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Major Revisions requested MATH-D-18-00266

Letters in Mathematical Physics <em@editorialmanager.com>
Reply-To: Letters in Mathematical Physics <kanishkaa.sridhar@springernature.com>
To: Mikhail Skopenkov <skopenkov@rambler.ru>

Wed, Jun 17, 2020 at 4:29 PM

Dear Dr Skopenkov,

We have received the reports from our advisors on your manuscript, "Discrete field theory: symmetries and conservation laws", which you submitted to Letters in Mathematical Physics.

Based on the advice received, I have decided that your manuscript could be reconsidered for publication should you be prepared to incorporate major revisions. When preparing your revised manuscript, you are asked to carefully consider the reviewer comments which can be found below, and submit a list of responses to the comments. You are kindly requested to also check the website for possible reviewer attachment(s).

While submitting, please check the filled in author data carefully and update them if applicable - they need to be complete and correct in order for the revision to be processed further.

In order to submit your revised manuscript, please access the following web site:

https://www.editorialmanager.com/math/

Your username is: skopenkov
If you forgot your password, you can click the 'Send Login Details' link on the EM Login page.

We look forward to receiving your revised manuscript before 17 Jul

2020.

With kind regards,
Giuseppe Dito
Managing Editor
Letters in Mathematical Physics

Comments to the author (if any):
While the two reviewers differ in their overall view, they are agreed on the key points that the claims of the paper are given in an overly sensational way and that the mathematics is not presented at the level of generality claimed, nor with the level of rigor claimed. They are also agreed that the results of the paper are (or would be should the mathematical correctness be resolved) quite interesting. This paper will need substantial revisions to be acceptable.

Reviewer #1: The preprint concerns a discussion of well-known field theories in terms of discretisations using mostly cubical constructions and notions from algebraic topology.

The author claims his ideas are far simpler than that of other authors. This claim cannot be verified. There are claims of a discrete energy conservation theorem not based on a symmetry and if true, this would be extremely exciting. However, the exposition is so riddled with imprecisions, mis-defined terms, undefined terms and undefined calculations, that I did not get very far into the paper.

1.1 Quick start. It would help if the author provided Maxwell's equations in 2+1 space. I think there should be 4 equations but there are only 2 discrete ones given, and the reader is left to assume that F is the 2-form in 2+1 space often written in shorthand as E+B. I did not see the point of the T function.

1.3 Main idea. It is pointless trying to motivate and illustrate when none of the terms referred to in the Figures are defined. To illustrate my frustration at trying to guess the quantities in Figure 3: I can guess that f is a face and v a vertex, but what is j? In one formula we have \partial j, indicating j is a chain, and in another we have \delta j, indicating j is a co-chain. This is bizarre, as is the cap-product formula in Figure 3's caption. The cap product is a map taking a chain of degree p and a cochain of degree q and giving you a chain of degree p-q. It is **not** really like the interior product of the exterior calculus, as claimed in Table 1, as the interior product has as its end result a form and not the input to a form. In any event, it is not the case that a cap product is defined on pairs of co-chains and neither does it give you a function defined on a vertex, as such a function would be a 0-co-chain. Unfortunately, the author's own def'n for the cap product, Def'n 2.4 makes no sense as it is written, relying on it does on a lexicographic (undefined here, but looks to be user-defined) ordering of vertices.

The author defines a k-dim'l field or k-cochain as a real valued function on k-faces. Actually, co-chains are linear maps on linear sums of k-faces, However, the author then writes C^k(M;R)=C_k(M;R). But the left hand side of this equation is the standard notation for co-chains, which are functions on chains, while the right hand side is the standard notation for chains, ie the linear sums of the faces themselves. These two spaces are totally different!! Further, co-chains in C^k(M;R) assign constants to each one-element chain, and not, as is implied, a variable function.

It does not seem to me that the author has the standard definitions of what he is talking about clear in his/her own mind. It is simply not the case, as claimed in 1.6, that the paper is written with mathematical rigour. Rigour begins with the standard definitions and with precise statements. I am not averse to the use of diagrams of cubical elements as seen on page 3 as the input to functions, to illustrate a point. But the absolute basics of mathematical rigour involves defining your terms: this includes saying what space every object is in, where the maps are going to and from, and why each calculation is well-defined. It is not up to the reader to guess. It is up to the author to write clearly.

Reviewer #3: Please see attached file