Report on the manuscript “An unexpected cyclic symmetry of $I\mathfrak{u}_n$” by Dror Bar-Natan and Roland van der Veen

Let $\mathfrak{u}_n$ be the Lie algebra of upper-triangular $n \times n$ matrices and consider the Lie algebra $I\mathfrak{u}_n = \mathfrak{u}_n \ltimes \mathfrak{u}_n^*$ where $\mathfrak{u}_n^*$ is regarded as an Abelian ideal acted on by $\mathfrak{u}_n$ via the coadjoint representation.

In Theorem 1, the authors exhibit an order n automorphism of $I\mathfrak{u}_n$. Using natural choices of bases in $\mathfrak{u}_n$ and $\mathfrak{u}_n^*$, the automorphism is given by a permutation of basis vectors shifting the indices along the cycle $(12\dots n)$ and the proof consists in elementary calculations showing that the map preserves the Lie bracket.

In Theorem 2, the authors show that their automorphism extends to an automorphism of $\mathfrak{gl}_{n+}^{\epsilon}$, a deformation of $I\mathfrak{u}_n$. The proof is basically the same.

As far as I can judge, the main output of the manuscript is the very fact that $I\mathfrak{u}_n$ (and its deformation $\mathfrak{gl}_{n+}^{\epsilon}$) has an automorphism of order n. Both the construction and proof are simple. I find the result interesting, however it does not look as exciting as it could because the authors do not explain its significance in possible applications. They only mention that automorphisms of $I\mathfrak{u}_n$ (and similar algebras) are expected to become symmetries of some knot invariants, and that’s all. Thus a convincing motivation is missing.

I think the manuscript (which is in fact a short note) is worth publishing somewhere but I am not sure that “Transformation Groups” with its high standard is appropriate for that.

Comments

(1) I think it would be better to specify restrictions on the ground field even if the construction discussed in the paper is valid for any (almost?) one.

(2) In my opinion, the term “length” does not look quite appropriate for the function $\lambda(x_{ij})$, at least I don’t see any geometrical meaning for it. It appears to measure the distance to the “diagonal” so a name like “depth” or “height” seems to be more suitable. I don’t insist.

(3) In the definition of $\lambda(x_{ij})$ for $i > j$, why not to write simply $n + (j - i)$ or $(j - i) + n$? This would emphasize that the function is just $(j - i)$ modulo n. I don’t insist again.

(4) Before the proof of Theorem 1, the Lie algebra $I\mathfrak{u}_n$ was explicitly described, therefore the commutation relations in the proof can indeed be checked with some explicit computations. However the same plan in the proof of Theorem 2 does not look so much convincing because the Lie algebra $\mathfrak{gl}_{n+}^{\epsilon}$ was described only implicitly, like “there exists a unique bracket on $\mathfrak{g}$ satisfying certain properties”. I think the latter claim requires more explanations or precise references. Otherwise it is unclear how to obtain the relations listed in the proof of Theorem 2 and how to really check them.

(5) References, item [BR]: a typo in “Ressayree”

(6) I don’t understand the role of the material on pages 6–9. The authors call it “Verification Notebook” and provide some *Mathematica* calculations in it for particular cases. At least a summary of those calculations is missing. Without explanations it is unclear how to read and perceive the code. Anyway, I think this kind of stuff is quite suitable for informal discussions of the paper (for example, preprint versions or additional materials on the authors’ websites) but does not belong in a journal article (at least without explanations of why the content is necessary to keep as a part of the publication).