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Monday, October 22, 2012

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I am happy to strongly recommend **Simon Rubinstein-Salzedo** for postdoctoral positions, working with any outstanding research group in number theory.

I know Simon from my time at Stanford University, where he was a graduate student and I was a postdoc. Our research interests are not identical – I am mostly an analytic number theorist and Simon is mostly algebraic – but each of us was also interested in the other's specialty (indeed, Simon frequently attended the analytic number theory seminar), and I spent a lot of time discussing math with Simon.

One opportunity was in a graduate course in additive combinatorics which I taught, and which Simon attended. Although I was eager to try this out at my department's suggestion, it quickly became apparent that I was not legitimately an expert in the subject, and many students lost interest and eventually stopped attending. However, Simon (along with a couple of other students) stuck around until the end, and continued to participate very actively until the end, consistently asking good questions and not allowing me to get away with mistakes. By the end, the course had turned largely from a lecture to a discussion. For example, I chose to present a contemporary paper (Helfgott, *Growth in* $\mathrm{SL}_2(\mathbb{Z})$) which I had not previously read. Simon was constantly asking questions and adding to the discussion, and by the end it seemed that he had understood the ideas of a paper as well as I had.

(There was no written work assigned, so I can't evaluate him on that basis.)

Most of all, I am in a good place to recommend Simon because we have some research interests in common. Simon and I are both interested in some problems in "arithmetic statistics" revolving around counting number fields, torsion elements of class groups, and other objects of number-theoretic interest.

First of all, let me mention that this is an *extremely* active area of research. It is closely related to the ongoing spectacular work of Manjul Bhargava, which could very possibly win him the Fields Medal in 2014. Directly generalizing work of Gauss, Bhargava formulated "higher composition laws", and he, his students, and his collaborators have proceeded to prove a variety of related fascinating arithmetic density theorems.

For example, consider the following question. Let K be a "random" number field. Then the ideal class group $\mathrm{Cl}(K)$ is known to be a finite abelian group. Consider its p-Sylow subgroup $\mathrm{Cl}_{p^\infty}(K)$ and an arbitrary abelian group A whose order is a power of p. What is the probability that $\mathrm{Cl}_{p^\infty}(K) \simeq A$?

In 1983, Cohen and Lenstra presented a family of conjectures (the *Cohen-Lenstra heuristics*) which answer the question for any A and p. Roughly speaking, the answer is that all possible groups G occur with probability inversely proportional to $|\mathrm{Aut}(A)|$.

To make this precise it is necessary to give a definition of "random". One widely studied situation is the following: For a parameter X, suppose that K ranges over all quadratic fields with $0 < \pm\mathrm{Disc}(K) < X$. Then, for any group A and prime p, the proportion of such fields with $\mathrm{Cl}_{p^\infty}(K) \simeq A$ should tend to a limit as $X \to \infty$, as predicted above.

Our knowledge in this (or any related) situation is quite feeble. For 2-torsion groups a satisfactory answer is provided by Gauss's genus theory, for 3-torsion we at least have a good handle on the average size on

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the 3-torsion subgroup $\mathrm{Cl}_3(K)$, and Heath-Brown has proved some results regarding 4-torsion – but beyond this, essentially nothing is known.

However, this has not deterred active interest in the Cohen-Lenstra heuristics. Indeed, two years ago I was one of forty attendees at an AIM workshop devoted entirely to the Cohen-Lenstra heuristics, which attracted leading lights such as Bhargava, Cohen, and Lenstra, and where Simon was one of the speakers.

At the workshop it was painfully apparent that we must further develop the related algebraic machinery if we want to make further progress, and Simon has a preprint (*Invariants for A_4-fields and the Cohen-Lenstra heuristics*) which does precisely that.

For the sake of brevity, consider the following special case (which Simon highlights, but his paper is not confined to a discussion of this case). Consider *cyclic cubic* fields with 2-class group $C_2 \times C_2$; the Cohen-Lenstra heuristics predict that these should consist of roughly 7.6% of cyclic fields, but Gunter Malle computed from numerical data that the truth appears to be closer to 13%. Why should this be? More generally, the Cohen-Lenstra heuristics predict that the average number of surjections from a 2-class group of a cyclic cubic field to $C_2 \times C_2$ should be $1/4$, but the truth appears to be $1/2$. Malle predicted that this should be due to the presence of 2nd roots of unity in the ground field.

More precisely, Simon argues that this discrepancy may be explained in terms of the *reduced Schur multiplier*, which to my knowledge is defined for the first time in his paper. The *Schur multiplier* is the second group homology $H_2(G, \mathbb{Z})$ (as usual, related to central extensions of G) and the *reduced Schur multiplier* $H_2(G, c, \mathbb{Z})$ is the quotient of G by a certain commutator group associated to a conjugacy class c of G.

Then, motivated by geometric considerations in the function field setting (related to connectedness of a related Hurwitz space, recently studied by Ellenberg, Venkatesh, and Westerland), Simon associates an invariant $\mathfrak{z}(\varphi)$ to each surjection $\varphi : \mathrm{Cl}(K) \to C_2 \times C_2$ and conjectures that this invariant is equidistributed over $H_2(G, c, \mathbb{Z})$. Simon presents and implements an algorithm for computing this invariant (which is far from trivial), and presents substantial numerical data in support of his conjecture.

Simon's other two other preprints (both on the arXiv) are less closely related to my active research interests, and I have not yet had the chance to study them closely. Nevertheless, I find the ideas quite interesting. In *Totally ramified branched covers of elliptic curves*, he describes a technique for computing totally ramified covers of elliptic curves, with specified monodromy, which are branched at one point. Over $\mathbb{C}$ an elliptic curve can be written as $\mathbb{C}/\Lambda$ for a lattice Λ, which may be visualized as a parallelogram with the edges identified. A cover branched at one point is called an *origami*, and these may be visualized as sets of multiple parallelograms, together with combinatorial identification explaining how the edges are identified. Such diagrams carry a wealth of combinatorial information, allowing one (for example) to compute the monodromy at the branch point.

Skipping to the punchline, these diagrams allow him to explicitly compute examples of such branched covers, far from a trivial task. Once formulas are written down, the proof that these examples satisfy the required conditions is not difficult; the most interesting section of his paper is in the last section, where he

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describes certain degeneration techniques that allowed him to discover these examples.

His last preprint, *Period computations for covers of elliptic curves*, addresses a related problem, and he produces an explicit example of a branched cover of an elliptic curve ramified at one point, whose local monodromy is a 3-cycle. As in the previous paper, the proof is easy, and the interesting part of the paper describes how he found such an example. His work applies techniques borrow from transcendental number theory, first computing the equations of his branched cover as real numbers to high precision, and then using techniques from continued fractions to determine the algebraic numbers approximated by these real numbers.

Several things stand out to me in Simon's application. The first is his expertise in algebraic number theory, interpreted quite broadly and including its relation to other topics within and beyond number theory. I am myself quite interested in algebraic number theory; currently I am writing my second and third paper on the subject. But I would rate Simon's knowledge of the subject to be above my own.

The second is his natural and overwhelming sense of mathematical curiosity. It is quite common for mathematicians to develop tunnel vision, and to only care about subjects close to their own research. Simon is very much the opposite: he was a regular at Stanford's tea hour, and he was an absolute delight to discuss any subject in math with.

The third is his attention to detail. Simon was interested in some of my own preprints dealing with various aspects of counting fields and related problems. I sent them my preprints, and responded with a great deal of insightful, relevant, and detailed feedback. My papers are definitely better because Simon read them and offered suggestions.

Finally, I would like to draw attention the large number of extracurricular math activities on his CV. For example, he spent four summers teaching high school students topics such as algebraic topology, number theory, abstract algebra, and *yes, you read that correctly, algebraic topology*. Although I spent only one day doing this with him (at the Julia Robinson Math Festival), I have absolute confidence that Simon inspired and excited his students, and left them a seed of genuine understanding of advanced mathematics. Simon's enthusiasm was matched only by his tirelessness: please look at the long list of outreach activities on his CV. If you are looking to increase your department's outreach profile, I would be surprised if any of your other applicants can begin to compete with Simon.

I would be neglectful to not also mention that I quite like Simon as a person, find him engaging and wonderful to talk to, and have always found him to be kind, modest, and friendly. (And I regret that I never dared to challenge him in chess, even if a match would have ended quickly and badly for me.)

Simon is currently employed at a one-year position at Dartmouth. He was not as successful on the job market last year as I expected (I was shocked he was not offered a three-year position), and I was very surprised when we almost had the chance to hire him at the University of South Carolina. If our department had a position this year (unlikely) I would strongly push to hire Simon, but I anticipate that he will have far

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more enticing offers.

His one weakness as a candidate last year was probably that he did not work very ”professionally”: it seems he spent his time engaging in outreach and simply learning interesting mathematics, without regard to whether it might lead to publications. Faced with the realities of the job market, he has made up for this weakness in a hurry: he has finished the three preprints described above (all on the arXiv), presumably with more to come soon.

As a professor at a mid-tier institution, I suspect that Simon is about as strong of a candidate as I will ever have a chance to recommend. His research and scholarship are very strong, and the depth of his outreach efforts is startling. Simon is a fantastic mathematician with a very bright future, and within a few years I anticipate that he will earn a tenure-track position in a strong Ph.D.-granting research university. He has my overwhelming recommendation.

Sincerely,

Frank Thorne

Frank Thorne
Assistant Professor of Mathematics