.

2.2. Families in New Westminster

When time came for me to attend school, and perhaps for other rea-
sons, we returned to New Westminster, then a small, in my memory,
delightful city with chestnut-lined streets, the trees planted in what
was called a boulevard, between the sidewalk and the curb.

New Westminster, during the war years, was agreeable for a
child. There were very few automobiles. 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. 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 on the other side of 12th, but it never occurred to
me to visit them on my own.

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 mother's family large, my father's smaller but
substantial, so that all in all there were thirteen aunts and uncles
together with their husbands or wives and children.

In England my grandparents may have been Methodists but in
Canada they were members of the United Church. My father's
parents were not so much stern as pious. In my father there were

only un-reflected remnants of the piety. 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. As a child I had found my father's parents and his sisters and brother, in comparison with my mother's family, a little colorless and distant.

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 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.

My grandmother was already a widow when I was born and even before my grandfather's death 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 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.

I was enrolled by my mother, a Catholic, 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 the elementary school to the companion academy, St. Peter's I believe, which was less friendly, with a morose beadle, prompt to 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.

2.3. White Rock—the town of my youth

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 logs 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.

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 pier was, at least after I learned to swim and dive, an attraction. I found my way there when free of responsibility.

On Sunday, the main street, which ran 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. 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.

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.

There were other sources of income, a newspaper route for Vancouver's *The 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 means 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.

School, now a public school, was mixed. I enjoyed that. Except that it was a place frequented by girls and my friends, meant little to me. For a short period I was large for my age, I was also younger than almost all my classmates and maladroit. 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.

2.4. Grade 12—a very special and important year

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. 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.

In the course itself, Vogler 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.

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.

By the time I read Meredith, I had met the girl, Charlotte, whom I was 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, having lost his mother as an infant, grew up in a Gaelic-speaking foster family on Prince Edward Island, 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.

My wife's father had only one year of schooling. 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. 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.

2.5. **The aptitude tests at UBC (1953)—a decisive event**

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 or even a PhD. The later meant nothing to me, but I did not acknowledge this. I decided on the spot that I would become a mathematician or physicist. The PhD, 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 PhD 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.

2.6. **A talent for languages revealed via mathematics**

At some point in the first or second year, because of my intention to choose honors in mathematics, I spoke with Professor 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 crook-handle.

He gave me a piece of advice for which I am grateful to this day. He declared that to be a mathematician one had to learn French, German and Russian. The romantic desire to penetrate the present and the 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.

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 Almet Hamidi Tanpinar, not to speak of Robert de Roquebrune or Michel Tremblay, or Mommsen and Michelet. I am afraid such possibilities are no longer available. 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.

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. I fear, however, that I was more attracted to the mathematical explanations than to the physical phenomena themselves.

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 Street 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.

2.7. **An incomplete master's thesis**

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. Weyl opened my eyes. Lefschetz's book would have been my introduction to topology. I never did take to topology. Whether Lefschetz is to blame or my intellectual limitations I cannot say.

My master's thesis, which was influenced by an undergraduate seminar at UBC, was not a 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.

2.8. **Yale University—an excellent choice**

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 institutions: Harvard, Wisconsin, and Yale. I was accepted by all three.

Wisconsin was without aid and 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 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 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.

2.9. A stressful oral examination

At Yale I was on my own and allowed to follow my own inclinations. I finished at Yale in two 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.

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 and it began badly because I could not prove the simplest things about Noetherian rings. I had obviously read Northcott's book on ideal theory too quickly and too superficially.

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. 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. I am particularly grateful to him for discovering that, even if I did not know what I should have known, I did know something.

Formally, the director of my thesis was Cassius Ionescu Tulcea. The first half of my thesis was a solution of an open problem in the somewhat obscure domain of Lie semigroups and their representations. It became my first paper, appearing in the Canadian Journal of Mathematics. The second half of my thesis [was] never properly published, but [is] 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.

2.10. A fateful future

What I really hoped to do when I completed my PhD 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, who had, for reasons I never completely understood, taken a dislike to me. His disfavor was a great favor.

I accepted the offer from Princeton, where I had the great 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.

Robert P. Langlands is currently Professor Emeritus at the Institute for Advanced Study in Princeton, New Jersey, where he has been a professor since 1972.

3. Overview

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.

3.1. **The letter to André Weil (January 1967).** When asked where those ideas in his conjectures came from, Langlands often answered, "Those ideas were in the air, and I was in the right place and at the right time."

In the early 1960s, Langlands was a junior faculty member at Princeton University. Emil Artin's influence was certainly in the air even though he had left Princeton University in 1958. Artin was noticeably the most respected algebraic number theorist of his time and an authority on abelian class field theory. Artin and his school had seriously investigated the possibility of a nonabelian class field theory but was not able to create one.

John Tate, one of Artin's eminent students showed (in his famous Princeton university thesis (1950)), how to use an adelic treatment to reformulate and reprove Hecke's complicated results about his L-functions.

One of the "desirable" and "fashionable" research topics at that time, especially in Princeton, was to generalize Tate's thesis from $\mathrm{GL}(1)$ to $\mathrm{GL}(n)$. The leaders of that group were Godement and Tamagawa. Even though they both were striving for the same goal, they worked totally independently of each other.

The leading mathematician of the other desirable topic at that time was Atle Selberg, who was a professor at the Institute for Advanced Study in Princeton, New Jersey. From the 1950s to the 1960s, Selberg's work focused on investigating Eisenstein series for the group $\mathrm{SL}_2(\mathbf{R})$. What he was looking for, around 1960, was a generalization of $\mathrm{SL}_2(\mathbf{R})$ to other groups.

Let me just remind you that there was a sequence of developments in the 1930s, 1940s, and 1950s that would inspire a lot of the later work: Hecke's theory; Siegel's many papers on reduction theory and related matters; Maass's extension of Hecke's ideas to non-holomorphic forms on GL(2) and his introduction of the associated spectral theory of which the Eisenstein series form a part; Selberg's solution of a problem raised by Maass but not solved by him, the

construction of the continuous part of the spectrum with the help
of the Eisenstein series, and his introduction of the trace formula.

I learned something about this from Selberg's paper, not only
for GL(2) but also for groups of higher dimension, but I no longer
know exactly what. I had also, for other reasons, read some pa-
pers on domains of holomorphy for functions of several variables.
Putting this together, I had proved some theorems about the ana-
lytic continuation of Eisenstein series in several variables. I would
guess that Selberg had also proved them, but one never knew with
Selberg.

Many mathematicians from both groups tried their hands at one or the other
of these topics, obtaining partial results. But it was Langlands who, in 1966 at
the age of 30, amalgamated and vastly generalized the essential ideas from his
contemporaries as well as the recent past:

- the ideas of Selberg, Harsh-Chandra, and Gelfand which are rooted in
  Eisenstein series, harmonic analysis, and representation theory of certain
  classes of noncompact groups;
- the usage of adelic structure on groups championed by Godement as well
  as Tamagawa and Satake; and
- Artin's legacy on class field theory and his quest for a nonabelian class field
  theory.

Among what Langlands had created was a series of interlocking conjectures which
later became the foundation of the Langlands program. Those conjectures seek to
connect deep arithmetic questions with the highly structured theory of infinite-
dimensional representations of Lie groups. The latter is part of harmonic analysis.
Those visionary conjectures have exposed, quite unexpectedly, the deeply entwined
nature of several seemingly unrelated branches of mathematics. In 1967, when those
conjectures were first introduced to the public, they encountered some resistance.
However, during the past half century, mathematicians around the world have been
inspired to ask questions and solve problems, and their solutions have altered the
landscapes of (a) number theory, (b) arithmetic geometry, as well as (c) representa-
tion theory. As an example, let us mention that according to a theorem of Kenneth
Ribet, the celebrated Fermat's Last Theorem, an unsolved problem over 300 years,
follows from a conjecture of Shimura–Tanijama–Weil (proved by Andrew Wiles and
Richard Taylor), which could be viewed as a special case of the general Langlands'
conjectures. In addition, the Langlands–Tunnell theorem, proving another case of
the Langlands' conjectures, was an important step in the Wiles–Taylor proof.

These conjectures had a profound effect, especially on number theorists, on how
they thought about their subject. The conjectures suggested a way to obtain arith-
metic information from the rich and rigid structure of algebraic groups and their
infinite-dimensional representations, which were mostly unfamiliar to number the-
orists at that time.

In January 1967 Langlands formulated a series of conjectures in his famous letter
to André Weil who was, at that time, a world-famous mathematician and a professor
at the Institute for Advanced Study in Princeton, New Jersey.

We find, in that famous letter, in a hastily written dense format, an impressive
collection of novel and ingenious ideas which, in 1967, were far ahead of their time:

(1) A general and comprehensive notion of automorphic L-functions attached to automorphic representations (plus some additional representation theoretic datum) simultaneously generalizing Hecke's L-functions as well as those of Artin. Such a generalization had been out of reach since Hecke's work in 1936. The notion of L-group[1] (and the dual group), which underpins both the automorphic representations and their L-functions, was a deciding factor in Langlands' discovery of his automorphic L-functions.

(2) The principle of functoriality and nonabelian class field theory, which explains, albeit conjecturally, the connections between automorphic representations on groups whose L-groups are related via an appropriate notion of morphism. Moreover, a special case of the functoriality conjecture generalizes Artin's abelian reciprocity law, and asserts, conjecturally, Artin's reciprocity conjecture for nonabelian field extensions, which may be viewed as a general formulation of nonabelian class field theory.

Topic (2) is linked to topic (1) and they were discovered almost simultaneously. Both topics will be revisited in later sections.

The automorphic L-function, $L\,(s,\,\pi,\,\rho)$, as Langlands discovered, depended not only on the automorphic representation π, but also on an auxiliary finite-dimensional representation ρ of a canonically associated dual group or L-group.

Langlands originally named his L-function the Artin–Hecke series because it significantly generalized both of these L-functions into a single definition, via the representations ρ and π. This is quite remarkable in the sense that Langlands' L-function is defined as an Euler product akin to Artin's L-function, and hence it is arithmetic in nature, but it also possesses analytic characteristics, which are more aligned with Hecke's L-function. This unique feature of combining algebra and analysis plays a central role in Langlands' L-function.

From the fall of 1960 until the spring of 1967, Langlands, a promising young mathematician, worked and lived in Princeton, except for the academic year 1964–65 which he spent in Berkeley, California. For the rest of section 3, we have Langlands' own recollections, from that period, on some aspects of his professional life.

3.2. Salomon Bochner—an exceptional mentor (1960–1967)

> In the first few months, I was, as a very junior mathematician, invited to speak in the analysis seminar conducted by Robert Gunning. The talk was about a theorem that I had proved as a graduate student while studying Selberg's paper. I believe that Salomon Bochner was very favorably impressed by my independence.
>
> I had worked alone as a graduate student but, more importantly, on at least two different subjects. He encouraged me in a number of ways. He not only urged me to read Hecke's work on the Dedekind ζ-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. I was scared stiff. [Bochner's] encouragement and suggestions played a decisive role in my first years at Princeton.

---

[1] The name L-group was suggested by Hervé Jacquet.

We have learned from Langlands himself that Bochner had been an encouraging and caring mentor. The other mathematician who had an important influence on Langlands' work, at that time, was Atle Selberg. As we know, Langlands had started learning about Selberg's work as a graduate student at Yale University, mostly by reading Selberg's papers. As for their personal contact in Princeton, as far as we know, Selberg was reserved and aloof.

3.3. A memorable meeting with Atle Selberg (1961)

> Selberg, I am sure, had invited me to his office at the suggestion of Salomon Bochner. I was able to follow Selberg's oral presentation of the proof of the analytic continuation of the Eisenstein series for discrete subgroups of $SL(2, \mathbf{R})$ with quotient of finite volume, in my first, and surprising as this may be, my last mathematical conversation with him, since our offices were, many years later, essentially side-by-side for a good long time.
>
> The ideas involved are just those of spectra theory for second-order self-adjoint equations on a $\frac{1}{2}$-line, but I had never really seen these before and certainly not in the hands of a master.
>
> It was a great pleasure to speak, for the first time in my life, with a strong mathematician about serious mathematical matters, or rather to listen to him, and I was tremendously impressed. It was a defining experience. I went away with a reprint of his paper and began to study it carefully, especially the trace formula.

What Selberg was lecturing on at that meeting was the Selberg spectral theory on $SL_2(\mathbf{R})$. This topic is the subject of section 3.7, and Langlands' generalization of Selberg's work is the subject of section 3.8. In sections 3.4–3.6, we list the topics that are relevant to the works of both Selberg and Langlands.

3.4. Eisenstein series.
An Eisenstein series of weight $2k$ on a lattice Λ in C is defined by

$$E_{2k} = \sum \lambda^{-2k}, \qquad \lambda \in \Lambda, \;\; \Lambda \neq 0, \;\; k \text{ a positive integer.}$$

It is invariant under integral linear change of lattice basis and homogeneous with respect to complex rescaling of the lattice. It is also holomorphic, i.e., it satisfies the Cauchy–Riemann equation.

The *real-analytic* Eisenstein series, which was introduced by Maass, is an Eisenstein series without the constraint of being holomorphic; instead, it is an eigenfunction of a non-Euclidean second-order Laplace operator. It played a central role in Selberg's spectral theory on $SL_2(\mathbf{R})$, and later was vastly generalized by Langlands in his spectral theory.

By their definition, Eisenstein series converge absolutely only in some right half-plane and are not even square integrable. In applications it is therefore necessary to extend their convergence to the entire complex plane by meromorphic continuation, and this was an important and challenging undertaking for both Selberg and Langlands in their investigations on spectral theory.

3.5. Automorphic forms.
Since Maass introduced nonholomorphic modular forms of one variable, there was the need to deal with this together with the holomorphic theory.

Gelfand (together with Fomin) translated the notion of modular form to a class of vectors in certain Hilbert space. In this new setting, the theory of holomorphic modular forms is on the same footing with the nonholomorphic modular forms discovered by Maass; that is, the only difference is their Laplace eigenvalues. Also, the moderate growth condition translates into being square integrable. The result is that a modular (cusp) form becomes identified with an element of an infinite-dimensional Hilbert space.

The resulting notion became known as an automorphic form. In this sense, Gelfand started the spectral theory of modular forms.

> The modern analytic theory of automorphic forms has its origins largely in the work of Hecke and Siegel: Hecke with his L-functions and Siegel who worked with a large number of groups—general linear groups, orthogonal groups, and symplectic groups—and, in general, brought the nineteenth century into the twentieth.
>
> This—according to my understanding and reading, but this was never systematic—laid the foundation for the modern theory. That one decisive step was the extension of the reduction theory to general reductive groups by Borel and Harish-Chandra (in *Arithmetic subgroups of algebraic groups*, Ann. Math., 1962).
>
> The general L^2-theory, thus the theory for general reductive groups, especially the notion of cusp form, seems to have had its origins in two papers, one by Godement (in Sém. H. Cartan, 1957/58, Ex. 8, pp. 8–10) and the other by Harish-Chandra (*Automorphic forms on a semisimple Lie group*, 1959).

3.6. **The rise of automorphic representation.** From the 1940s to the 1950s there was a flurry of development of modular forms in two separate directions. On the one hand, Maass and Selberg established the theory of real-analytic modular forms. On the other hand, Siegel, with his reduction theory of many variables, had successfully generalized the theory of modular forms to several complex variables. One of the sticky points was that Siegel modular forms were necessarily analytic functions. This meant that transferring their definitions over to groups was not straightforward, since there may not even be a complex analytic structure. Those two points of view were amalgamated with the work of Harish-Chandra, whose use of harmonic analysis allowed him the flexibility to work in both higher-dimensions like Siegel, and also with relaxed analytic conditions like Selberg. These simultaneous relaxations, together with the contributions of Gelfand, gave rise to the theory of automorphic representation.

3.7. **Selberg's spectral theory on $\mathrm{SL}_2(\mathbf{R})$ (1950s–1960s)**

> The Hecke theory and thus the possibility of generalizing it was in the air at the time, in part because the possibility of Hasse–Weil L-functions was in the air. In part because Hecke's work was becoming more familiar to mathematicians. This may have been encouraged by the ideas of Maass, taken up by Selberg. Hecke's L-functions and the converse theorems worked for imaginary quadratic extensions. It was Maass, not Selberg, who created a theory that functioned for real quadratic extensions. Maass was however a weaker analyst than Selberg, and neither he nor his students were

able to establish a theory for the continuous spectrum in the general case (of discrete subgroups of SL(2)). Selberg undertook this, but as far as I know with knowledge of Maass's papers, the continuous spectrum is given by series that for subgroups of $\mathrm{SL}(2, Z)$ are Eisenstein series in the classical sense. For other discrete subgroups of $\mathrm{SL}(2, \mathbf{R})$, Maass, I believe, was not able to establish the necessary analytic continuation. That was done by Selberg, who perhaps did not give enough credit to Maass. Roelcke, a student of Maass, also examined the pertinent spectral theory. By the way, Maass was influenced by Hecke, indeed a student of Hecke. For Hecke the L-functions came first. This was probably the case for Maass as well.

We learned from Langlands that the central analytic problem of establishing the spectral theory, continuous and discrete, was broached first by Maass and, through his influence but with stronger results, by Selberg, who was looking for a spectral decomposition of the space $L^2(\Gamma \setminus G)$, where Γ is a congruence subgroup of G, $G = \mathrm{SL}_2(\mathbf{R})$, and $L^2(\Gamma \setminus G)$ is an infinite-dimensional Hilbert space of square-integrable automorphic forms.

It was Selberg who first appreciated that the classification problem of automorphic forms was essentially an eigenfunction problem for a certain operator. In the case of real-analytic Eisenstein series, this operator is a non-Euclidean second-order Laplace operator which gave both discrete and continuous spectra. Selberg knew that the continuous spectrum can be described completely by the real-analytic Eisenstein series, which is an eigenfunction of the Laplacian operator. In general, the discrete spectrum is a mystery. Selberg's way of treating the discrete spectrum was to introduce the Selberg trace formula.

In summary, Selberg succeeded in finding a complete spectral basis for the non-Euclidean Laplace operator on $\mathrm{SL}_2(\mathbf{R})$, and hence paved a way to construct every possible square-integrable automorphic form on $\mathrm{SL}_2(\mathbf{R})$.

> In those same two years, I had begun to study the Hecke theory. Gunning, with whom I often spoke, had written very convenient and accessible notes on it. Also sometime in those first months or years in Princeton, I acquired and began to read the various Paris seminars of Cartan, Godement, and others inspired by the work of Siegel, Hecke, Selberg, and the work of the pioneers of representation theory Gelfand and coauthors, Bargmann and Harish-Chandra.
>
> I also began to think about the general theory of Eisenstein series taking advantage of the many results on theta series of various kinds in the papers of Siegel. I would occasionally mention these to Selberg, but he always replied that it was a general theorem that was needed. When the general theorem was offered, he said nothing, but the general theorem came later.
>
> It is, by the way, curious that Selberg himself never could deal with the general theory. He himself was never clear about what he knew and did not know.
>
> My guess is that he never understood the notion of a cusp form or the mutual relations between parabolic subgroups. This meant that

> he could not separate the new problems posed in several variables
> from those already settled as a consequence of the theory for one
> variable.

Selberg was extremely interested in the generalization of his result on $\mathrm{SL}_2(\mathbf{R})$ to higher-rank groups; however, he was prevented by many complicated obstructions. He is reputed to have said, "Eisenstein series are Dirichlet series in r complex variables. It seems very difficult to attack the problem of continuation of these."

3.8. Langlands' spectral theory (1962–1964).

It was Langlands who undertook the project to resolve the problem of the "general theorem" that Selberg had in mind. It was the higher-rank problem for arbitrary reductive groups. Langlands not only gave a complete description of the spectral decomposition of $L^2(\Gamma \setminus G)$ but also succeeded in identifying the spectral basis of $L^2(\Gamma \setminus G)$ for arbitrary reductive groups G, where Γ is a discrete subgroup of G such that $\Gamma \setminus G$ is of finite volume.

This project required a drastic change in language: from a complex analytic framework to a Lie group-theoretic setting. The necessary tool to deal with the latter was mainly provided by Harish-Chandra's work. At the same time, Langlands had to lay the foundation of the theory of general Eisenstein series for general groups.

Eisenstein series themselves have a certain domain of convergence, and unfortunately the spectrum resides outside of this range of convergence. Thus, constructing a spectral basis is tantamount to meromorphically continuing Eisenstein series.

Langlands initially encountered the problem of meromorphic continuation of real-analytic Eisenstein series on $\mathrm{SL}_2(\mathbf{R})$ as a graduate student at Yale from reading Selberg's paper on the topic. He had some initial success in extending Selberg's ideas to several variables. In doing so, it was clear to him that the problem of several variables was more complex than previously expected.

The theory of Eisenstein series on higher-rank groups turned out to be significantly more involved than on $\mathrm{SL}_2(\mathbf{R})$. Langlands' PhD thesis (see section 2.9) had prepared him to handle the challenging situation here. Langlands succeeded in decomposing $L^2(\Gamma \setminus G)$ into both the continuous spectrum and the discrete spectrum. Attempts to parametrize the continuous spectrum by Eisenstein series presented its own problems because the continuous spectrum broke up into a descending tree, where each level represents a factor of the previous one, reducing the problem to lower rank, making them amenable to induction arguments. Roughly speaking, Langlands' proof was an elaborate induction on the rank of the group. However, its significant layer of combinatorial complexity was one of the complications facing Langlands. Moreover, some extra attention is needed when some Eisenstein series actually arise as the "residues" of some other Eisenstein series on larger groups. This phenomenon had not been recognized until then.

> I read Gelfand's address to the ICM in Stockholm, finally understood correctly the notion of a cusp form in general. Since, as I observed, I had some passive experience with the spectral theory of self-adjoint operators and with holomorphic forms of several variables, several months with my nose to the grindstone and a refusal to be discouraged by temporary setbacks—for the proof presented a good number of unexpected obstacles—gave me in the spring of 1964 a complete proof of analytic continuation.

>I was exhausted and, moreover, quite dissatisfied with the account of the proof but with no energy and no desire to revise the exposition. If Harish-Chandra had not taken time from his own researches to work through and present at least a part of my paper—that pertaining to Eisenstein series associated to cusp forms—no one may have taken me seriously. To Bochner and Harish-Chandra I owe an enormous amount.
>
>So what did I offer or have available that was not available to Selberg? Of course, one thing I had was an understanding of the style of Harish-Chandra and the context in which he worked. Another, was a clear notion of cusp-form.

At this point Langlands had worked solely with the archimedean place. However, this would be the archimedean component π_∞ of what later become the "automorphic representation". The nonarchimedean parts of an automorphic representation were also being investigated, around the same time, mainly by Tamagawa and Satake (section 3.12).

Langlands' spectral theory was undoubtedly his first major achievement.

3.9. UC Berkeley—a disappointing year (1964–1965)

>In the fall of 1964, I went to 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 hope, having established the analytic continuation of the Eisenstein series, to turn immediately to the trace formula, but it was too daunting.
>
>Although, as I appreciate in retrospect, it was not an entirely unsuccessful year. 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 realization of the discrete series representations constructed by Harish-Chandra not long before. I ran a seminar together with P. Griffiths on abelian varieties, but in the end he did much more with the material than I did. Nevertheless I did not attach much importance to them and was discouraged. I did not have the feeling that things were working out.

3.10. The Boulder conference in Boulder, Colorado (summer 1965).
In the summer of 1965, Langlands attended the Boulder conference in Boulder, Colorado, which was organized by Borel and Mostow. His reflections suggested that it was an important event to him.

>I came of age at a time when Hecke, Siegel, Maass, and Selberg had revived a theory that goes back to Eisenstein, Dirichlet, and many others and which would now be referred to as the theory of automorphic forms. At the same time Artin and Chevalley had not been able to create a nonabelian class field theory and Artin had even written that he had come to believe that there was no such theory.

At the Princeton University Bicentennial Conference on the Problems of Mathematics held in 1956, on p. 5 of a pamphlet published on that occasion, Brauer reported his proof of Artin's conjecture about induced characters. Brauer's proof asserts that characters known to be rational combinations of certain special characters are in fact integral rational combinations. Brauer's result represents a decisive step in the generalization of class-field theory to the nonabelian case, which is commonly regarded as one of the most difficult and important problems in modern algebra.

Artin stated,

> My own belief is that we know it already, though no-one will believe me—that whatever can be said about nonabelian class field theory follows from what we know now, since it depends on the behavior of the broad field over the intermediate fields—and there are sufficiently many abelian cases. The critical thing is learning how to pass from a prime in an intermediate field to a prime in the large field. Our difficulty is not in the proofs, but in learning what to prove.

We have Langlands' comment on Artin's quotation:

> It is evident that Artin's notion of a nonabelian theory is not ours. His notion is closely tied to contributions of the initial creators of the abelian theory; ours lies more in Artin's own contributions.

The Boulder conference had inspired Langlands to investigate the following two problems:

> I participated in the Boulder conference and learned, somewhat belatedly, to think in terms of reductive algebraic groups. After having been introduced, in one way or another, in the early 1960s to the papers of Siegel, Selberg, Hecke, and Harish-Chandra and to class-field theory, I had the background to reflect on the problems (1) to create a general theory of L-functions, thus for all possible groups G appearing in the theory of automorphic forms, or better, automorphic representations in general, but along the line of the Hecke L-functions; and (2) to find a nonabelian class field theory, even though my understanding of the abelian theory was weak.
>
> As I recall, in the early sixties, a number of mathematicians had created a structure theory for groups over p-adic fields. These were described in the very successful Boulder conference. Some of the representation theory for real groups, created by Harish-Chandra, by the Russian school, and, to a lesser extent, by others, had been extended to groups over p-adic fields, in particular the theory of spherical functions to which, among others, Satake had contributed.

The first problem was popular at the time, and many unsuitable approaches were discovered. The solution of the second problem was the unfulfilled goal of Emil Artin.

The next section, L-functions, is a supplement to the above-mentioned problems (1) and (2).

3.11. *L*-functions—Euler, Riemann, Dirichlet, Frobenius, Artin, Hecke, and Langlands. We have mentioned in section 3.1 that the distinct feature of Langlands' automorphic *L*-functions is their dual nature—both arithmetic and analytic. As we will see, they are vast generalizations of classical *L*-functions attached to the names listed above, from Euler to Artin and Hecke.

3.11.1. The Euler–Riemann ζ-function. The Euler–Riemann ζ-function was first studied by Euler for real values of *s* only. He showed, using only the fundamental theorem of arithmetic, that the series had an Euler product expression over all prime numbers *p*, that is

$$\zeta(s) = \sum_n n^{-1} = \prod_p (1 - p^{-s})^{-1}, \qquad R(s) > 1, \quad n = 1, \ldots, \infty.$$

An immediate result is that the number of primes *p* is infinite.

   Later, Riemann, with the theory of complex analysis, was able to extend the domain of *s* from real values to complex values. He also showed that ζ has "nice" analytic properties:

   (1) analytic continuation to a meromorphic function of *s* in the entire complex plane with a simple pole at $s = 1$; and
   (2) functional equation relating values at *s* and $1 - s$.

Those two analytic properties together with the Euler product will be the *characteristic feature* of any future *L*-functions (or *L*-series).

3.11.2. Dirichlet L-series. Sometime later Dirichlet introduced complex valued functions $\chi(n)$, *n* is a positive integer, such that $\chi(nm) = \chi(n)\chi(m)$, $\chi(n + N) = \chi(n)$, and $\chi(n) = 0$ if $\gcd(n, N) > 1$. Such a χ is called a *Dirichlet character* with modulus **N**.

   By generalizing Riemann's construction, Dirichlet introduced his *L*-series and showed that it has an Euler product expansion over all primes *p*,

$$(3.11.1) \qquad L(s, \chi) = \sum_n \chi(n) n^{-s} = \prod_p (1 - \chi_p p^{-s})^{-1}, \quad n = 1, \ldots, \infty.$$

   Dirichlet showed that $L(s, \chi)$ has analytic continuation to a meromorphic function on the complex plane and a functional equation. In fact, the Dirichlet *L*-series behaves very much like the Riemann ζ-function, and Dirichlet showed that the number of primes in an arithmetic progression is infinite.

3.11.3. Artin L-functions—Galois representations and Frobenius class. Let *E* be a Galois extension of **Q** such that *E* is the splitting field of a monic, irreducible, integral polynomial $f(x)$ of degree *n*. The Galois group $G = \text{Gal}(E/\mathbf{Q})$ of *E* (or *f*) is the group of automorphisms of *E* leaving **Q** fixed. Since the elements of *G* permute the roots of *f*, we have an injective homomorphism of *G* into the symmetric group S_n on *n* elements. We say **E** (or **f**) is *abelian* if **G** is an *abelian group*.

   A *representation* of a group *G* is simply a homomorphism σ of *G* into the group of invertible linear transformations of a vector space $\text{GL}(V)$, $\sigma : G \to \text{GL}(V)$.

   A *subrepresentation* is one which stabilizes a subspace of *V*. A representation is *irreducible* if it cannot be written as a direct sum of proper subrepresentations.

Representations distinguish abelian groups from nonabelian groups, because of the following:

Schur's Lemma. Every irreducible representation of a group G is one-dimensional if and only if G is abelian.

It is clear that abelian groups can be described by characters (one-dimensional representations) alone while representations of nonabelian groups are necessarily higher dimensional.

Artin was interested in relating prime numbers p to elements of the Galois group of an irreducible polynomial f of degree n by considering the way in which f factors modulo p. That is

$$(3.11.2) \qquad f(x) \equiv f_1(x) \cdots f_r(x) \ (\mathrm{mod}\ p),$$

where each f_i is an irreducible polynomial of degree n_i, and $n = n_1 + n_2 + \cdots + n_r$.

An *n-dimensional Galois representation* is a homomorphism r of the Galois group $\mathrm{Gal}(E/\mathbf{Q})$ into $\mathrm{GL}_n(C)$, i.e., $r : \mathrm{Gal}(E/\mathbf{Q}) \to \mathrm{GL}_n(C)$, where E is a finite Galois extension of $\mathbf{Q}$. If E is an abelian extension over $\mathbf{Q}$ and r is irreducible, then r is a character $\sigma : \mathrm{Gal}(E/\mathbf{Q}) \to C^\chi$.

It was Frobenius who introduced a canonical way of turning primes into conjugacy classes in Galois groups over $\mathbf{Q}$. He had shown that there is a unique element g of the Galois group $\mathrm{Gal}(E/\mathbf{Q})$, defined by $g(x) = x^p \ (\mathrm{mod}\ P)$, for all integers x of E, where p is an unramified prime and P is a prime ideal lying over p in E, which when considered as an element of S_n is defined by the partition

$$\prod p = \{n_1, \ldots, n_r), \quad \text{where } n_1 + \cdots + n_r = n,$$

and it is *well-defined up to conjugation.*

We denote the *conjugacy class* of g associated to p by Frob_p, and it is called the *Frobenius class*. Any element of the conjugacy class is called a *Frobenius element* of p.

Remarks.

(1) $\mathrm{Frob}_p = 1$ if and only if $f(x)$ splits into linear factors modulo p in (3.11.2).
(2) Suppose E is the Nth cyclotomic field. Then its Galois group over $\mathbf{Q}$ is abelian and isomorphic to $(Z/NZ)^x$, and conjugacy classes in the Galois group are just elements. Now suppose p does not divide N. Then the Frobenius class is $p \mod N$. This example suggests not only that the distribution of Frobenius conjugacy classes in Galois groups over $\mathbf{Q}$ generalizes Dirichlet's result about primes in arithmetic progressions, but also that the Frobenius class is linked with the Dirichlet character.

Artin was inspired by the two notions mentioned above: The first is the notion of a *group representation* which is distinct from a character and which could distinguish between abelian and nonabelian extensions. The second is the Frobenius class which is well-defined in nonabelian extensions. Historically, Artin's L-functions were developed by Artin in part as an attempt to understand nonabelian class field theory.

A central ingredient in Artin's construction of his local L-function was an n-dimensional Galois representation $r : \mathrm{Gal}(E/\mathbf{Q}) \to \mathrm{GL}_n(C)$, where the Frobenius conjugacy class Frob_p will be mapped to a semisimple conjugacy class $r(\mathrm{Frob}_p)$ inside $\mathrm{GL}_n(C)$. Semisimple conjugacy classes in $\mathrm{GL}_n(C)$ have a particularly simple

representation: a diagonal matrix with the roots of the characteristic polynomial of the conjugacy class along the diagonal.

Artin defined its local L-function in $\mathbf{Q}$ as

$$(3.11.3) \qquad L_p(s,r) = \left[\det(1 - r(\mathrm{Frob}_p)p^{-s}\right]^{-1}, \quad p \text{ unramified},$$

in terms of the associated characteristic polynomial, where $r(\mathrm{Frob}_p)$ is a semisimple conjugacy class in $\mathrm{GL}_n(C)$.

The global Artin L-function, defined for unramified primes in $\mathbf{Q}$ only, is the Euler product

$$(3.11.4) \qquad L(s,r) = \prod_p L_p(s,r) = \prod_p \left[\det(1 - r(\mathrm{Frob}_p)p^{-s}\right]^{-1}.$$

Artin worked from the beginning with finite Galois extensions E over an arbitrary number field F, a finite field extension of $\mathbf{Q}$ rather than $\mathbf{Q}$ itself. Algebraic number theory assures us that the definitions we have so far can be extended from $\mathbf{Q}$ to F.

Let S be the set of prime ideals for F that ramify in E, then for any prime ideal $P \notin S$, there is a canonical conjugacy class Frob_P in $\mathrm{Gal}(E/F)$.

From the n-dimensional representation $r : \mathrm{Gal}(E/F) \to \mathrm{GL}_n(C)$, Artin defined his local and global L-functions in F as follows:

$$(3.11.5) \quad L_F(s,r) = \prod_P L_{F,P}(s,r) = \prod_P \left[\det(1 - r(\mathrm{Frob}_P)(NP)^{-s}\right]^{-1}, \quad P \notin S,$$

where NP is the number of elements of $\mathcal{O}/P$, and $\mathcal{O}$ is the ring of algebraic integers of F.

Artin conjectured that $L_F(s,r)$ has nice analytic properties. Since it is defined as an Euler product, it is fundamentally an arithmetic object, and the kind of analysis Dirichlet applied to his L-function was not suitable for $L_F(s,r)$. However, when E/F is an abelian extension, Artin was able to link his L-function with Hecke's degree-1 L-series which has nice analytic properties. This was achieved via the Artin reciprocity law. Let us introduce Hecke's L-series before stating the Artin reciprocity law.

3.11.4. *Hecke L-series*

3.11.4.1. *Hecke's degree-1 L-series.* A significant generalization of the works of Riemann and Dirichlet was taken up by Hecke who generalized the Dirichlet L-series to number fields. A Hecke L-series for an abelian number field extension F over $\mathbf{Q}$ takes the form

$$(3.11.6) \qquad L(s,\chi) = \sum_U \chi(U)N(U)^{-s} = \prod_P (1 - \chi(P)N(P)^{-1})^{-1},$$

where the Hecke character χ is a character on the idele class group, U is an (ordinary) ideal, and P is an unramified prime ideal of the ring of algebraic integers of F. Hecke showed that $L(s,\chi)$ has a degree-1 Euler product, and it possesses desirable analytic properties such as analytic continuation and a functional equation.

We remark that both Dirichlet's L-series and Dedekind's ζ-function are special cases of Hecke's degree-1 L-series:

(1) Taking the number field F to be $\mathbf{Q}$ and the character χ to be nontrivial, then $L(s,\chi)$ specializes to the Dirichlet L-series.
(2) Taking the character χ to be the trivial character, then one recovers the Dedekind ζ-function.

3.11.4.2. *Hecke's degree-2 L-series—Ramanujan, Mordell, and Hecke operators.*
Ramanujan, in 1916, was the first to observe that modular forms could have L-series
attached to them. He actually conjectured that the classical Delta-function has a
degree-2 L-series with Euler product expansion. A year later, Mordell proved this
by introducing a series of operators known as Hecke operators (an early version).
Hecke operators as we now know them are a vast generalization of Mordell's version
by Hecke in 1936.

Relying on the Hecke operators, Hecke showed that given a modular cusp form
for $\mathrm{SL}_2(Z)$, $f(z) = \sum a_n e^{2\pi i n z}$, $n = 1, \ldots, \infty$, of weight k and level N, its associated
Dirichlet L-series has a *degree-2 Euler product* expansion if f is an eigenfunction
for all the Hecke operators $\mathbf{T_p}$ with $\mathbf{T_p f = a_p f}$ for all primes p, that is

$$(3.11.7) \qquad L(s,f) = \sum_n a_n n^{-s} = \prod_p (1 - a_p p^{-s} + p^{k-1-2s})^{-1}.$$

Hecke was also able to show that $L(s,f)$ could be meromorphically continued
to the entire complex plane and possessed a functional equation. Hecke's degree-2
L-series shed a new light on the theory of automorphic forms and their L-functions.
Interestingly, neither Hecke nor Artin was aware of the other's work on L-functions
even though they were working not far from each other. Their L-functions were
finally linked by Langlands as a special case of his functoriality conjecture.

3.11.5. *The Artin reciprocity law and class field theory.*

> I will tell you a story about the Reciprocity Law. After my thesis,
> I had the idea to define L-series for nonabelian extensions. But
> for them to agree with the L-series for abelian extensions, a certain
> isomorphism had to be true. I could show it implied all the standard
> reciprocity laws. So I called it the General Reciprocity Law and
> tried to prove it but couldn't, even after many tries. Then I showed
> it to the other number theorists, but they all laughed at it, and
> I remember Hasse in particular telling me it couldn't possibly be
> true. Still, I kept at it, but nothing I tried worked. Not a week went
> by—for three years!—that I did not try to prove the Reciprocity
> Law. It was discouraging, and meanwhile I turned to other things.
> Then one afternoon I had nothing special to do, so I said, "Well,
> I try to prove the Reciprocity Law again." So I went out and sat
> down in the garden. You see, from the very beginning I had the
> idea to use the cyclotomic fields, but they never worked, and now I
> suddenly saw that all this time I had been using them in the wrong
> way—and in half an hour I had it.[2]

The Artin reciprocity law. Suppose E is an abelian extension over F, and let S
be the set of prime ideals for F that ramify in E. Then for any Galois representation
r over F, $r : \mathrm{Gal}(E/F) \to C^\chi$, there is a Hecke character χ over F such that

$$(3.11.8) \qquad\qquad\qquad r(\mathrm{Frob}_P) = \chi(P), \quad P \notin S,$$

and this led to

$$(3.11.9) \qquad\qquad\qquad L_F(s,r) = L(s,\chi).$$

Both (3.11.8) and (3.11.9) are called the *reciprocity law.*

---

[2]Emil Artin, as recalled by Mattuck in *Recountings: Conversations with MIT Mathematicians*,
2009.

Artin's reciprocity law identifies Artin's abelian L-function $L_F(s, r)$ in (3.11.5) with Hecke's abelian L-series $L(s, \chi)$ in (3.11.6). This identity enables $L_F(s, r)$ to acquire nice analytic properties such as analytic continuation and a functional equation from $L(s, \chi)$.

Let us list a few consequences of Artin's reciprocity law:

(a) The reciprocity law (3.11.8) informs us that abelian Galois extensions E over F are limited to those attached to Hecke characters χ. From now on let E be an *abelian* extension over $\mathbf{Q}$, then (3.11.8) simplifies to

$$(3.11.10) \qquad\qquad r(\mathrm{Frob}_p) = \chi(p),$$

where p is a prime in $\mathbf{Q}$ and χ is a Dirichlet character for $\mathbf{Q}$ with modulus N.

Let us also recall that $\mathrm{Frob}_p = 1$ if and only if $f(x)$ splits into linear factors in (3.11.2)

(b) Let $S(E/\mathbf{Q})$ be the set of unramified primes p such that $\mathrm{Frob}_p = 1$. Then $S(E/\mathbf{Q})$ is also the set of primes that splits completely in E. From Tate's work [T, p. 165] we know that the map $E \to S(E/\mathbf{Q})$ is injective. So we have a criterion which limits the number of abelian Galois extensions E of $\mathbf{Q}$.

(c) Suppose $f(x)$ in (3.11.2) is a quadratic polynomial. Then (3.11.10) tells us that *a prime is a sum of two squares if and only if it is congruent to* 1 *modulo* 4.

(d) Artin's reciprocity law implies the *Kronecker–Weber theorem*: *Any finite abelian extension of* $\mathbf{Q}$ *is contained in the cyclotomic extension of* N *th roots of* 1, *for some* N.

The Artin reciprocity law is the essence of *class field theory*. How should we describe class field theory? Let us take a look at what Langlands has to say:

> Class field theory, which establishes a decisive connection between abelian extensions of number fields on one hand and their ideal class groups on the other hand, classifies and constructs all abelian extensions of number fields. So problem arose after the completion of the theory to classify and construct (in some sense) all finite extensions. In 1956 at the bicentennial conference of Princeton University, Artin suggested that perhaps all we could know and all we needed to know in general was implicit in our knowledge of abelian extensions, so that there was in fact little left to do, although it was not clear what it might be.

3.11.6. *Emil Artin and the pursuit of nonabelian class field theory.* In the second of his *Two lectures on number theory, past and present*, André Weil summarized the state of affairs up to the mid-1930s, after Artin's reciprocity law had been proven, by saying:

> Artin's reciprocity law, which in a sense contains all previously known laws of reciprocity as special cases, deals with a strictly commutative problem. It establishes a relation between the most general extension of a number field with a commutative Galois group on the one hand, and on the other hand the multiplicative group over that field. Where do we go from there? Well, of course we take up the noncommutative case.

The Artin school had hoped in vain that their theory of simple algebras, and the resulting nonabelian cohomological theory would lead to a nonabelian class field theory, but this never materialized.

A generalization of Artin's reciprocity law to nonabelian extensions of number fields is the Langlands' functoriality conjecture, which, in a special case, is Artin's reciprocity conjecture, and identifies, conjecturally, Artin's nonabelian L-functions with Langlands' automorphic L-functions which are analytic in nature. In this respect, Langlands' functoriality conjecture represents a general formulation of nonabelian class field theory—a theory that had eluded Artin's grasp more than half a century ago.

3.11.7. *Langlands' automorphic L-functions.* We have seen that Hecke's degree-1 L-series was the analytic counterpart of Artin's abelian L-functions. The analytic counterpart of Artin's nonabelian L-functions is Langlands' automorphic L-functions $L\,(s, \pi, \rho)$. This identity enables Artin's nonabelian L-functions to have analytic continuation and functional equation, albeit conjecturally.

A novel feature of Langlands' L-function $L\,(s, \pi, \rho)$ is the presence of its dual representations: the automorphic representation π and the finite-dimensional representation ρ. *Automorphic representations* are the nonabelian generalizations, largely due to the introduction of adelic theory, of *Hecke characters.* The finite-dimensional representation ρ is, roughly speaking, an extension of the Galois representation associated with the Artin L-function, and the Frobenius classes play an important role. In this sense, *Langlands' L-functions* represent an unprecedented generalization and fusion of ideas of, among others Dirichlet, Frobenius, Hecke, and Artin. This topic will be revisited in section 3.15.

Why should L-functions deserve so much attention? Well, besides their applications to number theoretical problems, L-functions are particularly relevant to the principle of functoriality. For example, take two L-functions, which are concrete objects to study, their equality is equivalent to the identity of their attached objects. Those objects could appear quite different and, hence, much harder to handle.

3.12. **The adelic theory.** The adelic theory played an important role in Langlands' discovery of his L-functions. Chevalley had introduced the adeles into number theory with the intent to streamline notions in class field theory. An important application of the adeles came in Tate's Princeton University thesis (1950). Tate recast Hecke's work about his L-functions using the theory of adeles.

In 1963 Tamagawa, and independently Godement, extended Tate's work on Hecke's L-functions from $\mathrm{GL}(1)$ to an inner form of $\mathrm{GL}(n)$. In particular, he showed that there is a Hecke algebra for which one can produce (in a similar vein to Hecke) a degree n, pth Hecke polynomial. Further, Tamagawa was systematically introducing adelic methods and the theory of spherical functions into the theory of automorphic forms. In particular, Satake (1964) translated Hecke theory into the language of spherical functions (the local analogue of Hecke operators), and he also localized Tamagawa's arguments with spherical functions and generalized them from $\mathrm{GL}(n)$ to general p-adic groups.

> Spherical functions were in the air and I used that. These, I suppose, relied on a more or less obvious transfer of the theory of spherical functions from the reals to the p-adics that is implicit

in the theory of Eisenstein series. It is difficult to transfer one-
self back to that time, but this transition was made more or less
unconsciously.

3.13. A promising career in jeopardy (fall 1965–summer 1966). Langlands
started to think seriously about the two problems mentioned in section 3.10 soon
after the Boulder conference, but he was not successful. He grew more and more
discouraged to the point that he was seriously considering abandoning mathematical
research. Consequently, some months later, he decided to spend a year in Ankara,
Turkey:

> There were in the 1960s still residues of romantic notions of British
> imperialism, embodied in figures like Gertrude Bell or T. E. Law-
> rence. So one could dream, even with a wife and four small children,
> of escaping into the life and language of some exotic land and be-
> ginning anew. I did; my wife, more generous than wise, did not
> discourage me, and we made plans to spend a year, at first several
> years, but the department in Ankara only agreed to a provisional
> 1-term appointment. The specific choice of Turkey was the result
> of accidental factors.

The decision had been taken by the summer of 1966. Langlands was, no doubt,
attempting to liberate himself. The decision itself freed him, and all ambitious
projects were dropped. He took up again the study of Russian which he had
abandoned for many years.

3.14. Constant term calculations—an unexpected path to success (fall
1966). In the fall of 1966, Langlands did not spend much time with mathematics.
His interest was in the studies of Russian and Turkish.

> I took a beginner's course in Turkish and a course in Russian with
> which I already had a little experience. The teacher was Valentine
> Tschebotaref-Bill. I liked her as a teacher and I think she was fond
> of me. However, I still had a little time for mathematics but no
> serious goals. As far as I can recall, I began idly, simply to fill
> the time, to calculate the constant term of the Eisenstein series
> associated to maximal parabolics of split groups. I had no goal in
> mind, just nothing better to do. 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 meromor-
> phic functions, already a convincing beginning. The ideas must
> have matured over the course of the summer and fall of 1966, my
> thoughts quickening toward the year's end. The strange way in
> which mathematical insights arise.

Langlands realized that the constant term he was calculating possessed features
that could be related to the automorphic L-function he was searching for. This
revelation both inspired and energized him, and in the next few months he worked
feverishly to reach his goal.

> I abandoned the Turkish course and the Russian course from one
> day to the next, although I was already committed to Turkey. This
> angered Ms. Tschebotaref-Bill, and with good reason. She had
> been generous to me. I was unable to explain the situation to her
> satisfactorily.

The unexpected success from his constant term calculations was a total surprise,
perhaps not only to Langlands himself but also to everyone who was trying to find
an acceptable definition for an automorphic L-function.

> I had not recognized in 1966, when I discovered after many months
> of unsuccessful search for a promising definition of automorphic L-
> function, what a fortunate, although, and this needs to be stressed,
> unforeseen by me, or for that matter anyone else, blessing it was
> that it lay in the theory of Eisenstein series. What is to be very
> much stressed is that there is no intrinsic reason that there should
> or must be a connection between the local factors of the Euler prod-
> uct and the representation theory of reductive groups. There is, so
> far as I know, absolutely no reason that this is so. There is no com-
> parable phenomenon for the group GL(1) alone. The phenomenon
> cannot be discovered before one has passed—in the spirit of Siegel
> and his nineteenth-century precursors—to automorphic forms on
> reductive groups. It is a phenomenon for which neither the Bour-
> baki nor the Artin–Chevalley school were prepared, and which they
> still cannot readily accept. These schools were enormously success-
> ful, and their members are still very influential.

3.15. **A successful search for automorphic L-functions (winter), the Sa-
take isomorphism, and the Langlands isomorphism.** The Eisenstein series
that Langlands was working with was the most general Eisenstein series, which was
generalized by him in his construction of Langlands' spectral theory (section 3.8).
The new feature of this Eisenstein series, in 1966, was its adelic format. Langlands
was calculating the p-factors of the Euler products in the constant term which arose
in the functional equation of his adelic Eisenstein series. They appeared in the form

$$(3.15.1) \qquad \prod \xi_i(\alpha_i s)/\xi_i(\alpha_i s + 1), \quad i = 1, \ldots, r,$$

where $\xi_i(\alpha_i s)$ is a polynomial. They were defined on $F/\mathbf{Q}$, for reductive groups,
where F is a finite field extension over the rational numbers $\mathbf{Q}$.

> If I had not searched assiduously for a general form of the theorems
> of Hecke and of the founders of class field theory, or had not been
> familiar with various principles of nonabelian harmonic analysis
> as it had been developed by Harish-Chandra, in particular with
> the theory of spherical functions, I might have failed to recognize
> the importance or value of (3.15.1). It is the relation expressed
> by (3.15.1) that suggests and allows the passage from the theory of
> Eisenstein series to a general notion of automorphic L-function that
> can accommodate not only a nonabelian generalization of class-field
> theory but also, as it turned out, both functoriality and reciprocity.
> It was the key to the suggestions in the Weil letter.

Langlands was intrigued by two unexpected features in these polynomials:

(a) the degree of the polynomial in the numerator of (3.15.1) was not the dimension of the representation of the group G as he had expected, instead, it was the dimension of the representation of the dual of that group; and

(b) these polynomials in (3.15.1), as characteristic polynomials of semisimple conjugacy classes on a dual group, shared some of the same features as the characteristic polynomials in Artin's construction of his L-function via Frobenius conjugacy classes.

Langlands' observation (a) led him to focus not only on the dual group $\hat{G}$ but also on the link between the local factors of the Euler product and the representation theory of reductive Lie groups. Such a link was established via the creation of the L-group ^LG. A conjugacy class $c(\pi_p)$ in the L-group is called the Langlands class.

3.15.1. *The L-group.* The L-group ^LG is defined usually over field extensions K/F as the semidirect product of the dual group $\hat{G}$ with the Galois group $\mathrm{Gal}(K/F)$ (or the Weil group $W(K/F)$),

$$(3.15.2) \qquad\qquad ^LG = \hat{G} \rtimes \mathrm{Gal}(K/F),$$

where K/F is a Galois extension and $F/\mathbf{Q}$ is a finite field extension with $\mathbf{Q}$ the field of rational numbers.

We note that when $K = F$, then we have $^LG = \hat{G}$. Since the dual group of a reductive group was not established at that time, Langlands' creation of both the dual group and the L-group were two of his major achievements.

> I then realized not only that these functions were quotients of Euler products, but also that the numerator, whose form was similar to that of the denominator, could be described in terms of representation theory. The relevant representations were algebraic representations of a complex algebraic group—the L-group. I not only understood the form of these functions, I was also able to prove—thanks to the general theory of Eisenstein series—that they admit meromorphic continuations.

It was Langlands' creation of the L-group, together with his observation (b) about the similarity between his characteristic polynomials and those of Artin's, that eventually led him to the definition of the automorphic L-function $L(s, \pi, \rho)$.

3.15.2. *The Satake isomorphism.* Aside from the L-group, the other essential ingredient in the construction of Langlands' local L-functions is the Satake isomorphism.

> 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-bi-invariant differential operators.
>
> [The Satake isomorphism] is suggested immediately by the analogous lemma for spherical functions over a real field. The step from it to what I have called the Frobenius–Hecke conjugacy class (i.e., the Langlands class) rather than the Satake parameter—a term often used by others—is technically minute, but entails a fundamental conceptual change. Without any sign of the L-group and without

the desire to find an adequate notion of automorphic L-function, there was no need for it.

Let us take a closer look at Langlands' words in the above quote, especially about "The step [that] entails a fundamental conceptual change."

(a) Satake was motivated to construct the Satake isomorphism from his interest in the Euler factors appearing in Tamagawa's L-function associated to automorphic representation on an inner form of $GL(n)$.

(b) Satake describes the contribution of his paper *Theory of spherical functions on reductive algebraic groups over p-fields* (Publ. Math. IHES, 18 (1963)), in the following words:

> Then our main theorem asserts that $\mathcal{L}(G, U)$ is isomorphic to the algebra of all w-invariant polynomials functions on $\text{Hom}(M, C^*) \cong C^a, \dots$, thus $\mathcal{L}(G, U)$ is an affine algebra of (algebraic) dimension a over C.

The algebra $\mathcal{L}(G, U)$ is the algebra of spherical functions. That is, however, suggested immediately by the analogous lemma for spherical functions over the real field.

(c) Satake proved the nonarchimedean analogue of the Harish-Chandra isomorphism, relating the Hecke algebra with a certain polynomial ring. The similarity of Satake's target ring to that of a general representation ring was striking. However, Satake had no motivation to suspect that it was a representation ring because "without the desire to find an adequate notion of automorphic L-function, there was no need for it."

(d) The conjugacy class $c(\pi_p)$ (i.e., the Langlands' class), for unramified p, was an object in the L-group. It was Langlands who established a bijection between $c(\pi_p)$ and the unramified local automorphic representation π_p, which is a character on the Hecke algebra H_G. This bijection was *the step* taken by Langlands which *entails a fundamental conceptual change.*

By what we have just seen, the following questions come up naturally:

- How can the Satake isomorphism, "without any sign of the L-group", link the Hecke algebra with a conjugacy class in the L-group $^L G$?
- Since a conjugacy class in the L-group has no direct connection with the Satake isomorphism, what to the "Satake parameter" referring to?

  The expression "Satake parameter" is completely inappropriate and could very well be replaced by "Frobenius–Hecke parameter". It is an extension of the Frobenius parameter adapted to the Hecke theory both in the original and extended forms. This parameter as an element of the definition of automorphic L-functions was discovered in the context that is the continuation of contributions of Frobenius and Hecke.

3.15.3. *The Langlands isomorphism.* Let us give a very brief account on Langlands' construction of the bijection in 3.15.2(d).

- The Satake isomorphism, as we have seen, only relates the Hecke algebra H_G with a certain polynomial ring.
- Langlands realized, from his constant term calculation and observation 3.15(a), that the Satake target ring was in fact the representation ring of a certain complex algebraic group.

- In searching for that "certain" complex algebraic group, Langlands created the L-group.
- With the L-group in place, a bijection was established between the Satake's target ring and the representation ring of the L-group, denoted by $R(^L G)$.
- Linking this bijection with the Satake isomorphism, one gets an isomorphism relating the Hecke algebra H_G with the representation ring $R(^L G)$:

(3.15.3) $$H_G \approx R(^L G).$$

We call the isomorphism in (3.15.3) the *Langlands isomorphism.*

- The local automorphic representation π_p is a character on the Hecke algebra H_G. However, via the Langlands isomorphism (3.15.3) one can view π_p as a character on $R(^L G)$.
- Essentially by definition, characters of a representation ring correspond bijectively to conjugacy classes of the underlying group; therefore, π_p corresponds bijectively to the conjugacy class $c(\pi_p)$ inside the L-group, and this establishes the desired bijection in 3.15.2(d). We refer to $c(\pi_p)$ as the conjugacy class associated with π_p for unramified p.

3.15.4. *Definitions of Langlands' L-functions.* Langlands' definition of L-functions actually presupposed local factors of ramified primes as well as the archimedean prime π_∞. However, the unramified L-functions $L_p(s, \pi_p, \rho_p)$ remain the most important component. In our version of Langlands' definitions of $L_p(s, \pi_p, \rho_p)$ and $L(s, \pi, \rho)$, we restrict ourselves to dealing with unramified primes p in $\mathbf{Q}$ only.

Since it is desirable to have the conjugacy classes $c(\pi_p)$ in a general linear group $G_n(C)$, rather than the complex (disconnected) group $^L G$, where $^L G = \hat{G} \rtimes \mathrm{Gal}(F/\mathbf{Q})$ and $F/\mathbf{Q}$ is a finite Galois extension, we let ρ_p be a finite-dimensional representation $\rho_p : {}^L G \to \mathrm{GL}_n(C)$.

This step is in analogy with Artin's construction of his L-functions which had been attached to the finite-dimensional representations of a finite Galois group. The image $\rho_p(c(\pi_p))$ is a semisimple conjugacy class inside $\mathrm{GL}_n(C)$: a diagonal matrix with the roots of the characteristic polynomial of the Langlands class $c(\pi_p)$ along the diagonal.

We express $\pi = \bigotimes_p \pi_p$ as a (restricted) tensor product where p is an unramified prime. Langlands defined his L-functions as follows:

Definition.

 (a) $L_p(s, \pi_p, \rho_p) = [\det(1 - \rho_p(c(\pi_p))p^{-s}]^{-1}, \quad p$ unramified.
 (b) $L(s, \pi, \rho) = \prod_p L_p(s, \pi_p, \rho_p) = \prod_p [\det(1 - \rho_p(c(\pi_p))p^{-s}]^{-1}$, which converges in some right half-plane.

We remark that p^{-s} will be replaced by $N(p)^{-s}$ in the above definitions when $\mathbf{Q}$ is replaced by a finite extension of $\mathbf{Q}$.

One of the reasons that we are engaged with automorphic representations is because the family $c(\pi)$ attached to π is a good object to study: It is believed that $c(\pi)$ is fundamentally connected to the arithmetic world, and that it carries concrete and analytic data. For example, any family $\{c(\pi_p) : p$ unramified$\}$ attached to $\{\pi_p : p$ unramified$\}$ determines a homomorphism from a Hecke algebra H_G into C. Elements in H_G are called Hecke operators. The conjugacy classes $\{c(\pi_p) : p$ unramified$\}$ thus provide eigenvalues of Hecke operators.

Langlands' success in attaching his L-function to an automorphic representation was an assertion that automorphic representations rather than automorphic forms were desirable objects to study.

Special cases of $L_p(s, \pi, \rho)$.

(a) Let $G = \{1\}$ be the trivial group. Then $\pi_p = 1$ and $\rho_p = 1$, and

$$L_p(s, 1, 1) = (1 - p^{-s})^{-1}$$

is the p-Euler factor of the Riemann ζ-function.

(b) Let G be trivial, and let $^L G$ be the Galois group over some unramified extension. Then

$$L_p(s, 1, \rho) = [\det(1 - \rho(\mathrm{Frob}_p) p^{-s})]^{-1}$$

is the local Artin L-function over $\mathbf{Q}$, where Frob_p is the Frobenius class.

(c) Let $G = \mathrm{GL}_2$, then π_p is just a Hecke character and the local Langlands' L-function over $\mathbf{Q}$ is the local Hecke L-function over $\mathbf{Q}$ (see equation (3.11.7)).

3.16. The genesis of the functoriality conjecture (winter break 1966). Almost immediately after the definition of automorphic L-function was found, Langlands turned his attention to the problem of establishing his L-functions' analytic properties such as meromorphic continuation to the entire complex plane and the functional equation relating the values at s and $(1 - s)$.

We learned from Langlands (see section 3.14) that the constant terms he was calculating were quotients of Euler products, and these Euler products could be continued to the entire plane as meromorphic functions. To him, that was a convincing beginning.

> What I held in my hands was a satisfactory definition of the function $L(s, \pi, \rho) = \cdots$. However, the functions L_v were still unknown at a finite number of places.[3] At this point I had no idea how to define them. The element $\gamma(\pi_v)$ belongs to a conjugacy class in a certain complex Lie group that is nowadays denoted by $^L G$. Even though I had proved the existence of a meromorphic continuation for a significant number of these functions, I had no idea how to prove the existence of such a meromorphic continuation in general, or whether their analytic continuation could be proved at all.

3.16.1. *The Tamagawa lecture.*

> This is what I thought about standing by the window. Suddenly, everything was immediately present in my mind, at least according to my own recollection:
>
> *Tamagawa has on some occasion that could not have been too long before December 1966, but I am not sure, delivered a lecture in the auditorium of the old Fine Hall in which he discussed the function $L(s, \pi, \rho)$, where G is an inner form of $\mathrm{GL}(n)$ and ρ is the defining representation of $^L G = \mathrm{GL}(n, C)$, and had treated the problem of analytic continuation. I saw no reason that his proof shouldn't also be valid for $\mathrm{GL}(n)$, as indeed it does, as later shown by Godement and Jacquet. [G-J]*

---

[3]Ramified primes and the archimedean prime

3.16.2. *Langlands' epiphany and his letter to André Weil.* This insight quickly led Langlands to the next step which was to replace Artin's reciprocity law with functoriality. We have the following vivid recollection of Langlands during those last few days at the end of 1966:

> The mathematics department at Princeton University was housed in the old Fine Hall, now Jones Hall, at the time a lovely building. As you enter it, on the right was a small office. It was a beautiful office. On the right was a seminar room, with a large table, chairs, and a large blackboard. The windows in both rooms were leaded, in a medieval style, with views of the presidential gardens. Since I lived on Bank Street, a few steps from the corner of Nassau Street and University Place, and there were four children at home, I often worked there evenings and on holidays.

The next paragraph deserves our attention:

> In particular, working there during the Christmas vacation of 1966, looking out the window, it occurred to me that what was needed was a generalization of Artin's original idea that his abelian L-functions were equal to the L-functions defined by ideal class characters. I realized that the conjecture I was in the course of formulating implied the Artin conjecture. Thus, what was needed was that the Artin's reciprocity law has to be replaced by what I now refer to as functoriality. It was certainly one of the major moments in my mathematical career.

While looking out the window, the epiphany of Langlands at that moment was what we might call a turning point in the history of mathematics. It was the moment that the genesis of Langlands' most celebrated creation—functoriality—was first revealed. Finally, Langlands' one last decisive insight was his reflection on the Tamagawa's lecture:

> 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 $^{L}G = \mathrm{GL}(n, C)$ 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 continuation of the Artin L-functions. The Artin reciprocity law has to be replaced by what I now refer to as functoriality.
>
> 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.
>
> 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 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.

Whether Weil took the letter seriously or not, the Langlands program was launched soon afterward.

At the inception of the Langlands program, with the introduction of Langlands' newly discovered *automorphic L-functions* attached to general reductive groups, one of the aims of the program was to search for a list of properties these L-functions were expected to satisfy. This list constituted a bulk of the original Langlands' conjectures, particularly the *functoriality conjecture* (section 3.17), which can be regarded as a foundation of the Langlands program.

It is not surprising that the nonabelian theory was not developed until the late 1960s. The theory of infinite-dimensional representations of reductive groups was first developed actively in the 1950s, and the p-adic case was not studied intensively until the 1960s; both those theories provided the necessary tools for Langlands' formulation of functoriality conjecture in January 1967.

3.17. The functoriality conjecture—from Hecke to Langlands. Let us see what James Arthur [A] has to say about functoriality:

> The principle of functoriality was introduced by Langlands as a series of conjectures in his original article [L]. Despite the fact it is now almost fifty years old, and that it has been the topic of various expository articles, functoriality is still not widely known among mathematicians.
>
> The principle of functoriality can be regarded as an identity between automorphic L-functions for two groups. Let G and G' be connected, quasi-split groups over $\mathbf{Q}$, and let φ be an L-homomorphism from $^L G'$ to $^L G$, which is compatible with the maps $\rho : {}^L G'$ to $\mathrm{Gal}(K/F)$ and $\rho' : {}^L G$ to $\mathrm{Gal}(K/F)$, then functoriality asserts that for every automorphic representation π' of G', there is an automorphic representation π of G such that $c(\pi_p) = \varphi(c(\pi'_p))$.

3.17.1. *From Hecke's L-series to Langlands' functoriality.* Artin's reciprocity law (3.11.9) identifies Artin's abelian L-function with Hecke's degree-1 L-series, and Langlands' epiphany was to replace Artin's reciprocity law with functoriality. In this section, we present an example of Langlands from [L1] which shows, if only as a rough sketch, how he arrived at functoriality—a sublime mathematical insight— from injecting his novel ideas, step by step, into a degree-2 Hecke L-series (see section 3.11.4).

If I replace s by $s + k - 1$ in the Hecke form

$$\prod_p [(1 - \alpha_p p^{-s})(1 - \beta_p p^{-s})]^{-1}, \quad \alpha_p \beta_p = p^{k-1},$$

then

$$\alpha_p \to \alpha'_p = \alpha_p/p^{k-1}, \qquad \beta_p \to \beta'_p = \beta_p/p^{k-1}, \quad \alpha'_p \beta'_p = 1,$$

and the functional equation is between s and $1 - s$.

Let γ_p be the diagonal matrix with α'_p and β'_p in the diagonal, which may be treated as a conjugacy class in $\mathrm{GL}(2, C)$, and the Hecke form may be written as

$$\prod_p [\det(1 - \gamma_p p^{-s})]^{-1}.$$

More generally, although Hecke did not do so, we may replace γ_p by $\rho(\gamma_p)$ where ρ is any finite-dimensional representation of $\mathrm{GL}(2, C)$.

(3.17.1) $$L(s, \pi, \rho) = \prod_p [\det(1 - \rho(\gamma_p) p^{-s})]^{-1}.$$

Here $\gamma_p = \gamma_p(f) = \gamma_p(\pi)$, where the automorphic form f, an eigenform of the Hecke operators, is replaced in the notation by the representation π it determines.

For any reductive group G/F and any automorphic representation of $G(A_F)$, the theory of spherical functions allows us to define $\gamma_p(\prod) = \gamma(\prod_p)$ as a conjugacy class in a finite-dimensional complex group, the L-group, for almost all p. The calculations on Eisenstein series I described give Euler products that can all be expressed as the right side of (3.17.1), i.e., $\prod_p [\det(1 - \rho(\gamma_p) p^{-s})]^{-1}$. They suggest that this function can be analytically continued and has a functional equation of the usual kind. This question posed, a way to answer it suggests itself.

At this point the Tamagawa lecture (see section 3.16.1) was a decisive insight that led Langlands to connect his Hecke series with his ideas in the next step:

Then in analogy with the Artin reciprocity law, all we would need to do to show the analytic continuation of $L(s, \pi, \rho)$ is to establish the existence of an automorphic representation of $\mathrm{GL}(n), n = \dim \rho$, such that

$$\{\rho(\gamma_p(\prod))\} = \{\rho_p(\prod)\} \quad \text{for almost all } p.$$

It is a small step—at least conceptually—from this possibility to the possibility of functoriality in general.

Taking the above ideas one step further, Langlands in [L2] succeeded in formulating his definition of functoriality:

I might equally well think that given two reductive groups H and G over a number field F, and given a map $\varphi : {}^L H$ to ${}^L G$ compatible with the maps $\rho : {}^L H$ to $\mathrm{Gal}(K/F)$ and $\rho' : {}^L G$ to $\mathrm{Gal}(K/F)$, and an automorphic representation $\pi_{H,v}$, then there also exists an automorphic representation $\pi_{G,v}$ such that we have

(3.17.2) $$\varphi(\Upsilon(\pi_{H,v})) = \Upsilon(\pi_{G,v})$$

for almost every place v.

Let us point out that equation (3.17.2) is the *essence of functoriality*. To state functoriality in terms of L-functions, we take the following steps:

(1) $\Upsilon(\pi_{H,v})$ and $\Upsilon(\pi_{G,v})$ are conjugacy classes in ${}^L G_H$ and ${}^L G_G$ attached to $\pi_{H,v}$ and $\pi_{G,v}$, respectively. The above quotation of Langlands informs us that we are not only given the condition $\rho'(\varphi(\Upsilon(\pi_{H,v}))) = \rho(\Upsilon(\pi_{H,v}))$, but also the assumed

existence of $\pi_{G,v}$ together with condition (3.17.2). An immediate consequence is the following

$$(3.17.3) \qquad\qquad \rho'(\Upsilon(\pi_{G,v})) = \rho(\Upsilon(\pi_{H,v})).$$

(2) The L-function given by (3.17.1) was Hecke's degree-2 L-series reformulated by Langlands. In the context of reductive groups H and G, the local L-function in (3.17.1) takes the form

$$L_p(s, \pi_{H,p}, \rho_p) = [\det(1 - \rho_p(\Upsilon(\pi_{H,v})p^{-s}))]^{-1}$$

and

$$L_p(s, \pi_{G,p}, \rho'_p) = [\det(1 - \rho'_p(\Upsilon(\pi_{G,v})p^{-s}))]^{-1}.$$

Langlands' functoriality conjecture (local and global).

(a) Let $\pi_{G,p} = \pi_{G,v}$ and $\pi_{H,p} = \pi_{H,v}$. Suppose we are given φ, ρ, ρ', and $\pi_{H,p}$ as before, and assume there exists an automorphic representation $\pi_{G,p}$ with condition (3.17.2) for each unramified p. Then from (3.17.3) we have

$$L_p(s, \pi_{G,p}, \rho'_p) = L_p(s, \pi_{H,p}, \rho_p).$$

(b) Let $L(s, \pi_G, \rho') = \prod_p L_p(s, \pi_{G,p}, \rho'_p)$ and $L(s, \pi_H, \rho) = \prod_p L_p(s, \pi_{H,p}, \rho_p)$, then under the same assumptions as in (a), we have

$$(3.17.4) \qquad\qquad L(s, \pi_G, \rho') = L(s, \pi_H, \rho).$$

Both (3.17.2) and (3.17.4) are known as Langlands' functoriality conjecture.

3.17.2. *Some consequences of functoriality.* We conclude by mentioning two immediate consequences of functoriality:

(1) A result of Godement and Jacquet [G-J] states that the L-functions of $\mathrm{GL}(n)$ have nice analytic properties provided the irreducible unitary representations of $\mathrm{GL}(n)$ are automorphic.

(2) Using (1) together with the expression in (3.17.4) imply that functoriality reduces the study of L-functions for arbitrary reductive groups G to the known results of $\mathrm{GL}(n)$.

(3) Artin's reciprocity conjecture is a special case of the functoriality conjecture. Suppose $H = \{1\}$ (the trivial group) and $G = \mathrm{GL}(n)$. Then $\varphi : {}^L H \to {}^L G$ reduces to the map $\rho : \mathrm{Gal}(K/F) \to \mathrm{GL}(n)$.

Since π_H is trivial and $\rho' : \mathrm{GL}(n) \to \mathrm{GL}_n(C)$ is the "standard" representation, it follows from functoriality and condition (3.17.4) that there exists an automorphic cuspidal representation $\pi(\rho)$ such that

$$(3.17.5) \qquad\qquad L(s, \pi(\rho)) = L(s, \rho).$$

Expression (3.17.5) is an assertion of Artin's reciprocity conjecture.

In summary, we repeat that one of the original aims of *abelian class field theory* was to establish nice analytic properties for Artin's abelian L-functions. A satisfactory solution was provided by Artin's reciprocity law which identifies Artin's arithmetic L-functions with Hecke's analytic L-functions.

For nonabelian field extensions, Artin's reciprocity conjecture identifies Artin's L-functions with Langlands' automorphic L-functions which are analytic in nature. This identity enables Artin's L-functions to have analytic continuation and functional equation, albeit conjecturally. In this respect, the Langlands functoriality

conjecture, which has Artin's reciprocity conjecture as a special case, represents a general formulation of nonabelian class field theory.

Acknowledgments

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References

[A]   J. Arthur, *Functoriality and the trace formula*, available at `www.math.toronto.edu/arthur/`.

[G-J] R. Godement and H. Jacquet, *Zeta functions of simple algebras*, Lecture Notes in Mathematics, Vol. 260, Springer-Verlag, Berlin–New York, 1972. MR0342495

[L]   R. P. Langlands, *Problems in the theory of automorphic forms*, Lectures in Modern Analysis and Applications, III, Springer, Berlin, 1970, pp. 18–61. Lecture Notes in Math., Vol. 170. MR0302614

[L1]   R. Langlands, *The genesis and gestation of functoriality*. TIFR, Mumbai, Feb. 2005, available at `www.math.tifr.res.in/sites/default/files/maths/TheGenesis.pdf`.

[L2]   R. Langlands, *Funktorialitat in der Theorie der atomotphen Formen: Ihre Entdeckung und ihre Ziele* [from a translated text], available at `http://publications.ias.edu/rpl/`.

[T]   J. T. Tate, *Global class field theory*, Algebraic Number Theory (Proc. Instructional Conf., Brighton, 1965), Thompson, Washington, D.C., 1967, pp. 162–203. MR0220697

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