COMMENTS

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Comment on "Collisionless shock and supernova remnant simulations on VULCAN" [Phys. Plasmas 8, 2439 (2001)]

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This recent paper reports some real advances in experimental technique, but is misleading or incorrect in several places. First, the design assumes without discussion that the magnetic field will completely penetrate the plasma, but this is not likely. Second, when the magnetic field is present the surfaces of the converging plasmas will be Rayleigh–Taylor unstable. Third, any shocks produced in experiments like those reported may be collisionless but have no relevance to shocks in supernova remnants. Fourth, the experiment is not a meaningful hydrodynamic simulation of a supernova remnant. Finally, the hydrodynamic simulation results reported are also in error, leading to incorrect values for some scaling parameters. © *2002 American Institute of Physics.*
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The recently published paper[1] of Woolsey *et al.*, referred to here as "paper W," discusses the production of strong magnetic fields over mm-scale volumes and the application of such fields to colliding plasma experiments. This work, and the experimental evidence regarding the interaction of expanding plasmas in such a field, is in the opinion of this author, first-rate experimental plasma physics. Unfortunately, the remainder of paper W, from its title, to its scaling arguments relating to Supernova Remnants (SNRs), to its theoretical results, has a number of problems. Some of these are discussed in this Comment.

The essential scheme of the experiment is to form a magnetic field of about 20 T throughout a volume that contains two thin (100 nm) CH sheets spaced by 1 mm in vacuum, after which lasers explode the sheets. This causes the two plasmas from the CH sheets to expand into the magnetized region. These plasmas collide, presumably causing shocks to form that propagate back into the expanding plasmas.

In the design section of paper W, it is apparently assumed that the magnetic field will immediately penetrate these plasmas, so that the shocks that develop after the two plasmas collide will be magnetized shocks. This is the apparent assumption (not discussed in that part of the paper), because the field magnitude throughout the plasma is given in Table II as 20 T, which is the nominal value of the vacuum field. This assumption is completely unreasonable. Given its high conductivity, the plasma will exclude the magnetic field. This has nothing to do with magnetohydrodynamic theory or the ion gyroradius, but rather is a consequence of Maxwell's equations and Ohm's law.

Quantitatively, one can evaluate the field penetration depth, using, for example,[2] the field diffusion coefficient given in Ryutov *et al.* One finds that on the 100 ps time scale of interest, the field can penetrate only the coldest leading edge of the two plasmas, and only before they begin to compress and heat as they approach one another. (In the calculations reported here, the plasma is taken to be pure hydrogen, because the faster hydrogen atoms should form the leading edge of the expansion, as previously observed.[3]) In the presence of a sufficiently large source of anomalous resistivity, some field penetration could occur. But if the authors had intentionally designed this experiment so that it required anomalous resistivity in order to succeed, then they should have said so in the paper. They also should have explained (a) why they believed such anomalous resistivity would be present, and (b) how one could know the value of the anomalous resistivity with sufficient accuracy to obtain meaningful and significant results from the experiment. One must conclude that the values of the magnetic field, the gyroradii, and β given in Table II are all probably incorrect and certainly unreliable and unsubstantiated.

It is worth noting that although the magnetic field will not penetrate the expanding plasma, it might mix with it through the magnetic Rayleigh–Taylor instability. The authors disregard this possibility, on the grounds that "the magnetic field will be pushed aside as the plasmas converge." Unfortunately, they are incorrect. Whether or not the converging plasmas succeed in pushing aside the magnetic field, they do push on the field. By Newton's third law, the field pushes back. In resisting the plasma expansion, the field will cause a pressure gradient that opposes the density gradient, producing a positive growth rate for the Rayleigh–Taylor instability at the surface of the plasma. The related theory is well understood.[4,5] This instability has been observed in the Crab Nebula.[6] It probably has also been observed in Z-pinch

plasmas.[7] It is to be hoped that, in future work, the authors assess the growth rate of this instability and investigate whether such experiments could have real relevance to the dynamics in the Crab.

Moving on from magnetic fields to collisions, the authors argue that the experiment is collisionless, based on a calculation that the ion–ion collision length λ_{ii} is comparable to the system size L. This comparison, if correct, would show that any shocks that formed must do so by noncollisional effects. In this sense, such shocks would be collisionless shocks. However, the authors want to make a stronger claim. Their stated intent is to produce collisionless shocks that are relevant to SNRs. Collisionless shocks in SNRs, and in related astrophysical systems, develop on spatial scales determined by MHD and related kinetic instabilities. The scale of the shock transition in such systems is the ion gyroradius. To perform experiments that are relevant to such systems, one must produce a system in which λ_{ii} is large compared to the ion gyroradius, r_{Li}, and in which r_{Li} is very small compared to L. The design of the authors, which claims to have $r_{Li} \approx L$, manifestly fails to accomplish this goal. Indeed, a recent paper,[8] which analyzed in detail the tradeoffs involved in simulating astrophysical collisionless shocks, concluded that this goal cannot be met by laser experiments working with mm or cm volumes. One concludes that the experiment described here, which has $r_{Li} \approx L$, cannot possibly produce results that are relevant to collisionless shocks in SNRs.

The authors also argue that the experiment represents a hydrodynamic simulation of an SNR, supporting this claim with a table. Yet this experiment has only one feature in common with an SNR, which is a strong shock wave that slows and heats an expanding plasma. If it were collisionless in the same sense as the SNR shock, which is not the case here, then such a system would be of considerable interest to SNR physics. However, except for the presence of a strong shock, this experiment is not meaningful as a hydrodynamic simulation of an SNR. Table III in the paper (and the text as well) show that the authors misunderstand the concept of Euler similarity as developed[9] by Ryutov *et al.* One is permitted only one comparison between two systems that may be similar. The authors make two comparisons, in Table III. The initial conditions matter; in particular the density profiles must be similar. However, the profile in the experiments described here never resembles that of an SNR beyond the presence of a strong shock. In addition, the number, Eu, should *not* be defined in terms of a sound speed. The pressure and density need not be taken from the same location, but in the usual event that the spatial profiles of all the parameters are not completely identical they must be chosen at locations where their values matter for the evolution of the system. Also, they must be chosen at corresponding locations in the two systems that are being compared. The authors show no awareness of this. The analysis and discussion in the paper is not a meaningful application of the Euler similarity. One concludes that the experiment described here is not a meaningful hydrodynamic simulation of an SNR.

With regard to the description of the experiment, Table I and Fig. 2 are also substantially in error, with regard to the upstream ion temperature. Fundamentally, the expanding plasmas that collide in this case (with no magnetic field) are supersonic. As a result, no hydrodynamic information can propagate upstream, in the laboratory frame, except as a shock wave. However, the second frame in Fig. 2 shows a large increase in ion temperature that propagates upstream beginning before the interacting plasmas produce a shock wave. This temperature, however, does not affect the ratio of the pressure and the density profiles. This is not physically correct, as the electron temperature is not the dominant source of pressure here. Table I likewise shows a very large value for the upstream ion temperature (50 keV). This is not a sensible result. I was able to reproduce the evolution of the density, velocity, and pressure in these simulations using the Lagrangian hydrodynamics code HYADES.[10] (However, to get the timing right required me to assume that the wavelength on target was 0.53 μm, not 1.05 μm as the text indicates.) The ion temperature produced in this calculation behaved as one would expect from the above discussion. The inferred sound speed was consistent with the value given in Table IV of paper W, which is more evidence that the ion temperature shown in Table I is incorrect. Using the correct value of the ion temperature (about 100 eV), one finds that λ_{ii} in the upstream plasma is 0.3 μm, so that this plasma is rather collisional. One concludes that the second line of Table I is substantially in error.

Regarding the title, this discussion has shown that these experiments are not supernova remnant simulation experiments. They might produce collisionless shocks, but if so these shocks will not be relevant to SNRs. It is regrettable that the authors chose to make these unsustainable claims rather than to focus upon the impressive advances in experimental technique that they did obtain.

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