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# Comments on "Comment on Propagation and Dissipation of Alfvén Waves in Coronal Holes" by Suresh Chandra

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**Abstract:** We comment on the recently published paper by Chandra (Open Astronomy Journal, 2009, **2**, 16-18), and show that his results are erroneous in the context of the propagation and dissipation of Alfvén waves in polar coronal holes under individual effects of magnetic diffusivity and viscosity.

## 1. INTRODUCTION

Recently, Pekünlü *et al.* [1], and Dwivedi and Srivastava [2, henceforth DS], have interpreted respectively the plateau formation in the nonthermal velocity [3] and line-width decrement [4] in polar coronal holes as a likely signature of Alfvén wave dissipation which may play an important role in understanding the solar wind acceleration and coronal hole heating mechanisms.

Chandra [5, henceforth SC] has investigated analytically the individual roles of magnetic diffusivity and viscosity on the propagation and dissipation of Alfvén waves in polar coronal holes, and claimed that results reported by DS are not reliable either due to mistakes in the computer program or error in the used physical parameters. On the contrary, it is SC's results which are erroneous in the context of the propagation and dissipation of Alfvén waves in polar coronal holes, and we comment on his erroneous results in this rebuttal paper.

## 2. FLAWED AND PHYSICALLY INVALID RESULTS AS REPORTED BY CHANDRA (2009)

SC did not solve the dispersion relation [Eq. 4 of his paper] numerically. Instead, he has considered the individual effects of viscosity and magnetic diffusivity, and made approximations to obtain some analytical equations for group velocity, damping length scale, and energy flux density. He has derived erroneous results by solving these approximate analytical expressions, using the plasma parameters reported in Table **1**.

### 2.1. The Case of Magnetic Diffusivity Only ($\nu = 0$)

Surprisingly, SC has written in Section 3.2 of his paper that he has applied the approximation $\omega\eta << V_A^2$ based on the values given in Table 1. The erroneous results under this assumption are shown below:

(**A**) (**i**) Wrong values of damping length scale (D) in Fig. 1(a)

We calculate the values of damping length scales (D) at a few locations from the data presented by SC in his Fig. 1(a). Since it is not clear from the SC's paper for which period he has produced Fig. 1(a), we show 'D' for all the three reported periods :

(**a**) At R= 1.05$R_o$, $\omega^2 D/4\pi \sim 0.9 \times 10^{19}$ m $s^{-2}$, hence D $\sim 2.87 \times 10^{14}$ m or $\sim 4.15 \times 10^5 R_o$ (if $\tau_a = 0.01$ s), $\sim 4.15 \times 10^3 R_o$ (if $\tau_a = 0.001$ s), $\sim 4.15 \times 10^1 R_o$ (if $\tau_a = 0.0001$ s).

(**b**) At R=1.20 $R_o$, $\omega^2 D/4\pi \sim 3.6 \times 10^{19}$ m $s^{-2}$, hence D $\sim 1.14 \times 10^{15}$ m or $\sim 1.65 \times 10^6 R_o$ (if $\tau_a = 0.01$ s), $\sim 1.65 \times 10^4 R_o$ (if $\tau_a = 0.001$ s), $\sim 1.65 \times 10^2 R_o$ (if $\tau_a = 0.0001$ s).

(**c**) For R=1.35$R_o$, $\omega^2 D/4\pi \sim 3.1 \times 10^{19}$ m $s^{-2}$, hence D $\sim 9.87 \times 10^{14}$ m or $\sim 1.43 \times 10^6 R_o$ (if $\tau_a = 0.01$ s), $\sim 1.43 \times 10^4 R_o$ (if $\tau_a = 0.001$ s), $\sim 1.43 \times 10^2 R_o$ (if $\tau_a = 0.0001$ s).

These are wrong values of damping length scale which are of the order of a few tens to millions of solar radii. This unexpected result is erroneous both physically and quantitatively.

(**ii**) The group velocity of damped Alfvén waves cannot be equal to the local Alfvénic velocity in the solar plasma in which the collisional dissipating properties (viscosity, magnetic diffusivity etc.) are effectively presented [1, 6]. This is the case for the ideal plasma in which no dissipation mechanism works, i.e., angular frequency ($\omega$) is linearly proportional to the wave number (k) [7]. However, this is not the case with SC who has considered resistive coronal hole but certainly not an ideal environment. Hence, the result presented in Fig. 1(b) of SC is not valid.

(**iii**) Wrong values of "W" in Fig. 1(c)

SC has estimated wrong values of energy fluxes in the coronal context. He has used wrong and dimensionally incorrect formula for the energy flux, which is actually the volumetric energy. The actual formula of the energy flux must have the units ergs $cm^{-2}$ $s^{-1}$ (in CGS) or Joule $m^{-2}$ $s^{-1}$ (in SI) [8, 9]. However, Fig. 1(c) caption and SC's formula for energy flux shows the dimension "kg $m^{-1}$ $s^{-2}$", which is ergs $cm^{-3}$ (in CGS) or Joule $m^{-3}$ (in SI). Hence, the formula for energy flux is wrongly derived in this section. It should be noted that DS have used the correct expression for the estimation of energy fluxes. SC is

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probably not aware of the coronal requirements of the energies. This is of the order of ~ $10^6$ ergs $cm^{-2}$ $s^{-1}$ for active regions, ~ $10^5$ ergs $cm^{-2}$ $s^{-1}$ for the quiet Sun, and ~ $10^4$ ergs $cm^{-2}$ $s^{-1}$ for coronal holes [10].

**(B)** The group velocity and energy flux density of damped Alfvén waves in the resistive coronal hole plasma must be frequency and/or wavelength dependent [1], however, in less amount as these waves are weakly dispersive in the coronal context. Hence, SC's absolute claim for frequency-independent group velocity and energy flux in the dispersive coronal hole plasma is erroneous.

### 2.2. The Case of Viscosity Only ($\eta = 0$)

SC has written in the Section 3.1 of his paper that he has applied the approximation $\omega\nu >> V_A^2$ based on values given in Table 1. We calculate the values of damping length scales (D) at a few locations from the data presented by him. Since it is not clear in SC's paper, for which period he has produced Fig. 1(d), we show 'D' for all the three reported periods :

**(a)** At R = 1.05 $R_o$, $D(\omega)^{1/2}/(8\pi^2)^{1/2}$ ~ 1.25 × $10^5$ m $s^{-1/2}$, hence D ~4.43 × $10^4$ m or 0.044 Mm (if $\tau_a$ = 0.01 s), ~ 0.014 Mm (if $\tau_a$ = 0.001 s), ~ 0.0044 Mm (if $\tau_a$ = 0.0001 s).

**(b)** At R = 1.20 $R_o$, $D(\omega)^{1/2}/(8\pi^2)^{1/2}$ ~ 8.0 × $10^5$ m $s^{-1/2}$, hence, D ~ 2.835 × $10^5$ m or 0.283 Mm (if $\tau_a$ = 0.01 s), ~ 0.089 Mm (if $\tau_a$ = 0.001 s), ~ 0.028 Mm (if $\tau_a$ = 0.0001 s).

**(c)** At R = 1.35 $R_o$, $D(\omega)^{1/2}/(8\pi^2)^{1/2}$ ~ 11.0 × $10^5$ m $s^{-1/2}$, hence, D ~ 3.9 × $10^5$ m or 0.390 Mm (if $\tau_a$=0.01s), ~0.123Mm(if $\tau_a$ = 0.001 s), ~ 0.039 Mm (if $\tau_a$ = 0.0001 s).

These damping length scales of weakly dispersive waves are not violating the coronal physics. However, they are unexpectedly very much shorter than the scale heights in the inner corona. The use of the formula of energy flux is again incorrect and thus derived values in Fig.1(f) is also incorrect.

## 3. CONCLUSIONS

It should be noted that Pekünlü *et al.* [1], and DS have reported the same form of their main dispersion relations [Eq. (13) of Pekünlü *et al.* [1]; Eq. (1) of DS; Eq (4) of SC]. Similar to Pekünlü *et al*. [1], DS have also solved the general dispersion relation and obtained their main results under the combined effect of viscosity and magnetic diffusivity to compare with the observations. The qualitative nature of both the results in the same spatial range is the same under the combined effect of viscosity and diffusivity. These were the main results of DS, while they have only discussed the limiting cases of the individual effects of viscosity and magnetic diffusivity as sub results. DS have put the magnetic diffusivity ($\eta$) = 0, and kinematic viscosity ($\nu$) = 0 in their general dispersion relation (cf., their Eq. 1), and obtained the polynomial forms of their equations (cf., their Eqs. 2a-2b; 3a-3b). DS did not make any approximations to obtain polynomials which directly come from the general dispersion relation, hence they are physically valid. Using the plasma parameters obtained from different empirical relations mentioned in their paper, DS have solved numerically those polynomials to obtain their sub-results also correctly. It is also to be noted that the difference in the magnitude of plasma parameters only introduces different magnitude of wave parameters, and should not affect the basic physical characteristics of the waves.

Since SC's results are erroneous, he could not even compare his results with the sub-results of DS who directly solved the polynomials without applying any approximations. Hence, the base of SC paper commenting on the results of DS, is both unreliable and unfortunate. On the contrary, it is SC's results which are seriously flawed both physically and quantitatively in the coronal context. SC has wrongly interpreted the work of DS which gives the theoretical support to the observational findings of two well established results [4, 11] under the combined effect of kinematic viscosity and magnetic diffusivity. SC has also wrongly interpreted the well established plasma diagnostics [12], and temperature profile derived by Pekünlü *et al.* [1] which is based on the seminal work of David *et al.* [13].

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