

The author is grateful to the reviewer for the thorough, useful review. The reviewer concisely and absolutely correctly summarized the essence of the paper in the introductory sentence.

Below follow detailed responses to his/her comments.

1. The author agrees with the Reviewer that the approach by Gawlik and Gay-Balmaz seems to be the closest to the one used in the current work. A valid point was made by the Reviewer that this is not correct that "... non-canonical variations can be avoided in the present manuscript because time is kept continuous". The author agrees; however, the paper didn't mean to claim that. Such an impression was, apparently, due to the fact that the paragraph mentioning continuous time immediately followed the citation of the Gawlik and Gay-Balmaz work. To clarify the issue and emphasize the difference between the Gawlik and Gay-Balmaz's work the sentence starting in line 50 is modified as follows:
"This interesting technique differs significantly from the approach pursued in this paper where the action is calculated in the canonical coordinates and there are no restrictions on the coordinates/momenta variations".
2. The author agrees that the procedure of Lagrangian reassignment aka semi-Lagrangian advection is well known for ages. The following sentence is added to the introductory paragraph of subsection 3.1: "The familiar procedure of the Lagrangian reassignment (aka semi-Lagrangian advection, Lagrangian remapping, etc.) is detailed in this subsection in conjunction with the linear interpolation of the coordinates/momenta employed in this work".
3. The term "forward operator" is used in the paper five times. In two cases its relation to the interpolation is explicitly mentioned. In the three other cases (lines 8, 46, 353) a qualification "i.e., the mode of interpolation" is added.
After mentioning "discrete set of parameters" in line 37 the following qualification "i.e., the finite number of degrees of freedom" is added.
4. The author agrees with the Reviewer; "in a broad sense" statement is replaced by "from the standpoint of action minimization".
5. Similar comment (# 21) was made by Reviewer 2. The author agrees that comparison of the numerical results obtained in Sec. 4 vs. established algorithm would be highly desirable. Such undertaking is, however, too hard for a single researcher who doesn't have an access to the existing dynamical cores nor experience in running them. Thus, the only test of validity of the numerical solution in Sec.4 provides conservation of mass. On the other hand, the main purpose of the paper is not to prove correctness of the developed code but to demonstrate feasibility of a code based on the suggested approach including an option of splitting/recombining tetrahedra.
6. Investigation of conservation properties although very important *per se* is beyond the scope of this work. The statement in lines 186-189 only emphasizes their dependence on presence of continuous symmetries.
7. Reviewer 2 was interested in certain details of possible realization of the algorithm. Since corresponding comments take of only a couple of sentences, the author would suggest to leave the sentence in line 274 as is.
8. The phrase "For historical reasons" is removed from the text.