Selberg's Eigenvalue Conjecture

On the occasion of Robert Osserman's retirement

Peter Sarnak

In the late 1970s, when I was a student at Stanford University, I attended a seminar run by Bob Osserman on eigenvalue problems—more specifically—bounds for eigenvalues. This seminar was instructive for me and my fellow students. It kindled my keen interest in spectral geometry. One of the results reported in this seminar was due to Osserman; I would like to review it as a starting point for this lecture.

Suppose W is a smooth, simply connected domain in $\mathbb{R}^2$. We are interested in the smallest positive eigenvalue for the Laplacian Δ on W with Dirichlet boundary conditions: $\lambda_{DIR}(W)$. In other words, the bass note of the drum W:

$$\lambda_{DIR}(W) = inf_{f|\partial W=0} \frac{\int_W |\nabla f|^2 dV}{\int_W |f|^2 dV}. \tag{1}$$

Under what conditions does $\lambda_{DIR}(W)$become arbitrarily small? It turns out that it is not the area of W, but rather, the in-radius $r(W)$ (the radius of the largest inscribed ball) that is relevant. In 1965, Makai [22] solved a long standing problem showing that the bass note can be made arbitrarily small only if the region includes an arbitrarily large circular drum: that is, if the in-radius goes to infinity. In 1978 Hayman [12] rediscovered Makai's result and following this, Osserman [25] gave sharp bounds relating these quantities, and (which is what is important to us here) he generalized these to spaces which are curved. Osserman clarified the use of isoperimetric inequalities and, in particular, Bonneson type inequalities in this context. His result for simply connected domains W in the hyperbolic plane $\mathbb{H}$ (that is, the upper half plane with the line element $ds = |dz|/y$) is as follows:

$$\lambda_{DIR}(W) \geq \frac{1}{4(tanh\, r(W))^2}. \tag{2}$$

This one-fourth is the magic number, and the issue of this one-fourth is the content of this lecture. So, from (2), $\lambda_{DIR}(W)$ decreases to $1/4$ only if $r(W)$ goes to infinity (actually, this suffices as well).

Let's pass now to the following picture which leads us to the fundamental conjecture of Selberg. Let X be a surface (without boundary) covered by $\mathbb{H}$, that is, X is a hyperbolic surface $\Gamma\backslash\mathbb{H}$ where Γ is a discrete subgroup of $SL_2(\mathbb{R})$. Again we look at the smallest eigenvalue $\lambda_1(X)$ of the Laplace Beltrami operator, but now the Dirichlet boundary conditions are replaced by $\int_X f\,dV = 0$.

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$$\lambda_1(X) = inf \frac{\int_X |\nabla f|^2 dV}{\int_X |f|^2 dV}, \tag{3}$$

where the infimum is taken over all functions f that have compact support in X and satisfy $\int_x f dV = 0$. It is understood here that dV, ∇, etc., are all defined using the hyperbolic metric descending from H.

It is easy to see from many points of view that $\lambda_1(X)$ can be made arbitrarily small, even for X of a fixed genus. For example, consider the degenerating family of such surfaces of genus two (figure 1, with the separating geodesic γ pinching to a point). As the length of γ tends to zero, $\lambda_1(X) \to 0$, for we can take as the test function in (3) a function f which is nonconstant only in the collar, and thus with $\int |\nabla f|^2 dV$ proportional to the length of γ. Thus, as the surface gets thinner and thinner, the length of the inner circle tends to zero and $\lambda_1 \to 0$ also. For details and further examples, see [26, 31].

Thus the number 1/4 does not seem to play the important role which it did in Osserman's theorem. However, interesting questions arise when we consider specific surfaces of this type which arise through number theoretic constructions. To best present the ideas, we restrict our attention to one such family: those $X = \Gamma \backslash \mathsf{H}$ where Γ is one of the groups

$$\Gamma(N) = \left\{ \begin{pmatrix} ab \\ cd \end{pmatrix} | a, b, c, d \in \mathbb{Z}, ad - bc = 1, \right.$$
$$\left. a \equiv d \equiv 1, b \equiv c \equiv 0 \mod N \right\}. \tag{4}$$

Let us denote $\Gamma(N)\backslash\mathsf{H}$ as $X(N)$. These are the (principal) congruence surfaces (or modular curves, as they are often called) which are central from many points of view, for example, in the formulation of the Shimura-Taniyama conjectures and the recent work of Andrew Wiles. Since $\Gamma(N)$ is a subgroup of finite index of the modular group $\Gamma(1)$, $X(N)$ is a finitely sheeted covering of $X(1)$. The $X(N)$ are noncompact but of finite area. We define $\lambda_1(X(N))$ by the Rayleigh quotient as in (3): it is the smallest eigenvalue for the Laplacian on $X(N)$. Now, the continuous spectrum of the Laplacian is well understood [13], [16]: it consists of the segment $[1/4, \infty)$. Thus the issue is really one of the discrete spectrum. Denote by $\lambda_1^{disc}(X(N))$ the smallest positive discrete eigenvalue of Δ on $X(N)$. It has been known for some time (Selberg, Roelcke) that $\lambda_1^{disc}(X(1)) > 1/4$. In fact, by a careful analysis of nodal lines Huxley [10] [11] has shown that $\lambda_1^{disc}(X(N)) > 1/4$ for $1 \leq N \leq 17$.

Consider the problem of estimating $\lambda_1(X(N))$ as $N \to \infty$ by geometric means. The area $|X(N)|$ of $X(N)$ is easily estimated. Indeed, the covering $X(N) \to X(1)$ is Galois, with deck group $PSL_2(\mathsf{Z}/N\mathsf{Z})$. The cardinality of the fiber of the cover is the number of 2×2 matrices over a ring with N elements, about N choices for each entry, with one relation: $ad - bc = 1$, so is about N^3. Thus $|X(N)|$ is about $N^3|X(1)|$. So the area of $X(N)$ goes to infinity with N, and one might guess that $\lambda_1 \to 0$. Indeed, Buser [5], by relating such problems to the combinatorics of cubic graphs, was led to the seemingly natural conjecture that $\lambda_1(X_g) \to 0$ as $g \to \infty$ where X_g is a hyperbolic surface of genus g.

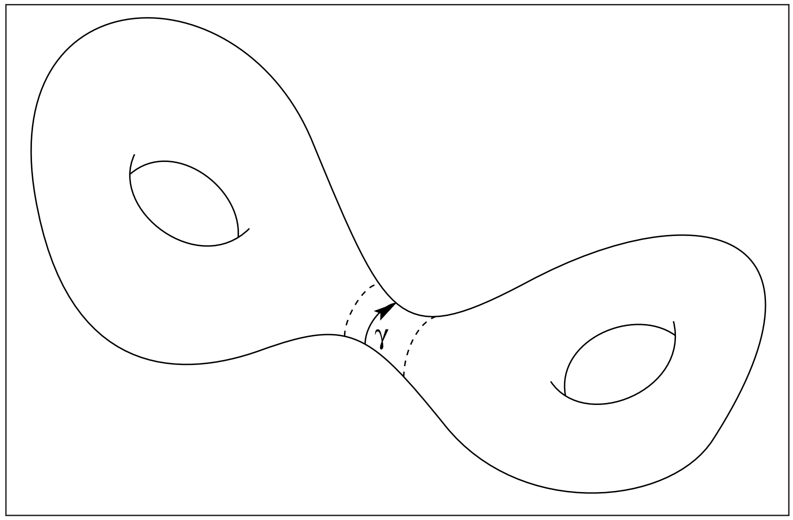


Figure 1

However, we have the following:

Conjecture 1 (Selberg [30], 1965). For $N \geq 1$,

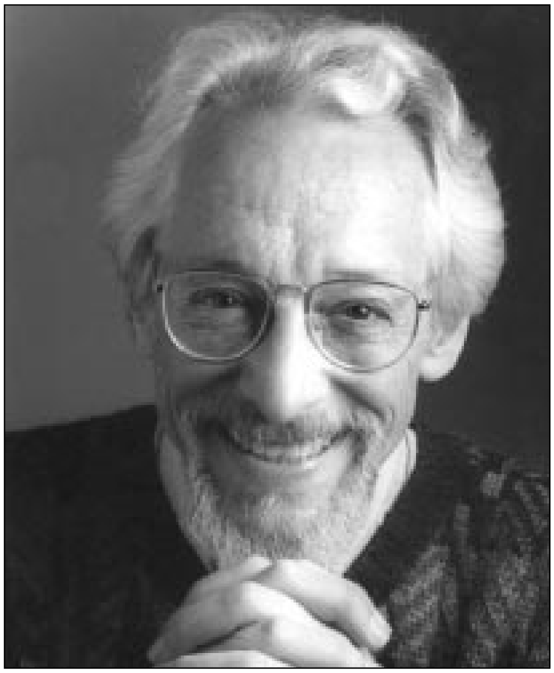
$$\lambda_1(X(N)) \geq 1/4.$$

I think this is the fundamental unsolved analytic question in modular forms. It has many applications to classical number theory (see [16, 30], for example). If true, it is sharp. In the first place, the continuous spectrum of $X(N)$ begins at 1/4 (so that $\lambda_1(X(N)) \leq 1/4$); and secondly, for certain N, $X(N)$ has discrete eigenvalues at $\lambda = 1/4$ (see Maass [21]). As we understand it today, Conjecture 1 is part of the general "Ramanujan" conjectures. From a modern representation theoretic point of view this conjecture is a natural generalization [27] of the famous Ramanujan conjectures solved by Deligne. The latter is more tractable, since Deligne was able to exploit algebraic-geometric interpretations of the classical Ramanujan conjectures. This path is not (as yet) available for Selberg's conjecture. Selberg backed up Conjecture 1 by proving the remarkable

Theorem 2 (Selberg [30], 1965).

$$\lambda_1(X(N)) \geq 3/16 = 0.1865.$$

In 1978 Gelbart and Jacquet [7], using methods very different from Selberg's, showed that one can replace the equality above by an inequality; but other than this, no improvement has been made until very recently, as I will describe toward the end of the lecture.



Robert Osserman

Selberg's approach was to relate this problem to a purely arithmetical question about certain sums of exponentials, called Kloosterman sums. This allowed him to invoke results from arithmetic geometry. The key ingredient giving the estimate is a (sharp) bound on Kloosterman sums due to Andre Weil [34]. This bound in turn is a consequence of the Riemann hypothesis for the zeta function of a curve which he had proven earlier. On the other hand, to go further than Theorem 2 by this approach, one needs to detect cancellations in sums of such Kloosterman sums, and arithmetic geometry offers nothing in this direction. This is the reason that the approach through Kloosterman sums has a natural barrier at 3/16. It is interesting that Iwaniec [15] has given a proof of Theorem 2 which, while still being along the lines of Kloosterman sums, avoids appealing to Weil's bounds.

I want to explain why Conjecture 1 is hard. Let's look at the fundamental domain of $X(N)$. Pretend that there are no cusps, so that we have some big polygon with many sides and a large body. The in-radius of this polygon is essentially half the girth of $X(N)$, that is, the length of the shortest closed geodesic on $X(N)$. This can easily be estimated from the definition of the group $\Gamma(N)$: the trace of the matrix $\gamma \in \Gamma(N)$ determines the length of a closed geodesic on $X(N)$. By a simple congruence analysis one finds that the girth is about $4/3 \log |X(N)|$, the area of $X(N)$. The number 4/3 is critical, as we will see.

That roughly is the geometric picture of $X(N)$. What then is responsible for the lower bound for $\lambda_1(X(N))$? $X(N)$ is gotten by certain identifications of the sides of the polygon, these being dictated by the arithmetic of linear fractional transformations mod N. A key feature is that this arithmetic identifies the sides "randomly". If you take a random symmetric $N \times N$ matrix with, say, three 1's in each row and column and all other entries zero, then its biggest eigenvalue is 3, but the next largest eigenvalue will be bounded away from 3 by a fixed amount independent of N. This is quite unexpected and is perhaps why Buser was misled. The latter is proven by combinatorial arguments, see [28] for example. It is this feature that is responsible for the conjecture in its crude form. Also the "random" identifications prevent one from finding test functions for which the Rayleigh quotient is small. With these comments it is perhaps no surprise that one can use the Ramanujan Conjectures to construct explicit graphs which mimic many desirable properties of random graphs [2, 19].

For many years Joseph Bernstein and David Kazhdan have been telling me about an interesting geometric approach to prove a lower bound for $\lambda_1(X(N))$ and related surfaces. While it appears difficult to push this through with geometric ideas alone, I will show you that, with some elementary arithmetic, this can be done. Look at $X(p)$, p a large prime (you can also deal with general N, but N prime is simpler). The deck group of the covering $X(p) \to X(1)$ is $PSL_2(\mathsf{Z}/p\mathsf{Z})$. Now suppose that there is a bad eigenvalue λ, i.e., $0 < \lambda < 1/4$. First, let's show that λ must be of high multiplicity. Let V_λ be the corresponding eigenspace. The Laplacian on $X(p)$ commutes with the deck transformations, and consequently $PSL_2(\mathsf{Z}/p\mathsf{Z})$ acts on V_λ. If this action is trivial, then a corresponding eigenfunction on $X(p)$ will live on $X(1)$, however, we have seen that for $X(1)$there is no such eigenfunction. So V_λ must contain a nontrivial irreducible representation of $PSL_2(\mathsf{Z}/p\mathsf{Z})$. A result going back to Frobenius asserts that any nontrivial irreducible representation of $PSL_2(\mathsf{Z}/p\mathsf{Z})$ has dimension at least $(p-1)/2$. We conclude that $\dim V_\lambda \geq (p-1)/2$. The idea then is to show that for λ small one cannot accommodate an eigenvalue with such large multiplicity.

To complete the argument, we need some suitable bounds on such multiplicities, $m(\lambda, X(p))$. Xue and I [29] proved the following bounds (which are valid in much more general settings).

For $\varepsilon > 0$ there is C_ε such that

$$m(\lambda, X(p)) \leq C_\varepsilon |X(p)|^{1-2\nu+\varepsilon} \tag{5}$$

where $\lambda = 1/4 - \nu^2$ $(0 \leq \lambda \leq 1/4)$.

The proof of (5) is elementary: one expresses positive sums over the spectrum in terms of matrices in $\Gamma(p)$ and finds that the inequality (5) reduces to estimating the number of integers a, b, c, d satisfying (4) and lying in a certain region. Estimating the number of such integers is straightforward. Recall that $|X(p|)$ is of the order of p^3. Combining (5) with the lower bound $(p-1)/2$ on the multiplicities, we get

$$p - 1 \leq K p^{3(1-2\nu+\varepsilon)}.$$

For p large, this is possible only if $3(1-2\nu+\varepsilon)$ is not less than one, from which we conclude

$$\lambda_1(X(p)) \geq 5/36 - \varepsilon. \tag{6}$$

This is not as good as Selberg's Theorem but is almost a geometric proof (Huxley [11] obtains a similar lower bound by a related argument).

Incidentally it was Kazhdan's hope that the growth of the injectivity radius of $X(N)$ combined with the lower bound on the multiplicity would be enough to give a lower bound for $\lambda_1(X(N))$. However, as Brooks [4] has shown the girth being $4/3 \log|X(N)|$ is just too small to push this through. That is, if the girth were any fraction larger than $4/3$ of $\log|X(N)|$, one could give a purely geometric (together with the symmetry analysis) proof that $\lambda_1(X(N)) \geq \delta > 0$.

I would now like to describe some recent results. I will begin with the joint work of Luo, Rudnick, and me which concerns overcoming the 3/16 barrier. I might add that whenever I had set out to achieve this, I came up with nothing. It was while trying to do something quite different that we stumbled upon a completely different approach to this problem, an approach which is, in fact, much more general.

Theorem 3 (Luo, Rudnick and Sarnak [20], 1994).

$$\lambda_1(X(N)) \geq \frac{171}{784} = 0.2181\ldots,$$

taking us about half way between Selberg's Theorem and his conjecture. This may be a good place to make some comments about certain results in analytic number theory. The subject has the bad reputation that people work very hard in improving some exponent which in the end very few people care about. There is some truth to that, but the main interest is not so much in improving exponents but rather in introducing new methods and techniques. Moreover, there are problems of this sort—pregnant, I like to call them—where there is some natural barrier to which existing technology has led and where going around that barrier leads to the complete resolution of a well-known problem. There are by now a good number of such problems in existence and even a few which have followed this course to complete resolution. The Theorem of Iwaniec [14] and Duke [6] is a good example. Concerning Theorem 3 we can use it, by reversing the reasoning in Selberg's Theorem 2, to give for the first time results on cancellation in Kloosterman sums on progressions. But this is not something I would like to enter into here.

The method leading to Theorem 3 works in the setting of the more general symmetric spaces $H^n = SL_n(\mathsf{R})/SO_n(\mathsf{R})$. $H^n, n \geq 2$ is the space of positive definite (symmetric) matrices of determinant equal to 1. The group $SL_n(\mathsf{R})$acts on H^n by $Y \to {}^t gYg$. The line element on H^n is $ds^2 =$ trace $(Y^{-1}dYY^{-1}dY)$, generalizing the upper half plane ($n = 2$). One can ask similar questions about the spectrum of the Laplace-Beltrami operator Δ on $SL_n(\mathsf{Z})\backslash H^n$and $\Gamma\backslash H^n$ where Γ is a congruence subgroup of $SL_n(\mathsf{Z})$. Actually, in this setting there are other invariant differential operators besides Δ and there is, so to speak, a "Ramanujan" conjecture for each such invariant-operator. In [20] an analogue of Theorem 3 is established towards these Ramanujan conjectures; for $n \geq 3$ these are the first results in this direction.

The approach to these general Ramanujan Conjectures is via L-functions. Attached to an eigenfunction as above is the standard L-function (Hecke, Godement-Jacquet [7]) and a Rankin-Selberg L-function [30, 17]. Basically, one shows that certain L-functions do not exist. In this connection let me mention a recent result of Steve Miller, a second-year student at Princeton. His method is based on analysis of L-functions associated to such eigenfunctions, with roots in a method of Stark and Odlyzko [24], giving bounds for discriminants of number fields.

Theorem 4 (A) (Miller [23], 1995). For $n \geq 2$

$$\lambda_1^{cusp}(SL_n(\mathsf{Z})\backslash H^n) > \lambda_1(H^n).$$

Here λ_1^{cusp} is the smallest eigenvalue of the Laplacian on the "cuspidal" subspace of $L^2(SL_n(\mathsf{Z})\backslash H^n)$. This cuspidal subspace L^2_{cusp} is the invariant subspace which consists of all functions on $SL_n(\mathsf{Z})\backslash H^n$ which have zero periods in all cusps (see [9]). It sounds (and it is!) technical; suffice it to say that this subspace is the basic building block in understanding the spectrum of Δ on $L^2(SL_n(\mathsf{Z})\backslash H^n)$. $\lambda_1(H^n)$ is the bottom of the L^2-spectrum of Δ on H^n. It can be computed explicitly: for $n = 2$, $\lambda_1(H^2) = 1/4$ (as one can easily see from Osserman's Theorem), so that Theorem 4(A) is a generalization of $\lambda_1^{disc}(X(1)) > 1/4$. In fact the general Ramanujan conjecture is the assertion that the spectrum of an invariant differential operator on $L^2_{cusp}(\Gamma\backslash H^n)$, Γ a congruence subgroup of $SL_n(\mathsf{Z})$, is contained in its spectrum on $L^2(H^n)$. Thus Theorem 4(A) establishes this for Δ and for $\Gamma = SL_n(\mathbb{Z})$.

Now that you have seen Theorem 4(A) you might say, "Wait a minute, maybe I can prove there are no cuspidal harmonic forms as well or perhaps even compute the cohomology of $SL_n(\mathbb{Z})$!"

Theorem 4(B) (Miller [23], 1995)[2].

$$H^p_{cusp}(SL_n(\mathbb{Z}), \mathbb{R}) = 0,\ \ 2 \le n \le 22, p \ge 0$$

By $H^p_{cusp}(\Gamma, \mathbb{R})$ we mean the space of L^2-harmonic p-forms on $\Gamma\backslash H^n$ which have zero periods along all cusps; for a precise definition see Borel, [5]. Very roughly speaking this is the cohomology of $\Gamma\backslash H^n$ not coming from the boundary.

For $n = 2$ the above is the well-known fact that $SL_2(\mathbb{Z})$ has no holomorphic cusp forms of weight two (or equivalently $SL_2(\mathbb{Z})\backslash H$ is of genus zero). For $n = 3$ the above follows from Soulé [33], who in fact computed the full integral cohomology of $SL_3(\mathbb{Z})$[3]. For $n = 4$ this is due to Ash [1], who has informed me that his methods are not feasible for $n \ge 5$ (perhaps he could do $n = 5$ if the national security depended on it!). Of course where successful his methods give much more information, including determining some very interesting cuspidal cohomology on certain congruence subgroups. Miller's proof fails miserably for n larger than 23 and it is unclear to me whether this is for a good reason (i.e., that the result is false for n, say, equal to 24) or whether the statement in Theorem 4(B) is valid for all n.

Photo courtesy of C. J. Mozzochi.

Atle Selberg

To end, let me briefly explain the idea of the proof of Theorem 3. I will do so with a model problem which will allow me to illustrate a key point. Recall the Riemann zeta function $\zeta(s)$ and its relatives, the Dirichlet L-functions, $L(s,\chi)$.

$$\zeta(s) = \sum_{n=1}^{\infty} n^{-s} = \prod_p (1 - p^{-s})^{-1}$$

$$L(s,\chi) = \sum_{n=1}^{\infty} \chi(n) n^{-s} = \prod_p (1 - \chi(p)p^{-s})^{-1}$$

Here χ is multiplicative $(\chi(mn) = \chi(m)\chi(n))$ of primitive period q $(\chi(m+nq) = \chi(m))$. If $\chi(-1) = 1$ the function

$$\xi(s,\chi) = (\pi/q)^{-s/2}\Gamma(\frac{s}{2})L(s,\chi)$$

is entire $(\chi \ne 1)$and satisfies a functional equation

$$\xi(1 - s, \bar{\chi}) = \varepsilon_\chi \xi(s,\chi).$$

with $|\varepsilon_\chi| = 1$.

The big conjecture here concerning these functions $\xi(s,\chi)$ is the Riemann Hypothesis asserting that their zeros are on $Re(s) = 1/2$. Now one reason that we are so interested in the eigenfunctions ϕ on $\Gamma\backslash H^n$ is that they give rise to L-functions, $L(s,\phi)$. In fact all L-functions are expected to be of this form (Langlands [18]).

$L(s,\phi)$ is of a form similar to $L(s,\chi)$ except that it has n-local factors for each prime p and n-"Gamma" factors in the definition of $\xi(s,\phi)$. Moreover $\xi(s,\phi)$ is entire and satisfies a functional equation. If we have an eigenfunction ϕ on $\Gamma\backslash H$ with eigenvalue $\lambda = 1/4 - r^2$, then the Gamma factor associated with $L(s,\phi)$ is $\Gamma((s-r)/2)\Gamma((s+r)/2)$. Since $\xi(s,\phi)$ is entire we see that the pole of this Gamma function forces $L(r,\phi)$ to be zero. So if $0 < \lambda < 1/4$, then $L(s,\phi)$ will have a zero at $0 < r < 1/2$; that is, $L(s,\phi)$ violates the Riemann Hypothesis. Given the slow progress on the Riemann Hypothesis, this might not seem a promising approach. However, a key observation is that such a zero of $L(s,\phi)$ is very stable. That is if $L(s,\phi)$ is expressed as a series

$$L(s,\phi) = \sum_{n=1}^{\infty} a_n n^{-s}$$

and if χ is a character as above with $\chi(-1) = 1$, then one can form

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[2] *As this article goes to press, I have learned from J. P. Serre that S. Fermigier in a preprint (1994) has obtained a similar result.*

[3] *See also R. Schwarzenberger [32].*

$$L(s, \phi \otimes \chi) = \sum_{n=1}^{\infty} a_n \chi(n) n^{-s}.$$

Moreover this new L-function has all the same properties (entire, with a functional equation) and most importantly its Gamma factor is still $\Gamma((s-r)/2)\Gamma((s+r)/2)$. It follows that $L(r, \phi \otimes \chi) = 0$ for all such χ! So we now have a whole family of terrible zeros and we have the opportunity to show that there is at least one χ for which $L(r, \phi \otimes \chi) \neq 0$. By considering averages of $L(r, \phi \otimes \chi)$ over χ this can be done at least for certain r. Theorem 3 is proven by applying this idea on the L-functions associated with ϕ.

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