Answers to review report of the paper

High order numerical methods for Vlasov-Poisson models of plasma sheaths

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The authors wish to thank the referees for their report which helps us to improve the paper. Below, we discuss all the corrections (in red in the revised version) suggested by the referees.

1 Remarks of referee 1

1. The choice of the different boundary conditions must be more justified in terms of physics.
   The boundary conditions we are using are commonly employed in the literature on the simulation of plasma sheaths, for example in the paper by Devaux-Manfredi [DM08]. Other related references have been added [SZG+22] and [Cag18].

2. p6 : It is written that "The code works in parallel using MPI". I really don't see where MPI can add anything to the 2D code proposed here.
   We are not sure to understand the question of the referee. Indeed, some simulations using a large number of points are performed ($N_x = 512$, $N_{v_i} = 8192$ and $N_{v_e} = 16385$) which of course leads to quite long simulations and the use of MPI obviously enables to get the results in a shorter time, even in 2D.

3. p7 - caption of figure 1: What does it means "truncated to 10 ?".
   This sentence simply means that the plot of $f(t, x, v)$ is replaced by $\min(f(t, x, v), 10)$ to ease the reading. The caption of Figure 1 has been modified.

4. p8 - "Validation of the SL scheme" section :
   (a) Can you explain why in the case of the SL scheme the solution is not evaluated at the points but at the centre of the cells with an additional

quadrature ?

In spite of the fact the stationary solution (3.3) is integrable, it is not defined at the grid points (x_i, v_j) satisfying $x_i^2 + v_j^2 = 1$ so we instead consider the center of the cells (or staggered grid) through a finite volume interpretation to avoid this stiffness. Regarding the quadrature, this convention corresponds to the trapezoidal rule. A sentence has been added in the revised version.

(b) Not sure that the discussion to explain how to compute a scheme order is mandatory here.

According to the reviewer comments, we reformulate the caption of Figure 4. Let us remark the comments of Figure 4 are kept since the way the error is presented is a bit unusual and thus requires some additional explanations.

5. As a general comment for section 2, I don't see the interest of the tests performed with FD method here if no discussion and comparison with the SL scheme is added.

Since there is no test in Section 2, the reviewer may refer to Section 3.1 in which some validations of the FD and the SL methods are performed on the one species test. It is true that no comparison are performed but since this test has a exact solution, it enables us to validate the convergence of both numerical approaches independently before tackling the more complex two species problem.

6. p8 - section 3.2 : $\mu = 1/100$ is at least an order of magnitude higher than the realistic values. The choice of this value must be motivated and the robustness of the proposed scheme should be tested with more realistic values.

Of course, more realistic values might be considered, but this would require more refined mesh which would require to run additional heavy tests (to check the convergence for example). We think this value $\mu = 10^{-2}$ is a good compromise and sufficient to observe the different behavior for ions and electrons, which may serve as benchmark for other numerical scheme.

7. p10 : I am very surprised by the choice of the maximum velocity values :

(a) why do you choose different values for ion and electron in the case of the

SL scheme and not for the FD scheme ?
The FD code is not optimized as the SL code and for simplicity in the programming step, it is sufficient to consider the same (large enough) velocity domain for both electrons and ions.

(b) If you can choose different values for the ion and electron why do you choose a such big value for ions. Looking at figure 5 (left) it seems clear that the distribution function is already null for $v_i > 5$.
Like the referee, we also thought that it was possible to consider smaller velocity domain for the ions. However, due to the ionization term νf_e in (1.2a), it is not since f_i is not equal to zero even for large velocity and this has to be taken into account. A sentence has been added p10.

8. p10: Concerning the number of points:

(a) Why do you choose $N_x = 512$, $N_v = 513$ for the FD scheme and you choose more points for the SL scheme?
Thank you for this question. Actually, since the FD code is not parallelized, considering refined meshes is very costly. Moreover, since the FD code is based of an explicit treatment of the transport term, a stringent CFL has to be respected, making the runs very costly, even with $N_x = 512$, $N_v = 513$. On the contrary, the SL code does not present this drawback and refined meshes can be considered.

(b) Why do you finally choose Run2 as a reference and not another Run0. A discussion on the results for the different cases should at least be added.
Our goal in this 'Short time' test is not to comparer the FD and SL codes but rather to illustrate the evolution of the FD solution against a reference solution. The paragraph 'Short time' has been modified.

9. p11 - Figure 6: The plots, as presented, only give a qualitative comparison. For all row, please show on the same plot the difference between both methods to give a more qualitative comparison. This comment also applies to all the following figures.
The goal of Figures 6 is to illustrate that qualitatively, the results are in good agreement. In particular, we found interesting to show that for relatively short time ($t \leq 0.1$), the two methods gives very close results, which is not the case for larger time. We do not think plotting the difference between

FD and SL results will be constructive. Moreover, let us remark that the numerical parameters are not the same, so that some errors may affect the diagnostics.

10. On figures 7 and 8 the results are not at all the same between FD and SL. Why? It is essential to comment on these results.
The results presented in Figures 7 correspond to $T = 0.2$ whereas those of Figure 8 correspond to $T = 20$. We actually choose this way to present the comparison to illustrate the fact that for short time $t \leq 0.1$, the two methods are in very good agreement whereas for 'large' times, it is not the case any more and the FD method turns out to be not converged after $t \approx 0.2$. Some explanations are given in the paragraph called "Short time" p10.

11. As a general comment, the results all over the paper need to be analysed and commented on more seriously.
As suggested by the reviewer, additional comments have been added in the revised version (Short time and Long time paragraphs p10-11).

12. The number of references to other works is poor and must be increased.
Acoording to the referee comments, additional references have been added.

13. Finally, I definitely need to be convinced by something new in this work. If the case, this should be emphasized both in the abstract and in the conclusion.
Our goal was to propose high numerical techniques to approximate the solution of a two-species problem involving boundary conditions. However, the use of high order methods requires some strategies that a first order method does not need (typically for the boundary). Moreover, several methods have been compared for the numerical treatment of the Poisson equation. Hence, without any reference solution, it is not easy to validate these different techniques. We think the SL code that has been developed is a good tool to investigate stationary sheath solutions that are known to be difficult to capture. As suggested by the referee, the abstract and conclusions have been modified.

2 Remarks of referee 2

1. What is the objective of the project regarding the different boundary conditions: To compare them, or to find one which gives satisfactory results ? And what is the final assessment of the authors regarding their objective ?
   Using high order methods to approximate transport problems with boundary conditions requires some dedicated strategies. Recently, some theoretical works proposed some ways to keep high order accuracy even at the boundary and our goal was to test them in the two-species Vlasov context.
   Regarding the boundary conditions for the Poisson equation, our goal was to compare them and investigate the better one in terms of stability and accuracy (see Figure 3 for FD and Figure 12 for SL).

2. it would be useful to summarize at the end of Section 1 which boundary conditions will actually be used in the numerical scheme: (1.5), (1.8), (1.11)?

   As suggested by the referee, we clarified the boundary conditions discussion for the Poisson equation. Moreover, Remark 2.1 makes the links between the Poisson equation and the different boundary conditions. Finally, in the numerical part, we now refer more clearly to the different boundary conditions.

3. same remark for the formulas for E given in Section 2. In particular, since (2.1) modified according to Remark 2.1 and (2.2) are both derived from the symmetry assumption and they both involve a constant that depends on time, are these formulas equivalent ?
   Solvers (2.1) and (2.2) both yield symmetric solutions, but differ in that (2.2) may produce discontinuities at 0. This point has been clarified in the discussion of Figure 3.

4. In the FD and SL schemes the authors should be clearer about which formulas are used. In the FD scheme this is not written, and in the SL scheme the authors write that the Poisson equation is solved using (2.1), and later that only (1.8) will be used: does that mean via (2.1) ? With or without the modification suggested by Remark 2.1 ?
   The comparison of the different boundary conditions in Figure 6 showed that the results are very similar, with a slight loss of symmetry for the condition $E(t,0) = 0$. This comparison has been relocated at the beginning of the numerical section for clarification, with the following effect on numerotation:

Former Figure number	1-5	6-11	12
New Figure number	unchanged	7-12	6

The discussion has been completed to highlight the effective choice of the condition $\int_{x=-1}^{1} E(t,x)dx = 0$. Let us remark that this condition is used in Figures 7-12.
Let us summarize the answers to the referee comments 2-3-4

- modification of the paragraph "Boundary conditions" (p4, end of Section 1) to clarify the different boundary conditions.
- modification of Remark 2.1, following the paragraph "Boundary conditions".
- addition of a sentence in Subsection 2.2 to precise different solvers to compute E will be compared.
- discussion for Figure 3 (bottom of p7) about the Poisson solver (2.2).
- Figure 12 became Figure 6 to first discuss the effect of the boundary conditions for the Poisson solver.

5. I also have a question about the ionization term: Could the authors comment on how it is derived ? For densities which vanish close to the domain boundaries, (1.2) does not preserve the total charge $\int\int (f_e + f_i)dxdv$, which seems unphysical. Could the authors discuss this lack of total charge conservation ? On p14 it is further written that the ionization parameter needs to be adjusted to ensure the stationarity of the solution: this should probably be explained in Section 1.
   In general, the presence of a sheath induces a mass loss at the boundary. To ensure mass preservation, an ionization term is introduced in such a way the neutrality is ensured in the plasma core $\int (f_e(t, x = 0, v)dv = \int (f_i(t, x = 0, v)dv$. In practice, this leads to a relation on ν (see [ALPM+20] for more details). We do not investigate this in the present work but intend to explore this aspect.

6. Finally the authors should comment on how well their numerical results model a non-neutral sheath. Since here the normalized Debye length is $\lambda/2$, the sheath occupies a large portion (probably all) of the computational domain. Looking at Figure 8 one sees indeed a larger charge at distance $\lambda/2$ from the boundaries (although the charge is positive everywhere, which is probably caused by the non-conservative ionization term): are these results satisfactory ?
   Regarding the two-species model, we can guarantee that the high order numerical method converges when the numerical parameters go to zero towards a solution (which can be supposed unique) of the problem. In this sense, the

results we obtained are satisfactory. More tests would be required (considering smaller λ or μ) to check if the solution is satisfactory from a physical point of view.

Minor comments:

1. p1: the last sentence of the abstract is obscure.
   The last sentence of the abstract has been modified.

2. p1: Introduction: what is "a sufficiently large domain" ?
   This sentence has been changed.

3. again in the introduction, it would be useful to specify which parameters (mass ratio and normalized Debye length) will be used in the numerical experiments, in particular if they are realistic or not.
   As suggested by the referee, we added a sentence saying that the mass ratio and normalized Debye length are non necessarily physically relevant.

4. p2: When several references are cited together, chronological order should be preferred.
   We would like to warmly thank the reviewer for his careful reading.

5. p2: "we studied": does this cover all the authors of the current article ?
   No, it is not. We changed the sentence.

6. p2: "Then, we are concerned" $\rightarrow$ "Thus, ... "
   Thank you. We changed.

7. p2: "their density" $\rightarrow$ "its density".
   Thank you. We changed.

8. p5: what is "a large enough maximum speed" ?
   As usual in Eulerian Vlasov simulations, a grid in velocity has to be considered and a maximum value of v has to be fixed to ensure the unknown is correctly represented in the interval. The sentence has been modified.

9. p5: in eq (2.3), how is E_j^n defined ? also, I guess scalar products are missing.

   E_j^n is supposed to be an approximation of $E(t^n, x_j)$ from one of the three strategies proposed in Section 2. A sentence has been added p5 (see also the answer of comment 4).
   Actually, there is no missing scalar product since the vector notations in (2.3) are chosen in such a way we do not need.

10. p5: "coordwise" → "element-wise" ?
    Thank you. We changed.

11. p5: "The upwind scheme is known to be ...": please cite a relevant reference.

    Thank you. We changed the sentence: The upwind scheme is known to be diffusive, and stable under the CFL condition → The upwind scheme is stable under the CFL condition.

12. p14: "we then make vary" → "we then vary" / "we remark" → "we observe".

    Thank you. We changed.