Numerical real algebraic geometry

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Summary

Numerical methods are the future of computation in algebraic geometry. From now on, increases in computing power will likely be due to increased parallelization. Gröbner bases, the current dominant paradigm for computation in algebraic geometry, have limited potential in this regime as they do not appear to be parallelizable. In contrast, numerical continuation is easily parallelizable and so continuation algorithms will reap the benefits of increases in computer power. This project aims to help build the infrastructure for this numerical future. Specifically, it will result in the research and development of algorithms and software tools for this numerical future, in the application of these tools to problems in pure algebraic geometry, in the dissemination of this research, and most importantly for this infrastructure, in the training of young researchers in this area.

One aim of this project will be to develop and implement a radically new continuation algorithm that computes only the real solutions to a system of polynomial equations, in contrast to traditional homotopy continuation, which necessarily computes all solutions, both real and complex. This continuation algorithm is a by-product of a new bound for the number of real solutions to a system of polynomial equations, and its complexity depends upon that bound. To develop this new continuation algorithm, this proposal seeks funds to support a postdoc and students to work with PI Sottile and his collaborator Bates to develop and implement this new algorithm. A second focus also involving the postdoc and students will be the use of numerical homotopy continuation and exact symbolic methods to study the Galois groups of Schubert problems, which is a question from pure mathematics. This will require the development and implementation of new software tools, particularly a Littlewood-Richardson homotopy algorithm (based upon Vakil's geometric proof of the Littlewood-Richardson rule) for computing solutions in Schubert calculus, as well as research into certifiability of numerical results. Both projects are several-year tasks requiring software development that involve team-based research and will result in several publications, software packages, and lead to one or more Ph.D. Theses.

The intellectual merits of this project include the development of a new type of continuation algorithm that is adapted to real solutions, as well as the new Littlewood-Richardson homotopy algorithm and perhaps the first serious application of numerical algebraic geometry to a question in pure mathematics.

The broader impacts of this project include the creation of a software package which implements this new continuation algorithm for finding real solutions, the introduction of new tools for inquiry in algebraic geometry, a large public database of Galois groups of Schubert problems, and the training of students and a postdoc in the increasingly important area of numerical algebraic geometry.

Project Description

1. Results from previous NSF support

My scientific activity since August 2002 has been supported by three grants. One, "CAREER: Computation, Combinatorics, and Reality in Algebraic Geometry, with Applications" was for $344,577 and ended in July 2007. The second, "Applicable Algebraic Geometry: Real Solutions, Applications, and Combinatorics" DMS-070150, began August 2007, is for $202,895 over three years. The third is under the Texas Advanced Research Program (#010366-0054-2007) provides $68,000 for Texas A&M students. These grants have led to 35 completed manuscripts, with 26 in print, one completed book and one book in progress. This research has included Hopf algebras, algebraic and geometric combinatorics, nonlinear computational geometry, Schubert calculus, real algebraic geometry, and applications of algebraic geometry. It has involved 36 collaborators on the completed projects and at least 16 on work-in-progress. In that period, I have given over 100 scientific presentations, including two short courses and about a dozen major invited talks at international meetings.

These grants have directly supported 6 graduate students (2 who received Ph.D.s—one with a different advisor), 3 postdocs (1 month summer salary each), 2 undergraduates, and indirectly supported many of my 29 junior (pre-tenure track at the time of collaboration) collaborators through research visits and by providing equipment for those based at UMass and TAMU. This support has enabled me to help organize 24 scientific meetings, including semesters at MSRI and the Centre Bernoulli in Lausanne, Switzerland.

Here, I will discuss relevant projects. Numerical citations are grant-supported work listed at the end of this section, and alpha-numeric citations appear in the bibliography.

Numerical computation of Galois groups of Schubert problems. While Galois groups of enumerative geometry problems were studied by Jordan in 1870 [Jor70], Harris laid their modern foundations in 1979 [Har79], showing that the algebraic Galois group equals a geometric monodromy group, and computing Galois groups of several such problems.

Leykin and I realized that we can use numerical homotopy continuation to compute Galois groups of Schubert problems, as computing monodromy is a well-established technique [SVW01] accomplished by numerically continuing solutions along loops. Using off-the-shelf software (the Maple interface for PHC [LV06, Ver99]), we wrote code based upon the Pieri homotopy algorithm [HSS98, HV00] to solve some moderately-sized Schubert problems and compute their Galois groups. The largest problem solved had 24024 solutions, and the largest whose Galois group we could compute had 17589 solutions. In every case we computed, the Galois group was the full symmetric group. We used continuation to compute permutations in the Galois group, and then GAP [GAP06] to test whether these permutations generated the full symmetric group. This software limited us to *simple* Schubert problems on Grassmannians, which are Schubert problems that may be formulated as complete intersections. Our results are described in [31] and they form the foundation for one of the main research projects in this proposal.

New fewnomial upper bounds. In 1980, Khovanskii [Kho80] proved that a system of n polynomials in n variables involving $l+n+1$ different monomials has at most

$$2^{\binom{l+n}{2}}(n+1)^{l+n}$$

nondegenerate *positive* solutions. This astronomical bound was not believed to be sharp.

Bertrand, Bihan, and I gave the bound $2n+1$ for all real solutions, when $l = 1$ and the set of monomial exponents is primitive [13]. Bihan then gave the sharp bound of $n+1$ for positive solutions when $l = 1$ [Bih07]. Bihan and I then [20] lowered Khovanskii's bound to

$$\frac{e^2+3}{4} 2^{\binom{l}{2}} n^l \ . \tag{1}$$

For this, we reduced the system to one consisting of l functions in l variables depending upon a vector configuration Gale dual to the exponents in the original system. Only solutions in a particular polyhedron are relevant, and we used polyhedral combinatorics, toric geometry, and the Khovanskii-Rolle theorem to obtain this bound.

This prepared the ground for further work. These bounds were shown to be asymptotically sharp and used to bound the topology of a fewnomial hypersurface [21]. The method, transforming a fewnomial system into a Gale-dual system, works over any field [26].

Most relevant for this proposal is that the proof leads to a new continuation algorithm which computes only the real solutions to a system of polynomials, and has complexity that depends upon the new fewnomial bound (1). Bates and I have a toy implementation in Maple to accompany a paper [37] presenting this algorithm, which we call *Khovanskii-Rolle continuation*. While writing the code, Bates and I discovered how to bound *all* non-zero real solutions to a primitive fewnomial system [22]. This bound is

$$\frac{e^4+3}{4} 2^{\binom{k}{2}} n^k \ ,$$

which is nearly identical to (1), except that (1) has e^2 in the constant, while the bound for all non-zero solutions in $\mathbb{R}^n$ has e^4. This advance would not have happened were we not implementing the Khovanskii-Rolle algorithm.

Experimentation in the real Schubert calculus. I have long studied real solutions to Schubert problems. This includes large-scale computation to test, formulate, and support conjectures. This was inspired by a conjecture of Boris and Michael Shapiro, that Schubert varieties given by real flags osculating a rational normal curve would have only real points of intersection. While this seemed too good to be true (if not actually fanciful), extensive symbolic computation gave deeply convincing evidence for their conjecture and efforts to prove it led to over 20 published papers. Most notable are some of my work [Sot99, Sot00] proving reality in the Schubert calculus and work of Eremenko and Gabrielov [EG02a, EG02b], Mukhin, *et. al* [MTV, MTV07], and Gordon, *et. al* [GHY07], which includes proofs of this conjecture. This story is the subject of a Current Events Bulletin Lecture that I will give at the AMS meeting in January 2009, and an accompanying survey article [35].

The original Shapiro conjecture was for all flag manifolds, but computer investigation found a counterexample, and the work described established it for the Grassmannian. Computations suggested a modification, the *Monotone Conjecture*, that may hold for the flag manifold. With (then) undergraduate student Sivan, graduate student Ruffo, and postdoc Soprunova, we undertook a large-scale project to study the Monotone Conjecture for many Schubert problems on small flag manifolds. This consumed over 15 GigaHertz-years of computing, solving over 500 million systems of polynomials to study the conjecture and related questions for over 1100 Schubert problems on 25 small flag manifolds. The computations are archived at `www.math.tamu.edu/~sottile/pages/Flags/Data`. This work involved scientific contributions from members of our research team who were at vastly different stages of their careers. For example, the undergraduate Sivan formulated and discovered the seminal example, which became the cover illustration for Experimental Mathematics [14].

This inspired Eremenko, *et. al* [EGSV06] to prove the Monotone Conjecture for a (small) subclass of flag manifolds. They actually prove a different generalization of Shapiro's conjecture involving secant flags for the Grassmannian of codimension-2 planes. Their work in turn led me to organize a new research team to investigate this *Secant Conjecture*, and its natural common generalization with the Monotone Conjecture.

In the past project [14], our ambition for computer experimentation exceeded my capabilities and vision of an efficient way to automate and organize the computations. However, Hillar, an NSF postdoc who worked with me, devised a very general framework for automating the computation—coordinating hundreds of computers, archiving, and displaying the results. This computation is in progress, running on personal and departmental computers, and nightly on a Beowulf cluster of 191 nodes whose day job is calculus instruction. This has involved three postdocs and four graduate students (three of whom have moved on to other jobs). Progress can be monitored from our team's web page, at `www.math.tamu.edu/~secant`.

Algebraic Geometry in Algebraic Statistics and Geometric Modeling. Luis Garcia of Sam Houston State University and I hold a grant from the State of Texas. We aim to exploit the common element of real toric varieties in geometric modeling (Bézier patches) and algebraic statistics (discrete exponential families) to transfer ideas and techniques between the fields. The manuscript [29] with Garcia laid the foundations for this project, which has resulted in two papers. One [34] classifies toric surface patches with linear precision, introducing a new trapezoidal patch, and another [36] applies ideas from toric varieties and dynamical systems to better understand the structure of Bézier patches. This grant however only provides support for students. I support two graduate students and Garcia supports three undergraduates. We jointly supervise the students, with a particular goal of integrating the students from SHSU into my research group at Texas A&M. This includes monthly group meetings and joint attendance at the January AMS-MAA meetings.

Scientific projects supported by recent grants.

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16. *Irrational proofs of three theorems of Stanley*, with M. Beck. European Journal of Combinatorics, **28** (2007), 403-409.
17. *A Pieri-type formula in the K-theory of a flag manifold*, with C. Lenart. Transactions of the AMS, **359** (2007), 2317-2342.
18. *Line tangents to four triangles in three-dimensional space*, with H. Brönnimann, O. Devillers, and S. Lazard. Discr. Comput. Geom., **37**, No. 3, (2007), 369–380.
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20. *New Fewnomial Upper Bounds from Gale dual polynomial systems*, with F. Bihan, Moscow Mathematics Journal, **7** (2007), Number 3, 387–407.
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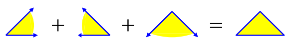
28. 

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30. *A Littlewood-Richardson rule for Grassmannian permutations*, with K. Purbhoo. Proc. AMS, to appear.
31. *Galois groups of Schubert problems via numerical homotopy continuation*, with A. Leykin, Math. Comp., to appear.
32. *General isotropic flags are general (for Grassmannian Schubert calculus)*, 4 pages. Journal of Algebraic Geometry, to appear.

33. *Betti number bounds for fewnomial hypersurfaces via stratified Morse theory*, with F. Bihan, 7 pages.
34. *Linear precision for toric surface patches*, with K. Ranestad and H.-C. Graf von Bothmer. 25 pages.
35. *Frontiers of reality in Schubert calculus*, 25 pages, Current Events Bulletin. 2008.
36. *Some geometrical aspects of control points for toric patches*, with G. Craciun and L. Garcia, 20 pages, 2008.
37. *Khovanskii-Rolle continuation for real solutions*, with D. Bates. In progress.
38. *Discrete and Applicable Algebraic Geometry*, with T. Theobald. Book in progress, currently 180 pages.

2. Proposed research

Numerical methods are the future of computation in algebraic geometry. This proposal will facilitate that numerical future through the training and professional development of students and postdoctoral researchers, through the development and implementation of algorithms, and through computer-aided research projects that showcase the possibilities of this numerical future to the rest of mathematics, and in particular to algebraic geometry.

To that end, this proposal will partially support a postdoc at Texas A&M University who will have a low teaching load and summer support. This will enable them to work on software development. It will also support one graduate student, undergraduate student full time in Summers and part-time during the academic year. It will support the research of the PI for one month each Summer. Necessary computer equipment and travel funds are included.

2.1. **Why numerical algebraic geometry?** Currently, most computation in mathematics with polynomials is symbolic, often utilizing Gröbner bases. This is particularly true in computational algebraic geometry and in many applications of algebraic geometry. Gröbner basis computation has horrible (doubly exponential in space and time [MM82]) worst-case complexity, and even the average-case is at least single-exponential. In the previous era of computer development when processor speed was increasing exponentially, this complexity was not considered a serious drawback.

Processor speed is no longer increasing as the non-linear dependence of power consumption and heat generation on speed renders faster processors infeasible. Consequently, chip manufacturers increase computer power through multi-core processors and users are left to satisfy their needs with massive parallel clusters. Future increases in computation will come from algorithms that exploit this parallel reality of computer architecture.

Gröbner bases do not appear to be parallelizable—at least no efficient parallel algorithms or implementations have been developed. This and the complexity of Gröbner basis computation suggests that Gröbner bases and the symbolic algorithms which rely on Gröbner bases will not benefit from future increases in computer power.

Fortunately, numerical homotopy continuation [AG90, Li03] provides an alternative foundation for computation with polynomials. This was originally developed to solve square (number of equations equal to number of variables) systems of polynomial equations. The paper, "Numerical Algebraic Geometry" by Sommese and Wampler [SW96] signaled an important shift—algorithms based on numerical homotopy continuation to compute, study, and manipulate algebraic varieties. While this development is intellectually significant in that it provides an alternative toolset for studying algebraic varieties, its real value is that it is the computational algebraic geometry of the future, as numerical continuation is easily parallelizable in that its elementary operations may be performed independently on different processors, with no interprocessor communication.

Thus numerical algebraic geometry has the potential to displace symbolic algorithms based on Gröbner basis computation as a primary tool for computation in algebraic geometry. Future serious large-scale computations will be based on numerical computation as Gröbner bases are limited by the physics of chip design.

2.1.1. *Challenges for this numerical future.* Before we can arrive at this numerical paradise there are many scientific challenges to overcome. One is the (fairly obvious) further development of numerical algebraic geometry, extending the range of applicability of numerical algorithms. This is the current main thrust of work. For example, the computation of exceptional sets of morphisms [SW], following singular solutions of systems of equations [LVZ06], and primary decomposition [Ley08] are now possible.

Another challenge is to refine these techniques and apply them to new scientific domains. Numerical methods, while fast, have the troublesome habit of failing now and then. While Shub and Smale [SS93] developed α-theory for the certification of numerical methods (guaranteeing that Newton iterations applied to an approximate solution will converge to an actual solution), this has not yet been implemented in user-friendly software. This lack of certifiability is one reason that numerical algebraic geometry has barely been used as a tool in pure mathematical research. A cause for this state of affairs is that the software is naturally driven by its users, who are typically applied scientists for whom the probabilistic nature of their answers is not a problem, but a decrease in performance necessitated by increasing the quality of the answers would be a serious problem. Thus the incorporation of certificates into software for numerical algebraic geometry is caught in a catch-22 situation.

A third challenge is that numerical algebraic geometry relies on a single type of algorithm, numerical homotopy continuation. The basis is even narrower—most continuation uses the polyhedral homotopy method of Huber and Sturmfels [HS95]. This amazing algorithm is often efficient and has the advantage that it is easily used as a black box, as it exploits combinatorial structures in the equations. There are however many problems for which polyhedral homotopy is inefficient. In the Schubert calculus the Pieri homotopy algorithm [HSS98, HV00] sometimes provides an efficient alternative. For example, the Schubert problem in [31] with 24024 solutions is efficiently computed using Pieri homotopies, but polyhedral homotopies must follow 1995356 paths, for a 1.2% efficiency. Pieri homotopies cannot solve all Schubert problems. Vakil's geometric Littlewood-Richardson rule [Vak06a] is the basis for an algorithm to solve all Schubert problems (Vakil, Verschelde, and I have developed but not implemented these *Littlewood-Richardson homotopies*). Another class are problems which demand real solutions—by its very nature, numerical homotopy continuation must compute all solutions, both real and complex, as it also must solve systems with general complex parameters. For this reason, it is inefficient for systems with few real solutions (see [20,22] and [Kho80]).

A fourth and serious obstacle to the development of numerical algebraic geometry is that there is not much of a pipeline for post-Ph.D. training and professional development of young researchers. This new field is somewhat orphaned as it is neither traditional numerical analysis nor algebraic geometry. Consequently, it is hard for a young researcher in numerical algebraic geometry to obtain a research position.

This proposal will further the development of numerical algebraic geometry along the last three lines identified above. One project will develop and implement a new continuation algorithm (Khovanskii-Rolle continuation) that only computes real solutions to systems of polynomial equations. Bates and I have produced a toy implementation [32] in Maple, but there are technical and theoretical issues which need to be understood and overcome before a fully working version can be produced. This project is described in Section 2.4.

The second project will use numerical algebraic geometry to compute Galois groups of Schubert problems, a problem in pure mathematics. We will develop software tools, including Littlewood-Richardson homotopies, software with certificates, and overdetermined path following. Together with symbolic methods, these will be used to investigate the Galois groups of tens or hundreds of thousands of Schubert problems. This large-scale experimentation may use scores of GigaHertz years on hundreds of computers, giving a detailed census of these Galois groups. This will showcase the power and possibilities of numerical methods as a research tool in algebraic geometry. This project is described in Section 2.5.

While these research goals are interesting and important for the development of this field, it is the last challenge which this proposal will most squarely address. It will help support a postdoctoral researcher working in this field, aiding their professional development by broadening their mathematical horizons and providing careful mentoring. It will train at least one Ph.D. student in this field, support an undergraduate, and it would help me to orient my research more towards this numerical future of algebraic geometry, and in particular to learn more about writing numerical software.

I want to hire a postdoc for this work because I simply do not yet possess enough programming skills to undertake this project on my own, yet I am extremely interested in carrying out this research and working more in numerical algebraic geometry. The postdoc would work with me on this research and on the implementation of our software. We would learn from each other, and in particular, I would acquire the skills to train future graduate students and postdocs. This is a model that has already been successful; in my work with Hillar on the Secant Conjecture (see Section 1), I learned perl, MySQL, and PHP, and how to use these software languages to manage computation, manipulate data, and display the results.

2.2. **Personnel.** The proposal will support people engaged in the proposed research. A main part is support for a postdoc who will work on these research projects. Last year I visited three leaders in the field of numerical algebraic geometry to interview their graduate students, identifying one student of each who is most appropriate for this project.

Andrew Sommesse of Notre Dame directs the software project Bertini for numerical algebraic geometry and is the Ph.D. advisor of my collaborator Bates. Andrew's student Jon Hauenstein is graduating this year and already works with Bates. Jon is personable and good at teaching (my department is very strict about our postdoc's teaching). His undergraduate training included substantial computer experience, and he has been a key figure in the development of the software package Bertini.

The best numerical homotopy continuation software in terms of sheer power and speed comes from the team of T.Y. Li of Michigan State. One of his former students, Dr. Tsung-Lin Lee, is a postdoc at Michigan State. According to T.Y.: "without him our code, HOM4PS-2.0, would never be as speedy as it stands. Most importantly, his enthusiasm about computation is rarely seen among young researchers." I have met Lee, and I can work well with him. Besides helping him grow mathematically by introducing him to new research topics, I would provide careful mentoring and advice on beginning one's career.

Jan Verschelde of the University of Illinois-Chicago writes the easiest to use and most popular homotopy continuation code, PHCPack. His student Yun Guan is graduating this year and she is experienced with parallel implementation of numerical homotopy software. She has also investigated some subtle pitfalls of numerical software, including undetected path crossing and stepping over singularities. This background would be useful for the project to introduce certificates of successful computation into numerical software.

If none of these (or other suitable new Ph.D.'s) are available, I would support Zach Teitler, a postdoc at Texas A&M, during his remaining year here and look for a suitable candidate for the final two years of this project. Zach was trained in pure algebraic geometry (he is a student of Lazarsfeld), but he is a skilled programmer and his combination of strong mathematical background and programming skills has been key for our work on the Secant Conjecture (described in Section 1).

Abraham Martin del Campo-Sanchez, a current student, is interested in computation, and may choose to work in numerical algebraic geometry, either for his Ph.D. research, or as a secondary project (my students work on several lines of research). I will likely recruit a new student for this project either from within the graduate students at Texas A&M or from outside; I have been quite successful at attracting graduate students.

Recruiting good undergraduates is easy, as I teach Honors calculus and multivariate calculus to math majors, and my students often ask if I have research jobs available.

The two main projects described here involve continuing work with collaborators Dan Bates and Anton Leykin, respectively. I have included letters of support from them with more details about our collaborations. Students and postdocs working with me would have the opportunity to visit Bates or Leykin for research collaboration.

2.3. **Mentoring.** While some aspects of the professional development of junior personnel working with me are discussed elsewhere, I will now provide more details.

People working on this project will be integrated with my research team, which includes four students and two postdoctoral collaborators at Texas A&M and the group at SHSU. We meet regularly to discuss our work and professional matters. This includes informal lunchtime meetings, a management technique I learned from Peter Gritzmann while on sabbatical in his Lehrstuhl in Munich. At these meetings we share advice on professional matters including research, writing papers, refereeing, giving and watching talks, courses, mathematics, and anything else.

It is my responsibility to train those who work with me in all aspects of our profession. I meet weekly with each student and postdoc. There, we discuss their (or our, if we are collaborating) research, and anything else that arises professionally, from their classes, to their teaching, to talks they may be giving, to papers, job applications, and research grants. For all of these tasks, I critique their work and encourage their efforts. On top of these tutorials, everyone who starts with me is given some of the books that are good for professional development [BC99, Con96, Kra99, Kra96, Kra04, SW00, Zin05].

This is more than just verbal advice. I get my students and postdocs invited to give talks at seminars and conferences, help them plan, prepare, and practice their talk, and support their travel. I pass a referee job off at least once to each as a tool to discuss with them the ins and outs of refereeing. I have now helped four postdocs write research grants; two are pending and two (Garcia and Hillar) have been successful.

My joint grant with Luis Garcia of Sam Houston State University (a 1-hour drive away) involves face-to-face meetings once a month. At these, his three undergraduate students and my two graduate students discuss their work with us, giving short presentations or explaining some mathematics. We engage the undergraduates in joint work on publishable research. We also travel together to conferences, such as the yearly AMS-MAA January meeting.

I plan to hire an undergraduate student for research work full-time in the summer and part-time during the school year. They will be part of these activities and work on a multi-year project, with the goal of learning interesting mathematics and collaborating on publishable research. I enjoyed such year-round work as an undergraduate physics major. This gave me an interesting job while at University, a direct connection to the workings of research, and access to high-level faculty. Such experiences are lacking in the traditional mathematics REU, but would be provided to the students working with me.

2.4. **Khovanskii-Rolle Continuation.** In numerical homotopy continuation a target polynomial system you wish to solve is deformed into one whose solutions are known (the start system), and then the known solutions are tracked along a path connecting the two systems. Much ingenuity has been applied to finding good deformations and start systems, fast and adaptive trackers, and dealing with singularities such at the beginning and ending of paths. In this important algorithm, *all* paths are tracked to find the solutions of the target system. This is inefficient and inelegant for applications that seek only real solutions. Currently, there are three methods to compute only real solutions. One, cylindrical algebraic decomposition [Col75], has remarkably poor complexity and is infeasible in dimensions above 3. Another method is exclusion [SW96, §6.1], which recursively subdivides a bounded domain, excluding subdomains that cannot harbor solutions. This is an active area with research into better exclusion tests [Geo01, MP05] and complexity [AEG02]. A third method, based on semi-definite programming, was recently proposed [LLR08].

My work with Bihan and Bates leads to the *Khovanskii-Rolle continuation algorithm* that only follows real paths and which finds all real solutions. Bates and I have completed a proof-of-concept toy implementation [37] that works when $l = 2$ (l is defined below). While promising, we forsee significant further work to produce workable code. There are also theoretical questions which need to be addressed to improve the robustness of this algorithm. Producing this code and studying this algorithm is a pillar of this proposal. I will explain this algorithm below and then discuss some research directions.

This new algorithm should be better understood and developed, not only for its own sake, but also to understand how it compares to other methods for finding real solutions. Its development may lead to advances in more traditional continuation. Lastly, its implementation (as well as implementing two exclusion algorithms described below) will provide me, my collaborators, and others with tools for our own research. These includes Swaroop and Bhattacharyya, colleagues in mechanical engineering who very much want tools for finding real solutions in bounded domains.

My collaborator Bates is also proposing the development of Khovanskii-Rolle continuation in his NSF proposal. We chose to submit separate proposals as this represents a small part of his work, and because my proposal is largely to support people working under my direction. The careful reviewer will notice much similarity between our descriptions of the algorithm as we each modified the same example from our joint paper.

2.4.1. *Gale duality.* The Khovanskii-Rolle algorithm begins by transforming a system of n polynomials in n variables, each with $l{+}n{+}1$ monomials into a *Gale-dual* system of l polynomials in l variables [26]. Here is an example. Let $n = l = 2$ and consider the system

$$2x^4y^{-1} - 3x^3y^2 - 4x^4y + xy^2 - \tfrac{1}{2} \quad = \quad x^3y^2 + 2x^4y - xy^2 - \tfrac{1}{2} \quad = \quad 0\,. \tag{2}$$

in $(\mathbb{C}^\times)^2$. We diagonalize it to obtain

$$x^3y^2 \;=\; x^4y^{-1} - x^4y - \tfrac{1}{2} \qquad\text{and}\qquad xy^2 \;=\; x^4y^{-1} + x^4y - 1\,.$$

If we set $s := x^4y^{-1}$ and $t := x^4y$, then this becomes

$$x^3y^2 \;=\; s - t - \tfrac{1}{2} \qquad\text{and}\qquad xy^2 \;=\; s + t - 1\,. \tag{3}$$

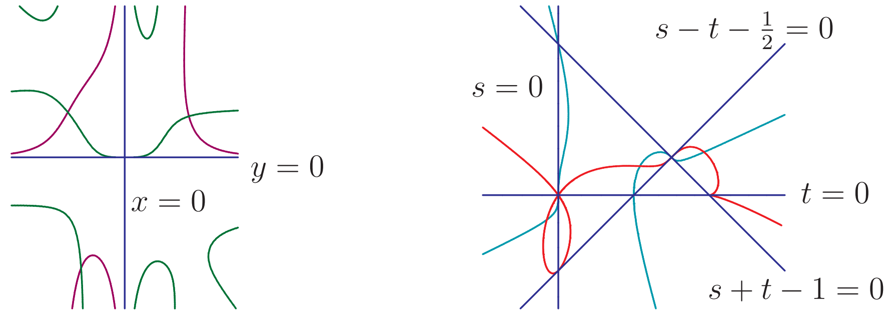
Since the monomials x^3y^2, xy^2, x^4y^{-1}, and x^4y satisfy

$$(x^4y^{-1})^2(xy^2)^3 \;=\; (x^4y)^2(x^3y^2) \qquad\text{and}\qquad (x^4y^{-1})(x^3y^2)^3 \;=\; (xy^2)(x^4y)^3\,,$$

we may use (3) to obtain the *Gale (dual) system*,

$$s^2(s+t-1)^3 \;-\; t^2(s-t-\tfrac{1}{2}) \;=\; s(s-t-\tfrac{1}{2})^3 \;-\; t^3(s+t-1) \;=\; 0\,. \tag{4}$$

Under the substitution $s := x^4y^{-1}$ and $t := x^4y$, the system (2) in $(\mathbb{C}^\times)^2$ is equivalent to the system (4) in the complement of the line arrangement $st(s+t-1)(s-t-\frac{1}{2}) = 0$.



Polynomial system (2) and its Gale system (4).

Under Gale duality, nonzero solutions to the original system become solutions to the Gale system that lie outside some hyperplanes $p_i = 0$, with positive solutions corresponding to those in the polyhedron $p_i \geq 0$. Here, the hyperplanes are the lines and the polyhedron is the central quadrilateral. This reduces the problem of finding real or positive solutions to a system of polynomials to finding real solutions to the Gale system in a hyperplane complement or in a chamber. The proofs of the bounds [20,22] for the number of real solutions to Gale dual systems immediately give the Khovanskii-Rolle continuation algorithm.

2.4.2. *An example of Khovanskii-Rolle Continuation.* Let us begin with a Gale system dual to system of 4 polynomials in 4 variables,

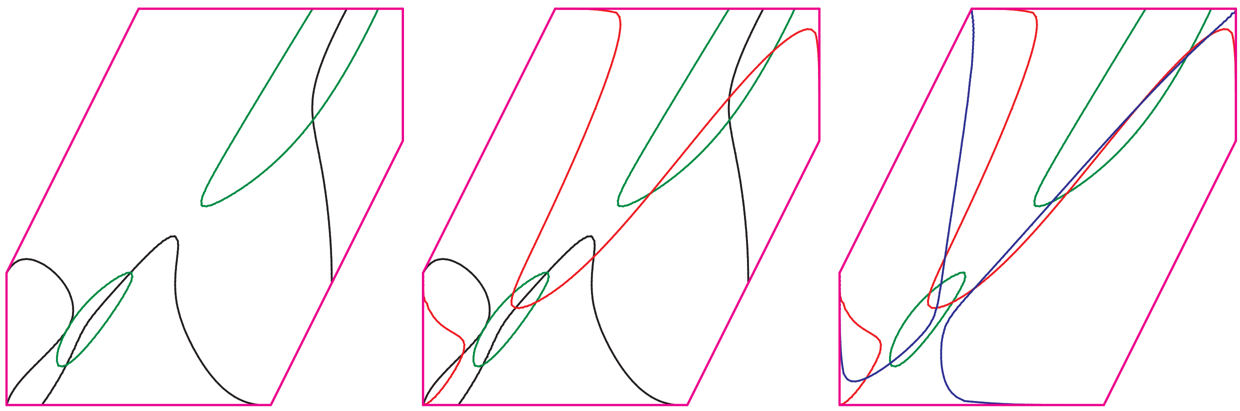
$$\begin{aligned} F_1 &:= (3500)^{12}x^{27}(3-x)^8(3-y)^4 \;-\; y^{15}(4-2x+y)^{60}(2x-y+1)^{60} \;=\; 0\,,\\ F_2 &:= (3500)^{12}x^8y^4(3-y)^{45} \;-\; (3-x)^{33}(4-2x+y)^{60}(2x-y+1)^{60} \;=\; 0\,. \end{aligned}$$

This was chosen to have an exceptionally large number (5) of real solutions, which leads to the extreme $(33,45,60)$ exponents. Consider the Jacobian determinants $\Gamma_2 := \mathrm{Jac}(F_1, F_2)$ and $\Gamma_1 := \mathrm{Jac}(\Gamma_1, F_1, \Gamma_2)$,

$$\begin{aligned} \Gamma_2 \;=\; & 2736 - 15476x + 2564y + 32874x^2 - 21075xy + 6969y^2\\ & - 10060x^3 - 7576x^2y + 8041xy^2 - 869y^3 \;+\; 7680x^3y - 7680x^2y^2 + 1920xy^3\,. \end{aligned}$$

$$\begin{aligned}\Gamma_1 \;=\; & 8357040x - 2492208y - 25754040x^2 + 4129596xy - 10847844y^2 \\ & - 37659600x^3 + 164344612x^2y - 65490898xy^2 + 17210718y^3 + 75054960x^4 \\ & - 249192492x^3y + 55060800x^2y^2 + 16767555xy^3 - 2952855y^4 - 36280440x^5 \\ & + 143877620x^4y + 35420786x^3y^2 - 80032121x^2y^3 + 19035805xy^4 - 1128978y^5 + 5432400x^6 \\ & - 33799848x^5y - 62600532x^4y^2 + 71422518x^3y^3p - 13347072x^2y^4 - 1836633xy^5 + 211167y^6 \\ & + 2358480x^6y + 21170832x^5y^2 - 13447848x^4y^3 - 8858976x^3y^4 + 7622421x^2y^5 - 1312365xy^6 \\ & - 1597440x^6y^2 - 1228800x^5y^3 + 4239360x^4y^4 - 2519040x^3y^5 + 453120x^2y^6\,.\end{aligned}$$

The linear factors of F_1 and F_2 give lines in $\mathbb{R}^2$. Under Gale duality, the positive orthant in $\mathbb{R}^4$ maps to the hexagon in the figure below (best viewed in color!). The solution curves of these four equations are also drawn below. The algorithm begins by solving $\Gamma_1 = \Gamma_2 = 0$



$\Gamma_1 = 0$, $\Gamma_2 = 0$ $\qquad$ $\Gamma_1 = 0$, $\Gamma_2 = 0$, $F_1 = 0$. $\qquad$ $\Gamma_2 = 0$, $F_2 = 0$, $F_2 = 0$.

in the hexagon, finding the points where Γ_2 meets the edges of the hexagon, and computing all vertices. This involves solving one polynomial system with Bézout number 32 but 5 solutions in the hexagon, six with Bézout number 4 and six with Bézout number 1. The Khovanskii-Rolle Theorem guarantees that, along the curve $\Gamma_2 = 0$, its intersections with the curve $F_1 = 0$ falls between its intersections with the curve $\Gamma_1 = 0$, or between such an intersection and the boundary. (You may see this in the central picture above.)

Thus, curve-tracing from points of the hexagon where $\Gamma_1 = \Gamma_2 = 0$ and where $\Gamma_2 = 0$ meets the boundary is guaranteed to find each of the points $\Gamma_2 = F_1 = 0$ twice. These points are recognized during tracking by monitoring the function value of F_1. Similarly, the solutions $F_2 = F_2 = 0$ are interspersed on the the curve $F_1 = 0$ between the vertices of the hexagon and the points $\Gamma_2 = F_1 = 0$. Another round of tracking provides the desired solutions of the Gale system, and thus solutions to the original system via Gale duality. Apart from the vertices, the curves traced are generically everywhere nonsingular.

This basic idea of bootstrapping from solutions to a system of Jacobians to solutions to the original system is based upon the Khovanskii-Rolle Theorem: Consecutive points where $h = 0$ along a curve $g = 0$ are separated by zeroes of the Jacobian $\mathrm{Jac}(g, h)$. This algorithm is feasible because Jacobians of Gale polynomials have low (in fact bounded) degree. These same ideas work for Gale systems in any dimension and reduce solving a Gale system to some path-tracking from the solutions to low degree polynomial systems on the different flats of a hyperplane arrangement. The total number of solutions to these systems bounds the number of solutions to the Gale system, and is the source of the fewnomial bounds [20,22]. Since this also bounds the number of paths to be tracked, the fewnomial bound also bounds the complexity of the Khovanskii-Rolle algorithm.

Our next step is to make a proper implementation (not a Maple worksheet) of this algorithm that will in principal work for all n and l. We do not intend to create a standalone package, but rather a module that may be linked to software, such as Sage [Ste08] or Macaulay 2 [GS]. Our primary goal is to develop and test this algorithm.

This is, however, only the beginning of the story. There are many points that remain to be studied. For example, we do not yet know how to prevent path jumping during the continuation. This may be prevented in homotopy continuation as the paths are obtained by deformation, but that is not the case here. While I am currently unfamiliar with work on these questions, we will begin by studying the existing literature about tracking implicit curves before trying to reinvent this wheel.

In the example, the curves $F_i = 0$ are singular at the vertices of the hexagon—exactly where we begin our tracking. This is a general feature of the curves we need to track in Khovanskii-Rolle continuation as the Gale polynomials are singular along the codimension 2 flats of the hyperplane arrangement. However, in the neighborhood of a point on such a flat, the variety of a Gale polynomial is a hypersurface parametrized by monomials (a toric variety), we may change coordinates locally for singular tracking. We did this in the example, but in dimensions higher than 2 we expect this idea to require finely tuned heuristics, if not more theoretical justification.

There will be other intellectual challenges to overcome for implementing and refining the Khovanskii-Rolle continuation algorithm. We will investigate and solve them as they arise.

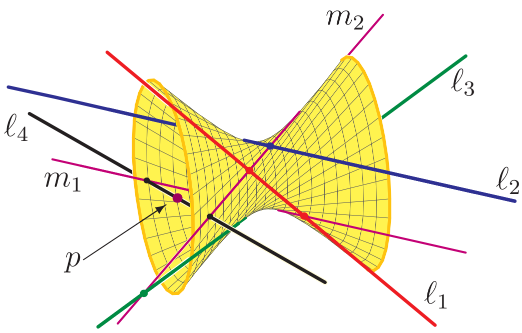
2.4.3. *Applications.* One goal of implementing the Khovanskii-Rolle continuation is that my own research into the real solutions of sparse polynomials systems could use some software to reliably and efficiently find the real solutions to a system of equations. There are two other algorithms based on exclusion that my team may investigate, implement, and compare to each other and to existing methods.

One is an exclusion algorithm to find the solutions to a Gale system in the polyhedron $p_i > 0$, where p_i are the linear factors in the Gale polynomials. While this polyhedron may be unbounded, there is a fractional-linear coordinate change under which it will become bounded. The challenge for this new application of exclusion will be to find an exclusion criterion adapted to Gale systems.

A second algorithm has wider application: We may reinterpret a system of equations in the positive orthant $\mathbb{R}^n_>$ as equations on the positive part of a toric variety. Rather than consider the toric variety as being parametrized by monomials, with the image of $\mathbb{R}^n_>$ the positive part of the toric variety, we instead parameterize the positive part by the toric Bézier functions of Krasuaskas [Kra02, Kra06] that arose in geometric modeling. Pulling the equations back to the polytope reduces the problem again to finding real solutions to a system of equations in a bounded domain. Again, the challenge is to find an exclusion criterion adapted to the equations that arise.

2.5. **Galois Groups of Schubert Problems.** The power of Gröbner bases lies in their teasing delicate invariants from a list of polynomial equations, which explains their utility for cohomology and free resolutions. The natural strength of numerical methods lies in (perhaps massively parallel) path-following. Thus computing monodromy of branched covers, or for us Galois groups, is a natural application of numerical methods. This method is significant as computing monodromy is notoriously difficult. I will showcase the potential of numerical computation in algebraic geometry by computing Galois groups of many Schubert problems.

The Schubert calculus [KL72] is a method to compute the number of solutions to *Schubert problems*, which are a class of geometric problems involving linear subspaces. The prototypical Schubert problem is the following: How many lines in space meet four given lines? To answer this, note that three lines ℓ_1, ℓ_2, ℓ_3 lie on a unique doubly-ruled hyperboloid.



The lines ℓ_1, ℓ_2, and ℓ_3 lie in one ruling, while the lines that meet them for the second ruling. The fourth line ℓ_4 meets the hyperboloid in two points. Through each point there is a line in the second ruling, and these are the two lines m_1 and m_2 meeting our four given lines.

The Galois group of this Schubert problem is the group of permutations that are obtained by following the two solutions over loops in the space of lines $\ell_1, \ldots, \ell_4$. Rotating ℓ_4 about the point p gives a loop which interchanges the two solution lines m_1 and m_2, showing that the Galois group is S_2, the full symmetric group on two letters.

More generally, given a fixed flag $F_\bullet$ of subspaces in $\mathbb{C}^n$, a *Schubert condition* ω specifies how a linear space (or a flag of linear spaces) meets $F_\bullet$. In the example, the fixed line ℓ is the flag $F_\bullet$ and ω is the condition that a general line meets ℓ. The linear spaces satisfying a condition ω for a flag $F_\bullet$ is a set of points on some Grassmannian or flag manifold defined by equations that may be read off from the data $F_\bullet, \omega$. A list of Schubert conditions $\omega_1, \ldots, \omega_s$ so that for general flags $F^1_\bullet, \ldots, F^s_\bullet$ there are finitely solutions (linear spaces satisfying the given conditions for the given flags) is a *Schubert problem*. The solutions are permuted if the flags move along a loop in the parameter space. These permutations generate the *Galois group*, which encodes subtle information about the Schubert problem.

Using numerical continuation to follow the solutions as the flags are moved along loops gives permutations that generate a subgroup of the Galois group. We do not know if this subgroup is the full Galois group, except when the subgroup is the full symmetric group. In [31], we always found permutations generating the full symmetric group, typically only needing two or three permutations. These were all *simple Schubert problems*, and our results showed that these problems have no additional structure. However, Derksen, and then Vakil [Vak06b] and also Ruffo, *et. al* [14] found Schubert problems with unexpectedly small Galois group. I plan a census of tens or hundreds of thousands of Schubert problems, trying to determine the Galois group of each. These data will reside in a publicly accessible MySQL database. (I use this model in an experiment to study the Secant Conjecture, see Section 1.) This will be a large multi-year project that should lead to a number of research papers and possibly theses.

This census will give a list of Schubert problems that likely have an unexpectedly small Galois group. I plan to understand why these Galois groups are unexpectedly small. At least these data should lead to a precise conjecture about such Schubert problems.

We will acquire these data via several methods. One exact method that works for any small problem, is to compute a single instance of the Schubert problem with rational number coefficients to obtain a univariate eliminant, $f(x)$. Billey and Vakil [BV08] noted that the Galois group of f is a subgroup of the Galois group of the parent enumerative problem. Such eliminants are readily computed for many problems with fewer than 50 solutions and for some that are much larger. The bottleneck is that existing software for the Galois group of a polynomial f can only handle a limited range of degrees (the best that I know has $\deg(f) \leq 12$). While we will seek the best software, we hope that our new application will spur software development (perhaps within Sage [Ste08]) to raise this limit on degree.

A second exact method is a result of Vakil [Vak06b]. Given a Schubert problem on a Grassmannian, Vakil constructs a combinatorial structure. When this possesses certain properties, the Galois group contains the alternating group. Implementing this would be good work for an undergraduate student, as it requires little besides decent programming skills. It would also help us to start to design and populate our Galois group database. Coskun [Cos08] has a geometric Littlewood-Richardson rule for two-step flag varieties and Mihalcea has announced one for the Lagrangian Grassmannian. Proving analogs of Vakil's result would extend this method to Schubert problems on these other flag varieties.

The signature method for this problem uses numerical homotopy continuation to compute elements in the Galois group directly by following solutions along paths in the space of flags. This computation is not exact as numerical path tracking may sometimes fail in that the tracker can jump paths. Standard methods to reduce this problem include re-running the computation, using adaptive step size, or tightening numerical tolerances. The Shub-Smale α-theory [SS93] provides a certificate that Newton iterations applied to an approximate zero of a complete intersection will converge to an actual zero. In principle, this could be used to certify that numerical path-tracking has not failed. As far as I know, this has not been implemented as it is likely to result in slow, almost to the point of inefficient, continuation. I plan to implement such certificates, both to show that it is possible, and to use them to obtain exact results about Galois groups of Schubert problems. Implementing α-theory and applying it would be a major project and lead to an interesting and important publication.

Schubert problems are typically not complete intersections—the number of equations exceeds the number of variables. A *simple Schubert problem* is one that is a complete intersection. For these, off-the-shelf tracking software can compute monodromy permutations, given a single solved instance of that Schubert problem. In [31], we only studied simple Schubert problems on Grassmannians. We will study all feasible simple Schubert problems, including those on different flag manifolds.

More generally, we will need to obtain or write continuation software that efficiently follows overdetermined paths to compute Galois groups when the Schubert problem is not a complete intersection. This is where the real interest lies, as simple Schubert problems are conjectured to always have Galois group the full symmetric group. In this and other aspects of this proposal I would draw upon the expertise of my friends and collaborators within the numerical algebraic geometry community.

Computing monodromy permutations requires a single solved instance of the Schubert problem. For simple Schubert problems without too many solutions, existing software can reliably solve a single instance. For other problems, we will need better algorithms or different software. In [31], we implemented a simplified version of the Pieri homotopy algorithm [HSS98, HV00]. This version makes sense for some other flag manifolds, and we will implement and use it where possible. For more general problems on Grassmannians we will need the more general Littlewood-Richardson homotopies.

While Vakil's geometric Littlewood-Richardson rule involves vivid geometry and it is clear that there is a homotopy method based on it, Vakil, Verschelde, and I discovered that it is quite subtle to understand in a way that may be implemented. Briefly, in traditional homotopy the space is fixed and the equations are perturbed, while for the Littlewood-Richardson homotopies the equations are fixed and the space gets deformed. Implementing these Littlewood-Richardson homotopies in collaboration with Vakil, Verschelde, and the postdoc will be a priority, as they are a necessary input for the computation of Galois groups.

While these algorithms will enable the computation for Schubert problems on the Grassmannian and some simple Schubert problems on other flag manifolds, we need to develop algorithms to extend this project deeper into the zoo of all Schubert problems.

We will develop our software on personal computers and test it on small clusters in the Texas A&M mathematics department. This project may require massive computing resources. If not too large (hundreds of GigaHertz-years), then we will use our department's teaching computer lab, which transforms into a Beowulf cluster of 191 computers each night. If the computation is larger, then we will seek time on an appropriate supercomputer, such as the NSF-funded TeraGrid `www.teragrid.org/`.

2.6. **Related activities of the PI.** During the period of this proposal, I will engage in my usual range of professional activities, including organizing conferences. I mention some that are relevant to this proposal. I continue to be on the Steering Committee for the MEGA (effective methods in algebraic geometry) conferences, which are biannual and European. The next is in June 2009 (just before the project would begin), and I give a short course in Leiden just before that meeting. In the Autumn of 2009, I am co-organizing an Oberwolfach seminar (graduate course) on algorithms in real algebraic geometry, at these courses, I will lecture on the Khovanskii-Rolle continuation algorithm.

Following on from the 2006-2007 year on applications of algebraic geometry at the IMA are several activities. One is a semester at the Institut Mittag-Leffler the first half of 2011. I have been invited to attend and plan to spend a substantial part of the semester there, which is in negotiation with my family. This may coincide with a sabbatical leave. To showcase the work in this proposal and more generally applications of algebraic geometry, I will organize one or more special sessions at AMS meetings topics related to this proposal. This will provide a forum for the people working with me to speak on their work to a national audience.

Another, more interesting, activity is that I am heading an application to the SIAM to form an activity group on (applications of) algebraic geometry. This planning will include a major international conference on this topic in the Autumn of 2011.

3. Budget justification

This proposal primarily asks for funding to support personnel who will carry out research under the direction of PI Sottile. Sottile only requests one month of summer salary and support for some project-related travel, as he holds an individual research grant (DMS-070150) for the first 15 months of this proposal that supports his other activities and students. The summer support will enable him to devote the extra time to this project that writing software entails.

The centerpiece of this proposal is the partial support of a postdoc for three years to work on this project with Sottile. This includes 1/3 academic year support and summer salary. Sottile's department will seek to provide additional teaching support for the postdoc, which will probably be two courses per year. This will enable the postdoc to devote sufficient time to this project, as it will involve serious software development.

The second core component of this proposal is graduate student support. It asks for full support of one graduate student, including salary, tuition, and fees. Learning the necessary computer programming (or the math) will require that the student devote themselves full-time to this project. If I attract more than one student to these topics, then this support would be split among the students.

This proposal also requests year-round (40 hours a week at \$12/hr for 12 weeks each Summer, and 10 hours a week for 28 weeks during the academic year) support for an undergraduate research assistant, who will be a full (but junior) member of my research team. This will provide a promising student with interesting job while at University and a continuous direct connection to the workings of research and access to research faculty.

All participants (except Sottile) will need a laptop, and this proposal will also purchase a multi-core server both for development and testing of parallel software and for dedicated computing in our experimental projects. Yearly MAPLE licenses cost \$50 per machine per year.

Team travel to national and international conferences is an important part of this proposal. This includes the MAA-AMS January meeting for the undergraduate student and possibly also the MAA Mathfest. The graduate student will go once to ISAAC or other appropriate international conference, as well as some AMS meetings and national conferences. The postdoc will need to attend the ISSAC conference, SIAM conferences on Numerical Analysis, and some AMS meetings and national conferences. This budget envisions 1 or 2 conference trips per year for the students, 3 for the postdoc (1 international) and 2 for Sottile (1 international). Included in travel are trips to work with collaborators or to bring them to Texas A&M.

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