Reviewer 1.

1. *I still have substantial concerns about the fundamentel approach. Treating a non-canonical system in canonical coordinates is non-trivial and requires one to pass to Clebsch variables (cf. J. E. Marsden and A. Weinstein. Coadjoint orbits, vortices, and Clebsch variables for incompressible fluids. Physica D: Nonlinear Phenomena, 7(1-3):305–323, may 1983.). In the non-canonical setting, the work by Gawlik and Gay-Balmaz (and earlier work by Gawlik et al, Pavlov et al.) required considerable technical complexity (e.g. in terms of the Hodge star operator) to make the non-canonical system well defined in the discrete setting. The manuscript (and potentially a longer reply to this reviewer) needs to explain why a canonical treatment is possible although one deals with a non-canonical system/description.*

   Response

   Under a "non-canonical system" the Reviewer, probably, had in mind a Hamiltonian system represented in non-canonical coordinates. Judging from the title, the paper by Marsden and Weinstein mentioned by the Reviewer considers an incompressible fluid, where the Lagrangian description of fluid dynamics provide canonical coordinates only after imposing restrictions. This leads to introduction of Clebsch variables. However, in the case of a compressible fluid considered in the paper the Lagrangian displacements and the products of density and corresponding Lagrangian velocities provides a canonical description. Canonical coordinates are not, of course, unique, and Clebsch variables can be introduced in the compressible case as well, and the 2D numerical example considered in the paper in fact does use them.

   I would like to emphasize that the evolution due to canonical dynamics based on the Lagrangian coordinates is used in the paper within each time step; before next time step the Lagrangian reassignment (aka semi-Lagrangian advection, Lagrangian remapping, etc.) has to be applied.

2. *The manuscript would still substantially benefit from a deeper and more numerical results (see my previous review).*

   Response

   Although the author in principle agrees with this comment, I would like to emphasize that the point of this paper is not a development of a competitive numerical algorithm but rather a demonstration of a feasibility of building an algorithm based on the least action principle. The author believes that the numerical example presented in the paper basically fulfils this task, demonstrating also a capability of a refinement of an unstructured grid in the course

of calculation and account for an orography, which are claimed to be advantages of the suggested approach.

Still, consideration of a well-established example and comparison of the results would, of course, make the paper much more convincing. This is, unfortunately, unrealizable for the author for a practical reason: I am planning retirement in a few months, and corresponding effort would take much longer. Thus, if consideration of a well-established example deemed to be necessary, the paper should be apparently rejected.

Reviewer 2.

The author is very glad to know that the approach considered in the paper is found of interest by an expert in the field, and he is very grateful to the Reviewer for his/her encouraging remarks.

In the introductory part of the review among other comment three important questions were raised: (a) a necessity of considering a "standard" test case (a similar concern was expressed by the Reviewer 1 above; (b) the issue of uniqueness/non-degeneracy of the Lagrangian remapping; (c) consideration of the potential performance benefits of corresponding computer realization of the approach. These issues will be addressed when replying to "Technical considerations" below.

1. The dot symbol was added in Eqs. (17) and (21).

2. A contribution from the boundary term in the second Hamiltonian equation written out after eqs. (16) is absent because this equation corresponds only to the points lying inside the volume (in a more technical terms: since $\delta\vec{\xi}$ is an arbitrary function of coordinates $\vec{a}$, one can set it equal to $\delta\vec{\xi}\delta\left(\vec{a}-\vec{a}_0\right)$ where a point $\vec{a}_0$ lies within the volume. The boundary integral in this case turns to zero. The resulting volume integral of the variation of the Hamiltonian density δH over the whole volume is by definition the variational derivative $\delta H / \delta\vec{\xi}$ at the point $\vec{a}_0$ times $\delta\vec{\xi}$). The contributions from the first (boundary) term in the RHS of (8) contributes only to the points belonging to the boundary of the volume begetting corresponding boundary conditions).

   As far as numerical example goes, the boundary conditions correspond to zero mass flux at the top and bottom points of the atmosphere, and the motion was assumed periodic in horizontal direction (i.e., along x-axis). Corresponding clarifying sentence was added after the old line 340.

3. The simplest way to make sure that conditions (27) select inner points of a tetrahedron is to make an affine transformation of the a-space mapping the vertex $\vec{a}_4$ onto the origin, vertex $\vec{a}_1$ onto a unit along _x-axis (i.e. into the point $\vec{e}_x = (1,0,0)$, vertex $\vec{a}_2$ onto the point $\vec{e}_y = (0,1,0)$ and vertex $\vec{a}_3$ onto the point $\vec{e}_z = (0,0,1)$ of a new Cartesian coordinates. Then (26) becomes: $\vec{r} = \vec{e}_x\tau_1 + (\vec{e}_y - \vec{e}_x)\tau_2 + (\vec{e}_z - \vec{e}_y)\tau_3$, whence $x = \tau_1 - \tau_2$, $y = \tau_2 - \tau_3$, $z = \tau_3$. In the new coordinates the internal points of the tetrahedron are selected by the conditions $x > 0, y > 0, z > 0, x + y + z < 1$ which in fact coincide with (27).

4. To consider the issue of uniqueness/non-degeneracy of the mapping (29), let us consider trajectories of fluid particles. The fluid particle that at $t = 0$ was located at the vertex with coordinates $\vec{a}_i$ at a later moment of time t will be located at a point $\vec{r}_i(t) = \vec{a}_i + \vec{\xi}_i$. Points $\vec{r}_i$ form vertices of a shifted tetrahedron onto which the initial tetrahedron is mapped. Note, that shifts $\vec{\xi}$ of the fluid particles inside a tetrahedron are assumed to be linear functions of the shifts of the fluid particles located at the vertices of the tetrahedron: this is our basic (linear) approximation of the forward operator (see the first paragraph of Sec. 3). Thus, the initial tetrahedron with the vertices at the points $\vec{a}_i$ is linearly (more precisely affinely) mapped onto a shifted tetrahedron with vertices at $\vec{r}_i$; the transformation is linear regardless of the trajectories of the fluid particles at the vertices $\vec{a}_i$ being linear or curved. In particular, faces and edges of the initial tetrahedron are mapped onto corresponding faces and edges of the shifted tetrahedron. Since shifts of the internal points of the tetrahedron are linear functions of $\vec{a}_i$, the Jacobian of the linear transformation of the initial tetrahedron within it is constant (the constants for different tetrahedrons are, of course, also different, and they depend on time t). Thus, piecewise linearity of the forward operator ensures that the mapping of the whole initial volume onto the shifted volume is also piecewise linear, and the mapping is one-to-one provided neither tetrahedron in the course of evolution degenerates (i.e. tetrahedra volumes never turn to zero). The latter is warranted for sufficiently small t, since at $t = 0$ the transformation is identical. A time step has to be sufficiently small anyway, since the shifted tetrahedron after a time step should not deviate too far from the initial one; otherwise numerical integration will be in error. Non-degeneracy of the initial tetrahedrons can be easily checked since trajectories of the fluid particles at the vertices are calculated in the course of numerical integration. Note also that the degeneration of the initial tetrahedron means that density inside it becomes infinite; if time steps of numerical integration are selected correctly, this could never happen.

5. The interesting issues raised by the Reviewer at comment 5 go well beyond my area of expertise. On the other hand, they are of primary importance if the approach under consideration is going to be implemented. Although I quite understand the reason the Reviewer declined my invitation to co-author this paper, this is disappointing to me, and the invitation is still open.

6. The desirability of consideration of established test cases was also raised by the other reviewer. Below I copy my response.

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   Still, consideration of a well-established example and comparison of the results would, of course, make the paper much more convincing. This is, unfortunately, unrealizable for the author for the following practical reason: I am planning retirement in a few months, and corresponding effort would take much longer. Thus, if consideration of a well-established example deemed to be necessary, the paper should be apparently rejected.

   All suggestions listed in “Editorial considerations” are taken into account in the revised manuscript.