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Interactive comment on "Mirror mode physics: Amplitude limit" *by* Rudolf A. Treumann and Wolfgang Baumjohann

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We thank the reviewer for his comments. we are sorry for responding late, but a health problem intervened before we could reply.

We friendly ask R2 to consider the general comments which apply to all reviewers for the overlapping points.

Below are just a few concise remarks specific to R2.

1. why one needs to consider electrons? why not? first for curiosity which is a typical property of theoreticians or interested non-mechanical researchers. second because the growth rate of the general ion mirror mode also contains the anisotropy of electrons (e.g. noreen et al. 2018). so no need for any further justification.

just for a comment on shoji et al. by taking electrons as an isotropic fluid they eliminate all electron effects except quasineutrality. these simulations are nice but just show what has been put in (as all simulation do: they never discover anything beyond what is already contained in the equations they solve). with fluid electrons there will not be any electron effects nor ion sound waves for interaction. so electron pairing is excluded from the beginning.

we note in passing that our theory formally also applies to ions (which R2 is not aware of). we, however, consider only electrons, not ions which would require to check for waves they could interact with and for the different inertia. all going beyond the intention of our basic physics investigation as they would unreasonably increase the volume of the paper. the learned R2 might have believed we would not be aware of this possibility. we apologize of leaving the impression we would be ignorant.

we could add a number of other remarks on these simulations (which are a typical case of formally performing simulations, showing many figures but incompletely discuss the physics, clearly simulations always show something, i.e. solutions of the equations as figures. these need to be explained in terms of physics) but we refrain from it. we just say that in those sim's the large imm amplitudes are caused by the requirement of pressure balance which in the open system sucks in neutral plasma from the surrounding environment, without explanation why. in closed boundary sim's this will no be the case. R2's description is not further illuminating here. his last comment in his point 1 is superficial for the above reasons. no further comment on this.

2. this whole point is based on misunderstanding the physics. we have elucidated a physical effect here, not simply done some trivial simulation. R2 should read our general comments. true: we consider only parallel elstat waves as these apply to bouning particles in correct parallel physics (the only important physics her, which excludes any elmag waves). electromagnetic forces play no role in any generation of electric potentials, and there is no interest in radiation here; the comment on the mirror force is superficial, not withstanding that it is implicit to our main argument on bouncing parti-

cles. it is anyway surprising that it took half a century until in mirror modes the bounce is taken into account. We do not comment on langmuir and whistlers here as this is rhetoric nonsense notwithstanding that we commented on langmuir in the paper.

3. this is the reasonable point R2 makes of which we have been very well aware.

we refer R2 to the general comments. in brief, the gyrophase in elstat wave parallel resonance interaction clearly plays no role. there is no bunching of particles. in bounce any motion is gyroaveraged. so this point as made by R2 does not apply.

what applies but is not mentioned by R2 is our use of "coherent" which is meant to apply to the pairs locked near mirror points where their motion practically reduces to a common gyration (phase independent on this average bounce timescale as explained in the paper and general comments) at common speed v_e. this causes a surface or shell current which contributes a local diamagnetism, which however is very weak of the order of landau diamagnetism and negligible. we put this part in rewritten version into the appendix.

the essence of this and point 4 of R2 is correct: we have no true Meissner effect here, not only because the plasma is classical but because the diamagnetism is too weak (because ions are not fixed in a crystal here). thus susceptibility is simply pressure balance.

the important point is (still restricting for our purposes to electrons only, though it might also appy in similar vain to ions, as said above) that electron pairing in quasilinear stability with vanishing ion anisotropy $A_i \approx 0$ and emerging large pair anisotropy $A_{e,pair} = T_e/m^*u^2 \gg 1$ gives rise to a substantial increase of the ion-mirror growth rate which without pairing woul not be not known.

hence in pairing, even for electrons only, the mirror mode continues growing fast beyond the quasilinear level even in closed boundary conditions.

it is clear that in simulations this requires inclusion of the fully kinetic electron dynam-

ics. stabilisation under closed boundary conditions implies then destruction of the pair anisotropy which implies destruction of pairs and heating the electrons until new pressure balance at enhanced pressure, decreased internal magnetic field. otherwise it implies inflow of quasineutral plasma from the surrounding along the magnetic field to come up for the lack in pressure. this is the most probable mechanism. our expectation concerning susceptibility as interesting as it would have been, was overoptimistic. however the elucidation of this new and probably singular effect in plasma is a nice result.

once more thanks to R2 for the inquiry. though it was not his doubts but our own unrest with the original claim which forced us to rethink the physics of the reason for further growth with this satisfactory result.

we refrain from the demand of performing simulations which we leave to the efforts of simulationists. we dot see this as our duty which is not mechanically performing simulations but producing new possibly applicable physics. this has always been the duty of theorists until simulations came up which are now believed to be the non plus ultra but never produce any more insight than is contained in the input equations and boundary conditions and in addition requires the frequently lacking ingenuity of correct and exhausting interpretation of the figures obtained within the bounds of the input. just showing figures is not enough in physics.

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