Comment on "Evolution Equations of Nonlinearly Permissible, Coherent Hole Structures Propagating Persistently in Collisionless Plasmas"

I H Hutchinson

Plasma Science and Fusion Center,
Massachusetts Institute of Technology,
Cambridge MA 02139, USA
Email Address: ihutch@mit.edu

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Recent critical remarks, published in this journal, about the present author's analysis of electron and ion holes and their stability are addressed and shown to be misunderstandings and misrepresentations.

Comment

In a recent publication[1], Schamel and Chakrabarti, make a point of contradicting some features of my papers concerning electron and ion holes. While I have in many places acknowledged the important pioneering work of Dr. Schamel concerning electron holes, there remain various important aspects of his claims on which we disagree. Normally I am content to present my analysis and findings objectively in my own papers, in the expectation that the give and take of the progress of science will eventually clarify which perspective or opinion is most satisfactory. However, the referenced paper misrepresents my positions so egregiously that I feel it is essential to comment and correct the misrepresentations.

In my 2017 tutorial paper about electron holes[2], I tried, for the benefit of the community, to explain the decades-long dispute (in which I had no part) between what Schamel calls respectively the "pseudo-potential method" and the "BGK method". (Actually Bernstein Greene and Kruskal[3] introduced both methods and called them the "differential equation" and "integral equation" methods.) I said, and I remain firmly convinced, that these are both valid ways to obtain equilibrium electron hole solutions, and they have different strengths and weaknesses. The pseudo-potential approach specifies the trapped particle velocity distribution function $f(v)$ and solves the steady Vlasov-Poisson system to obtain the corresponding self-consistent potential $\phi(x)$. Its strength is that $f(v)$ can be specified so as to avoid some singularities; however it requires the solution of an eigenproblem to ensure that the trapping boundary energy lies at the assumed value in the prescribed $f(v)$; and its weakness is that historically monotonic trapped $f(v)$ of specific form were generally assumed, which substantially constrains its characteristics. (Recent generalizations by Schamel of the $f(v)$ form permit, for example, $f(v)$ discontinuities, thus weakening one of its strengths and strengthening one of its weaknesses.) The BGK method, in contrast, specifies the potential profile and solves for the required trapped distribution function. Its strength is that seemingly any potential shape is permitted, and that no eigensolution is required; its weakness is that unless certain plausibility constraints are applied to $\phi(x)$, notably that it falls off asymptotically exponentially with the Debye shielding length, then slope singularities in $f(v)$ arise at the trapping boundary (and possibly at other potential extrema).

In [1] Schamel and Chakrabarti say concerning my comparison of the two methods that "Hutchinson ... does not adequately acknowledge their differences. In particular, he sees no need for an independent derivation of a NDR". It is true that I see no need for an eigensolution in the BGK method if one prescribes the asymptotic form of ϕ correctly. It is not true that the BGK method requires "independent

derivation of a NDR"; instead the asymptotic constraint becomes what Schamel calls the NDR (Non-linear Dispersion Relation). Nor is it true that in every case "it would be better to start directly with the pseudo-potential method". Both methods have advantages, but for different purposes. I also disagree with their abstract's extreme claim that "structure formation is inevitably associated with particle trapping, which can only be properly addressed by the pseudo-potential method". Trapping and electron hole formation is generally highly dynamic and non-linear, often involving merging of smaller holes into larger ones; and neither equilibrium construction method can meaningfully represent the formation processes.

They further say "he keeps making mistakes in his calculations" with the only justification being a reference to a longer version[4] of [1]. (The authors call [1] an "abridged version" of [4]). In the longer version, the only passage that appears to refer to my "mistakes" is that in a footnote of my tutorial paper[2] I remark on, and explain in sufficient detail that others can check, an algebraic error I thought I had detected in a Taylor expansion in a paper by Korn and Schamel [5]. On rechecking that observation, I find that, though there was an error in their development where I said, their subsequent equations (A4 on) and final expressions were actually correct. I therefore withdraw the remark with apologies to Korn and Schamel, and acknowledge that the final coefficient in equation (12) of [2] should be 1/16 as they maintain, not 1/32 as I wrote. Fortunately, as I said, "these errors are not of great significance".

The difference in our perspectives is starkest when Schamel and Chakrabarti dispute the findings of [6] where I explain that there is a sign error in the original paper of Schamel and Bujabarua 1980, in their application to ion hole temperature ratio requirements of what Schamel calls the NDR, and what I see (in the BGK approach) as the constraint on the asymptotic form of $\phi(x)$ (effectively the same equation). To come to some resolution of this important issue, which has often been cited, perhaps a more direct comparison of our hole treatments might be helpful.

My version of the asymptotic constraint (i.e. the distant boundary condition) when employing the BGK method is simply Poisson's equation

$$(1/q_r)d^2\phi/dx^2 = n_f + \tilde{n} - n_r, \tag{1}$$

where $q_r = \pm 1$ is the charge (sign) and n_r is the density of the repelled species (ions for a positive potential); n_f is the attracted species density including a flat trapped $f(v)$ equal to the passing value $f(\mathcal{E} = 0)$; and $\tilde{n}$ is the (usually negative) deviation of the actual trapped density caused by the deviation of trapped $f(v)$ from flat: effectively the phase space density hole. Asymptotically, the potential decays exponentially to zero at large $|x|$: $\phi \propto \exp(-|x|/\lambda)$; so $d^2\phi/dx^2 = \phi/\lambda^2$. Moreover $n_r \simeq 1 + (dn_r/d\phi)\phi$, and $n_f \simeq 1 + (dn_f/d\phi)\phi$, while if $f(v)$ is continuous with finite derivatives at the trapping boundary $\mathcal{E} = 0$, then $\tilde{n} \sim \phi^{3/2}$. Thus in the limit $\phi \to 0$ it is required that $1/\lambda^2 = 1/\lambda_{Da}^2 + 1/\lambda_{Dr}^2$ where $1/\lambda_D^2 = \lim_{\phi\to 0}|dn/d\phi|$, and λ_D is the Debye length of each species (for general velocity distribution functions). This requirement is nothing other than familiar Debye shielding of potential in a plasma. But it invokes continuity of attracted species $f(v)$ at $\mathcal{E} = 0$ to justify ignoring the then higher order $\tilde{n}$. Note that $\theta = T_r/T_a$ is the ratio of the repelled species temperature to the attracted temperature. For an ion hole this is T_e/T_i.

Schamel's version of the NDR is most easily related to mine from the longer version paper [4], to whose equations and notation I now refer. When his parameters k_0, $\tilde{C}$, D_1 and D_2 are zero we have "The privileged $\mathrm{sech}^4(x)$ solitary mode", eq (12). Its potential form is $\phi(x) = \psi\,\mathrm{sech}^4(\sqrt{B}x/4)$, and gives a NDR eq (13)

$$-\frac{1}{2}Z_r'(\tilde{v}_D/\sqrt{2}) - \frac{\theta}{2}Z_r'(u_0/\sqrt{2}) = B - \Gamma. \tag{2}$$

Here the $-Z_r'$ function terms (subscript r denotes real part in his notation) represent $\lim_{\phi\to 0}|dn/d\phi| = 1/\lambda_D^2$ for the two species, which are Maxwellians with shifts $\tilde{v}_D$ and u_0; and evidently from the potential form, B is equal to the asymptotic inverse potential scale length squared, my $1/\lambda^2$. Thus if $\Gamma = 0$, the equation is identical to my asymptotic constraint. But Γ ($\equiv \frac{\sqrt{\pi}}{2}\exp(-v_D^2/2)\gamma$) represents (see [4], eq 1)

a constant times $\sqrt{|\mathcal{E}|}$ added into the trapped distribution function but not to the passing distribution. Therefore, my requirement of continuity and non-singularity of $f(v)$ across $\mathcal{E} = 0$ amounts to the assumption that $\Gamma = 0$.

By contrast, in their arguments to defend the temperature ratio condition that I have contradicted, Schamel and Chakrabarti say "We neglect for simplicity in (1) the right hand side and get $k_0^2 - \frac{1}{2}Z_r'(v_0/\sqrt{2}) - \frac{\theta}{2}Z_r'(u_0/\sqrt{2}) = 0$" and then "For ion acoustic waves ... using $-\frac{1}{2}Z_r'(u_0/\sqrt{2}) \approx 0$ one gets $-\frac{1}{2}Z_r'(u_0/\sqrt{2}) = -1/\theta(k_0^2 + 1)$. This is essentially (14) of his paper...". This statement is incorrect even when $k_0 = 0$, the solitary limit. Actually, my eq (13) (not 14, which has nothing to do with the issue) is the one to compare; and it has *not* neglected all the right hand terms that in Schamel's notation are represented by B. Instead, it recognizes that the $B \equiv 1/\lambda^2$ term for a solitary potential must be positive, and it supposes the repelled species is an unshifted Maxwellian, giving $-\frac{1}{2}Z_r'(\bar{v}_a/\sqrt{2}) = 1/\lambda^2 - 1/\theta$ (where $\bar{v}_a$ is the Maxwellian shift of the attracted species). From this follows trivially that when $-\frac{1}{2}Z_r'(\bar{v}_a/\sqrt{2}) = -0.285$ (its global minimum over all $\bar{v}_a$) it is required that $\theta < 1/0.285 = 3.5$ as I have stated, not $\theta > 3.5$ as Schamel continues to maintain based on improperly neglecting the important right hand side term(s). Others before me, e.g. [7], whom I cite in [6], have given counterexamples of equilibria disproving Schamel's version.

I believe the source of the confusion lies in the reference to ion acoustic waves, which do of course require $\theta \gg 1$ for low damping. An ion hole, however, is not like an ion acoustic soliton, nor a nonlinear extension of ion acoustic waves, both of which can be treated using a fluid (single speed) ion representation. An ion hole is intrinsically a kinetic phenomenon involving a depression in phase space density (the hole) within the spread of the ion velocity distribution. So their statement "And of course the same holds for ion holes with $u_0 = 1.307$" is an incorrect assumption, based on a mistaken analogy with ion acoustic phenomena. It is ironic that Schamel and Chakrabarti later say of me "In summary, many of his ideas are still influenced by linear wave theory, which, however, is misplaced in this area." In fact, they are the ones misled by analogies with ion acoustic waves. I, by contrast, have consistently treated holes as requiring full-scale kinetic treatment of the attracted species, as is their nature.

A final misrepresentation I must contradict consists in Schamel and Chakraborti's remark saying that in my work 'invalid results such as "ultra- slow velocities exist only at the center of a double-humped ion distribution" are obtained.' In none of my published papers can I find the passage they place in double quotes implying that it is a quotation. The nearest thing I can find to a statement along these lines is that in [8] I write "for slow positive structures to exist stably, the background ion velocity distribution generally cannot be single-humped." However the crucial point is the word "stably". What my analysis shows is that in the absence of a local distribution minimum, slow electron holes are unstable to self-acceleration. The entire point of [8] and [9] is stability analysis, not merely equilibrium analysis. Slow hole lifetimes when unstable are relatively short, because they become fast.

I hope the present comment might help the community and Schamel and Chakraborti, to understand better the relationship between their work and mine, and become more accepting of the value of different perspectives, and of constructive criticism.

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