Review.

I would like to mention that, in Section 7.2, the authors misunderstood Milnor's result, as well as the result of [AM]. Indeed, Milnor didn't prove, as they claimed, that links up to link homotopy are characterized by their reduced peripheral systems, he only proved it for links with less than 3 components, and this is not true anymore for links with 4 components or more. And, what is actually proved in [AM] is that reduced peripheral systems do not characterize the link-homotopy class of links, but the link-homotopy class of their 4-dimensional spinning.

My opinion is that the characterization of classical braids as the only OU-presentable tangles is a new and interesting result, but it is only a small part of the paper. Two-thirds of the paper is devoted to the alternative proof of Chterental's theorem (and its comments). This is a significant and worthwhile simplification of it but, as a matter of fact, it is not a new result.

Also, the implementing section 6 surely do contain some "funny" computational outcomes, but as it takes almost one third of the paper; maybe the details of the Mathematica routines could have been postponed to some appendix or external reference. And above all, Section 2 is written is a very disturbing way. The authors start by stating obviously wrong results as a way to introduce their tools. I can't see the point of doing that when they could have just presented their gliding process and discussed then whether it converges or not.

In conclusion, I certainly think the two above-mentioned results deserve to be published, but I guess the paper needs to be reworked and maybe it does not hit the standard of CJM.

Response.

Dear Editors,

We accept your decision not to publish our paper in CJM. Indeed, we blundered in misunderstanding the results of Milnor and of Audoux-Meilhan (though these play a very minor role in our paper). Indeed, a significant part of the paper is devoted to re-formulating and re-proving the results of Chterental. The main point of our paper is that “Over-then-Under” ideas repeat in knot theory in many guises, so re-formulating Chterental’s results in our language is fully justified, but we might have gone overboard by taking a few pages to also prove these results from our perspective. And indeed Section 2 is written in a non-traditional style which is seen as “very disturbing” at least by some. With all that in mind, it is fair that our paper got rejected.

This said, we wholeheartedly disagree with the reviewer’s criticism of our Section 6, and we would appreciate it if you could share our comments with them.

Computation is not that thing that we do in the dark when nobody watches, or assign to our summer interns, hoping never to see the gory details. Rather, when a bit of mathematics is computable, we feel it is a grave omission not to include an implementation, and when the implementation is concise and follows the notation and logical structure of a paper, it can and should be an integral part of that paper. The programs aren't monsters that nobody wants to look at. Rather, if they are simple and complete (as they are in our paper), they are a proof of the overall simplicity and validity of the ideas, and they encourage others to play and further discover. All the more so in the case of our paper, where the implementations lead to new enumerations (the tables in sections 6.2 and 6.3) and to intriguing and appealing graph-valued invariants that cannot be computed otherwise and that may well lead to further study in the future (section 6.4).

Sincerely,

Dror, Roland, and Zsuzsi.