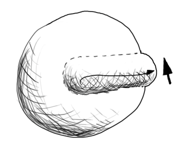
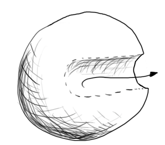
Previous Research

During my time at the University of Texas, I pursued a variety of different research projects, alone and in collaboration, on topics ranging from pure mathematics to exact arithmetic to user interfaces and everything in between. These experiences affirmed my belief that breadth, not depth, is the key to successful research today. Interdisciplinary and vertically integrated projects form the crucial links that bind together the enterprise of knowledge, and ensure its continuation. This philosophy drives and underlies my choice and execution of research.

Graphics Research: 3d Sculpting and Booleans

Anyone who has tried will tell you, making 3d models is hard. Artists need special training and years of experience to use current software effectively. Personally unhappy with the current state of affairs, I instigated a project 3 years ago to create a 3d sculpting program that would be as simple to use as a 2d paint program. From this simple starting point, I delved down into the intricacies of floating point computation, and scraped the austere heights of formal semantics. Along the way, I wrote an entire geometric boolean system from scratch, 3 separate proposals for department funding (2006-2008), and a technical paper (which I am currently revising for submission to SIGGRAPH). For this work, I have received department funding 3 years in a row, best math poster at the Natural Sciences Undergraduate Research Forum, and honorable mention from the CRA's outstanding undergraduate award.





My research on 3d sculpting and Booleans (set union, intersection difference in 3d) is a prime example of the importance of vertically integrated research, working across traditional boundaries. Three years ago, I began with a human computer interaction project, designing new interfaces for sculpting. Since I needed a system on which to test the interface, I was naturally led to building my own Boolean system—existing Boolean systems were either too slow or too unstable (formally not-robust) for interactive use. This led me (a year and a half ago) to investigate geometric robustness techniques, and apply them in a novel manner. The system I wrote is the fastest 100% robust system ever built (to my knowledge) and runs at comparable speeds to non-robust commercial systems. Having addressed the practical problem of fast and robust Booleans, I turned to the theoretical side of the matter. Booleans have been notoriously difficult to get right, making them a good candidate for formalization. Last summer, I devised a geometric extension to Boolean algebra in order to allow formal algebraic proofs of correctness for geometric Boolean algorithms. This formalism was possible entirely because I adopted the mathematical perspective of abstract algebra. When reflecting on my work, I realize that had I restricted myself to just interface, just Booleans, just robustness, or just geometric formalisms, I would not have been able to make as broad contributions. Because I took a particular vertical slice through these problems, I was able to bring novel viewpoints and contributions to these long standing problems.

Communicating Results

In isolation, all research is amazingly unimportant. For this reason, I put a lot of time and effort into presenting my work to others, and listening in return. I took an active role in my department and university's academic communities, using the particulars of my education to inform fellow researchers of diverse backgrounds.

Since arriving at the University of Texas four years ago, I have regularly attended and participated in the graphics group meetings. A month ago, I prepared and presented a two lecture mini-course on the application of classical geometry and exterior algebra in Computer Graphics. These talks helped clarify previously free-floating concepts like affine and projective space, and deepened my labmates' understanding of geometric theories. In a similar vein, I presented Maurice Herlihy and Nir Shavit's work on "The Topological Structure of Asynchronous Computability" to the operating systems research group. At the time, I was taking both distributed computing and algebraic topology. This put me in a unique position to explain the topological content which had eluded many of the systems researchers for so long.

Expanding on the same trope, I firmly believe in sharing my work with wider, less technically adept audiences. In 2007, I joined Junior Fellows, an interdisciplinary undergraduate research society. There, I presented on both my Booleans and knot theory research to audiences ranging from English to business to music students. Although I had to elide many technical details, the talks were an excellent opportunity to raise awareness and interest in both computer science and mathematics.

Knot Theory Research: Intrinsically Knotted Graphs

Premature specialization is the root of all evil. Besides computer science, I am also pursuing mathematical research. After taking knot theory last Spring, I began work with Dr. Cameron Gordon and a fellow student on the subject of intrinsically knotted graphs. I identified a new minor-minimal intrinsically knotted graph, and demonstrated that certain techniques to prove this fact would not work by using a computer program to solve a laborious counting problem. This saved a tremendous amount of time where my collaborators had been trying—unsuccessfully—to count by hand. Expanding on this idea, I devised a new proof strategy which allows for rapid exploration of hypotheses using this counting program. My computer science background not only made the programming trivial, but more importantly led me to think of the approach in the first place. Conversely, my growing familiarity with knot theory will better prepare me to recognize potential applications in computing. Although the topic seems rather removed from computation at first blush, knots have already been linked to quantum mechanics, quantum computing and a variety of combinatorics problems—most notably in connection to complexity theory.

Conclusion

These research experiences have left me adamant in my conviction to work across traditional disciplinary boundaries. Cross disciplinary work is very important to the health of computer science as a field. In mathematics, sub-specialization has become a huge impediment to effective communication. As Bill Thurston, a famous geometer, put it in his essay *On Proof and Progress in Mathematics*, "Basic concepts used every day within one subfield are often foreign to another subfield. Mathematicians give up on trying to understand the basic concepts even from neighboring subfields, unless they were clued in as graduate students." Over the coming decades, with the passing of the first generation of computer scientists, the threat of fragmentation will loom larger over computer science. Cross disciplinary work is the best antidote and protection we have against this threat.