UNIVERSAL MANIFOLD PAIRINGS IN DIMENSION 3

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Around 2004, Mike Freedman, inspired by a question of Alexei Kitaev’s, became interested in universal manifold pairings. This story is mainly about Mike’s tenacity but, to put it in context, I first need to explain what universal manifold pairings are.

Our starting point is the notion of a $d$-dimensional topological quantum field theory (TQFT). If you are interested in TQFTs (and many people are), then you should also be interested in universal manifold pairings. Manifolds of dimension $d$ have a cut-and-paste structure: they can be glued together along parts of their boundaries to yield new manifolds. For example, when $d = 2$, we can glue two disks together and obtain a sphere. A TQFT assigns an algebraic object (for example, a number or a vector space) to each manifold, in such way that gluings corresponds to algebraic operations such as inner products and tensor products (depending on the dimension of the manifold). For example, every closed (that is, with an empty boundary) $d$-dimensional manifold is assigned a complex number, every closed $(d - 1)$-manifold is assigned a vector space, and a $d$-dimensional manifold with boundary is assigned a vector in the vector space associated to its $(d - 1)$-dimensional boundary. If we glue together two $d$-manifolds with the same boundary to obtain a closed $d$-manifold, the number assigned to the glued-up manifold is equal to the inner product of the vectors assigned to the two pieces. A TQFT also assigns fancier algebraic objects to manifolds of dimension $d - 2, d - 3, \ldots, 0$, but here we will only be concerned with the $d$- and $(d - 1)$-dimensional parts of the TQFT. There are many different $d$-dimensional TQFTs, each making different assignments of numbers, vector spaces, and so on.

Some natural questions arise: To what extent does the TQFT’s replacement of topological objects with algebraic ones lose information? Can we find two different $d$-manifolds with the same boundary such that any possible TQFT assigns to these manifolds equal vectors? One way to address this type of question is to form a sort of universal TQFT. Instead of numbers, we use the ring $R$ of formal linear combinations of closed $d$-manifolds. Instead of a vector space, we assign to a closed $(d - 1)$-manifold the $R$-module $V(S)$ of all formal linear combinations of $d$-manifolds bounded by $S$. There is a pairing $V(S) \times V(S) \to R$ induced by gluing $d$-manifolds together along $S$, yielding closed $d$-manifolds. This is the universal manifold pairing we spoke of.

For a fixed dimension $d$, we can ask whether there exists a $(d - 1)$-manifold $S$ for which the universal pairing on $V(S)$ has null vectors, that is, vectors $x \in V(S)$ such that the pairing of $x$ and $y$ is zero for all $y \in V(S)$. Such null vectors $x$ would lead to universal relations that will hold in any particular TQFT. A weaker thing to ask for is a norm-zero vector $z$ in $V(S)$, that is, a $z$ which, when paired with itself, yields
zero in $R$. Such a norm-zero vector would lead to relations that hold in any TQFT with positive-definite inner products.

It is easy to show that for $d = 1$ or 2 there are no norm-zero vectors for the universal pairing. Mike’s first result (along with five coauthors — Mike is generous in sharing credit) was that null vectors do exist for $d = 4$ [Freedman et al. 2005]. Building on this result, one can show that any two closed homotopy-equivalent simply connected smooth 4-manifolds have the same TQFT invariants for any positive-definite TQFT. This negative result shows that, if you hope to use TQFTs to study exotic smooth structures on 4-manifolds, you will have to try something more complicated than the simplest strategy of employing a positive-definite 4-dimensional TQFT. (For example, you would need to try a non-semisimple TQFT, or a 5-dimensional TQFT.) Soon after, Kreck and Teichner [2008] showed that such norm-zero vectors exist for the universal pairing in dimensions 5 and higher as well.

This left universal pairings in a state roughly similar to the Poincaré conjecture (pre-Perelman). In the 1- and 2-dimensional cases, it was easy to show that norm-zero vectors did not exist. In dimensions 4 and higher, they did exist. Dimension 3 was the last remaining case, and it looked to be quite difficult.

In 2007, Mike told me he had a strategy for attacking the 3-dimensional case. A recent result of Agol, Storm and Thurston [2007] (building on work of Perelman) showed that there were no norm-zero vectors if one restricted to hyperbolic 3-manifolds glued along a geodesic boundary. The geometrization theorem for 3-manifolds tells us that any 3-manifold can be cut along spheres and tori, to yield pieces which are either spherical, Seifert fibered, or hyperbolic. Thus, a generic strategy for proving something about all 3-manifolds is to (1) prove it for spherical 3-manifolds, (2) prove it for Seifert-fibered 3-manifolds, (3) prove it for hyperbolic 3-manifolds, and (4) show that these three special cases can somehow be combined when gluing along spheres and tori. Typically, steps (3) and (4) are the hardest parts. In the present case, Mike saw a way to solve a special case of (3), and boldly assumed that the remainder of (3), as well as (1), (2) and (4), would fall into place. Mike’s plan was that he and Danny Calegari would finish step (3), while Mike and I would handle the combinatorial arguments of step (4).

I was privately quite skeptical of this. His arguments that step (3) (the hyperbolic case) was nearly done didn’t convince me, but I’m not an expert in hyperbolic 3-manifolds, so I was willing to concede that part. I was more concerned about step (4), which I thought would be a horrible mess. I remember feeling a bit smug and superior, thinking “Poor Mike, he’s wasting his time on this intractable problem instead of working on more realistic projects.” But Mike had been very generous to me over the years, so I felt I owed it to him to work on this despite my skepticism.

Mike would present a large number of ideas, and I would respond with (almost) the same number of counterexamples to show that these ideas couldn’t work. I was expecting that he would eventually run out of steam and leave me in peace but, instead, he kept going and going. Some small portion of his ideas seemed to indeed work, and the foundations of a complete proof began to slowly accrete.

Meanwhile, Danny was having a similar experience with Mike on the hyperbolic side of the argument. In order to feed into step (4), we needed to extend the Agol–Storm–Thurston result to cusped hyperbolic 3-manifolds. In Danny’s words,

One thing that impressed me is how sanguine Mike was about the technical obstacles we had to overcome. Our first strategy, to avoid having to prove the strict volume inequality for the
cusped hyperbolic pieces, depended on using eventual strictness for a sequence of orbifold fillings converging to the cusped guy. The problem is to compare the orbifold fillings for the double and the twisted double; there is no easy cut-and-paste comparison, because the “surface” is now a complicated 2-complex with interesting singularities along the orbifold. In the case where the orbifold filling has even degree, one gets a smooth immersed surface in a manifold cover, and, if one knew the surface subgroup was subgroup-separable, one could try to do cut-and-paste in a suitable finite cover. The first step of this potential argument depended on proving some version of LERF—I remember going through dozens of variations on the idea of using some weakened notion of LERF that would do what we wanted: passing to an amenable separating cover and doing some sort of averaging, replacing the hyperbolic manifold with a solenoidal hyperbolic lamination in which things were separated, etc. After trying about 20 variations on this idea over 8 months, it was clear that this just wasn’t going to work, and I was basically ready to give up. But Mike just said, “well, let’s just prove the strict volume inequality for the cusped guys,” and we were instantly off thinking about Ricci flow for singular metrics on noncompact manifolds with no uniform bounds on injectivity radius...”

Eventually, after many ups and downs, we had a proof. My initial assessment that the project was a big waste of time proved to be quite wrong. I don’t think I misjudged the difficulty of the problem. Rather, I misjudged Mike’s tenacity and resourcefulness. Here’s Danny again:

The great thing about this was how Mike was able to sustain this optimism and just try idea after idea after idea, never discounting an idea because it involved using/learning/generalizing some hard technical machinery, never discounting an idea because it seemed to depend on solving some known “hard” problem, but being prepared to drop an idea and move on to the next one when it didn’t work out.

References


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