I recommend the paper for publication with minor adjustments.
The paper proposes a new discretization algorithm for classical field theories that recovers several known discretizations in the literature. It states and proves a new discrete Noether theorem. Moreover, it introduces a new concept of discrete $(1, 1)$-tensors and defines a conserved discrete analogue of the energy-momentum tensor, which seems to be an interesting object. The paper is written in an expository style which is understandable also by non-experts and offers plenty of example. More difficult and abstract are introduced in several steps, which greatly helps reading and is very uncommon for a research paper. As such, I think it poses a very valuable addition to the literature.
However, I think that the paper has several limitations. Some of them should be ameliorated before publication, others merely mentioned. I will list my main conceptual objections below and some ideas how to address them:

(1) The introduction (page 2, before subsection 1.1) seems somewhat detached from the rest of the paper. For instance, the principles of discretization (starting around line 15) are not discussed anywhere else in the paper, and it is not clear to me how the discussion in the rest of the paper is influenced by or supports these principles. The introduction should be slightly updated to connect better to the main body of the paper. See point 4 below.
(2) Also in section 1, there are several fundamental claims for which the papers offers no motivation at all. E.g. page 2, line 8: "we think [nature] is discrete rather than fundamental" or slightly below: "we think [spatial symmetries] are approximate rather than fundamental". The paper offers no support for these claims. Nor are these claims supported enough to motivate the investigation in the paper. Such claims should either be dropped or motivated better.
(3) Similarly on page 4, line 32: "reconsider the old idea that the Universe is discrete rather than continuous". This might be personal taste, but I think such statements are sensational, rather than scientific. Generally, I think scientists should be more careful in distinguishing between reality and mathematical models for it.
(4) I really think the paper is lacking a "discussion" section somewhere where the obtained results are put into context with the literature and the principles discussed in the introduction. Partly the material is there in the "limitations", "background"

and "open problems" sections. Such a section should be added, either at the end of the paper or in the introduction.

(5) On a mathematical level: Many objects are defined only for $M = I_N^d$ only. I think this is completely fine for the objective of the paper. But at several points the author claims that the results are easily extended to general simplicial complexes, which I find somewhat nonchalant. I would either give some more details about the generalization or drop these remarks (specifically, Remark 2.16 on page 22, Remark 3.1 on page 25)

Following are some smaller remarks and typos:

- Some figures (2,3) appear before the relevant notation is introduced and are slightly hard to understand without them.
- page 13, line 33: Amper should be spelled Ampère.
- page 16, paragraph in lines 33-36: I think this point is important - it is very confusing at first why a tensor should be a function on $M \times M$ - as opposed to just M. I think emphasizing this point would facilitate understanding of this concept for the reader.
- Definition 2.14, page 20: I think it would be helpful for the reader to recall the relation between A and U here.
- Defintion 2.15, page 20: A slight pedantry: Probably the author means by T_uG the linear *sub*space parallel to the tangent *space* to G at u? The latter is not a subspace, only an affine space.