Dror Bar-Natan's Research Statement for 2005

http://www.math.toronto.edu/~drorbn/Misc/ResearchStatement05.pdf

This is a quick yet sincere introduction to the mathematician Dror Bar-Natan. You'll find it interesting if you are my student or if you are considering becoming my student or if you are a referee writing a letter about me.

Over the next few years I'm planning to work on the following subjects:

1. Khovanov homology, Khovanov-Rozansky homology and categorification of everything in sight.
2. Finite type invariants and related topics.
3. The Knot Atlas.
4. The omath.org project.
5. Other topics.

Khovanov, Khovanov-Rozansky, Categorification

In the 1980s a group of people, lead by Jones, Drinfel'd, Witten, Reshetikhin, Turaev and Vassiliev revolutionized knot theory (and other parts of low dimensional topology) finding a vast array of new and unexpected knot (and 3-manifold) invariants. Much work had gone into understanding these new invariants. A lot remains open, but yet, by the end of the 1990s it seemed that the surprise wore off and we got used to the fact that knots were related to Lie algebras and to quantum field theory; we even came to understand this relationship quite well.

Then in 1999 came Khovanov and got us all confused once again (confused is of course the best state a mathematician can be in; the struggle out of that state is the primary drive for progress). He found a chain complex, naturally associated with knot diagrams, whose homology is a knot invariant and whose Euler characteristic (interpreted in an appropriate way) is the good old Jones polynomial that started the revolution of the 1980s.

Why is that so exciting?

The first and probably least significant reason is that the newly discovered homology theory is a stronger invariant than the Jones polynomial and it is computable (though not too easily) even for pretty large knots. Thus we can expect years of study and hundreds of papers establishing this or that property of Khovanov homology for this or that class of knots. I have taken a share of that loot already and I hope to grab more over the next few years. My students will surely help.

The second reason is much better. Generally speaking, homology is "functorial". A map between spaces provides no relationship between their Euler characteristics, but always yields a map between their homologies. Without this we wouldn't be proving the Brouwer fixed point theorem in the first class of every algebraic topology course; it is the primary reason why homology is interesting. The excellent news is that Khovanov homology is likewise “functorial”, for the appropriate (4-dimensional) notion of “morphisms” between (3-dimensional) knots. Hence we can expect Khovanov homology to be qualitatively better than the Jones polynomial, leading to much more interesting topology. The early signs (a lovely theorem by Rasmussen) suggest that this is indeed the case. There ought to be further applications to the functoriality of the Khovanov homology and me and my students aren't going to just sit there while everybody else is having a free lunch.

The third reason is the most speculative, yet IMHO it is by far the most exciting. Nobody expected Khovanov homology. The Jones polynomial has its natural place in the world of quantum algebra and topological quantum field theories. Khovanov homology yet doesn't. Could it be that Khovanov homology is an accident? Not really, for in 2004 came Khovanov and Rozansky and showed that the HOMFLY polynomial has a lift to a homology theory, much like Khovanov lifts Jones. So the reasonable expectation is that Jones and HOMFLY lift to homological theories because their context, or at least a part of their context, can be lifted. That context is Lie algebras, quantum algebra and quantum field theory; we can now fairly expect that these great subjects are merely the “Euler” shadows of even bigger structures. Math hardly ever gets more exciting than this. The young and smart and the old and wise are converging and they will eventually unravel these bigger structures for everybody's joy. But it's in the back yard of what I've always done and I may still have something to say before it gets too crowded.

I should add a word about the Khovanov-Rozansky homology (KRH), whose Euler characteristic is the HOMFLY polynomial. There is something extraordinary about the KRH construction. KRH associates a complex with an ordinary differential satisfying $d^2=0$ to a knot or a link. But to a tangle, a “knot part”, it associates a differential satisfying $d^2=\omega$ where $\omega\neq 0$ (these are so called “matrix factorizations”). There is a general nature to the KRH use of such non-standard differentials. It seems surprising to me that such differentials were not used previously as steps towards the construction of “honest” differentials, and it seems unlikely to me that non-standard differentials will not find future applications. Yet while the idea behind those non-standard differentials is simple, there is no simple and conceptual explanation for why they must arise and the way they arise in “categorifying” the Lie algebra sl(n) which lurks behind the HOMFLY polynomial.

I know my size. The big dream of categorifying all of quantum algebra is too big for me. I'll be happy to watch and add my iota, but others will do the bulk of the work. But a simple and conceptual explanation of the KRH construction is within my range and had been my primary objective over the last two years. I remain very far but I remain convinced that when fully drawn, the picture will be beautiful.

Finite Type Invariants

Yesterday's fashion may be today's anachronism. Yet regarding finite type invariants I still have a number of things I'd like to know and a number of things I'd like to do.

1. I'd like to see a perturbative Chern-Simons construction of a universal finite type invariant of knotted trivalent graphs. This, along with a resolution of the “framing anomaly”, will lead to a clear and conceptual understanding of the conjectural equality of the Chern-Simons and the Kontsevich constructions.

2. Algebraic structures on spaces of knots lead to constructions of knot invariants – given an algebraic structure you present knot theory using generators and relations and then to produce an invariant, you merely have to "guess" its values on the generators in a way so that the relations are satisfied. Several relevant algebraic structures have been proposed in the context of constructing a universal finite type invariant, using things like braids/groups, tangles/planar algebras and knotted trivalent graphs / graph algebras. Some of these proposed constructions work, some don't, yet hardly anything is written up and there is no a-priori understanding of which constructions should / shouldn't work. Along with my students I hope to improve this situation.
3. Goussarov-Polyak-Viro show that every finite type invariant has a Gauss sum formula. I believe their result, but I also believe there must be a much simpler way to demonstrate it and it must be possible to make the procedure for finding Gauss sum formulas effective (i.e., to program it). Along with my students I hope to contribute here.
4. A few hundred papers have been written about the Kontsevich integral. There are effective ways to compute it, at least at low degrees, yet no one seems to have done so systematically. Along with my students I hope to do so.

The Knot Atlas

The Knot Atlas is a web-based knot theory atlas and database set-up by myself and by Scott Morrison and served to the Internet from a computer under my desk (visit it at http://katlas.math.toronto.edu!). The Knot Atlas is designed as a community project (a "wiki") - anyone can edit and add. With nurturing and care it will become the glue that holds the knot theory community together and the repository for all knowledge on specific knots and links. Along with students, I plan to provide the necessary nurturing and care over the next few years.

omath.org

Over the last 15 years I have written dozens of computer programs to do all sorts of mathematical things. Here's a two point summary of what I've learned:

1. Computers are an extremely valuable tool for mathematical research. So much of what we do is computable, and actually computing it very often leads to new conjectures and insights.
2. Our computational tools are miserably inadequate.

We consider computers to be **outside** of our field rather than a part of it, hence most of us know nothing about them. We (as a group) are happy when an undergrad writes a program to compute something for us; this done, we are happy to forget the program and use only the results. Hence a coherent uniform framework for mathematical computation does not yet exist. So every time I try to compute some complicated homology, I have to teach my computer linearity, Gaussian elimination, and tensor products practically from scratch. Having done so too many times already, I became reasonably good at it and rather quickly I can come to the main point, whatever the main point is at any given time. But for most mathematicians and most students of mathematics the entry barriers are way too high, their education is largely irrelevant and they get no credit for the effort. Hence so many math papers describe what amounts to be an algorithm, and so few actually implement it.

Thus in my opinion one of the biggest challenges in mathematics, perhaps the biggest challenge, is to turn computation from a theory to a practice. There is an education component to it, there is a sociological component to it and there is also a very significant research component to it. Two hundred years ago we felt the need for rigor and had to figure out how to implement it (and thus ε and δ were born, and gradually we **all** learned how to use them). Now there is a need for an additional form of rigor, computational rigor. We need to figure it out: What languages to use? How do we make sure that my implementation of tensor products is compatible with yours? How do we turn programming to a cumulative experience, like ordinary scientific publishing? And gradually we all need to learn to appreciate computational rigor.

(Just to be clear – my notion of "computational mathematics" goes way beyond numerical analysis, in which perhaps the issues are more adequately understood. The computations I do and the computational framework I miss is entirely in the pure side of mathematics: vector spaces, algebras, homologies, odd base fields and rings, combinatorial and graph-theoretical issues and the like).

But isn't Mathematica™ there already? And Maple? Unfortunately, while useful in the short term, the current crop of commercial mathematics programs is inadequate. In fact, they are an obstruction to real progress. Being commercial, these programs are closed; we cannot inspect, verify or modify their internals. We cannot trust their results and we cannot improve them when we learn better ways to do things. It's as if Cauchy and Legendre had the copy rights on ε and δ, and the rest of us had to pay them fees to use their notation and could use it only as prescribed by the original authors. The presence of the current crop of closed programs on one hand lowers our incentive to write open ones, and at the same time the out-of-community nature of these programs means that we are reluctant to build on top of them and to educate using them.

As my tiny contribution in this direction I plan to continue to contribute, directly and with the help of my students, to the omath.org project (which is currently lead by Scott Morrison and Yossi Farjoun and is hosted on a computer under my desk). The phrase "omath" stands for "open math". It aims to be an open source community developed replacement of Mathematica™ , Maple and their likes and a small step towards the eventual integration of computations into pure mathematics. See http://omath.org.

Other Topics

With luck, "other topics" will be most of what I'll do over the next few years. The best research cannot be planned in advance; I'm happy with my plans, yet if I'll end up doing something else, it's only because it'll be better!