Technical Comments

Comments on "Satellite Potential in an Ionized Atmosphere"

S. H. Lam*
Princeton University, Princeton, N. J.

In the April 1965 issue, the AIAA Journal published an article by N. C. Jen[1] on the problem of a spherical satellite moving in an ionized atmosphere. Since full-length journal papers are subject to "rigorous technical review," I was distressed to find in this paper a number of serious errors which apparently escaped the attentions of the editors.

Jen considered a collisionless plasma flow about a sphere. His goals were to obtain the self-consistent potential field distribution and in particular the satellite potential, the "Coulomb drag," and the wake trail patterns. In this note, I shall point out only the major errors.

1) Jen assumed that the potential dropped rapidly from the value at the satellite wall to a negligible value within a thin sheath of the order of several Debye lengths. External to the sheath, the region was considered field-free, and the charged particles behaved as neutrals. Since the sheath was assumed thin, the edge of the sheath then forms a near sphere of a slightly larger radius. By a somewhat tortuous but completely false argument, Jen arrived at the conclusion that the sheath acts as a specularly reflecting surface (item 3). At this point, one can already conclude, within Jen's model so far, that the solution of this problem in the allegedly field-free region is identical to that of a collisionless neutral flow over this near sphere of specularly reflecting surface. In particular, the drag will be completely determinate and independent of the satellite potential. However, the number density distributions of ions and electrons so calculated in the allegedly field-free region will be different since the speed ratios of the two flows are different. Thus, we arrive at a contradiction to his field-free assumption via Poisson's equation.

2) Jen assumed that the charged particles that struck the satellite wall would first recombine, then would be "dissassociated," and be "diffused out or reflected back (as charged particles), from the satellite surface. . . ." This novel model has never been suggested in the literature and is, therefore, an original alternative to replace the usual assumption that the charged particles simply recombine and are reemitted as neutrals. Note that Jen's new model gives rise to no net current regardless of the satellite wall potential, and his later requirement of zero current to the satellite in Eq. (13) becomes difficult to explain. Furthermore, Jen's statement that "the striking velocity and the reflecting velocity (of the charged particles) must be the same from the statistical view point," is, of course, completely false.

3) In requiring zero current across an elementary area on the satellite surface, Jen wrote Eq. (13) to determine the satellite wall potential (without justification or derivation):

$$(C_+ + W\cos\theta)e^{q\psi_0/kT} = (C_- + W\cos\theta)e^{-q\psi_0/kT}$$

where C_- and C_+ are mean thermal speeds of electrons and ions, W is the speed of the satellite, and ψ_0 is the satellite wall potential. Ignoring the difficulty mentioned in 2), we see that Jen has a satellite made of a nonconductor since ψ later turned out to be a function of θ. Furthermore, when $C_- > W > C_+$, at the rear of the satellite where $\cos\theta < 0$, no solution for ψ_0 exists. For precisely this case, however, Jen states that "ψ_0 becomes extremely large" and on the strength of this observation, rests all his conclusions about the wake trail befavior.

4) To compute the so-called "Coulomb drag," Jen invoked the following relation:

$$p = n_i kT$$

where p is called pressure intensity, n_i is the ion number density, and T is the undisturbed ion temperature. His Coulomb drag was obtained by integrating the component of this pressure force acting on the satellite wall. For good measure, Jen also suggested that, depending on whether $W \gtrless C_+$, the rear part of the satellite may or may not contribute to this integration. His Coulomb drag depends exponentially on ψ_0.

The previous criticism is by no means complete. For example, Jen somehow saw fit to invoke Boltzmann's H Theorem to justify Eqs. (6) and (7). No useful purpose will be served to identify all of the controversial points. It is difficult to believe that all of the points raised here could have been overlooked if this paper has, indeed, had a rigorous technical review.

Reference

[1] Jen, N. C., "Satellite potential in an ionized atmosphere," AIAA J. 3, 714–717 (1965).

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* Associate Professor. Member AIAA.

Reply by Author to S. H. Lam

Norman C. Jen*
City College of New York, New York, N. Y.

Lam has apparently misunderstood the whole paper but made a "vigorous technical review" of "collisionless neutral flow about a sphere" which is significantly different from the paper. The paper clearly indicated that the particles are electrically charged. In the field-free region, these charged particles are considered to be Maxwellian with large mean free paths. The plasma has the basic property of electrical neutrality.

The mechanism of reflection of electrons from the sheath has been analyzed and agrees with Spitzer's statement[1]: "In equilibrium a potential gradient arises near the wall, reflecting most of the electrons into the plasma, the number striking the wall being equal to the corresponding number of positive ions reaching the wall." This paper also indicated that the equipotential surface is nonspherical. On the other hand, Lam made a number of statements, such as "one can already conclude within Jen's model so far that the solution of this problem in the allegedly field-free region is identical to that of a collisionless neutral flow over this near sphere of specularly reflecting surface;" "the edge of the sheath then forms a near sphere of a slightly larger radius;" and "we thus arrive at a contradiction to his field-free assumption via Poisson's

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* Professor, Department of Civil Engineering, School of Engineering and Architecture. Member AIAA.