# Response to "Comment on 'Scaling of asymmetric magnetic reconnection: General theory and collisional simulations' " [Phys. Plasmas 16, 034701 (2009)]

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The comment by Semenov *et al.* has called into question our derivation of the outflow velocity in asymmetric magnetic reconnection. We present three reasons that the analysis presented in the comment is incorrect. Most importantly, the authors of the comment have incorrectly applied results from one-dimensional shock theory to the problem of conservation through a two-dimensional dissipation region. For completeness, we compare their predictions to numerical simulation results, finding that their theory does not describe the data. We conclude the analysis in the comment is without merit. © *2009 American Institute of Physics*. [DOI: 10.1063/1.3083264]

The comment by Semenov *et al.* questions the derivation of the outflow speed $v_{\mathrm{out}}$ during two-dimensional antiparallel asymmetric reconnection with arbitrary upstream densities and magnetic field strengths presented in Ref. 1. The derivation in question is a Sweet–Parker-type analysis based on conservation of mass, energy, and magnetic flux through the dissipation region.

The authors of the comment argue that the omission of magnetic flux across the outflow edge of the dissipation region in the theory invalidates the theory. To wit, this effect is also ignored in the original Sweet–Parker analysis[2] because it is negligible compared to the kinetic energy flux across the outflow edge. We will show that it is negligible in asymmetric reconnection as well, contrary to what was stated in the comment.

While it should be possible (although of questionable utility) to incorporate higher order effects into the Sweet–Parker analysis, we will show that the analysis presented in the comment has at least three fundamental errors which invalidate it. Most importantly, the authors inappropriately apply the Rankine–Hugoniot jump conditions for one-dimensional shocks to flux conservation through a two-dimensional dissipation region.

Lastly, it is important to note that the scaling results for the outflow speed $v_{\mathrm{out}}$ and the reconnection rate $E$ predicted in Ref. 1 have been successfully compared to numerical simulation results. Our own simulations confirmed these results for Sweet–Parker reconnection in resistive magnetohydrodynamics (MHD) simulations for asymmetric fields[1] and asymmetric densities[3] and for Hall reconnection in two-fluid (Hall-MHD with electron inertia) simulations for all combinations of asymmetries.[4] In addition, the reconnection rate $E$ (whose derivation depends on the relation in question) has been confirmed by simulations from other groups[5–9] in various settings using various simulation techniques.

The authors of the comment offer no simulation results in favor of their analysis, nor do they offer an explanation for why these disparate simulations agree with the theory in Ref. 1. To be completely transparent, we compare the predictions of the comment to our simulation results. We find that their theory does not describe the data.

The crux of the argument in the comment comes from the application of conservation of energy through the dissipation region. Equation (8) of our paper is

$$\oint_S d\mathbf{S}\cdot\left[\left(\mathcal{E}+P+\frac{B^2}{8\pi}\right)\mathbf{v}-\frac{(\mathbf{v}\cdot\mathbf{B})}{4\pi}\mathbf{B}\right]=0$$

(see Ref. 1 for definitions of quantities in question). Our scaling analysis argued that

$$L\left(\frac{B_1^2}{8\pi}v_1+\frac{B_2^2}{8\pi}v_2\right)\sim 2\delta\left(\frac{1}{2}\rho_{\mathrm{out}}v_{\mathrm{out}}^2\right)v_{\mathrm{out}}, \qquad (1)$$

which physically expresses that the magnetic energy entering the inflow edge of the dissipation region scales like the kinetic energy of the outflow.

The comment authors wish to include the magnetic energy due to the newly reconnected field line in the energy balance, their Eq. (5). We show that this effect is negligible. As in the comment, we restrict our discussion to the case of asymmetric magnetic field but symmetric density for simplicity. The ratio of the magnetic to kinetic energy flux across the outflow edge of the dissipation region scales like

$$\frac{B_y^2/8\pi}{\rho v_{\mathrm{out}}^2/2}\sim\frac{B_y^2}{B_1B_2}, \qquad (2)$$

where we used $v_{\mathrm{out}}^2 \sim B_1B_2/4\pi\rho$ from Eq. (15) of Ref. 1. Since $\nabla\cdot\mathbf{B}=0$, we expect $B_y/B_x=\mathcal{O}(\delta/L)$, where $\mathcal{O}$ means "on the order of." As such,

$$\frac{B_y^2/8\pi}{\rho v_{\mathrm{out}}^2/2}=\mathcal{O}\left[\left(\frac{\delta}{L}\right)^2\right]. \qquad (3)$$

For Sweet–Parker reconnection, $\delta/L$ scales like $\eta^{1/2}$ [see Eq. (35) from Ref. 1], so is very small. The largest this effect could be is for Petschek-like reconnection, but since $\delta/L \sim 0.1$, the contribution of the magnetic field to the energy flux out of the outflow edge of the dissipation region is only at the 1% level and is therefore negligible. The assertion in the comment that the magnetic field at the outflow edge is $(B_2-B_1)/2$ is simply not correct.

This being said, suppose one wanted to incorporate this effect into the theory to get a more precise scaling result. The manner in which the analysis in the comment was carried out is not correct, as can be seen in three ways.

(1) In Eq. (5) of the comment, the last term on the right hand side comes from the $(\mathbf{v}\cdot\mathbf{B})\mathbf{B}$ term in Eq. (8) of Ref. 1. However, at the outflow edge, their $B_{\text{out}}$ is in the $y$ direction, but $d\mathbf{S}$ is in the $x$ direction, so $d\mathbf{S}\cdot\mathbf{B}\simeq 0$ and there is no contribution.

(2) In Eq. (5) of the comment, the authors have included the effect of the magnetic energy flux across the outflow edge of the dissipation region, but have not included similar terms corresponding to the kinetic energy flux across the inflow edge of the dissipation region. The ratio of the kinetic to magnetic energy flux into the inflow edge of the dissipation region is scales like

$$\frac{\rho v_1^2/2}{B_1^2/8\pi} \sim \frac{B_2}{B_1}\left(\frac{B_2}{B_1+B_2}\right)^2\left(\frac{2\delta}{L}\right)^2, \tag{4}$$

where we used $v_1^2 \sim (cE/B_1)^2 \sim [B_2/(B_1+B_2)]^2 v_{\text{out}}^2 (2\delta/L)^2$ from Eq. (19) of Ref. 1. As such, the kinetic energy flux into the inflow region is of the same order correction $(\delta/L)^2$ as the magnetic energy flux out of the outflow edge. It is logically inconsistent to incorporate some higher order effects while not keeping other effects of the same order.

(3) Most importantly, in Eqs. (8) and (9) of the comment, the authors have used Rankine–Hugoniot conditions from one-dimensional shock theory. The expressions come from integrating the MHD equations across a one-dimensional discontinuity, which assumes that all the flux which enters from one side of the discontinuity must exit the other side. This is in stark contrast to the analysis in Ref. 1, which considers the flux through a *two*-dimensional dissipation region. Flux is allowed to leave the edges of the dissipation region, so the one-dimensional relations do not apply to the dissipation region.

To see this more clearly, Eq. (10) follows from Eqs. (8) and (9) only when using $\{v_n\}=0$ and $\{B_n\}=0$, where $\{F\}=F_{\text{in}}-F_{\text{out}}$ as defined in the comment. These expressions, again, come from one-dimensional shock theory. However, they are patently false when applied to the dissipation region, as the inflow speed $v_{\text{in}}$ does not equal the outflow speed $v_{\text{out}}$. Therefore, the use of Rankine–Hugoniot jump conditions in the present analysis is not appropriate.

Another way to see that the result in the comment for $v_{\text{out}}$ is incorrect comes from taking various limits. In the limit in which one of the two magnetic field strengths goes to zero, the outflow speed in Eq. (14) of the comment remains finite (nonzero), which also implies a nonzero reconnection rate. This is clearly unphysical, as no reconnection should proceed in the limit in which one magnetic field vanishes. In contrast, our Eq. (15) giving the outflow speed as $v_{\text{out}}^2 \sim B_1B_2/4\pi\rho$ correctly predicts no reconnection in the zero field limit.

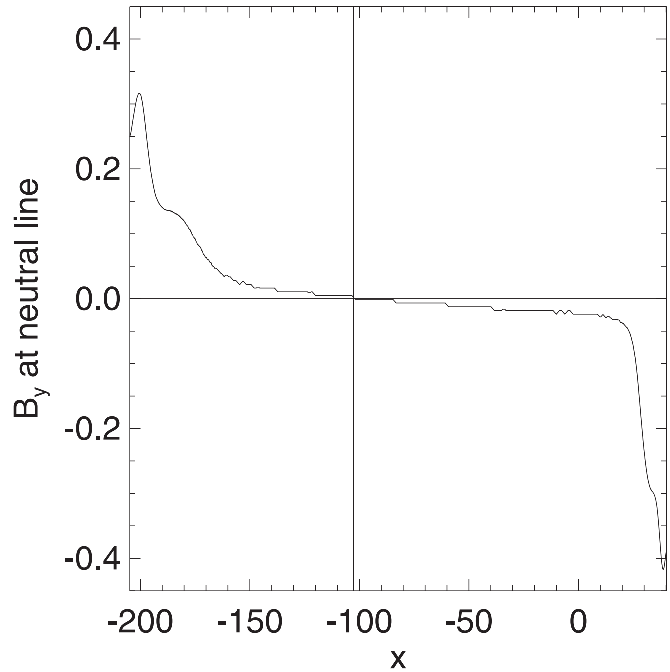


FIG. 1. Reconnected magnetic field $B_y$ at the location of the neutral line as a function of downstream distance $x$. The values reached are far smaller than that predicted in the comment.

For completeness, we compare the predictions of the comment to our simulation results. Figure 1 shows a plot of $B_y$ (which corresponds to their $B_{\text{out}}$) as a function of $x$ along the neutral line for the $[B_{01},B_{02}]=[1,3]$ simulation from Ref. 1. The vertical line shows the location of the X-line. The outflow edge of the dissipation region occurs near the break in $B_y$. The amplitude of $B_y$ at the break is on the order of 0.1, and probably closer to 0.05. This is nowhere near the prediction of $(B_2-B_1)/2=1$ predicted in Eq. (13) of the comment.

To test Eq. (14) from the comment for the scaling of the outflow speed, we begin by normalizing the predictions to the outflow speed of the symmetric ($[B_{01},B_{02}]=[1,1]$) simulation [labeled "(sym)"] as follows. The prediction from Eq. (14) of the comment is

$$\frac{v_{\text{out}}}{v_{\text{out}}(\text{sym})}\frac{B(\text{sym})}{B_1} = \frac{1}{2}\left(1+\frac{B_2}{B_1}\right), \tag{5}$$

while the prediction from Eq. (15) of Ref. 1 is

$$\frac{v_{\text{out}}}{v_{\text{out}}(\text{sym})}\frac{B(\text{sym})}{B_1} \sim \left(\frac{B_2}{B_1}\right)^{1/2}. \tag{6}$$

The data from the resistive MHD simulations in Ref. 1, as shown in Table I, are plotted in Fig. 2. The solid line represents the prediction of Ref. 1, while the dashed line represents the prediction of the comment. Clearly, the prediction in the comment does not represent the data.

Finally, the comment authors criticized our statement in Ref. 4 about the previous knowledge of asymmetric reconnection. However, the authors misquoted the paper. We were

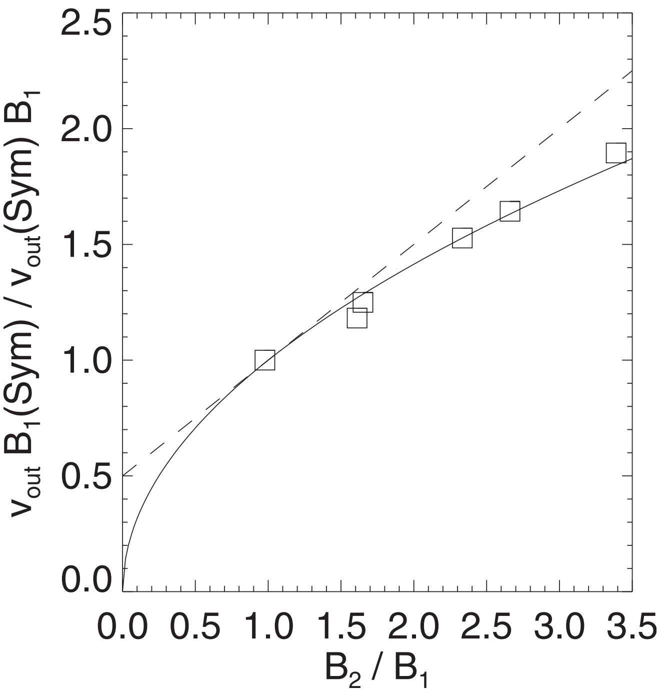


FIG. 2. Plot of the outflow velocity data from resistive MHD simulations normalized to reference symmetric simulation results. The prediction from the comment is the dashed line, the prediction of Ref. 1 is the solid line. The prediction of the comment does not represent the data.

very careful to state that "little was known about *the scaling* of asymmetric reconnection" (emphasis added), which we interpret as "the functional dependence of reconnection parameters on the upstream fields $B$ and densities $\rho$." (The statement in the comment left out "the scaling of.") The comment authors noted that several aspects, including stress balance have been previously addressed (see the comment for references). In our statement, we were interpreting "reconnection parameters" as the reconnection rate, outflow speed, and dissipation region structure (see also Ref. 3). We regret if our statement was interpreted to mean that no important studies on asymmetric reconnection had previously been carried out. Understanding the structure of discontinuities downstream of the dissipation region is a very important problem, which is why in Ref. 1 we had multiple references to previous analytical studies, including two papers by the comment authors.

In conclusion, we have shown that the inclusion of the magnetic energy flux out of the dissipation region is not necessary because it is negligible. The analysis in the comment purporting to do so is incorrect, and the results of their analysis do not agree with the simulation results. We conclude that the analysis in the comment is without merit.

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