Gauthier Update by Moskovich

November-09-10
11:40 AM

Following http://ldtopology.wordpress.com/2010/11/09/gauthier-update/

Low Dimensional Topology

not Fatal, it was never more than a normalization issue in the "input" to LMO/Århus.

November 9, 2010

Gauthier update

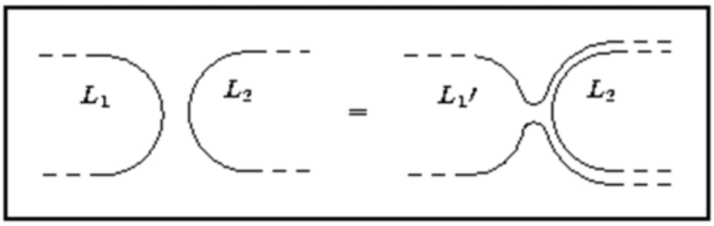
Filed under: Uncategorized — dmoskovich @ 11:09 am

A few weeks ago, we discussed a preprint by Renaud Gauthier which claimes a fatal error in the construction of the LMO invariant, having to do with invariance under the Kirby II move of a certain renormalization of the framed Kontsevich invariant which is used to construct the LMO. In this post, I would like to take stock, to explain Gauthier's claim and Massuyeau's response, and to tell you where I think we stand now.

LMO review

The foundation of 3-manifold quantum topology is Edward Witten's Quantum Field Theory and the Jones Polynomial, in which he constructs a Jones polynomial for 3-manifolds at the physical level of rigour. A whole mini-discipline of mathematics sprang up around understanding Witten's invariant mathematically, and we're still a long way from achieving that goal. If we restrict and restrict and restrict, until we only consider integral homology 3-spheres and the contribution of the trivial flat connection to Witten's invariant, then (modulo various minor issues) Witten's invariant becomes (equivalent to) something called the Reshetikhin-Turaev invariant. These invariants- Witten's invariant, the Reshetikhin-Turaev invariant, and indeed the LMO invariant- are built by exploiting the fact that a 3-manifold can be represented via Dehn surgery as a framed link in S^3. Topologically, this works by considering $S^3\times[0,1]$ with the framed link sitting in $S^3\times\{1\}$, and attaching a 2-handle to each link component as specified by the framing. The top boundary of the space which you end up with is your 3-manifold M. For intuition, think one dimension down, where you would be attaching 1-handles to pairs of points on the top boundary of "disk cross unit interval" to obtain some surface as the new top-boundary. Kirby's theorem tells us that two framed links present the same 3-manifold M only if they are related by blow-ups and by Kirby II (handleslides):

Nay, R-T is not just the flat connection. Though we mostly do perturbation theory just near the flat connection.



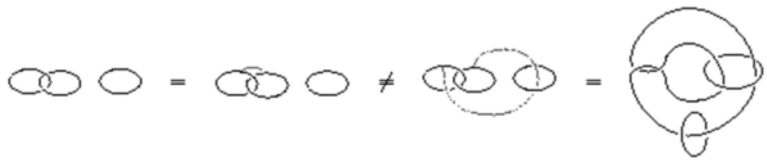
The recipe for constructing a 3-manifold invariant from a surgery presentation L (*i.e.* from a framed link) is to take your favourite link invariant, to extend it to a framed link invariant, and to mod out its values by relations induced by the Kirby moves. The best case is when your framed link invariant is invariant under Kirby moves. Quantum invariants are local (recall as we discussed here that this is perhaps their defining property), so the above recipe can be carried out explicitly.

The universal finite-type invariant for links is the Kontsevich invariant. You can extend it to a framed link invariant $\hat{Z}_f$. There's a standard way to make it invariant under Kirby I. But- and this is the key point- $\hat{Z}_f$ is not invariant under Kirby II. A key insight of Le, Hitoshi and Jun Murakami, and Ohtsuki (I don't know whose insight it was first… but one of these people) is that $\hat{Z}_f$ DOES become invariant under Kirby II if you renormalize by summing in a certain element ν for each component of L, which converts $\hat{Z}_f$ into a new framed link invariant $\check{Z}_f$. Everything works out wonderfully and you get the LMO invariant, which is the universal quantum invariant for 3-manifolds. It fits in well with 3-manifold topology: it recovers the Reshetikhin-Turaev invariant and the Alexander polynomial, and its degree 1 part recovers the Casson invariant.

Gauthier's claim

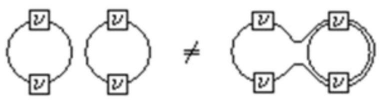
Renaud Gauthier claimed that the normalization of $\check{Z}_f$ is wrong, and that it is not invariant under Kirby II moves. The claim was then that each component should have been summed in with an element ν^{-1} instead of with ν; and that doing so makes the framed Kontsevich invariant of L invariant under Kirby II.

There's something which we have to understand about Kirby II before evaluating this claim: namely that the handleslide is not a well-defined move between links. Consider two link components $K_{1,2}$ which are a long way apart, and between are lions, tigers, and bears. To slide K_1 over K_2, first a part of it must be brought near a part of K_2, and there are many ways of doing this. Two different approach paths may lead to two different links. Consider:

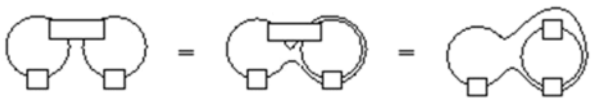


To upgrade Kirby II to a well-defined move on links, which is a prerequisite for calculating the correct normalization, a path between a point in K_1 and a point in K_2 must be specified. LMO do this by equipping K_1 and K_2 with points which are infinitesmally close together. Gauthier doesn't do this, and he arrives at different results. Observe:

In Gauthier, K_1 and K_2 are far apart (Proof of Proposition 4.1.1, pages 58-59):



But in LMO they are close together:



Massuyeau said this in a comment to the last post; and it accounts for the disrepancy between Gauthier's results in Section 4, and LMO.
This is unsurprising given that, computationally, the LMO construction is compatible with the Reshetikhin-Turaev invariant, and therefore with Witten's invariant. It would shake our very view of physics if it were to contain a substantial error.

The end of the story?

Despite the apparent failure of Gauthier's claim of an LMO error, this affair does raise a number of important and disturbing points:

1. The language of LMO is very confusing. The calculations always seem to hang by a narrow thread, and the fact that everything cancels out at the end seems little short of a miracle. A better language is sorely needed; and LMO needs to be rewritten in that language. Zsuzsana Dancso is doing this.
2. I don't know a conceptual reason why $\hat{Z}_f$ needs to be renormalized, and why the renormalization factor was

Dancso is only re-visiting LMMO, no LMO

to be ν for each link component (as opposed to ν^{-1} or to something entirely different). Thus our confidence that the LMO normalization is correct relies on computational verification as opposed to on understanding. This is unsatisfactory.

3. Perhaps the LMO normalization is not unique? It is a-priori possible that many different renormalizations of the framed Kontsevich invariant could give rise to different 3-manifold invariants. None of these (except for $\check{Z}_f$) wod be compatible with physics, but so what? It doesn't mean they can't exist!

This, of course, is very unlikely.

These are the important foundational questions which Gauthier's preprint raises. They can no longer be ignored.

Comments (1)

1 Comment »

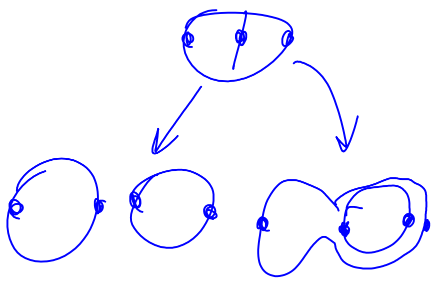
1. For point (2), there is a good explanation. If you look at what's happening on the physics side, this factor of ν comes from the invariant of the solid torus. Namely, if you do surgery on a knot, you're cutting out a solid torus and gluing it back in in another way. The invariant of the glued manifold is the pairing of the invariant of the complement and the invariant of the solid torus. The invariant of the complement is essentially the non-renormalized Kontsevich integral, and the invariant of the solid torus is that of the unknot, namely ν. You have to think a little more to see that the pairing is the right pairing.

   I mostly worked all this out some years ago, but never had an excuse to write it down.

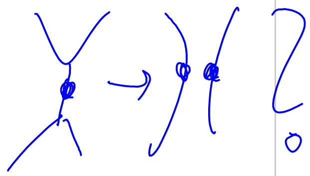
   Comment by Dylan Thurston — November 9, 2010 @ 11:26 am | Reply

I'm not sure I understand, but anyway, I'm not sure I like this.

The LMMO statement is entirely knot-theoretic, and it ought to have a conceptual explanation entirely within knot theory. Assuming the "dots" business that Dancso and I are close to finishing, we have a reasonably concise explanation of LMMO:



But why must unzip aquire dots?



This we only understand at a computational level.