## 1. Introduction

1.1. **Background.** $L$-functions and modular forms underlie much of twentieth century number theory and are connected to the practical applications of number theory in cryptography. The fundamental importance of these functions in mathematics is supported by the fact that two of the seven Clay Mathematics Million Dollar Millennium Problems [20] deal with properties of these functions, namely the Riemann Hypothesis and the Birch and Swinnerton-Dyer conjecture. The Riemann Hypothesis concerns the distribution of prime numbers. The correctness of the best algorithms for constructing large prime numbers, which are used by the public-key cryptosystems that everybody who uses the Internet relies on daily, depends on the truth of a generalized version of this 150-year-old unsolved problem [28].

The Birch and Swinnerton-Dyer conjecture deals with elliptic curves and connects the structure of the group of rational points on an elliptic curve to properties of its $L$-function. The importance of elliptic curves in number theory is illustrated in the key step of the solution by Andrew Wiles [107] of the famous 350-year-old Fermat's Last Theorem. Wiles proves that every (semistable) elliptic curve $E$ is modular, which means that the $L$-function of $E$ equals the $L$-function associated to a modular form. He then uses Ribet's proof that any counterexample to Fermat's assertion allows one to construct a semistable elliptic curve $E$ that cannot be modular.

$L$-functions also encode deep relationships between invariants of algebraic number fields, as in Dirichlet's class number formula. This formula is just one of many formulas for special values of $L$-functions. Other formulas include the Bloch-Kato conjecture and Stark's conjecture, both of which have resisted proof, and which illustrate the subtle information that is encoded by $L$-functions.

Virtually all branches of number theory and arithmetic geometry have been touched by $L$-functions and modular forms. Besides containing deep information concerning the distribution of prime numbers and the structure of elliptic curves and abelian varieties, they appear, for example in the twentieth century classification of congruent numbers, a problem first posed by Arab mathematicians one thousand years ago [58] (which asks which integers are the area of a right triangle with rational sides), and show up somewhat mysteriously in relation to the Mahler measures of certain polynomials [11].

Modular forms and $L$-functions seem aware of one another in many ways, including the Shimura correspondence, the modularity theorem of Wiles et al., the formulas of Kohnen-Zagier and Gross, symmetric powers, and the factorization of Artin $L$-functions. To give two examples, Clozel, Harris, Shepherd-Barron and Taylor recently proved much of the famous Sato-Tate conjecture, which deals with the distribution of the Fourier coefficients of classical modular forms (or equivalently, the Dirichlet coefficients of its associated $L$-function), by proving that all the associated symmetric power $L$-functions are meromorphic with functional equation, and holomorphic for $\Re(s) \geq 1$. Also, a theorem of Iwaniec, and Sarnak [54] relates the existence of so-called Landau-Siegel zeros of degree 1 Dirichlet $L$-functions to averages of degree 2 $L$-functions associated to classical modular forms.

$L$-functions arise in many ways, and these seem to connect to one another in ways that are not well understood: from modular forms, in terms of the solutions to algebraic varieties, and via representations of Galois groups. Two of the central ideas in number theory, the full Shimura-Taniyama conjecture for abelian varieties of $GL(2)$ type (now an unpublished theorem of Khare, Wintenberger, et al.), and the Langlands program, describe how these points of view are related. The Shimura-Taniyama conjecture for elliptic curves that Wiles et al. proved is the special case of the former conjecture applied to elliptic curves.

In spite of their central importance, we have only scratched the surface of these crucial and powerful functions. We plan to investigate them as thoroughly as possible.

1.2. **Overview.** We are proposing a major new project to develop theory and organize methods for understanding and computing with $L$-functions and modular forms. Broadly speaking, we plan to chart the landscape of $L$-functions and modular forms in a systematic and concrete fashion. We will develop important theory, create and improve algorithms for computing with $L$-functions and modular forms, discover properties of these functions, and test fundamental conjectures.

The proposed project will result in the creation of a vast amount of data about a wide range of modular forms and $L$-functions, which will far surpass in range and depth anything computed before in this area.

We will generate this data in a systematic fashion and organize it in a freely available online data archive, along with the actual programs that were used to generate these tables. By providing these tables and tools online, we will guarantee that the usefulness of this project will extend far beyond the circle of researchers on this FRG. Our archive will be a rich source of examples and tools for researchers working on $L$-functions and modular forms for years to come, and will allow for future updates and expansion.

These tables will be easily searchable both through online utilities and through a wiki that will organize our many tables into a theoretical framework. We also plan, at the end of our project, to produce a more refined published encyclopedia of $L$-functions and modular forms. Our section on Dissemination provides details about our data and software archive, as well of our online and published encyclopedias.

1.3. **Timeliness and origin of this proposal.** This proposal grew out of a workshop that was held July 30–Aug 3, 2007 at the American Institute of Mathematics. The purpose of the workshop was to examine the current state of theory, algorithms, and data for $L$-functions and modular forms, to define priorities in these areas, and to outline a large scale collaborative project aimed at carrying out research and implementing many of the ideas discussed during the workshop. Many of the PIs and senior personnel listed on this grant were active participants of that workshop.

Several factors make this an appropriate time for a concerted effort at organizing methods for $L$-functions and modular forms. The theory of degree 1 and 2 $L$-functions has reached a more mature form, but many of the basic formulas are still conjectural in higher degrees. Furthermore, many crucial and central conjectures have received scant, if any, testing in the case of higher degree, and limited testing in the case of degrees 1 and 2. We are now at a stage, theoretically and algorithmically, that we can tackle these issues and give them the high level of attention that they require. Algorithms for computing with both $L$-functions and various modular forms have been developed and some of them implemented, but these exist as independent entities, while the world of $L$-functions and modular forms are interrelated. These basic algorithms have reached a mature enough state so that combining them will be feasible and will allow for a systematic exploration and tabulation of degree 1, 2, 3, and 4 $L$-functions, as well as some higher degree $L$-functions. There is a recognized need to bring together these different ideas in order to investigate the world of $L$-functions and modular forms. Furthermore, computing power makes many questions that were out of reach a few years ago feasible, especially for higher degree $L$-functions.

This FRG proposal is intended to allow many of the important ideas outlined in the AIM workshop to become a reality. At the workshop, participants discussed the current state and priorities for topics including: algorithms for $L$-functions, special values of $L$-functions, classical modular forms, classical Maass forms, higher rank modular forms, Artin $L$-functions, and encyclopedia and database issues.

We formulated a list of important theoretical and algorithmic developments that are needed, outlined problems to work on and conjectures to be verified, and made a list of existing implementations and databases and a corresponding wish list. We also initiated a wiki as way to record discussions, to communicate, and to create a framework to help us begin charting the world of $L$-functions and modular forms. Besides discussions in smaller groups, several discussions involving all of the participants were held to try to organize a large scale collaborative project, and we were led to formulate many of the goals that are described in this proposal.

1.4. **Goals.** We categorize $L$-functions in the first place by degree. The degree 1 $L$-functions are the Riemann zeta function and Dirichlet $L$-functions; they are connected to classical problems in the distribution of primes. Degree 2 $L$-functions conjecturally all arise from primitive, cuspidal modular forms, both the holomorphic forms and non-holomorphic Maass forms. The former include the $L$-functions of elliptic curves, while the latter are connected to harmonic analysis on the hyperbolic plane. For degree higher than 2, examples include symmetric powers and convolutions of degree 2 $L$-functions; however, there are examples of higher degree $L$-functions that do not arise in this way, such as those associated to Siegel modular forms or to higher rank Maass forms.

We plan to carry out a systematic study, theoretically, algorithmically, and experimentally, of degree $1, 2, 3, 4$ $L$-functions and their associated modular forms, including classical modular forms, Maass forms for $GL(2)$, $GL(3)$, $GL(4)$, Siegel modular forms, and Hilbert modular forms. We will also investigate symmetric square and cube $L$-functions, Rankin-Selberg convolution $L$-functions, the Hasse-Weil $L$-functions of algebraic varieties, Artin $L$-functions associated to 3- and 4-dimensional Galois representations, and, less systematically, look at a few high degree $L$-functions associated to higher symmetric powers and higher dimensional Galois representations.

Our work will fall into four categories: theoretical, algorithmic, experimental, and data. The theoretical work will be stimulated by our goal of charting the world of $L$-functions and modular forms, and by the following needs: to extend lower degree results to higher degrees, to develop formulas that can then be applied to algorithms for computing with these functions, and to understand lower degree conjectures by working out the correct formulation of higher degree conjectures.

The work we will do in each of these categories relates to the others and we have attempted, in our list below, to identify these many connections. The graph of how these projects relate is highly connected, and we have done our best to describe it in this linear narrative. Much of our work will be related to verifying major conjectures. While we order the conjectures first, this is simply to put our work in context, and the theoretical work we do will play a crucial role.

1.4.1. *The Generalized Riemann Hypothesis.* We will test the Generalized Riemann Hypothesis for millions of $L$-functions of degrees 1,2,3,4 to large height: on the order of $10^{13}$ zeros will be computed for degree 1, $10^{10}$ zeros for degree 2, $10^{8}$ zeros for degree 3, and $10^{7}$ zeros for degree 4. We will also test GRH for several higher degree $L$-functions and for some pathological $L$-functions associated to high rank elliptic curves. In each of these cases we will not merely check GRH by counting sign changes along the critical line: we will compute and store the zeros. These tables will be useful beyond this project. See Section 2.1.

To carry out this massive test, we will need to:

– *Vastly extend existing tables of classical modular and Maass forms by two orders of magnitude:* Algorithms for fast exact linear algebra will be important, and useful beyond this project. Better algorithms for quaternion algebra-based approaches to computing modular forms will also be important in some cases. See Sections 4 and 6.

– *Find thousands of* $GL(3)$ *and* $GL(4)$ *Maass forms:* These would be the *first* examples of such Maass forms ever computed and this will require important new algorithms. The efficacy of possible approaches by researchers on this proposal will be tested at the upcoming AIM conference on *Computing Arithmetic Spectra* in March, 2008, and we will be well positioned to carry out this computation once this FRG grant begins. See Section 7.

– *Find thousands of Siegel modular forms for genus 2, and also some for genus 3, as well as thousands of Hilbert modular forms.* At present only a handful have ever been computed. We also intend to develop a computationally useful theory of modular symbols for Siegel modular forms that would allow computation of Hecke eigenvalues for forms of genus 2 with level. See Section 5.

– *Develop and implement improved algorithms for L-functions, especially for degree* $> 1$*:* This in turn will drive important theoretical developments in the study of certain important special functions, generalized incomplete integrals which are expressed as inverse Mellin transforms of products of gamma functions. See Section 10.

– *Implement ideas of Elkies and Watkins* [44] *for finding curves of high rank and relatively low conductor.* This will be done to test the GRH for the corresponding $L$-functions that have extreme behavior at the critical point. Tables of such curves will also be useful beyond this project.

1.4.2. *Conjectures for special values: Birch and Swinnerton-Dyer, Bloch-Kato, and the Böcherer conjecture.* We will investigate the Birch and Swinnerton-Dyer conjecture for several thousand modular abelian varieties, and the Bloch-Kato conjecture for several hundred modular motives and symmetric power $L$-functions of elliptic curves. See Sections 3.1 and 4.2. We will test the Böcherer conjecture for thousands of spinor zeta functions associated to Siegel modular forms. See Section 3.2. These will be another driving force behind our need to extend our tables of classical and Siegel modular forms, and also for improving $L$-function algorithms.

1.4.3. *Conjectures concerning probability questions for L-functions.* We will investigate precise predictions for the asympotic number of vanishings of the central value of the quadratic twists of an $L$-function associated to a classical modular form, predictions for the frequencies at which elliptic curves attain a given rank, conjectures for many moment problems for $L$-functions, the correlation conjectures of Montgomery [73] and Rudnick-Sarnak [95], and the Katz-Sarnak conjectures for the density of zeros of $L$-functions [56] [93]. See Section 8. This will drive some notable developments:

– *Generalize explicit versions of Waldspurger's theorem to all fundamental discriminants and all levels:* In order to efficiently compute the central values for quadratic twists in the most general case, we will need to derive explicit versions of Waldspurger's theorem, like those of Kohnen-Zagier and Gross, for *all* fundamental discriminants, positive and negative, for *all* weight $k$ classical modular forms, and *all* levels, whether prime or composite. Many special cases have been worked out explicitly, for example weight 2 and any squarefree level. Possible approaches to take for the general case were discussed in July 2007 at a workshop held at Banff. See Section 3.3.

– *Develop a discrete analogue of the Odlyzko-Schönhage algorithm:* We will combine the aforementioned formulas with fast Fourier transform techniques to efficiently evaluate certain theta series that enter in the formula and to develop an algorithm to compute the corresponding degree 2 $L$-functions at the center of the critical strip for all fundamental discriminants $|d| < X$ in $O(X^{1+\epsilon})$ time, with the implied constant depending on the underlying modular form. This would provide the discrete analogue of the Odlyzko-Schönhage algorithm [78] for central values of quadratic twists.

– *Use this algorithm to test precise conjectures of Conrey, Keating, Rubinstein and Snaith, that have arisen in the past decade for the frequency of vanishing for these special L-values:* This will involve computing billions of twists of thousands of modular forms of various weights and levels. Only the main terms in these conjectures have been tested, and only for weight 2 and squarefree level. Large datasets are required because the secondary terms are of size $O(1/\log(X))$ and thus convergence to the main term is slow. In order to better test these conjectures we will work out precise formulas for the lower terms in the conjecture for the number of vanishings. These formulas are very complicated, and numerical tests for these detailed conjectures would provide strong support in their favor. See Section 8.3.

– *Katz and Sarnak Conjectures.* We will test the Katz and Sarnak [56] conjectures for the density of zeros of $L$-functions with an eye towards incorporating lower terms that have been worked out recently by Conrey and Snaith [27] for degree 1 and 2 $L$-functions. We will also cover new ground and test their conjectures for higher degree families of $L$-functions. This will stimulate work in finding new expressions for the lower terms for those more difficult problems.

– *Asymptotics for the number of elliptic curves of given rank:* Another important theoretical development will be improvement of predictions for the asymptotics for the number of elliptic curves of a given rank, a difficult and controversial problem. See [5] for a recent Bulletin paper about this controversy, in which a massive amount data of Stein and Watkins about ranks of elliptic curves was studied (by an undergraduate). The model of Conrey, Rubinstein, Snaith and Watkins [26] has some hope of giving this prediction. To further test these predictions, we intend to enumerate more elliptic curves and to develop and implement improved algorithms of Elkies and Watkins [44] using Heegner points. See Section 8.3.

– *How large can ranks be?* The probabilistic models for $L$-functions, together with the Birch and Swinnerton-Dyer conjecture suggest that ranks of elliptic curves over $\mathbb{Q}$ may be unbounded (see [48] for a discussion of extreme values of $L$-functions, which could reasonably be carried over to extreme values of the arguments of $L$-functions and so to bounds for multiplicities of zeros). At present lhe largest recorded rank is due to Elkies who has exhibited an elliptic curve of rank 28 [39]. We will attempt to extend that record further and in doing so look for ideas for how to construct an infinite sequence of curves with ever larger ranks.

– *Moments and value distributions:* We will test many other fundamental conjectures, for example arising from recent work of Conrey, Farmer, Keating, Rubinstein, and Snaith that finally managed, after 100 years, to identify the moments and value distribution of $L$-functions [57] [21]. See also [36] [114]. These have, to date, received only modest testing, and we would like to understand better the remainder term in these problems. The tools for computing $L$-functions and the tables of modular forms that we will generate will be crucial for carrying out this work. See Section 8.1.

– *Theoretically develop the moments for some collections of degree 3 and 4 L-functions:* Doing so will also provide us with a way to test in degree 3 and 4 the machinery that has been developed for working out detailed moment conjectures, and this in turn will stimulate the development of new theoretical tools. We will also take advantage of the work that we will be doing to generate thousands of examples of such modular forms, and algorithms for computing their $L$-functions, to test the new moment formulas we derive.

1.4.4. *Other conjectures.* The tables of modular forms and of eigenvalues associated to Maass forms that we produce will be used to test two other important conjectures.

– *Analytic properties of Hasse-Weil L-functions.* One of the central predictions of the Langlands program is that the $L$-functions associated to algebraic varieties, so-called Hasse-Weil zeta-functions, are equal to automorphic $L$-functions. Booker and Sarnak have developed a numerical test for this conjecture which we intend to check for many examples. See Section 11.

– *The Selberg eigenvalue conjecture.* Selberg conjectured that there are no eigenvalues of the hyperbolic Laplacian with $0 < \lambda < \frac{1}{4}$ for congruence subgroups of $PSL(2, \mathbb{Z})$. The extensive tables of eigenvalues that we will generate to compute Maass forms will be useful for testing the Selberg conjecture as well. See Section 6.

1.4.5. *Software and data.* The free open source computer software that we create to generate the data will be tremendously useful for anyone requiring their own extensive datasets of these special $L$-values. They will all be made free available with complete source code in a form that is ready to use, extend, improve, or change. When possible, they will also be made part of Sage [101] and of the $L$-functions Calculator, so that the source code will be peer reviewed.

We will develop methods for storing representations of modular forms, $L$-functions, and related objects in a way that they can be immediately reconstructed and further data can be computed about them. This is critical both to make it easy to extend data, and to use parallel computation in certain situations.

1.4.6. *From data to conjectures.* There are many cases where a systematic collection of important data has led to major conjectures; the fundamental nature of our methods and data will make it a treasure chest for many years to come. For example, the Birch and Swinnerton-Dyer conjecture was discovered empirically from tables of elliptic curves and their $L$-functions. The prime number theorem was conjectured by Gauss at the age of 15 by preparing large tables of primes and comparing prime counts in intervals against various functions. We know from the hand-written notes of Riemann, which are preserved in the Göttingen library, that he formulated the Riemann Hypothesis after carrying out a difficult computation by hand of the first several zeros of the Riemann zeta function and, in so doing, discovered what is now called the Riemann-Siegel formula for efficiently computing the zeta function.

In our own work, Stein computed invariants of modular abelian varieties, which led him to make a still-unproved modularity conjecture about Shafarevich-Tate groups, and to make a conjecture that refines the results of Mazur's Eisenstein ideal paper, which Emerton later proved [45]. Sarnak would not have had the courage to make his conjectures with Katz concerning the role, as suggested from function field analogues, of the classical compact groups in predicting the behavior of families of $L$-functions without Rubinstein's data to confirm their initial predictions. These conjectures ended up playing a central role in our understanding of the vertical distribution of the zeros of $L$-functions and inspired a decade of work that succeeded in describing the full asymptotics of the moments of the Riemann zeta function and of other $L$-functions [56] [57] [27] [21] [22].

This broad project requires input from many experts on computational number theory, in particular algorithms for $L$-functions, and their associated classical modular forms, Maass forms, and more general modular forms, as well as theoreticians who have a broad view of these subjects. To this end, we have assembled a world class team of experts in these areas. The PIs on our proposal are: Andrew Booker, Brian Conrey, Noam Elkies, Michael Rubinstein, Peter Sarnak, and William Stein. The senior personnel are: Winfried Kohnen, Fernando Rodriguez-Villegas, Nathan Ryan, Fredrik Strömberg, Mark Watkins, and David Yuen.

1.5. **Organizing our project.** In order to achieve our goals, we will organize our research team around several problems, assign people to smaller groups and designate group leaders. The choice of our groups is based on the skills and planned work of each member of our research team. Group leaders will report to the entire research team twice per year: at annual meetings of the researchers on this proposal, and at our larger annual workshops. See Section 16 for more details.

Both the logistical management and mathematical management of the project will be handled by three of our coPIs- Conrey, Rubinstein, and Stein. They will plan the annual meeting of the dozen researchers on this FRG proposal, our larger yearly workshops, and schools for graduate students. They will make decisions regarding the hiring of postdocs. They will also manage the research aspect, by leading about half of the smaller groups and by communicating with the other group leaders to make sure that work is progressing as planned. Organization and management are further detailed in Sections 16, 17 on Collaboration and on Management.

We will meet regularly in several different ways to guarantee that this work proceeds in an organized fashion:

1. *In larger workshops.* Each of these workshops will be preceded by a one week graduate student school that will involve lectures for half of the day and carrying out student research projects for the remainder of the day. The students will then stay on for the workshops and will be involved in research. The workshops will therefore involve 36 participants: the 12 researchers on this proposal, 12 additional colleagues, and 12 graduate students.

The workshops will take place during the summers of 2008, 2009, 2010 at the American Institute of Mathematics, to be organized by Michael Rubinstein and William Stein in consultation with Brian Conrey and Peter Sarnak. The purpose of these workshops will be to organize and carry out work in the direction of achieving our stated goals, to collaborate on research, to report on work done over the course of the previous year, and to receive feedback on our group project and research from colleagues. We plan to have fewer talks and spend the afternoons carrying out research in smaller groups and holding discussions.

2. *Large yearly meeting of our entire research group.* The dozen people listed on this proposal will meet in winter in order to collaborate on research for this project, to exchange ideas, to evaluate our progress and discuss our next steps. The first two such meetings will be hosted by Peter Sarnak, and will take place in Princeton in winter 2009, and 2010. We have yet to chose a location for winter 2011, but Conrey is willing to host it at AIM.

3. *Program at the Institute for Advanced Studies.* Sarnak is co-organizing with Bombieri a year-long program at the IAS in 2009–2010 that will play a role in collaboration on this project. This will be an ideal opportunity for Sarnak to collaborate with Booker, Conrey, and Rubinstein who will spend three months in Princeton, along with one of their postdocs. Another postdoc, to be supervised by Sarnak, will spend the 2009–2010 year in Princeton. One of our annual meetings of the FRG researchers will be held in Princeton during that semester, as will a research workshop of 20 participants that will examine our progress on finding and computing with Maass forms in the classical case and for $GL(3)$ and $GL(4)$.

4. *Two additional research workshops.*

In spring 2009, a mini-workshop consisting of about 15 participants will be held to examine computational issues surrounding certain high degree Artin $L$-functions. This workshop will be organized by Rodriguez-Villegas. Another workshop, to be held in late fall 2009, will take place in Princeton, as described above.

5. *Smaller group gatherings.* We will meet several times per year in smaller collaborative groups of two–seven people.

The FRG will also allow us to involve two–three postdocs and three graduate students per year, and also involve undergraduate students through an REU and through the hiring of two undergraduates per year to help carry out experiments. We describe this in more detail in the section devoted to collaboration and in our management plan.

Next we provide details for the goals that were mentioned in this section.

## 2. Computing zeros of $L$-functions

2.1. **Verifying the Generalized Riemann Hypothesis and computing zeros.** We propose to test the Generalized Riemann Hypothesis in a systematic way for many zeros of many $L$-functions, vastly extending existing tests of GRH and charting new territory for $L$-functions of degree greater than 2.

We will progress systematically through tens of millions of Dirichlet $L$-functions, millions of $L$-functions associated to newforms and Maass forms, millions of symmetric square and cube $L$-functions of degrees 3 and 4, thousands of Maass form $L$-functions for $GL(3)$ and $GL(4)$, and thousands of spinor zeta functions of degree 4 associated to Siegel-Hecke eigenforms forms of genus 2. We would like to emphasize that in the case of degrees 3 and 4, only a handful of spinor zeta functions have ever been computed, and not a single Maass form has ever been computed. Our goal of verifying GRH will lead to significant new algorithms for finding these; see Sections 5 and 7.

Our work will extend by many orders of magnitude the existing verifications of GRH and provide important data. Rather than checking GRH for an $L$-function by just counting sign changes along the critical line, zeros will be also be computed. These will then also be used to test various other conjectures regarding the distribution of the zeros such as the correlation conjectures of Montgomery [73] and Rudnick-Sarnak [95] or density conjectures of Katz and Sarnak [56]. Extensive tables of the zeros collected and the software used will be made freely available to people outside this project through an online data and software archive.

Below we describe in more detail the planned tests of GRH for each degree.

-*Degree 1.* We will test GRH and compute zeros for each of the following primitive Dirichlet $L$-functions of conductor $q$: the first $10^{12}$ zeros for each $L$-function with $q < 10$, the first $10^9$ zeros for $q < 30$, the first 30 million zeros for $q < 100$, the first 100,000 zeros for $q < 2000$, and the first 5000 zeros for $q < 10000$. The family of quadratic twists will be given special attention, with the first 10000 zeros computed for all $L(s, \chi_d)$ with $|d| < 10^6$. All zeros found will be archived except our large data sets for $q < 10$, where only about 1 TB of data will be archived.

-*Degree 2.* GRH will be tested for the millions of degree 2 $L$-functions associated to the classical modular forms and Maass forms we will tabulate. See Sections 4 and 6. The number of zeros for which we will test GRH will depend on the level, as the complexity of computing a degree 2 $L$-function grows with the square root of the level. Typically we will compute thousands of zeros for each such $L$-function, but many more zeros (millions) for a few examples with smaller level.

-*Degrees 3 and 4.* For several hundred thousand of the classical modular forms above, we will test GRH for the first few hundred zeros of the corresponding symmetric square and symmetric cube $L$-functions of degrees 3 and 4, and also compute hundreds of zeros of several hundred thousand Rankin-Selberg $L$-functions of degree 4. We will do the same for the thousands of Siegel modular forms of genus 2, Hilbert modular forms of real quadratic fields, and $GL(3)$ and $GL(4)$ Maass forms that we will compute. See Sections 5 and 7.

-*Other high degree examples.* We plan to look at a few higher degree Artin $L$-functions, see Section 9, as well as a few higher symmetric powers of degrees 5–8.

-*Examples displaying extreme behavior.* It would also be worthwhile to test the Riemann Hypothesis for $L$-functions where pathological behavior can easily be identified, for example near the real axis for the $L$-functions of elliptic curves of high rank. To this end we will find many elliptic curves of large rank relative to their conductor (the run-time is proportional to the square root of the conductor). In order to generate lots of curves of high rank relative to the conductor we will apply ideas of Elkies and Watkins [44] that seem to have a good chance of catching most if not all of the best such curves of rank at least up to 8 or so.

## 3. Special Values of $L$-functions

3.1. **Bloch-Kato and Beĭlinson Conjectures.** The Bloch-Kato [10] and Beĭlinson [4] conjectures are generalisations of the class number formula for algebraic number fields, the Birch and Swinnerton-Dyer [7] conjecture for elliptic curves, or even Euler's [46] famous relation $\zeta(2) = \sum \frac{1}{n^2} = \pi^2/6$ that is familiar to many calculus students. The full force of these conjectures quickly leads to the language of motives and $K$-theory [88], but we can be more direct in some specific cases, which open up several avenues for new theoretical and algorithmic developments and extensive enumeration of data.

*The Bloch-Kato conjecture for modular motives* If $f \in S_k(\Gamma_1(N))$ is a classical cuspidal modular newform, then a construction of Scholl associates to $f$ a modular motive $M_f$. Dummigan, Stein, and Watkins (see [41]) did explicit computations with such motives in order to verify – assuming Beĭlinson's conjecture – visibility of Shafarevich-Tate groups of such $M_f$ in several cases. This entailed developing theoretical and computational techniques to compute partial information about each of the quantities appearing in the Bloch-Kato conjecture for $M_f$ for many particular $f$. We intend to further refine these algorithms in order to systematically compute this partial information about the Bloch-Kato quantities for thousands of modular motives $M_f$, thus creating a table similar to the one in Cremona's elliptic curves book and online tables [30, 29] but for modular motives.

3.1.1. *Special values of symmetric power L-functions of elliptic curves.* Let $E$ be an elliptic curve over $\mathbb{Q}$ with complex multiplication. Bloch [8] investigated the relationship between $K_2(E)$ and the special value $L(E,2)$. This was given a conjectural generalisation (later modified due to calculations in [9]) by Beĭlinson [88, §5] to the case of projective varieties over number fields, with the most recent computations being by Dokchitser, de Jeu, and Zagier [38] in the case of hyperelliptic curves. Our goal is to study similar questions for symmetric powers of curves, which are a natural first step beyond the above.

The critical values of symmetric powers of elliptic curves $L$-functions have been considered by Dummigan and Watkins [42]. Here one can be fairly explicit about the entries in the conjectural formula for the special values, and give conjectural constructions in some cases. For instance, for even symmetric powers $n = 2l$ with $l$ odd we expect that

$$\frac{L(\mathrm{Sym}^n E, l+1)}{\Omega(n, l+1)} = \frac{\big(\prod_p c_p(l+1)\big)\#Ш(l+1)}{\#H^0(l)\cdot\#H^0\big(l+1\big)},$$

while for odd $n = 2l+1$ we expect that the central value satisfies

$$\frac{L(\mathrm{Sym}^n E, l+1)}{\Omega(n, l+1)} = \frac{\big(\prod_p c_p(l+1)\big)\#Ш(l+1)}{\#H^0(l+1)^2}$$

if this value is nonzero, with a more general formula relating $L^{(r)}(\mathrm{Sym}^n E, l+1)$ and an $r$-dimensional regulator in general. The periods $\Omega(n, l+1)$ are suitable products of the real and imaginary periods, the $c_p(l+1)$ are generalised Tamagawa numbers, the $H^0$ groups are generalisations of torsion, and $Ш(l+1)$ is a generalised Shafarevich-Tate group. The periods are easily computable, the Tamagawa numbers can be computed in many cases and well-approximated in others, the $H^0$ groups can be bounded and occasionally computed, and Ш is still mysterious. A new investigation would be to look at $\mathrm{Sym}^n E$ of odd parity for which a nontorsion point in the Griffiths groups can be constructed (this task is carried out with $n = 3$ for various $E$ by Buhler, Schoen and Top [17]), and compare the central $L'$-value to the height of this point.

Again one can delve into $K$-theory by considering $L(\mathrm{Sym}^n E, m)$ for other integers $m$; for $L(\mathrm{Sym}^2 E, 3)$ this was already considered nearly 20 years ago via Kronecker series by Mestre and Schappacher [70], but the correspondence with the results of Goncharov and Levin on elliptic polylogarithms was not included prior to the work of Zagier and Gangl [112]. Only one example (a curve of conductor 37) is computed there, and the authors note that getting the right conditions for integrality might be difficult in general. We would aim to imitate their calculation for all curves up to (say) conductor 100; with our $L$-function technology it should also be possible to look at some examples involving polylogarithms of higher degree.

3.2. **Böcherer's conjecture.** Böcherer has made an interesting conjecture concerning central critical values of odd quadratic character twists of spinor zeta functions $Z_F(s)$ attached to cuspidal Siegel-Hecke eigenforms $F$ of genus 2 and even weight $k$. The participants in this project will test this conjecture numerically for thousands such $Z_F(s)$ and also attempt to prove the conjecture.

Böcherer conjectured that $Z_F(k-1,\chi_D) = C_F|D|^{1-k}\left(\sum_{\{T>0|\mathrm{discr}T=D\}/\mathrm{SL}_2(\mathbb{Z})}\frac{a(T)}{\epsilon(T)}\right)^2$ where $T$ in the sum ranges over positive definite half integral (2,2)- matrices of discriminant $D$, $a(T)$ is the Fourier coefficient of $F$ corresponding to $T$, and $\epsilon(T)$ is the order of the unit group of $T$. Böcherer proved [11] his conjecture in the case where $F$ is the Maass lift of a Hecke eigenform f of weight $2k-2$ w.r.t. $\mathrm{SL}_2(\mathbb{Z})$. Later on, Böcherer and Schulze-Pillot [12] proved an identity similar to the above in the case of levels, where now $F$ is the Yoshida lift of an elliptic cusp form. Also in [12], a similar formula in the case where $F$ is a Siegel- or Klingen- Eisenstein series was shown to be true.

The proof in all the above cases makes essential use of the fact that the spinor zeta function in question is a product of "known" $L$-series. Nothing regarding Böcherer's conjecture seems to be known in the case where $F$ is a "true" Siegel modular form, i.e., is not a lift of an automorphic form on $GL(2)$ (and so $Z_F(s)$ is not expected to split). In [59], Kohnen and Kuss provided some initial numerical support in support of Böcherer's conjecture, verifying it to five decimal places for $D = -3, -4, -7, -8$, for six Siegel-Hecke eigenforms $F$.

The algorithms that will be developed for $L$-functions and Siegel modular forms will allow for much more thorough testing of this conjecture for thousands of $F$, more values of $D$ and to higher precision. There is also some hope of proving the conjecture. A student of Kohnen, Kilian Kilger, is attempting to prove the conjecture on average. The ideas in this direction come down to first "linearize" the conjecture suitably and then work with certain Poincare series. The next step would be to put in the theory of Hecke operators, and granting a multiplicity one theorem in genus 2 (announced some years ago by Weselmann, but so far never published), to try and prove the conjecture itself.

3.3. **Computing central critical values.** We plan to develop formulas for the central values of the $L$-functions of quadratic twists of classical modular forms, and to combine these with the fast Fourier transform to evaluate billions of such values for thousands of modular forms of weight 2,4, and 6 in order to test conjectures concerning the number of vanishings of these $L$-values, moment conjectures, and the Bloch-Kato conjecture. We will combine these formulas with fast Fourier transform techniques to efficiently evaluate certain theta series that enter and to develop an algorithm to compute the corresponding degree 2 $L$-functions at the center of the critical strip for all fundamental discriminants $|d| < X$ in $O(X^{1+\epsilon})$ time, with the implied constant depending on the underlying modular form.

Computing of the central value of the $L$-function of quadratic twists of an elliptic curve can be efficiently accomplished by using explicit versions of Waldspurger's theorem, like those of Kohnen-Zagier and Gross (see [90] for a survey). In essence, this method converts an a priori analytic question (computing a special value of an $L$-function) into an algebraic one (computing the Fourier coefficient of a modular form). The catch is, of course, that the needed half-integer weight eigenform can be hard to compute.

The method to handle half-integer or integer weight eigenforms used in [80], [67], [81], [82], [91] relies on the arithmetic of positive definite quaternion algebras. It has the feature that the modular forms are expressed as linear combinations of theta series, which is quite convenient for computing their Fourier coefficients (see for example [92] for one use of this method). The main ideas for the algorithms (going back to Pizer [83]) are fairly well understood and basically follow the theoretical development of the subject. The details, however, can be tricky. Some cases were recently implemented as GP-PARI routines by A. Pacetti and F. Rodriguez-Villegas, mainly for weight 2 (and correspondingly weight 3/2) and square-free level. Other instances appear in the above cited papers. Though a priori the quaternion algebra method only works when the central value is non-zero, introducing auxiliary twists allows us to treat the general case [67].

We propose to make a concerted effort to organize the various partially developed cases into one working unit. The main efforts would go into ironing out the necessary details to deal with: a) forms of weight $> 2$, b) arbitrary composite level, c) vanishing central value. We should also emphasize that in some instances we do not yet have an explicit proven version of Waldspurger's formula (for example, if the level is $p^2$ for a prime $p$). Hence, the proposed work is not purely computational; it involves working out the details on the theoretical side too.

3.4. **Heegner points.** Given an elliptic curve $E/\mathbb{Q}$ of rank 0 and conductor $N$, the method of Heegner points can be used to try to enumerate imaginary quadratic twists of rank 3. This was first implemented by Elkies for the congruent number curve, and has now been generalised by Watkins. The method uses the Gross-Zagier formula which gives that $L'(E_d, 1)L(E, 1)$ is proportional to the height of a canonically-defined Heegner point. Thus $L'(E_d, 1) = 0$ if and only if the Heegner point is torsion, which can be checked numerically via a complex approximation on the torus $E/\mathbb{C}$. We expect that almost all $d$ with $L'(E_d, 1) = 0$ will yield twists of rank 3, though (if lucky) we might get some of higher rank. The algorithm involves enumerating the class group of $\mathbb{Q}(\sqrt{-d})$, for each form finding a suitable $\Gamma_0(N)$-equivalent form, and then computing to $O(\log(Nd))$ digits the modular parametrisation $\sum_n \frac{a_n}{n} e^{2\pi i n\tau}$ for the associated $\tau$ in the upper half-plane.

## 4. Classical Modular Forms

Certain modular forms called *newforms* encode an extraordinary amount of deep arithmetic information. For example, newforms play a central role in Wiles' proof of Fermat's Last Theorem, and much research toward generalizing his methods to attack other Diophantine equations (see [33, 69, 19, 98]) depends on computations and tables of modular forms. The modularity theorem of Wiles et al. also forges a deep link between modular forms and the Birch and Swinnerton-Dyer conjecture.

A major conjecture of Serre, which was proved during the last 3 years (by unpublished work of Dieulefait, Khare, Kisin, and Wintenberger), asserts that all 2-dimensional odd representations of the Galois group of $\mathbb{Q}$ arise from classical modular forms via a construction of Deligne. Modular forms can also be used to construct optimal expander graphs that arise in information theory, construct optimal codes, are important in counting points on varieties over finite fields, and arise as generating functions.

Below we describe several major computations of modular forms that go beyond anything that has been computed before. In each case we discuss the computation, how it could be carried out, and some specific ways in which it would impact theoretical work. The projects described below would move forward the theory of modular forms, and improve fundamental algorithms in linear algebra and parallel computation that are likely to have an impact outside of number theory.

4.1. **Weight 2 newforms.** *Compute extensive data about all weight 2 newforms on* $\Gamma_0(N)$ *for all levels* $N \leq 10{,}000$, *and all weight* 2 *newforms with quadratic character and level* $N \leq 1,000$. We intend to do this mainly using modular symbols. The complexity is dominated by exact linear algebra, computation of sparse kernels, dense characteristic polynomials and decompositions of modules as direct sums of simple modules (rational canonical form). Basic linear algebra research of C. Pernet, who will be a postdoc at University of Washington 2008–2010 funded by NSF grant #0713225, is critical to this problem. To each newform there corresponds a modular abelian variety, and extending much previous work by Stein, we hope to compute as much as possible about the Birch and Swinnerton-Dyer invariants attached to these abelian varieties, then make conjectures and prove theorems based on the resulting tables; a nice example of this is [45], in which Emerton proved Stein's refined Eisenstein conjecture, which was made based on such a table. We would also construct networks that encode all possible congruences between all forms in this data, in order to understand better how Ribet's level raising and level lowering theorems can be refined.

*Investigating a generalization of Elkies' theorem.* Elkies proved in [43] that if $f$ is a newform of weight 2 with rational Fourier coefficients, then infinitely many prime-indexed Fourier coefficients $a_p(f)$ are 0. Heuristics and new data computed by Stein led him [62] to conjecture that an analogous statement is true when $f$ is a weight 2 newform whose Fourier coefficients generate a quadratic field. In particular, for such a form Stein conjectures that $a_p(f) \in \mathbb{Z}$ for infinitely many primes $p$. One could imagine a proof similar to Elkies' proof, with $X(1)$ replaced by the 2-dimensional moduli space of RM abelian surfaces of the appropriate kind, and CM points replaced by various modular curves lying on this surface. Such a proof would require new arithmetic techniques and results, which may be suggested by explicit computation of these modular curves.

*Euler systems and congruences.* Associate a Kolyvagin Euler system to the rational elliptic curve $E$. Then for each Kolyvagin class $c_{\ell,p^n} \in H^1(K, E[p^n])$ as summarized in [61] Ribet's level raising theorem [89] implies there is a corresponding congruence between the newform attached to $E$ and a weight 2 newform of some higher level. We will to find examples of these other forms and study their arithmetic properties as they relate to the Mordell-Weil group of $E$, especially in the context of Mazur's notion of *visibility* of Shafarevich-Tate groups [31, 1].

*Enumerate all weight 2 newforms with rational Fourier coefficients on* $\Gamma_0(N)$ *for* $N \leq 234{,}446$. John Cremona has computed all such newforms for $N \leq 130{,}000$ as the result of many years' work and optimized software running on a large cluster [30, 29]. This enumeration appears to have ground to a halt, and we would like to push the calculation further using new techniques, e.g., by combining the massive search of Stein-Watkins [102, 5] with better algorithms for computing Hecke operators that involve quaternion algebras and ternary quadratic forms ([80] and unpublished work of G. Tornaria), and sparse linear algebra techniques. The first known elliptic curve of rank 4 has composite conductor 234,446, so this calculation would also provide a complete enumeration of all elliptic curves up to the first of rank 4 (incidentally, Stein and two undergraduates enumerated all prime-conductor curves up to level 234,446, and none had rank 4).

*Compute all weight 2 newforms whose coefficients generate a field of degree* 2 *on* $\Gamma_0(N)$ *for* $N \leq 50{,}000$. Such forms correspond to 2-dimensional abelian varieties. The resulting data would have an impact on research on modular abelian surfaces, quaternionic multiplication, and other areas.

All the above data will be a critical input to projects discussed elsewhere in this proposal.

4.2. **Weight 4 newforms.** *Compute all weight 4 newforms on* $\Gamma_0(N)$ *with level* $N \leq 1000$. This computation would involve the same techniques as the weight 2 calculation, except that the linear algebra would involve much larger rational numbers, albeit on matrices with fewer rows and columns. In some cases all weight 4 forms could alternatively be obtained as product of weight 2 forms (or their images under Hecke operators).

To each weight 4 newform there is a corresponding motive (a higher weight generalization of an abelian variety), and conjectures about that motive due to Beilinson, Bloch, and Kato that generalize the Birch and Swinnerton-Dyer conjectures. Extending the work started in [41], we hope to compute extensive data about each such motive, enumerate the results in tables, formulate conjectures, compute algebraic parts of $L$-values, and more.

Chad Schoen is interested in using the above information about algebraic parts of $L$-functions and their twists to find examples of weight 4 newforms with nontrivial vanishing central critical value, since then a major challenge is to verify Beilinson's conjecture for the corresponding Chow group.

4.3. **Newforms of large weight.** *Compute all newforms on* $\Gamma_0(N)$ *with level* $N \leq 100$ *and weight* $k \leq 100$. To do this computation we let $R(N) = \oplus_{k\geq 0} M_k(\Gamma_0(N))$ be the ring of modular forms of level $N$, and use modular symbols to compute $M_k(\Gamma_0(N))$ for the first few $k$, then use this low-weight data to find explicit algebra generators for $R(N)$ for each $N \leq 100$. We then generate a $q$-expansion basis to high precision for the spaces of cusp forms for each weight $k \leq 100$, compute a Hecke operator on it, and use it to write down newforms. We will also carry out a similar computation for $\Gamma_1(N)$ for $N \leq 30$, say. The resulting data will be useful for investigating questions about images of Galois representations, properties of $p$-adic modular forms, etc., all of which require data about forms of high weight. The work of Bill Hart (who would visit Stein for one quarter supported by this FRG) and David Harvey (see `http://www.flintlib.org/`) on very fast polynomial arithmetic, as well as Clement Pernet's work [40] on linear algebra, will all be crucial for making this computation possible.

## 5. Siegel and Hilbert modular forms

We plan to compute thousands of examples of Siegel modular forms of genus 2, and also to develop a computationally useful theory of modular symbols for Siegel modular forms that would allow computation of Hecke eigenvalues for forms of genus 2 with level. We will also tabulate a few examples with genus 3.

The Fourier expansion of a Siegel modular form $F$ of genus $n$ is indexed on half-integral, positive semi-definite $n \times n$ matrices: $F(Z) = \sum_{T \geq 0,\ \text{h.i.}} a(T) e^{2\pi i \mathrm{Tr}(NZ)}$ where $Z$ is an element of the Siegel upper half space. A noteworthy aspect is that the coefficients can be indexed [51] by isometry classes of lattices $\Lambda$ (rank $n$ when cusp form) associated to a quadratic form $Q$ whose matrix representation on $\Lambda$ is $T$. The work of Skoruppa [99] allows one to compute all coefficients up to discriminant $D$ in time $O(|D|^2)$; a refinement by Kohnen-Kuss in [59] to $O(|D|^{3/2})$ allowed the authors to compute coefficients up to discriminant 3000000, and thereby compute eigenvalues $\lambda_p$ for $p < 1000$. This has only been done up to weight 36. In current work by Ryan-Yuen [97] a way to multiply expansions to avoid this redundancy is being developed. Expanded computations of the generators of the ring of modular forms of degree 2 coupled with this new way of multiplying expansions has already allowed [97] to easily get to weight 50 and computing thousands of coefficients is realistic.

Nathan Ryan, in joint with Paul Gunnells, is developing techniques to compute Hecke eigenvalues for Siegel modular forms on finite index subgroups of $Sp_4(\mathbb{Z})$. The techniques are analogous to the theory of modular symbols and arise from the cohomology of arithmetic groups. This would allow one to compute local factors of the spinor and standard $L$-functions of Siegel modular forms with level.

For genus 3, the dimensions of the spaces of cusp forms and the generators are known [110], although relations among the generators not known. Only a few computations have been done in this genus; Miyawaki computed some coefficients of weight 12 and 14 cusp forms, leading to his conjecture (which Ikeda proved) on Miyawaki lifts [72] [53]. The weight 12 cusp form is a Miyawaki lift and so its $L$-function is known, but the weight 14 cusp form is still a mystery. Further computations have not been done in genus 3. The bottleneck here is likely both getting the coefficients of the generators and multiplying the generators to fill up the space at a particular weight and getting enough coefficients to find Euler factors. In addition to this method, we can also use the "restriction technique" [84] [85] [86] to compute the Fourier coefficients; judging by what was done in genus 4 [86], it should be possible to use the restriction technique to compute thousands of coefficients for genus 3 forms through weight 24 and likely beyond.

In consultation with Lassina Dembele, we plan to fully implement his quaternion-algebra based algorithm for computing Hilbert modular forms and use it to compute Fourier expansions to high precision of several thousand Hilbert modular forms. These will be the first large tables of Hilbert modular forms; moreover, there are degree 4 $L$-functions associated to Hilbert modular forms, which we will study using the other methods outlined in this proposal.

## 6. Classical Maass forms

All known examples of $L$-functions of degree 2 are associated to automorphic forms on the group $\Gamma_0(N)$ with a character $\chi$ modulo $N$. There are two such kinds of automorphic forms: holomorphic (described in Section 4) and non-holomorphic. The non-holomorphic classical modular forms are also called *Maass forms* and are square integrable eigenfunctions of the hyperbolic Laplacian on quotients of the upper half plane by $\Gamma_0(N)$.

One goal of this project is to develop, improve, and implement algorithms for finding Maass forms. We propose to vastly extend tables of eigenvalues and corresponding Fourier expansions for Maass forms, computing the first 100 eigenvalues for all $N < 1000$, and the first 10,000 eigenvalues for all $N < 10$, and also compute many of the corresponding Fourier coefficients of the Masss form, millions for smaller $N$ and smaller eigenvalues, thousands in other cases.

The only known explicit examples of Maass forms are realized as lifts of Hecke Grössencharacters and were introduced already by Maass in [66]. All other examples are computed numerically. Maass forms are found using two main kinds of algorithms: the "automorphy" algorithm in the version by Hejhal [52] generalized by Strömberg [103] to subgroups of the modular group and arbitrary weight and multiplier system (character), and the trace formula algorithm of Booker-Strömbergsson [15]. Another kind of automorphy algorithm has also been developed by Farmer-Lemurell [47]. Certification of eigenvalues, i.e., proving that the numerically computed eigenvalue is correct up to a certain number of digits can be done, e.g., by the methods of Booker-Strömbergsson-Venkatesh [16] or by the trace formula methods of Booker-Strömbergsson [15]. It would be very useful to automate these methods and extend them to general groups and multipliers.

Another goal is to improve existing (or write new) algorithms to produce large sets of "non-congruence" data. For example to compute the first 100 eigenvalues for all subgroups (up to conjugacy) of the modular group of index $\leq 15$ (to actually find all conjugacy classes is nontrivial). Another example would be to compute all eigenvalues up to some fixed height (e.g., $\lambda - \frac{1}{4} = 400$) of the weight $k$ Laplacian $\Delta_k = \Delta - iyk\frac{\partial}{\partial x}$ on the modular group (or subgroups) for a sequence of small weights say down to $k = 10^{-50}$. In [104] such eigenvalue data for $k$ down to $10^{-9}$ were presented.

This data will feed into projects described elsewhere in this proposal, and in addition we will also test:

1. *The Selberg eigenvalue conjecture.* Selberg conjectured that there are no eigenvalues $0 < \lambda < \frac{1}{4}$ for congruence subgroups of $PSL(2,\mathbb{Z})$. Such eigenvalues, if they exist, are called *exceptional.* In contrast, one can show that for any $\epsilon > 0$ there is a (non-congruence) subgroup which has an eigenvalue $\lambda$ with $0 < \lambda < \epsilon$. (Exceptional eigenvalues have been found numerically by Strömberg.) In this project we

will verify the conjecture by trace formula methods of Booker-Strömbergsson [15] as well as make more extensive numerical studies of exceptional eigenvalues on non-congruence subgroups. Such studies might provide insights into the arithmetic/geometric nature of the conjecture.

2. *Statistical questions about Maass waveforms.* The statistical properties of the eigenvalue spacings as well as the value distribution of the individual eigenfunctions are important objects in "Quantum Chaos", i.e., the study of quantum mechanical systems corresponding to dynamical systems which are classically chaotic. One would like to find "indicators" on the quantum mechanical level which tells whether the corresponding classical system is chaotic or not. The main conjecture in arithmetic quantum chaos (i.e., quantum chaos on hyperbolic surfaces $\mathcal{M} = \Gamma\backslash\mathcal{H}$ corresponding to an arithmetic Fuchsian group $\Gamma$) tells us that the value distribution should approach a Gaussian distribution as $\lambda \to \infty$, and the eigenvalue spacing distribution should tend to Poissonian. Data provided by the proposed project would help in establishing numerical evidence of these conjectures. (Up to date the only numerical verification has been done for $PSL(2,\mathbb{Z})$ in force and to a smaller extent on, e.g., $\Gamma_0(5)$.) The Fourier coefficients of an individual Maass waveform are conjectured to have a "Sato-Tate" or semicircle distribution. While Taylor has proved this conjecture for a class of elliptic curves, those methods probably do not generalize to Maass waveforms. Limited numerical evidence supports the conjecture and we will do more extensive calculations, including the case of integer weight $k$ and eta multiplier [105].

## 7. Maass forms for $GL(3)$ and $GL(4)$

A major thrust of the project will be to develop algorithms for computing in higher rank, including Maass forms on $GL(3)$ and $GL(4)$. Thus far only special examples, such as symmetric power lifts from $GL(2)$ and forms associated to Galois representations, have been constructed; the computation of "generic" forms (which make up asymptotically 100% of all Maass forms, in a natural sense) has remained elusive. We believe that processor speeds and memory sizes have reached a point where it is feasible to tackle these problems, and there are at least three teams trying viable approaches to computing forms on $GL(3)$. Two coPIs on this proposal, Booker and Strömberg, are leading two of these teams, while another is being led by David Farmer at AIM. Farmer has told us that he will contribute his data to our project, so we have access to essentially all the sources for these important functions.

The efficacy of the various approaches will be tested at the AIM conference on *Computing Arithmetic Spectra* in March, 2008. By the time that work on the proposed FRG project commences, we should have a good idea of the best way to compute examples of Maass forms on $GL(3)$ of full conductor. Simultaneous work on concrete $GL(3)$ analytic theory will allow us to extend those methods to forms of higher conductor, at which point we will be able to compute many examples for the proposed database. Moreover, the experience that we gain from computations on $GL(3)$, as well as having a large database of forms on $GL(2)$, will aid in the development of algorithms for computing on $GL(4)$. In this way, our theoretical results and data will build up naturally from low rank examples to higher rank.

## 8. Testing probability models for $L$-functions

8.1. **Moments of $L$-functions.** Recent work has provided conjectures, inspired from random matrix theory but based on number theory heuristics, for the moments and value distribution of $L$-functions, with some modest numerical support for these conjectures [21] [22] [23]. See also [36] [114]. Many explicit examples are given, such as the moments of a fixed $L$-function along the critical line, or the moments of quadratic Dirichlet $L$-functions at the critical point. The modest numerical verification of these conjectures suggest that the full asymptotics have been identified. We will test these conjectures more extensively over very large ranges with an eye towards carrying out a statistical analysis of the error term and identifying its properties.

We will develop and test explicit conjectures for some collections of degree 3 and 4 $L$-functions. Two examples are the moments of the central values of spinor zeta functions of degree 4 associated to Siegel modular forms, and of the $L$-functions associate to $GL(3)$ Maass forms. This will take advantage of the work we will be doing to generate thousands of examples of such modular forms, and the new algorithms for computing their $L$-functions. Those examples will also provide a way to test in degree 3 and 4 the machinery that has been developed for working out detailed moment conjectures.

8.2. **Statistics of the zeros.** We will carry out extensive testing of the correlation conjectures of Montgomery [73] and Rudnick-Sarnak [95] for millions of $L$-functions. This has only been tested extensively in the case of the Riemann zeta function [77], and for a handful of degree 1 and 2 $L$-functions [92]. We will also test as the Katz-Sarnak [56] conjectures concerning the density of zeros of $L$-functions for families of $L$-functions with a view to incorporating new refined conjectures of Conrey and Snaith [27] that provide, for degree 1 and 2 $L$-functions, the lower order terms. We will also cover new ground and test their conjectures for higher degree families of $L$-functions. This will lead to new expressions for the lower terms for these more difficult problems.

8.3. **Predictions for the number of vanishings and of ranks.** Conrey, Keating, Rubinstein, and Snaith have conjectured the asymptotic number of times the central critical values of quadratic twists of an elliptic curve $L$-function vanish [23] [24] [25]. The prediction is that the number of quadratic twists by fundamental discriminants $|d| < X$ of even positive rank of an elliptic curve $E$ over $Q$ is asymptotically $c_E X^{3/4} \log(X)^{\beta_E}$. The power on the logarithm $\beta_E$ depends on the underlying curve $E$ in a predictable way [35]. The constant $c_E$ depends in three main ways on the elliptic curve $E$, two of which are fully understood [24]. Even though the numerics in this problem take a very long time to settle down, this prediction, up to the $c_E$, has been tested for thousands of elliptic curves with $|d| < 10^8$ and strong evidence has emerged in its favor [24].

The next goals to achieve would be:

1. to fully identify the constant $c_E$. The difficulty comes from some extra subtle arithmetic information that seems to be related to Delaunay's heuristics for Tate-Shafarevich groups [34]. This was noticed and tested by Rubinstein on his huge dataset of quadratic twists, but certain "exceptional primes" do not fit with this approach. Recently, Patricia Quattrini, a student of Rodriguez-Villegas, has explained the role played by Cohen-Lenstra heuristics for class groups in understanding the contribution of the exceptional primes [87], and we are now in a position to revisit the question of $c_E$.

2. to incorporate lower order terms into the number of vanishings. When the prediction was initially made, we only had knowledge of the relevant moment up to leading order. However, we now have knowledge of the full asymptotics of the moments [21] and it will be possible to improve the prediction to include at least the next two lower order terms.

3. to test these predictions numerically, initially using the existing large dataset of quadratic twists. Because of the slow rate of convergence, it would be useful to extend this data, at least for several elliptic curves, to include many twists of size $10^{12}$. We will accomplish this by developing algorithms for cental values as described in Section 3.3.

4. to work out the corresponding theory, in the same level of detail and carry out thorough numerical testing of our predictions, for the related problem of quadratic twists the the $L$-function of a weight 4 modular form. A similar conjecture was made in [23] for the number of vanishings that are expected in this case, but with an $X^{1/4}$ rather than an $X^{3/4}$ in the asymptotics. This has been tested for a single weight 4 modular form, but to limited height. We propose to carry out a more extensive testing of the conjecture for many modular forms of weight 4, with $X$ of size $10^9$. We will also investigate the constant factor in the asymptotics. Here, the arithmetic properties of the Chow group will enter, rather than the Tate-Shafarevich group. For weight at least 6 the theory predicts only finitely many vanishings and it would be worthwile to check that as well.

5. to identify the asymptotic number of times that specific ranks are attained. The difficulty here comes from the fact that, for non-zero rank, the Birch and Swinnerton-Dyer conjecture involves a non-trivial regulator. It appears that this interacts with the order of the Tate-Shafarevich group so that the product of the two tends to be large [26]. Understanding this interaction is crucial for understanding the discretisation of the corresponding $L$-value and for making a predictions for specific rank.

## 9. Artin $L$-functions

Together with the Hasse-Weil $L$-functions described in Section 11, the Artin $L$-functions associated to complex Galois representations lie at the heart of the Langlands program. In particular, it is expected that Artin $L$-functions may be found among the $L$-functions attached to modular or automorphic forms.

For our project, we will be especially interested in representations of dimensions 3 and 4, for which relatively little is known compared to lower dimensions. Work by Ash et al. [2] has been done to try to identify the $L$-functions associated to some examples of 3-dimensional representations. In another direction, Booker [13] has shown how to test directly the holomorphy of Artin $L$-functions as well as the associated Riemann hypothesis, in some cases. We plan to carry out both of these techniques in a systematic way for dimensions 3 and 4, greatly expanding the amount of data available in each case.

9.1. **High rank examples.** We also plan to study certain more extreme higher degree $L$-functions, and will hold a two week workshop in 2009 to test the limits of what is computationally feasible. Examples of the $L$-functions we have in mind are those associated to representations of Weyl groups of types $E_6, E_7$ and $E_8$, say. For some of these, specific methods are available that do not exist in general, making them more amenable to computations. Concretely, suppose $\alpha = (\alpha_1, \ldots, \alpha_r), \beta = (\beta_1, \ldots, \beta_r)$ are a parameters of a hypergeometric differential equation, all of whose solutions are algebraic. (Such equations have been completely classified, starting with the classical work of Schwartz and ending with that of Beukers and Heckmann [6], and are known explicitly.) For concreteness let us say that the corresponding monodromy group is the Weyl group of $E_8$ (a fairly large finite group of order about $7 \times 10^8$).

For example, the generating series $u(\lambda) := \sum_{n \geq 0} \frac{(30n)!n!}{(15n)!(10n)!(6n)!}\lambda^n$ of the numbers considered by Chebyshev satisfies a (hypergeometric) differential equation of this type. The parameters are $\alpha = (1/30, 7/30, 11/30, 13/30, 17/30, 19/30, 23/30, 29/30)$ and $\beta = (0, 1/5, 1/3, 2/5, 1/2, 3/5, 2/3, 4/5)$.

Associated to such a differential equation there is a one-parameter family of Artin representations of dimension 8. The trace of Frobenius on these representation can be computed in terms of corresponding *hypergeometric sums* (with the same parameters $\alpha, \beta$) discussed in [55]. In turn, the hypergeometric sums could be computed using the $p$-adic gamma function by means of the Gross–Koblitz formula (this was the method used in [18] for example).

We should emphasize that it would be essentially impossible to compute the trace of Frobenius on these representations directly. Though $u(\lambda)$ does indeed satisfy an algebraic equation, and its specialization at a given $\lambda \in \mathbb{Q}$ gives rise to a corresponding Galois extension of $\mathbb{Q}$ with Galois group the Weyl group of $E_8$ (generically), its degree is 483, 840! (The smallest degree possible in examples of the kind we are discussing is 27 for certain families of type $E_6$.)

Admittedly, much remains to be done in the study of these representations (what is the exact value of their conductor, for example?) and it is not entirely clear precisely how feasible the required computations are. However, we strongly believe that these ideas are worth pursuing and at the very least carrying them through will produce significant insights on the computational aspects of higher degree $L$-functions.

## 10. Analytic $L$-function algorithms

We propose to develop, improve, and implement highly efficient algorithms for working with $L$-functions, starting primarily from two existing approaches. These methods rely on knowledge of the $L$-function in question, specifically its functional equation and its Dirichlet coefficients, and close collaboration with personnel working on various modular forms will be required. Emphasis will be placed on developing *rigorous* methods and implementations whenever possible.

The broadest approach to evaluating $L$-functions relies on an approximate functional equation, such as the Riemann-Siegel formula in the case of the Riemann zeta function [78] or, more generally, smoothed versions such as that of Lavrik [64] [92]. A second approach is to use the explicit formula relating sums over the zeros of the $L$-function to sums over the Dirichlet coefficients of its logarithmic derivative. Methods using the explicit formula can be applied, for example, to isolating zeros of an $L$-function and to bounding discriminants of number fields.

10.1. **Smoothed approximated functional equations.** The smoothed approximate functional equation expresses an $L$-function as a convergent series involving its Dirichlet coefficients and functional equation. If $L(s)$ is an $L$-function with Dirichlet series $L(s) = \sum_{n=1}^{\infty} b(n)/n^s$, absolutely convergent if $\Re s > 1$, normalized so that the critical line is $\Re(s) = 1/2$, functional equation $\Lambda(s) := Q^s \prod_{j=1}^{a} \Gamma(\kappa_j s + \lambda_j) L(s) = \omega \overline{\Lambda(1-\overline{s})}$, and no poles, then the smoothed approximate functional equation, convergent for all $s$, is:

$$\Lambda(s)g(s) = Q^s \sum_{n=1}^{\infty} \frac{b(n)}{n^s} f_1(s,n) + \omega Q^{1-s} \sum_{n=1}^{\infty} \frac{\overline{b(n)}}{n^{1-s}} f_2(1-s,n)$$

where $g(s)$ is a function chosen to control the exponentially small size of the $\Gamma$ factors and the functions $f_1(s,n)$ and $f_2(1-s,n)$ are inverse Mellin transforms of $\prod_{j=1}^{a} \Gamma(\kappa_j(z+s)+\lambda_j) z^{-1} g(s+z)(Q/n)^z$ and of $\prod_{j=1}^{a} \Gamma(\kappa_j(z+1-s)+\overline{\lambda_j}) z^{-1} g(s-z)(Q/n)^z$.

When there is just one gamma factor, a natural choice for $g(s)$ leads to the inverse Mellin transforms being expressed in terms of the incomplete gamma function. This occurs in case of Dirichlet $L$-functions and the $L$-function of a holomorphic newform. To evaluate the incomplete gamma function, several approaches can be taken: series, asymptotic and uniform asymptotic expansions, continued fractions, and numerical integration [92]. These different approaches work well under different circumstances. These methods have been implemented in Rubinstein's $L$-function package [94], but some important theoretical work needs to be done even in the classical case of a single gamma factor. Explicit truncation bounds on the the various continued fractions and on the uniform asymptotics have not yet been worked out for all values of the parameters. This means that rigorous computations of an $L$-function cannot rely on these methods. We propose to address this by carrying out a detailed study of these truncation bounds.

In the case of multiple gamma factors, one is led to incomplete integrals of certain multi-dimensional integrals that are more challenging to evaluate. There is a series expansion and asymptotic expansion that can be used [37], but these are only useful when we wish to compute $L(s)$ near the real axis. We plan to apply Guthmann's uniform asymptotics [50] for these incomplete integrals. That would provide a moderately efficient approach to evaluating these $L$-functions, but to limited precision. It would also be useful to have an analogous theory of continued fractions for these more complicated incomplete integrals.

Another approach developed by Rubinstein [92] can be used when one or more than one gamma functions is involved, and proceeds through numerical integration of the inverse Mellin transform. This avoids many of the complications associated with the corresponding incomplete integrals. Poisson summation can be used to show that the numerical integration can be carried out quite efficiently, and can be reduced to evaluating a finite Dirichlet series at equally and moderately spaced points.

A crude form of this appoach has been implemented [94] and tested on a symmetric square $L$-function of degree 3, and a spinor zeta function of degree 4. In terms of efficiency it competes with the best algorithms, giving the same runtime as the Riemann-Siegel formula (in the case of a degree 1 $L$-function), however with a constant that is several thousand times larger. This approach would be dramatically improved if one were to combine it with the fast Fourier transform ideas of Odlyzko-Schonhage [78], and we plan to do so. We would like to emphasize that this would help reduce the constant in the run-time of our method by several orders of magnitude, even for a single evaluation, and make it practical to explore higher degree $L$-functions and the various conjectures described in this proposal.

10.2. **Explicit formula.** Weil's "explicit formula" is an identity of distributions relating the zeros of an $L$-function to the Dirichlet coefficients of its logarithmic derivative. The explicit formula has been exploited by Omar [79] and Booker [13] to obtain numerical estimates for low-lying zeros. Booker's method, which assumes GRH, gives a canonical way of choosing the "best" test function for localizing near $1/2 + it_0$.

This approach has been used to evaluate low-lying zeros of a few examples of $L$-functions of very large conductor. One goal of the project is to expand this significantly to a systematic, "black-box" approach useful for any $L$-function, and use it to verify conjectures of Katz and Sarnak concerning the density of zeros for some families of $L$-functions [56]. See Section 8.

Besides estimating zeros, the explicit formula can be used to bound various other quantities, such as the conductor of an $L$-function or its Langlands parameters at $\infty$. For example, this idea was used by Odlyzko [76] to obtain bounds on the discriminants of number fields, in one of the earliest computational uses of the explicit formula. Later, Miller [71] used the same idea to provide a lower bound for the Laplacian eigenvalue of a Maass form on $SL(3,\mathbb{Z})\backslash SL(3,\mathbb{R})$, thus proving an analogue of Selberg's eigenvalue conjecture in that case. Both of these cases used test functions of a very special form. Another goal of the project is to apply Booker's method of arbitrary test functions to these and other cases, in the hope that much sharper bounds can be obtained.

## 11. Analyticity of Hasse-Weil $L$-functions

The tables of modular forms that we produce will be useful for numerically verifying one of the central predictions of the Langlands program, namely that the $L$-functions associated to algebraic varieties, so-called Hasse-Weil zeta-functions, are equal to automorphic $L$-functions. In particular, they should have analytic continuation beyond their half-plane of convergence and satisfy a functional equation. Such a connection, if true, would have important implications for arithmetic geometry. One spectacular example of this is the theorem of Wiles et al. (formerly the Shimura-Taniyama conjecture). More generally, Khare, Kisin, Winteberger, and others have recently proved Serre's conjecture, which by work of Ribet implies that that every abelian variety over $\mathbb{Q}$ of $GL(2)$ type arises from a modular form.

Despite this success story, evidence for the general prediction remains scant. One goal of the project is to test systematically many more examples than are known at present, including examples of higher degree over $\mathbb{Q}$, and some over number fields. There are at least two approaches to follow. The first, which provides the most direct evidence, is to search for examples of both modular forms and varieties with matching data, e.g., for which the first 100 Dirichlet coefficients agree. For instance, Cremona [32] has computed matching examples of modular forms on hyperbolic 3-space and elliptic curves over imaginary quadratic fields. In some cases, information of this type may be used to *prove* special cases of the predicted correspondence.

A second approach, due to Sarnak, was used by Booker [14] to test some examples of curves of genus 2 defined over $\mathbb{Q}$. This approach works by verifying cancellation in smooth sums of the Dirichlet coefficients of the associated $L$-function; such cancellation is implied by the analytic continuation of the $L$-function, and may be predicted precisely from the conjectured functional equation. Although the evidence obtained by this method is much less direct (in particular, it does not identify the associated modular form), it is generally easier to implement and is feasible for varieties of much larger conductor.

We propose to use both of these strategies to compute many more examples than are currently known. For the approach followed by Cremona, this will entail developing algorithms for computing new kinds of modular forms. For Booker's approach, we will need to develop explicit versions of Sarnak's test for each particular case.

## 12. The need for a collaborative effort

Throughout this proposal we have desribed the many facets of $L$-functions and modular forms, how they interrelate and the many different arithmetic, analytic and algebraic aspects that we will investigate from several different points of view- theoretical, algorithmic, and experimental. The goals of this proposal are by their very nature collaborative. They involve two very rich subjects, $L$-functions and modular forms. We plan to study many kinds of $L$-functions associated to classical modular forms, classical Maass forms, Siegel modular forms, Maass forms for $GL(3)$ and $GL(4)$, Hilbert modular forms, Artin $L$-functions, Hasse-Weil $L$-functions of algebraic varieties, as well as symmetric power $L$-functions and Rankin-Selberg convolutions. We will test conjectures that require skill with the analytic, arithmetic, and algebraic aspects of $L$-functions and modular forms: the Generalized Riemann Hypothesis, various probability questions concerning $L$-functions, the Bloch-Kato conjectures, analytic properties of Hasse-Weil $L$-functions, the Artin conjecture, and the Selberg eigenvalue conjecture. This project is designed for a group effort and no single individual or collection of isolated individuals is capable of carrying out the broad and important work that we describe.

## 13. Timeline

In the Introduction we summarized many of the theoretical, algorithmic, experimental, and data goals that we plan to achieve, describing these around the major conjectures we will investigate, and explaining how these various goals relate to one another. Here we highlight some of the major developments year by year. The timing of our workshops, schools for graduate students, meetings, and the hiring of postdocs are described elsewhere in this proposal.

13.1. **Year 1 (summer 2008-spring 2009).**

- Implement improved algorithms for higher degree $L$-functions and begin our theoretical study of the related special functions as described in 10. At the same time, we plan to begin testing the GRH for degree 1 and 2 $L$-functions (Section 2.1). In the latter case we will begin with existing tables of modular forms and Maass forms, as well as Cremona's tables of elliptic curves, and then move on to our massive new tables, which will also be worked on during our second year.
- Start to carry out the massive computation of classical modular forms and Maass forms described in Sections 4 and 6. In many cases it will be necessary to do further theoretical and implementation work on modular and Maass forms algorithms, in order to make the computation sufficiently fast, and to prepare for further work in year 2.
- Work on developing explicit versions of Waldspurger's theorem for any weight, discriminant, and arbitrary composite level. We will begin to implement our ideas and generate extensive tables of these formulas for thousands of modular forms of weights 2,4 and 6 (Section 3.3). These will then be used in the second year.
- Begin work on predicting the moments of $L$-functions of $GL(3)$ Maass forms and spinor zeta functions of degree 4, so that as production of the relevant modular forms increases, we will be ready to test our formulas. We will also develop a computationally useful theory of modular symbols for Siegel modular forms (Section 5) that will allow computation of Hecke eigenvalues for forms of genus 2 with level.
- Work on developing predictions for asympotics of rank 3 curves. Develop and improve our Heegner point algorithms for computing derivatives of $L$-functions at the critical point and use them to tabulate these values, so that predictions about ranks can be tested in the second year (Sections 3.4 and 8). We will develop and implement algorithms for finding elliptic curves of large rank, with and without restrictions on the conductor. Some of these large rank curves will be used in year 2 to test the GRH in extreme cases (Section 2.1).
- Obtain the constant factor and also lower terms in the vanishings conjecture (Section 8.3). We will test these in the second year.

13.2. **Year 2 (summer 2009-spring 2010).**

- Finish the large-scale computation of a database of classical modular forms and Maass forms (Sections 4 and 6).
- Begin systematic computation of invariants of the associated abelian varieties and motives. In many cases this will require significant additional theoretical and programming work, which will be important in year 3.
- Tests of GRH for degree 2 will be based on our new massive table of classical modular forms. Computation of forms on $GL(3)$ (Section 7) will be well under way, as will the generation of large tables of Siegel and Hilbert modular forms (Section 5), and we will test conjectures on these, including the GRH. We will also begin to test the GRH for the symmetric square, cube, and degree 4 Rankin-Selberg $L$-functions in year 2.
- Develop the basic theory needed for computations on $GL(4)$.
- Work out lower-order terms in the vanishings conjecture. Develop predictions for asymptotics of rank $> 3$ curves. (Section 8).
- Develop and implement fast Fourier transform methods for computing central values of quadratic twists based on our explicit versions of the Waldspurger formulas, and use them to create massive tables of these values (Section 3.3). Use these to test vanishings conjecture, including the constant and the lower terms.
- Make use of data created in the first year and in year 2 to begin testing important conjectures: the automorphy of Hasse-Weil zeta functions (Section 11); Böcherer's conjecture (Section 3.2);

Montgomery, Rudnick-Sarnak, Katz-Sarnak, and Moment conjectures (Section 8); the Selberg eigenvalue conjecture (Section 6).

13.3. **Year 3 (summer 2010-spring 2011).** We will finish all computations described in Section 4, and carry out a systematic analysis of our data. We will complete our tests of the various conjectures that were started in year 2. We will look for surprising connections between the arithmetic data (e.g., BSD, Bloch-Kato) associated to classical modular forms, and the large amount of analytic $L$-functions data computed. (Stein has already noticed some preliminary surprising connections like this in data about convergence in the Sato-Tate conjecture.) We will push the algorithms that we have developed to study questions for some degree $> 4$ $L$-functions, such as the GRH and also the Bloch-Kato conjectures for higher symmetric powers (Section 3.1.1).

## 14. Dissemination

The participants in these projects will publish papers in research journals, and present lectures on their work at seminars, workshops, and conferences. Papers submitted to journals will be uploaded to the the arXiv and also made available through the $L$-function and Modular Forms Wiki (see below) and the AIM preprint server.

The proposed project will result in the creation of a vast amount of data about a wide range of modular forms and $L$-functions, which will far surpass in range and depth anything computed before in this area. Making this data and the tools used to create that data easily and freely available is a major challenge. To this end, an online archive for data and software will be created with front end applications to extract data from our archive. The basic structure of this database is described below.

In order to provide a theoretical context and framework for the large amounts of data and many useful algorithms that will be freely shared through the online online data and software archive, a wiki will be developed. The wiki will organize existing knowledge concerning $L$-functions and modular forms and provide another way to access relevant data, algorithms, and software based on context, and also provide the background needed to carry out research in this area.

The database, software archive, and wiki were begun during the AIM workshop in August, 2007. The wiki already has more than 20 research-level survey articles on automorphic forms and $L$-functions. We anticipate making the wiki public in 2008. The plans for data and software storage were begun during the AIM workshop and a technical committee was formed to complete the design. As of mid-September 2007, a first draft of the data uploading guidelines is available, and several data sets have been produced. The next stage is to complete the first draft of the computer code that handles the uploading and organization of the data. We expect that to lead to a revision of the data guidelines, and we hope to have the second round of guidelines and code for uploading ready when the wiki is made public.

At the end of this three year project, a published Encyclopedia of $L$-functions and Modular Forms will be produced, based on the wiki and archive of data and software.

14.1. **Data and Software.** In this section we discuss software, our database and standards for reproducibility, and how we plan to share our work.

14.1.1. *Software.* Our project proposal involves many calculations of modular forms and $L$-functions, and related quantities, on a scale that rivals anything similar done before. Substantial improvements to existing algorithms will be made, and new algorithms will be developed and implemented. Thus this project will lead to the creation of a huge amount of new software.

The software we produce will be released under either the GPL or (modified) BSD license. The best algorithms will be incorporated into two freely available packages that two of the coPIs are responsible for: Sage and the $L$-function Calculator. The code used to actually construct and manipulate the tables we produce will be made available for others to review, modify, and extend.

14.1.2. *Data.* As part of the spirited meeting that was held at AIM in August 2007, strategies for addressing the challenge of archiving and sharing our data were discussed. Some of the key decisions that came out of that discussion were the following:

1. Data we compute will be made freely available under an open copyright license. This will make it crystal clear to anyone considering using the data precisely how they are allowed to use it, and will make it transparent to those who submit data to our project exactly how that data can and cannot be used.

2. Data will be hosted on a central server that will be mirrored initially at five other sites. Locations for server and the mirrors are: Washington, Waterloo, Harvard, AIM, Princeton, and Bristol.

3. The data will be comprised of two tiers. The first tier will consist of a core subset of our data that will grow to about 1 TB and will be synchronized automatically over the Internet. Since only the difference between the server and mirrors will need to be updated, that can easily be accomplished. For example, typical transfer rates between universities within North America are 500 KB to 1 MB per second, and about 500 KB per second between Europe and North America.

The second tier will consist of our full data and will not be mirrored in an automatic fashion, as the bandwidth requirements would be too large. Instead, full data sets will be made available just on the server of the coPI responsible for that aspect of the project (for example, full tables of zeros of $L$-functions on Rubinstein's server). At the end of the three year project, copies of the full data set will be sent by courier to all the coPIs so that the complete data will be available at all servers.

5. There will be a simple web interface to the data, so that anybody can easily download it in raw file format. A preliminary interface has already been written by Robert Bradshaw at the AIM workshop mentioned above and is ready to be deployed. There will also be a separate much more sophisticated web interface to the same data, meant for multiple levels of interactive queries.

6. Data will be accessible to both Sage and the $L$-function Calculator either over the web or by downloading a local database file for much quicker access to particular data. Note that Sage itself provides a web-based notebook interface, so this will provide a third very sophisticated way to work with the data in our database, for those who are comfortable with programming. These tools will allow anyone in the world access to the most up-to-date tables and to carry out their own investigations.

7. The servers will also host publicly available copies of Sage and the $L$-function Calculator. This will allow users to compute directly on the server and extract useful results from the database without having to download large data sets if only a relatively easy computation is required.

14.2. **Wiki.** A wiki is a collaborative website which can be directly edited in a web browser by anyone with edit access to it. Perhaps the best-known wiki is Wikipedia. We will use the wiki to make it much easier for our widely distributed group to collaborate on the projects described elsewhere in this proposal. There will be wiki pages for each project, and as researchers make progress they will regularly update the corresponding pages. This will greatly simplify and speed up the sharing of results, enhancing both the collaboration and the dissemination of results.

A Creative Commons license will be applied so that contributors retain copyright on their material but license it so that it can be broadly used, for example, in the published encyclopedia described below.

14.3. **Published Encyclopedia.** We intend to gather together, systematically organize, and make rigorous the best content on the wiki, which we will publish with a traditional publisher. Because the wiki will be based on the Creative Commons license, the same will apply to our encyclopedia.

In the third year of this FRG project, a committee consisting of the six coPIs on this FRG will be assembled, as described in the management plan, to oversee the publication of the encyclopedia. This committee will decide on the content of the encyclopedia, and will solicit from amongst the main contributors to the wiki people who will be responsible for putting together material for the different sections and chapters based on the wiki. The committee will also be responsible for making all decisions regarding publication.

## 15. Prior Results

All twelve investigators have made significant contributions to these areas of mathematics. Citations are given throughout this proposal, in the references, and in the biographical statements.

## 16. Collaboration and Training

16.1. **Collaborative projects.** The model for organizing our collaborative work described in this proposal will be based in part on the model that was used to run the AIM workshop that inspired it. At the workshop, participants broke off into smaller groups to discuss the current state and organize priorities on topics that are similar to the topics of this proposal. Each group was assigned a group leader whose job was to lead discussions, organize work to be followed up on after the workshop, and to record decisions. We plan to do the same with this proposed project.

We will organize our project around several topics, assign people to groups and designate group leaders. The choice of our groups that we describe below is natural, based on the planned work of each member of our research team. Group leaders will be responsible for summarizing group activities during the year at our annual workshops, for organizing work within a group, for recruiting help from outside the group, and for making sure that group decisions are recorded and not forgotten. Conrey, Rubinstein, and Stein, who will manage the proposal (and also lead some of the groups), will first turn to group leaders to ask questions related to managing the overall direction of this project.

Groups will meet throughout the year. We will hold annual meetings of the twelve researchers listed on this proposal in order to allow for exchanges, collaboration, and feedback from the various groups. We will hold larger annual workshops. The purpose of these gatherings will be to help maintain momentum on this project, to delegate work to be done during the year, and to report on findings made since the time of the previous workshop. We will use the $L$-function and Modular Forms Wiki that was initiated at our workshop as a way to organize the topics of this project, to designate work, to record decisions, and to provide a larger vision to the project. An FRG mailing list will be used to allow the researchers on the proposal to keep each other informed.

We have decided to organize the dozen researchers on this proposal in the following way. Group leaders are designated in italics.

Verifying the GRH and computing zeros: Booker, *Rubinstein*, Watkins
Special Values of $L$-functions: Conrey, *Elkies*, Kohnen, Rodriguez-Villegas, Rubinstein, *Stein*, Watkins
Classical Modular Forms: Elkies, *Stein*, Watkins
Siegel and Hilbert modular forms: Kohnen, *Ryan*, Stein, Yuen
Classical Maass forms: Booker, Sarnak, *Strömberg*
Maass forms for $GL(3)$ and $GL(4)$: *Booker*, Sarnak, Strömberg
$L$-function Algorithms: Booker, Elkies, Rodriguez-Villegas, *Rubinstein*, Watkins
Artin $L$-functions: *Booker*, Rodriguez-Villegas, Sarnak
Analyticity of Hasse-Weil $L$-functions: Booker, Rubinstein, *Sarnak*
Probability models for $L$-functions: *Conrey*, Elkies, Rubinstein, Sarnak, Watkins

We would like to point out that there will be close collaboration between these groups. For example, the GRH group will carry out research on $L$-functions associated to various automorphic forms and will require assistance from several of the groups working on those, and also use results of the $L$-function algorithms group.

The large group on special values will be broken down further into conjectures for special values (Stein) and algorithms for special values (Elkies), but because these two will work hand in hand, it makes more sense to keep them as a larger group.

16.2. **Undergraduate research activities.** Conrey and Rubinstein will organize an REU at the American Institute of Mathematics in the summer of 2010. David Farmer who is at AIM will participate in running the REU and supervising undergrads. By then, many of our algorithms will have been implemented and easily available for use, and we will have collected extensive tables of values and zeros of $L$-functions. We plan to involve undergraduates in several projects to make use of these algorithms and to study the data.

Both Conrey and Rubinstein have significant experience involving undergrad students in research. Besides having run successful REUs in the past, they have supervised undergraduates in research on an individual basis. One such research project ended up forming part of the paper [25], and another was published by the student in Communications in Mathematical Physics [113].

Rubinstein and Stein each plan to hire one undergraduate, part time, during the year to help carry out experiments, and to build web based interfaces to our software and database.

16.3. **Graduate students.** A graduate student school and two graduate student workshops will precede or follow three of our research workshops (AIM summers 2008, 2009, 2010). We are proposing to bring a dozen graduate students to these research workshops. For the school, to be held in 2008, students will arrive one week earlier to learn about algorithms for $L$-functions and modular forms. The idea of the school will be to train graduate students in state of the art algorithms so that they can become involved in research in this area. The school will be organized by Stein and Rubinstein, who will give the lectures along with Booker, and Strömberg.

During the summers of 2009 and 2010, the graduate students will stay on for an additional week and, under the supervision of the PIs and senior personnel on this proposal, implement ideas developed during the workshop.

By combining the graduate school and workshops with our research workshops we will be able to take advantage of the gathering of colleagues, and involve students in cutting edge research.

We are proposing to support three graduate students per year on this FRG who will carry out work related to this project with the coPIs, though more students will be involved through the school and workshops.

16.4. **Postdoctoral training.** This FRG will help to fund a total of eight postdoctoral positions. These postdocs will work closely with the PIs to carry out research on this proposal.

Three of the postdocs, one per year, will carry out research on this FRG under the supervision of Brian Conrey and Michael Rubinstein. One of these postdocs will spend the fall of 2009 in Princeton with Booker, Conrey, and Rubinstein and they will collaborate on research with Peter Sarnak who is co-organizing a year long program on analytic number theory.

A fourth postdoc will spend the 2009–2010 academic year in Princeton collaborating with Peter Sarnak on research for this project and attending his program at the IAS.

Four other postdocs (one in year 1, two in year 2, and one in year 3) will be supervised by William Stein at the University of Washington. These will be half-time research postdocs.

We have identified, several highly qualified candidates for several of our postdoctoral positions. These include: Lassina Dembele, a Darmon student, who wrote his thesis on computing Hilbert modular forms on real quadratic fields which are associated to certain degree 4 $L$-functions and has created tables of such forms in collaboration with Fred Diamond; David Harvey, a Mazur student at Harvard, who is graduating this year, and is a world expert in applying Fourier transform methods to fast power series arithmetic (this is critical for computing modular forms to high precision); Craig Citro, a Hida student at UCLA, will graduate in 2 years and is skilled in modular symbols, $p$-adic modular forms, and Galois representations; Robert Bradshaw, a Stein student at UW, will graduate in 2 years, and is an expert in exact linear algebra, $p$-adic Coleman integration, and is a *phenomenal programmer* who would help tremendously in making the data produced by our project widely available and easy to use; Duc Khiem Huynh, a student of Nina Snaith, will graduate in 2 years and is interested in working on moments of spinor zeta functions; Soroosh Yazdani, a Ribet student currently at McMaster, is a postdoc working with Romyar Sharifi who is an expert on arithmetic of elliptic and modular curves; Salman Butt, a student of Rodriguez-Villegas at UT Austin who is working on computational aspects of elliptic curves and their $L$-functions and is familiar with super computing.

16.5. **Workshops and conferences.** We plan a variety of workshops and conferences.

1. *Annual summer workshops.*

Our proposal has six coPIs and six additional senior personnel. But in order to carry out a project of the scope we envision we will need to rely on the assistance of many more people. Our main venues for enlisting this help will be at the annual focused workshops. It is critical that, in addition to our team, we also seek the community's knowledge and advice and count on the collective expertise and energy of researchers all over the world to accomplish our goals. We will invite to the three workshops, to name a few possibilities: Jennifer Beineke, Alina Cojocaru, Chantal David, Daniel Bump, Henri Cohen, John Cremona, Henri Darmon, Tim Dokchitser, Dorian Goldfeld, Benedict Gross, Dennis Hejhal, Henryk Iwaniec, Barry Mazur, Hugh Montgomery, Andrew Odlyzko, Nicole Raulf, Holly Rosson, Nina Snaith, Andreas Strömbergsson, Meera Thillainatesan, Gonzalo Tornaria, Akshay Venkatesh, Helena Verrill, and Ulrike Vorhauer as well as enthusiastic and energetic postdocs and graduate students. In this way, through a concerted effort, we will make significant and timely progress on many fronts.

These workshops will be held at AIM in the summers of 2008, 2009, 2010. They will be held back to back each year with our week long school for 12 graduate students who will stay on for the research workshop. Thus they will involve 36 participants: the 12 researchers on this proposal, 12 additional colleagues, and 12 graduate students.

The purpose of these workshops will be to organize and carry out work in the direction of achieving our stated goals, to collaborate on research, to report on work done over the course of the previous year, and to receive feedback on our group project and research from colleagues. We plan to have fewer talks and spend the afternoons carrying out research in smaller groups and holding discussions.

These workshops will be organized by Rubinstein and Stein, though key decisions regarding the focus of the workshops, participants, and speakers, will be made in consultation with Conrey and Sarnak. They will involve 36 participants: the 12 researchers on this proposal, 12 additional colleagues, and 12 graduate students.

2. *Large yearly meeting our entire research group.* The dozen people listed on this proposal will meet in winter in order to collaborate on research for this project, to exchange ideas, to evaluate our progress and discuss our next steps. The first two such meetings will be hosted by Peter Sarnak, and will take place in Princeton in winter 2009, and 2010. We have yet to chose a location for winter 2011, but Conrey is willing to host it at AIM.

3. *Program at the Institute for Advanced Studies.* Sarnak is co-organizing with Bombieri a year long program at the IAS in 2009–2010 that will play a role in collaboration on this project. This will be an ideal opportunity for Sarnak to collaborate with Booker, Conrey, and Rubinstein who will spend three months in Princeton, along with one of their postdocs. Another postdoc, to be supervised by Sarnak, will spend the 2009–2010 year in Princeton.

One of our annual meetings of the FRG researchers will be held in Princeton during that semester, as will a research workshop of 20 participants that will examine our progress on finding and computing with Maass forms in the classical case and for $GL(3)$ and $GL(4)$.

4. *Two additional research workshops.*

In spring 2009, a mini workshop consisting of about 15 participants will be held to examine computational issues surrounding certain high degree Artin $L$-functions. This workshop will be organized by Rodriguez-Villegas.

Another workshop, to be held in late fall 2009, will take place in Princeton, as described in 3.

5. *Smaller group gatherings.* We will meet several times per year in smaller collaborative groups of two–seven people.

## 17. Management Plan

Because of the large number of people on this project and the large-scale nature of the collaborative effort, we recognize two levels of organization: *logistical management* and *research management*. Conrey, Rubinstein, and Stein will manage both aspects of the project. Each brings skills that are needed for management. Conrey, as director of the American Institute of Mathematics, has years of experience organizing and managing focused collaborative research bringing people with varied skills and backgrounds to a project. Rubinstein will sit at the intersection of most of the group work as a consumer of automorphic forms and a producer of $L$-functions. His $L$-function Calculator will play a central role in this project. Stein created the modular forms database, which is the canonical online source for classical modular forms data, implemented much software that is widely used for computing modular forms, and has demonstrated his management skills as the director of the Sage open source software development project, which has four workshops per year and over 50 programmers from across the globe who have now written several hundred thousand lines of code for the project.

17.1. **Logistical management.** Conrey, Rubinstein, and Stein will decide the hiring of postdocs. They will decide the scope, format, and scheduling of the yearly meetings of the dozen researchers on this FRG, and will run these meetings. Our annual workshops at AIM (summers 2008, 2009, 2010) and graduate student school/workshops will be organized by Stein and Rubinstein, though key decisions regarding the focus of the workshops, participants, and speakers will be made in consultation with Conrey and Sarnak. The workshop in 2009 on higher degree Artin $L$-functions will be organized by Rodriguez-Villegas. The budget will be be administered by AIM, and managed by Rubinstein, and Stein. They will also administer the online data and software archive as well as the $L$-functions and Modular Forms Wiki. Decisions regarding the publication of the Encyclopedia of $L$-functions and Modular Forms will be made by a committee consisting of the six coPIs on this proposal. This committee will assemble at the start of the third year of this project, at the summer 2010 AIM workshop, when we anticipate our wiki being in mature form. The role of this committee is described in the section on dissemination.

17.2. **Managing research.** Conrey, Rubinstein, and Stein will oversee the general direction of this project. Our annual get-togethers of the 12 researchers on this FRG will play an important role in making sure that the necessary steps are being carried out to achieve the goals we have described, as will our annual summer workshops at AIM, and these will be organized and headed by the three managers. We will organize our project around several topics, assign people to smaller groups and designate group leaders. The choice of our groups is based on the planned work of each member of our research team. The organization of these groups is detailed in the section on Collaboration. The managers of this proposal will lead about half of the groups, and will communicate throughout the year with other group leaders in order to make sure that work is progressing towards our goals. Group leaders will report at our annual get togethers and at our annual workshops. Our timeline and the development of our data and software archive, which will be connected to many of our projects, will serve as objective ways to evaluate our progress and keep us on track.

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