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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.

Anonymous Referee #2

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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 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

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Discussion 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.

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

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.

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 x B, and I do not think it is guaranteed div(-V_e x 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 != 0 if eq (4) is solved properly.

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'?

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

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

7. Why do you need sub-stepping? Why the sub-stepping time step is so small (line 176)?

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

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

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'.

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

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