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Reply

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1. Introduction

Bellan's Comment [this issue, hereinafter B] has suggested that our previous article *Wright and Allan* [1996a] (hereafter WA) contains errors. We find no substance to these criticisms. B concentrates upon normal modes, and fails to appreciate the central time-dependent result of our article: For typical conditions the inclusion of ionospheric dissipation terms (which are far more important than B's two-fluid terms) causes pulsations to decay before the process of phase mixing has time to develop spatial scales of order the electron inertia length, when two-fluid effects would become important. We also point out some basic errors in B's analysis of the fluid equations. Our previous conclusion that "The neglect of two-fluid effects... is an excellent approximation during the lifetime of typical pulsations found from dawn through noon and dusk" remains accurate.

We shall divide our reply into two sections. The first addresses the importance of two fluid effects in time-dependent systems, such as observed ULF pulsations; the second (which may be of less interest to the general reader) focuses upon properties of the single-fluid and two-fluid normal modes.

Before proceeding any further we note that of the three comments [*Rauf and Tataronis,* 1995; *Ruderman et al.,* 1995; *Goedbloed and Lifschitz,* 1995] written on Bellan's earlier work, two of them pointed out basic misconceptions regarding the ordering of plasma quantities in the fluid equations, in particular a supposed contradiction concerning quasi-neutrality. To quote *Ruderman et al.* [1995, p. 3547] "This illusory contradiction was widely discussed long ago and we are sorry to have to repeat the well-known explanation." Such orderings are derived in numerous plasma textbooks. B contains a similar basic misconception about the ordering of plasma quantities, and we are also are sorry to have to repeat well-known material here. Readers may like to move on to section 2 if they are familiar with the following material.

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B is confused over the orderings of terms in the ideal linear two-fluid Ohm's law (equation (2b) of WA);

$$\mathbf{E} + \mathbf{u}_\wedge \mathbf{B} = \frac{m_i m_e}{\rho q^2} \cdot \frac{\partial \mathbf{j}}{\partial t} + \frac{m_i}{\rho q}\left(1 - \frac{m_e}{m_i}\right)\mathbf{j}_\wedge \mathbf{B} \qquad (1)$$

where q is the magnitude of the unit of charge, and $\mathbf{B}$ is in the z direction. WA state that when the single-fluid limit is taken appropriately, the terms on the right are small compared to those on the left-hand side. When B disagrees with WA on this point, B disagrees with every single-fluid MHD text ever written. B takes the z component of the above equation to get B's (26). Only two terms remain; one from the left-hand side (E_z), and one from the right-hand side. Of course, these two terms must be of the same order, and so B claims that the surviving term on the left-hand side (E_z) cannot be neglected. The error B has made is to consider a single component of the equation in a direction in which the left-hand side vectors have a small magnitude. A cursory reading of almost any MHD text makes it clear that it is the magnitudes of the terms that should be compared. There is nothing wrong with considering a component of (1), but it must be done in conjunction with the other components too, which is not done in B – although this fact is acknowledged there. We shall provide this service here by continuing from B's (26) and equations (2) of WA. It is easy to show that

$$\frac{E_z}{E} \approx \ell_e^2 k_z k_\perp \qquad (2)$$

where $\ell_e = c/\omega_{pe}$ is the electron inertia length and E is the magnitude of $\mathbf{E}$. Thus, if the wavelength is much greater than the electron inertia length, E_z is much less than E. Equation (2) says in words: in the single-fluid limit the magnitude of E_z has nothing to do with the magnitude of E.

It is acknowledged in B that "...E_z and m_e are both small...." However, this is somewhat sloppy as the size of dimensional quantities is ambiguous. It is far better to look at dimensionless ratios, as in (2) above. B claims that the limit of $m_e/m_i \to 0$ has nothing to do with B (26) as the quantity does not appear explicitly. Assuming some characteristic time variation for the solution (e.g., $\exp(-i\omega t)$), it is trivial to write B (26) as

$$\frac{E_z}{j_z}\left(\frac{nq}{B}\right) = -i\left(\frac{m_e}{m_i}\right)\left(\frac{\omega}{\omega_{ci}}\right) \tag{3}$$

Thus it is evident that E_z does indeed depend upon the ratio m_e/m_i. B intimates that although E_z is small this does not mean that the parallel current is small. This is nothing new to the MHD cognoscenti: the Alfvén wave has long been celebrated for its ability to carry a field-aligned current in the ideal single-fluid limit ($E_z = 0$).

In B's abstract the magnitudes of the fourth and second derivative terms are discussed. It is impossible to determine the size of these terms unless the magnitude of d/dx is known. B seems to overlook this basic fact. WA (p. 24,994) state that "Two-fluid effects begin to become important when spatial scales [in x] of the order of the electron inertia length are achieved," and we stand by that statement. If $d/dx < 1/\ell_e$, then the order of the fourth derivative term is less than that of the second. We demonstrate this property using B's own analysis in the text around our (5) and (6).

An undesirable feature of B is that it concentrates almost exclusively upon the properties of a single normal mode, which can never describe a real time-dependent system. Motivated by observations of ULF pulsations WA tried to determine when two-fluid effects would become significant, and this is the crucial issue to be settled. All the discussion in B (except for the last small subsections) focuses on a single ideal normal mode; this is a distraction and does not enable B to comment on what is the main conclusion of WA.

2. Time-Dependent Equations

Numerical studies of the evolution of single-fluid and two-fluid MHD equations by *Wei et al.* [1994] show that there is no noticeable difference between the two solutions until the time at which spatial scales of order 10 electron inertia lengths ($\ell_e = c/\omega_{pe}$) are developed. At this point a two-fluid term in Ohm's law becomes significant. The simulations show how the (now invalid) single-fluid equations develop even smaller scales by continued phase mixing [*Mann et al.*, 1995]. In contrast, the two-fluid solution does not develop smaller scales but begins to radiate an electron inertial Alfvén wave.

A simple way of estimating the time it takes to phase mix to spatial scales of order $10\ell_e$ was given by *Mann et al.* [1995], based upon the single-fluid solution. B criticises our use of this relation as it is based upon single-fluid equations which B claims are not valid. However, we only use these equations for the early time development when spatial scales are greater than $10\ell_e$, during which time the single-fluid approximation is valid. Further support for the correctness of our calculation comes from us estimating at what time the single-fluid and two-fluid solutions of *Wei et al.* [1994] should begin to show differences. WA (p. 24,994) state "we calculate the phase-mixing time to reach a scale of $10\ell_e$ to be $\tau = 3.4T_p$." Indeed, *Wei et al.*'s [1994] results show no difference between the solutions at $t = 2T_p$, whereas the results at $t = 4T_p$ begin to show visible differences. The excellent agreement between our estimate of when single-fluid MHD becomes invalid and the independent results of *Wei et al.* [1994] indicate that our estimate is extremely reliable. If there had been any doubt, we would not have used it in WA.

The picture that emerges for the growth of a steadily driven Alfvén resonance is one where energy is fed into the resonant layer which has a width proportional to $1/t$ and the wave amplitude is proportional to t. WA (p. 24,994) clearly state that this single-fluid description is valid "until electron inertial scale lengths are reached, at which point radiation of inertial electron Alfvén waves suppresses further growth [of the resonance]."

B criticizes WA's use of *Wei et al.*'s results as these employed an unrealistically large value of the magnetospheric plasma resistivity. (Something that all numerical studies do.) We can estimate the scale length at which resistive effects become important in *Wei et al.*'s simulation (e.g., from equation (4) of *Wright and Allan* [1996b]). For *Wei et al.*'s parameters it turns out that this length is an order of magnitude smaller than the scale length at which electron inertial effects become important. This is why, as acknowledged in B, "... despite this [large resistivity], *Wei et al.* ... see electron inertial effects..." Thus WA are quite justified in using *Wei et al.*'s results to predict the onset of electron inertial effects.

If *Wei et al.*'s [1994] resistivity had been even larger, so that the resistive scale length was much greater than $10\ell_e$, they would not have seen electron inertial effects. Indeed, WA's argument is just this: WA (p. 24,995) continue "An ideal, cold, linear description of a plasma neglects many processes. For ULF Alfvén pulsations it seems that the most important omissions are probably dissipative and nonlinear terms. The neglect of two-fluid effects (i.e., electron inertia) is an excellent approximation during the lifetime of typical pulsations found from dawn through noon and dusk." The reason WA claim that dissipative effects are more important than two-fluid effects is because dissipation limits the lifetime of the pulsations and hence places a limit on the smallness of the scales that can be produced by phase mixing. For realistic parameters ULF waves survive for 10 or so cycles, whereas the waves would need to phase mix for at least 85 cycles (under extremely favorable conditions) to develop scales of $10\ell_e$.

B criticizes WA's time-dependent picture by saying that WA "abandon frequency domain analysis", and that "[WA's time-dependent description] does not stand scrutiny, because time domain signals can always be expressed as a Fourier sum of [normal modes]." B's problem is that it never considers a sum of the modes and so can never deduce the time-dependent behavior. Under certain circumstances a single normal mode may represent the state of a system for large times. This will be possible for ULF pulsations. However, the main point of WA is that dissipation is the most important correction to single-fluid MHD. Ergo, if we are to approximate the asymptotic behaviour of a ULF pulsation by a single mode, we must include dissipation in the normal mode equations. B has no dissipation in its equations, and so they are not as realistic as WA's description. Obviously, a single ideal two-fluid normal mode is of little relevance to the asymptotic state of dissipative ULF pulsations. Nevertheless, the vast majority of B, and consequently the rest of this Reply, are devoted to such a mode. The reader who is not interested in these ideal modes in their own right may skip the next section.

3. Normal Modes

Normal modes are solutions to the governing equations (either single-fluid or two-fluid) that have a time dependence of $\exp(-i\omega t)$. It is well known that the ideal single-fluid normal mode contains a singularity at the Alfvén resonance where the spatial scale becomes vanishingly small, and so must be smaller than ℓ_e. WA (p. 24,993) note that this would "suggest that the singular modes indicate a violation of the approximations employed in deriving them." However, this is (WA, p. 24,993) "a cause for concern only if you believe that a real system can behave as a solitary normal mode..... A real system is time dependent and never behaves as a single normal mode. Even if the system has been driven for a long time at a single frequency there will still be transients somewhere..."

So, are normal modes worth studying? Yes. WA (p. 24,993) continue "Normal modes do not correspond to reality, but are mathematical functions of the governing equations... Although a single normal mode cannot be used to describe the evolution of a real system, a summation or integral of the normal modes can... It does not matter if a singular mode has infinite amplitude and vanishing scale length. What matters is if the amplitude and scale lengths of the summed (physically meaningful) solution violate the single-fluid approximations."

WA begin by considering the properties of the two-fluid normal mode for arbitrary frequency and Alfvén speed. To decide if there is a singularity or not at the Alfvén layer, WA follow the textbook method of Frobenius: First, the governing equations are reformulated as a single ordinary differential equation. WA's equation (6) is

$$A_4\frac{d^4E_y}{dx^4}+A_3\frac{d^3E_y}{dx^3}+A_2\frac{d^2E_y}{dx^2}+A_1\frac{dE_y}{dx}+A_0E_y=0 \quad (4)$$

The derivation of the coefficients above for the general two-fluid equations is rather arduous. It is not practical to do this with pencil and paper; we employed computer algebra software. To decide if there is a singularity at the Alfvén layer, we see if the coefficient A_4 is zero there. For this reason, WA only give A_4 explicitly. A_4 does not vanish at the Alfvén layer so there is no Alfvén singularity in the two-fluid normal mode.

B claims we have not calculated the coefficients correctly, which is surprising as we only give A_4 explicitly in the two-fluid approximation. (The other coefficients are much longer than A_4, which itself is six lines long, and so were not given explicitly as they do not make very interesting reading and would have exceeded the *Journal of Geophysical Research*'s 9-page limit. This is something to be avoided in a Brief Report.)

We take B's suggestion that we miscalculated the coefficients very seriously and so have performed extra checks on WA's computer algebra programs. In particular, B claims that we do not recover the well-known uniform plasma limit (B's equations (8) and (9)). We are happy to be able to set the reader's, and B's, mind at rest: After taking the uniform plasma limit of WA's full fourth-order ordinary differential equation (ODE) we can confirm that the standard result is found. (All the coefficients have several common factors which are cancelled to yield this result.) Also, note that we have retained the general definitions for S, P, and D, rather than the approximate forms employed in B's equation (1).

We never doubted the correctness of WA's fourth-order equation coefficients for a two-fluid plasma as we had already performed several other tests before publishing the result. In a small aside occupying one paragraph, WA mentioned briefly one such test which involved taking the single-fluid limit. Much of B is based upon this paragraph, which is of no consequence to the conclusions described in section 2 of this Reply.

B criticises the dimensions of our two-fluid and single-fluid coefficients as not being consistent. B correctly surmises that we have cancelled some common factors after taking the single-fluid limit. Thus the single-fluid coefficients in WA equation (8) are self-consistent and employ a different normalisation from that employed in the two-fluid coefficient WA equation (7). The difference in normalization is not important since it is not meaningful ever to combine these two results.

B criticizes our choice of E_y as the subject of the fourth-order equation we derived in WA equation (6) as it is not a "fundamental quantity." We are not clear what constitutes a "fundamental quantity," and this terminology does not appear in textbooks devoted to simultaneous ODEs. B notes the trivial "$k_y = 0$, uniform-plasma, $\omega/\omega_{ci} \rightarrow 0$" limit for which the governing equations split into two decoupled second-order ODEs. Each equation describes one of the decoupled wave modes. (This is also true even for a nonuniform medium.) However, WA consider the much more general problem of a finite ω/ω_{ci}, two-fluid plasma with arbitrary k_y. WA constructed their fourth-order ODE in E_y to investigate the existence or otherwise of a singularity due to wave coupling. Thus it seems patently obvious that WA's single ODE is addressing the general situation in which there is wave coupling. In addition the choice of E_y as our subject for the ODE is just as suitable as any other component of the electric field that is nonzero to leading order: If there is a singularity, it will be evident in the Frobenius analysis of the E_y ODE. B suggests that "the coefficients of the fourth-order E_y equation are very complicated making calculations unintuitive and prone to error." This criticism is irrelevant to the correctness, or otherwise, of our result. We have checked our (computer algebra) calculations extensively and recover all standard results. B's intimation that the choice of another more "fundamental quantity" will simplify the coefficients is sheer speculation. Although B derives a fourth-order equation in E_z (B's equation (16)) this is done after the limit of $\omega/\omega_{ci} \to 0$ has been taken, which simplifies the equations considerably. Moreover, we noted at the beginning of our Reply that if the scale of the waves is larger than the electron inertia length, then E_z is zero to leading order. (Note that B acknowledges that "E_z [is] small.") This seems to make B's subject choice of E_z a particularly unsuitable one for this analysis.

B demonstrates a number of misinterpretations of WA's analysis. For example in B's cumbersome "β" notation it is stated that "WA omit the non-uniform plasma term β_2 in their equation (8), so to be consistent with this WKB-like assumption (made by WA), both β_2 and β_4 should be omitted from (21) giving..." WA never mention nor employ the WKB approximation. The only terms WA neglect are through taking the well-known single-fluid limit.

B develops equations describing a uniform two-fluid plasma (B's equations (12)-(15)), and "WKB" equations for a weakly nonuniform two-fluid plasma (B's equations (22)-(25)). B is concerned about the WKB-like description employed breaking down at the Alfvén layer, suggesting that his WKB solution is inadequate for describing the Alfvén layer. For simplicity, let us suppose that V_A is constant, and the medium uniform so that we can be assured of B's equations (22)-(25) remaining valid. It is, perhaps, worth using B's uniform medium results to show the correctness of WA's conclusions and that B's are wrong. For example, WA explain that when the single fluid limit is taken the terms containing A_3 and A_4 are smaller than the other terms by a factor of V_A^2/c^2. We note that B considers the ratio of the coefficients (which is not dimensionless) rather than, as we do, the the ratio of the terms containing the coefficients, which is dimensionless. B claims that B's equations (14) and (15) disprove WA's assertion. It is straightforward to show that B is wrong and WA are right: The medium is assumed to be uniform at this point by B (consequently $A_3 = 0$), so we may replace d/dx by ik_x, and find the ratio of the fourth derivative to the second derivative term in B's equation (15) is

$$\frac{k_x^2\omega^2}{|\omega_{ci}\omega_{ce}|(\omega^2/V_A^2 - k_z^2)} \sim C\frac{V_A^2}{c^2} \tag{5}$$

where $C = (ckk_x)^2/[\omega_{ci}\omega_{ce}(\omega^2/V_A^2 - k_z^2)]$. The similarity above results from the details of how the single fluid limit is taken: $V_A^2/c^2 \to 0$ and $\omega/\omega_{ci} \to 0$, but the ratio $\omega/(kV_A) \sim 1$. Put simply, $\omega \propto V_A$. The low Alfvén speed limit may be taken by letting the density increase (but keeping ω/V_A constant) in which case everything in C is constant, and we recover WA's ordering.

It is also equivalent to order in terms of frequency (as this is proportional to V_A), in which case it is evident that the fourth derivative term in the form given in B's equation (15) becomes negligible in the low-frequency limit. Yet another alternative is to retain ω_{pe}^2 in favor of $|\omega_{ci}\omega_{ce}|c^2/V_A^2$ in which case the ratio in (5) becomes $\sim k_x^2\ell_e^2$. The latter form confirms what WA and *Wei et al.* [1994] claimed at the outset: Equation (25) of B may be written

$$\omega^2 = \frac{k_z^2 V_A^2}{1 + (2\pi\ell_e/\lambda_x)^2} \tag{6}$$

Thus we find a positive note of support from B's analysis. It is only when the scale of the wavelength perpendicular to the field (λ_x) is of order the electron inertial length that there is a significant departure from the well-known single-fluid Alfvén wave dispersion relation $\omega^2 = k_z^2 V_A^2$. Thus, if $\lambda_x \gg \ell_e$ the two-fluid correction is negligible. Since this correction comes directly from the fourth order derivative term it confirms that this term is small even for $\omega^2 \approx k_z^2 V_A^2$, in contrast to B's claim that the fourth derivative will dominate! We can only assume that B has become confused by writing the Alfvén factor $(\omega^2/V_A^2 - k_z^2)$ in the denominator. It is probably clearer to multiply B's equations (14) and (15) through by this factor. (Of course, this will not affect the solutions to the equations.)

We find nothing in B's Comment to suggest our analysis and conclusions are wrong. In fact, we find the opposite: interpreting the correct parts of his analysis appropriately vindicates our original claims! We shall finish on this positive note of agreement.

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