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Interactive comment on "Vlasov simulation of electrons in the context of hybrid global models: A Vlasiator approach" *by* Markus Battarbee et al.

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We wish to thank the referee for their input and evaluation of our manuscript. Below, we have included the referee comments in italics and our own response in regular text.

This draft described a Vlasov solver for electrons. This electron Vlasov solver is implemented to work with Vlasiator, which is a Vlasov-hybrid (kinetic ions and fluid electrons) code, together. This electron solver is distinct from a typical Vlasov solver in two ways: 1) the initial plasma and electromagnetic fields are initialized from the Vlasiator simulation results and the magnetic field is fixed during the electron simulation, and 2) the electric field that is produced by the electron oscillation is taken into account for accelerating electrons. The fixed magnetic field limits the applicability of the model to

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short-time simulations. Including the electron oscillation electric field is a novel feature. Including electron dynamics into Vlasiator is definitely import, and I think the result of this research project should be eventually published somewhere. However, this manuscript needs significant improvement before it can be accepted. This is a paper presents the numerical algorithm for the electron solver. The numerical algorithm itself is not complicated at all, but the draft is not well-organized and it is extremely difficult for readers to understand the algorithm.

Thank you for the constructive criticism. We will strive to improve the presentation of the work, as indeed explanation and understanding of the method is what we wish to achieve.

Specific comments:
1. In the introduction part, some descriptions about previous works are not accurate or even wrong. 1) In line 27, the papers cited are not particle-in-cell codes. They are hybrid codes, just as indicated by their titles. In the space physics community, 'PIC' means both electrons and ions are represented by macro-particles.

We believe there may be different sub-understandings of these terms, as we are familiar with terms hybrid-PIC and full-PIC to differentiate between these two approaches. Both approach still include particles tracked across cells. We will clarify the terms in this manner.

2) line 39: Resolving Debye length is required by typical explicit PIC, but not implicit PIC. Please make it clear.

This was noted in the sentence starting on line 40, but we agree it can be misread and shall rewrite this to be more clear.

3) line 41: 'at the cost of loss of some electron physics.' The cost comes from a coarse grid and large time step instead of the implicit solver itself.

A good point, this shall be clarified.

4) line 63: '... a local six-moment...'. These high-order moments fluid codes can be used for global simulations. They are not 'local'. The authors may also want to cite the paper Wang, Liang, et al. "Comparison of multi-fluid moment models with particle-in-cell simulations of collisionless magnetic reconnection." Physics of Plasmas.

Thank you for the excellent suggestion and the correction. The referenced six-moment code was only presented via local cases, but we shall include references to global multiple-moment codes as well.

5) line 64: '...they do not capture reconnection'. What does 'not capture reconnection' mean? I cannot believe any high-order moments paper would make such a note. High-order moments methods go beyond Hall-MHD, and they are at least as good as Hall-MHD, which is already capable of producing some import reconnection features, such as the fast reconnection rate and the Hall magnetic fields.

This was written in response to the conclusions of the referenced Huang 2019 paper. We shall correct this section to correctly describe a wider range of multiple-moment codes.

6) line 52: 'with a proton-electron mass ratio of 25'. 25 is just a parameter for a specific simulation. It is not a feature of a model.

That is correct, but it was the parameter used in the simulation used in that publication. We shall rewrite this sentence to clarify this fact.

2. Section 2 and section 3 need to be re-organized. Section 2.1 describes Vlasiator, which is not new and it should be a separate section. It is better to combine section 2.2 and Section 3, since both describe the electron solver algorithm. In this new electron solver section, the authors should discuss the big picture of the electron solver with a few sentences first, for example, the authors should emphasize 1) this electron solver is also a Vlasov solver, just like the ion part, but the electric field is different, and 2) the initial condition settings. Then, the general approach (not just for a specific simulation!)

of initialing the electron solver and the details of the numerical steps should be carefully described.

Thank you for these suggestions. After consideration, we agree that moving section 2.2 into section 3 and adding initial descriptive text is a good choice. We will also provide more details regarding the initialisation and boundary conditions, as well as implement naming convention clarifications.

3. Line 140: 'we assume charge neutrality to hold as div(E) = net charge = 0, This simplifies our electric field calculations significantly as we do not need to implement a Poisson electrostatic solver.' This statement is not correct. 1) You can assume the net charge is zero in the Vlasov-hybrid simulation, but not in a simulation with the electron solver because it is not guaranteed. You cannot assume div(E) = 0, because they are NOT equal. With Hall term, $E = -V_e \times B$, and I do not think it is guaranteed div($-V_e \times B$) = 0. Actually, if you calculate div(E) in your 2D magnetosphere Vlasiator simulation, you may find div(E) is not zero somewhere. 2) You do not need a Poisson solver to keep div(E) = net charge $\neq$ 0 if eq (4) is solved properly.

Thank you for these comments. Just to clarify, we did not intend to imply we actually constrained div(E) to zero, (or a given value of ρ_q) but rather that we chose to assume that charge imbalances generated during this short simulation period would remain small, and thus, the electric field contribution due to them could be neglected. To probe this issue, we are in the process of investigating charge imbalance resulting from running our electron code. We shall discussion to this effect and quantify the magnitude of charge imbalance forming due to electron effects.

We acknowledge that a suitably well performing full-Maxwellian field solver should also be able to correctly model effects due to charge imbalance, and intend to investigate this in a future update of our model.

4. Line 150: 'As it is a feature of only the hybrid approach, it is not included in our electron solver.' I do not understand this statement, why it is a feature of 'only the

hybrid approach'?

This approach stemmed from the quasi-neutrality assumption, but upon further reflection, we have decided to implement an electron pressure gradient term into the solver after all. We shall add description of this term and results into the manuscript.

5. People usually use the uppercase Δ instead of the low case δ to describe numerical schemes. The authors should clearly define what is δV_e with proper superscripts and subscripts. For example: $\Delta V_{e,i}^n = V_{e,i}^{n+1} - V_{e,i}^n$.

In our approach we designated uppercase Δ as effects happening on the full grid level with lowercase δ steps being performed in substepping on a cell-by-cell basis. We shall add description and clarification in order to rectify these issues.

6. Why the RK4 scheme is chosen? Is not a 2nd-order scheme accurate enough for this purpose? Give an explanation, please.

Correct, the stability of the substepping is quite demanding. We initially investigated using Runge-Kutta-Nyström schemes, but upon testing found that the relatively simple and flexible RK4 scheme provided best results. The computational price of RK4 within this context is minimal in comparison with the Vlasov advection computations.

7. Why do you need sub-stepping? Why the sub-stepping time step is so small (line176)?

Each remapping of the gridded electron (or proton) distribution function (be it rotation, acceleration or advection) involves piecewise fitting of polynomials to small sections of the distribution function and integrating over sections of them. This is computationally expensive and also, if performed needlessly often, can lead to numerical diffusion. Also, after each full simulation time step (consisting of advection and acceleration remapping steps), we need to perform communication with other processes. These together indicate that any calculations which can be substepped, should be. We shall add discussion about this approach to the manuscript to clarify the issue.

8. What is a 'transformation matrix'? It has never been defined.

We apologise for this oversight. It is used to evaluate acceleration of the gridded distribution function used in the Slice-3D solver Vlasiator approach (combining rotation and field-parallel acceleration into one descriptive matrix which is then decomposed into three shear motions). We shall add description to this effect.

9. In figure 3, it seems both the velocity and electric field are growing slowly. What if running the simulation longer, for example, 10s?

We performed additional tests, running the single-cell tests for longer periods of time (1s, matching our target scenario). Indeed, oscillations begin to increase, but we were able to negate this by decreasing the RK4 substep length, maintaining stability even over extended periods of time. However, the growth is significant only when $\Omega_{ce}\Omega_{pe}^{-1} \approx 1$, which does not occur within our simulation domain. In the future, when we apply this method to larger domains, this validity needs to be ensured, or the substep length needs to decreased accordingly. We shall add discussion of this stability issue to the manuscript.

10. Line 343: 'our model is efficient, taking only 80 thousand CPU hours to perform the sample simulation presented in this paper'. Without comparison, I cannot see why '80k CPU hours' is 'efficient'.

We acknowledge that this point is perhaps not the most informative, but indeed, comparisons of similar electron approaches are not readily available. We shall amend the statement.

11. LIne 357: What is 'Upscaling the input moments'?

We were referring to potentially performing interpolation of proton input moments in order to increase the resolution of simulation initialisation values. We shall clarify this discussion.

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