Report on the paper
"Discrete field theory: symmetries and conservation laws"
by Mikhail Skopenkov

By "discrete field theory" the author means a field theory on a simplicial complex, which is defined via an action on the k-co-chain of the simplicial complex. This action is formulated by summing a Lagrangian over all vertices. Typical physical examples which can be described in this setting are electrical networks (Kirchhoff's law), QED and non-abelian lattice gauge theories. Generally speaking, most continuous field theories (like for example Klein-Gordon or Dirac fields) can be formulated in the setting of "discrete field theory" if the derivatives in the continuum Lagrangian are replaced by suitable difference quotients and then to reformulated with co-chains.

The author gives a nice motivation and proceeds by discussing several examples of physical interest. Then he enters the proofs of his results. The main result of the paper is a conservation law for an energy-momentum tensor (Theorem 1.3), which the authors states to be "completely new." It was also completely new to me. On the other hand, he honestly admits that "it has neither a clear physical meaning nor unique continuum analogue." I must say that I also don't see the connection to the usual energy-momentum tensor. Why is it even called an "energy-momentum tensor"? Or why is it even a "tensor"? I feel that, in order to justify this notion, one should get agreement with the physical energy-momentum tensor at least in the example of lattice gauge theories (at least in the limiting case when the lattice spacing tends to zero). Therefore, the interpretation of the main result seems unclear from the physical point of view.

On the other hand, the author submitted the paper to a pure mathematics journal where the connection to physics is irrelevant. Therefore, the basic question is whether the paper fulfills the criteria of a journal of analysis and calculus of variations. Taking these criteria seriously, I must say that I do not find the methods deep enough for justifying publication. Indeed, the main result is obtained by direct computations with co-chains. I do not see that any analytical tools are used. Therefore, I feel that "Calculus of Variations and PDEs" is not the right journal for the paper. My suggestion to the author would be to try to clarify the physical significance of the "energy-momentum tensor" and then to submit the paper to a more physically oriented journal.