TECHNICAL MEMORANDUM

Plasma Physics Laboratory
Princeton University
Princeton, N.J.

Comments on the One-Dimensional Sheath Problem

Peter L. Auer and Francis F. Chen

Tech. Memo. No 141 December 29, 1961

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PLASMA PHYSICS LABORATORY

Contract AT(30-1)—1238 with the
US Atomic Energy Commission

PRINCETON UNIVERSITY
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AEC RESEARCH AND DEVELOPMENT REPORT

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Introduction

One of the most fundamental problems in plasma physics which is still not completely solved concerns the formation of the sheath at a wall in one-dimensional geometry. The difficulty lies in the fact that in the strictly one-dimensional case, it is not clear that the sheath problem can be isolated from the mechanism of charged particle production in the body of the plasma. Interest in the one-dimensional problem arises from the use of high magnetic fields in modern plasma physics; for instance, the problem of a probe drawing saturation ion current in a strong magnetic field is essentially the same as the problem of the sheath drop at the wall of a plane-parallel discharge.

In a well-known paper, Bohm showed that at least one property of a sheath is independent of the ionization mechanism; this was the minimum velocity of ions entering the sheath. (This was actually first discovered by Langmuir in the 1920's.) Recently, F.F. Chen showed that Bohm's unrigorous treatment yields the correct result except in very weakly ionized plasmas. Simultaneously, P.L. Auer presented a paper at Munich ostensibly proving the opposite result: that ions entering a sheath have a maximum velocity. An exchange of letters followed which clarified this discrepancy as well as some other aspects of this problem. These letters are reproduced here for the benefit of anyone who may be interested in this problem.

The comments herein will be more comprehensible if the reader has an acquaintance with the following papers:

Bohm, D., Characteristics of Electrical Discharges in Magnetic Fields, ed. by A. Guthrie and R.K Wakerling, Chap. 3 (McGraw-Hill, New York, 1949).

Harrison, E.R. and Thompson, W.B., Proc. Phys. Soc. 74, part 2, 145 (1959).

Auer, P.L., "The Space Charge Sheath in Low Pressure Arcs", Proceedings of Fifth International Conference on Ionization Phenomena in Gases, Munich, 1961 (North-Holland Publ. Co., Amsterdam).

Chen, F.F., "The Sheath Criterion", Matt-77 (1961).

The following papers provide additional background:

Tonks, L., and Langmuir, I., Phys. Rev. 34, 876 (1929).

Allen, J.E., and Thonemann, P.C., Proc. Phys. Soc. B67, 769 (1954).

October 30, 1961

Dr. Francis F. Chen
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Dear Frank:

Thanks very much for your letter of October 17th and the enclosed reports. Although I haven't had a chance to study them in detail yet, in glancing over the one on sheath criterion it seems to me that your treatment has a lot in common with the one given by Allen and Thonemann (Proc. Phys. Soc., B, 67, 769, 1954). Thus, my objection to your treatment is the same as my voiced objection to the Allen and Thonemann treatment. The objection is partially philosophical and might give the impression of being hair-splitting. Nevertheless, briefly it goes as follows:

In my forthcoming Nuovo Cimento article I point out that the reason a constraint becomes imposed on the ions is because of the requirements of stress balance. I think your treatment is aware of this fact also. The question is, recognizing stress balance, what general statement can we make about the ions. It seems to me that arguing about ion terminal velocities is ambiguous. Consider an arbitrary plane in the plasma. To the left of it, within arbitrary small distances of this plane, we assume the plasma is neutral, i.e., neutral to a very high degree of approximation. Some distance to the right of this plane we have a collecting surface which is biased negative in order to collect ions. Let us assume that in the intervening space between plane and collecting surface ion production is virtually nil. The ion current flowing to the collector will be ambipolar in that the ion current is exactly balanced by the electron current. Nevertheless, for the most part this space will be ion space charge dominated since the electron density contributed by the electron current is vanishingly small in comparison with the ion density. Thus, this intervening space between imaginary plane and collector can be divided conceptually into two regions. A region immediately to the right of the plane is a transition region over which the ion space charge density must build up; i.e., there will be a region where the electron density drops much more rapidly than the ion density. If the Debye distance is small compared with other pertinent dimensions, then the transition region over which the ion space charge builds up will be of the order of Debye length. Now, assuming that the distance between our imagined plane and collector is small compared to ion collision mean free paths, then for the large part the second region of this space will be described by space charge limited current considerations, i.e., the ion current flows under space charge limited conditions.

As you well know, three parameters specify space charge limited current flow; these are the current density, the voltage drop, and the spacing distance. Thus, the terminal ion velocity at the collector plate will be given by the potential drop across the space charge dominated collision free region and the distribution in velocities of ions arriving at the edge of this region. But you will note that this potential drop is highly arbitrary. It can be just about anything you wish to make it, because the character of a space charge dominated region is such that if a given space charge limited current is to be collected the space charge sheath will contract or expand in such a way as to accommodate the applied voltage across the sheath. Therefore, the terminal velocity is to some extent a function of externally applied conditions.

On the other hand, the ion flux which is to be collected is a function only of the discharge parameters. I personally believe that a criterion in terms of the ion flux is superior to a criterion based on terminal velocities because the ion flux is not sensitive to externally imposed parameters and, more important, the ion flux is precisely what a simple probe measures. In one particularly simple situation where the ion distribution can be trivially related to macroscopic quantities everywhere, I have been able to demonstrate that the ion flux enters directly into the sheath criterion. The reason this criterion differs from the usual Bohm criterion is essentially because the ions do not arrive at the edge of the space charge sheath with a monoenergetic distribution.

I have not had an opportunity to consider the situation in which the body of the plasma is collision dominated and to discover how this affects the space charge sheath criterion. However, I feel that the actual nature of the velocity distribution must somehow enter the criterion and the desirable form of the criterion will involve the ion flux rather than the terminal ion velocity.

I am looking forward to further exchanges of ideas with you on this subject. With best wishes,

Sincerely yours,

P. L. Auer

December 8, 1961

Dr. Peter L. Auer
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Dear Peter:

I have finally had a chance to think about your comments in your Munich paper and in your letter to me on October 30. I found that your very clear and comprehensive treatment of the sheath problem (for the particular case of no collisions) was very enlightening to me, and what I have to say will not detract from the beauty of your analysis.

It will be convenient henceforth to refer to your Munich paper, Bohm's chapter in Gutherie and Wakerling, and my MATT report as (A), (B), and (C), respectively.

1) The most important contribution of (A) is in my mind the unambiguous definition of a sheath edge (Theorem II). I do not agree, however, that the results of (A) contradict those of (B) nor that (A) differs from (B) because ion velocities are considered in (A). In the first place, Theorem I in (A) states that

$$J_p \geqslant 0.488\ N_o\ v_a,\ \text{where}\ v_a \equiv (KT/M)^{1/2}.$$

If we take your definition of the sheath edge as the point where $\eta = \eta^* = 0.854$, then the density N_s there is

$$N_s = N_o e^{-.854},$$

and hence

$$J_p \geq 1.15\ N_s\ v_a.$$

This corresponds to an average ion velocity of 1.15 v_a, in close agreement with Bohm's value of 1.0 v_a. The difference, I think, is merely that you have defined precisely where N_s is to be taken, whereas Bohm did not.

Secondly, (A) cannot possibly have taken the distribution of ion velocities into account, since the result (Thm.I) is perfectly general, independent of the form of the production function $j_p'(\eta)$, which determines the ion distribution. In particular, if j_p' were restricted to be non-vanishing only for very small η, the ion stream would be almost monoenergetic, and the condition of (B) should be approached. As you pointed out in the last section of (A), the inequality of Thm.I approaches an equality when η_c is small. The spread in ion velocities merely determines by how much J_p exceeds the minimum value; the limiting value itself is unchanged and should correspond to that calculated by Bohm. (see postscript)

In my opinion, as long as $kT_i < kT_e$, which is the case of interest, the distribution of ion energies should have little effect on the critical current or velocity (these are related merely by N_s, which is known). To show this, consider a monoenergetic ion stream of velocity $v \sim v_a$. After acceleration in a potential drop the ion density will be decreased by a factor α. Now split the ion stream into two streams of velocity $v \pm \delta$, so that $\overline{v} = v$. After acceleration in the same potential drop, the density will be decreased by α'. It is easy to see that α differs from α' only by a term of order $(\delta/v_a)^2$, so that to the extent that kT_i is less than kT_e, $n_p(\eta)$ is not much affected by the ion velocity spread. This was mentioned in (C).

Third, the result of Thm.II of (A) that the ion velocity v_s at the sheath edge has an upper bound is a consequence of the particular ion acceleration scheme chosen. In fact, Thm.III is a most complicated way to get a trivial result. Certainly $1/2\ MU_p^2$ will be less than the greatest possible potential drop through which the ions could have fallen. If we insert the value of η^* in Thm.III, we find that $U_p(x) \leqslant 1.3\ v_a$. This is perfectly consistent with the result that $v_s \geqslant 1.15\ v_a$. The lower bound is imposed by the condition that the charge densities must be equal over a finite range of η — as in (B) and (C). The upper bound is imposed by the mechanisms available for the ions to acquire energy. Aside from some sloppy averaging I have done here, what you have shown is that the actual value of v_s lies between $1.15\ v_a$ and $1.3\ v_a$, the exact value depending on the production function $j_p(\eta)$. Now in general, ions will not always free fall but will be accelerated as well by collisions, oscillations, and so forth; and the upper bound on v_s may be different from what is given in Thm.III. In (C) I was concerned only with the lower bound, since it is more generally applicable, although I did make slight mention of the upper bound (page 5, line 15).

Fourth, in Sec. 3 of (A) the Bohm criterion is made to sound ridiculous because it implies an absolute upper bound on j_o (Eq. 24b). This is no more unreasonable than Thm.I, which gives an absolute lower limit to j_p. The point is that these j's are normalized to N_o, the density in the body of the plasma, and for a given J_p prescribed by the ionization mechanism these limits should really be interpreted as bounds on N_o. I was confused for a while as to why the inequality signs in Thm.I and Eq. 24b are in opposite directions. I think the difficulty is that the normalization density N_o cannot be determined in the Bohm type analysis, since, as you say, Eq. (21) cannot be extended into the plasma. Thus it is quite unfair to ascribe to to this analysis a bound on J_p; the analysis can give only a bound on η_s.

2) The collisionless case of this problem, as you have shown, can be treated quite neatly. However, this case does not correspond to a great many physical situations outside of the mercury arc. The collisionless approximation requires that $\lambda_i \gg R$, or $n_n \ll (R\sigma_i)^{-1}$, where n_n is the neutral density, R the distance between the midplane of the system and the wall, and σ_i the ion-neutral cross section. According to Thm.I, $J_p \geqslant (1/2)$ No v_a, to use round numbers. Since this must be supplied by ionization,

$$\int \nabla \cdot J_p \, dx = \int \frac{dn}{dt} \, dx = n_n \, n_q \, \sigma_q \, v_q \, R \geqslant \tfrac{1}{2} N_o \, v_a ,$$

Where n_q, v_q, and σ_q are the density, velocity and ionization cross section for the primary electrons. This assumes that all ionization is by a stream of fast electrons traveling parallel to the wall. Thus

$$n_n \geqslant \tfrac{1}{2} N_o \, v_a \, / \, (n_q \, \sigma_q \, v_q \, R).$$

To satisfy both conditions on n_n, we must have

$$\frac{n_q}{N_o} \gg \frac{1}{2} \, \frac{v_a}{v_q} \, \frac{\sigma_i}{\sigma_q} \, .$$

For helium, appropriate values for the quantities on the right give

$$n_q / N_o \gg 0.25.$$

This means n_e differs appreciably from the exponential relation. For mercury, the situation is much better, since the corresponding requirement is

$$n_q / N_o \gg 10^{-3}.$$

Thus the primaries need contribute 1% to the electron density at the center. This 1% would of course wreak havoc with the analysis of the sheath, but fortunately (A) is not concerned with the solution there. Up to the sheath edge the primaries would change the plasma density slightly but would not affect the space charge. If the ionizing electrons come from the tail of the Maxwellian distribution rather than from a cathode, the collisionless approximation then requires that

$$1 \gg \sigma_i \, v_a / \overline{\sigma v},$$

where $\overline{\sigma v}$ is the ionization probability averaged over the whole distribution. This condition is almost impossible to satisfy.

3) Now we come to the comments in your letter regarding the suitability of a criterion based on ion current rather than velocity. Here one should discriminate between the sheath on a plane probe and the sheath on the wall of a discharge. In the case of the wall, the potential between the center of the discharge and the wall is determined by the discharge conditions. Thus the ion terminal velocity as well as the ion current are perfectly determinate, and one can talk about either one. In the case of the probe, I agree with you perfectly that talking about ion terminal velocities is not rewarding; they can be made arbitrarily large by biasing the probe sufficiently negative. One would expect a saturation ion current would be drawn which is dependent only on the plasma parameters and which would vary with probe potential only insofar as the sheath infringes on the region of ion production. However, in neither (B) nor (C) does one talk about terminal velocities. The velocity at the sheath edge (which you so well defined) does not depend on probe potential but only on the plasma parameters. This velocity is related, in its average value, to the ion current by a density which should also depend only on the plasma; in the particular case of (A), the density is just $N_0 e^{-\eta^*}$. The spread in velocity is also determined by what happens in the plasma. Thus neither ion current nor ion velocity seems to me to be a more intrinsic property of the plasma than the other.

In the case of the wall, we can talk about stress balance. I do not know exactly what you mean by that, but I presume you mean something to the effect that the plasma pressure at the center of the discharge must somehow be transmitted to the walls. Since the pressure in the walls comes mainly from ion bombardment, we have an equation something like this:

$$(1/2)\, N_o\, kT = J_p M v_p .$$

It is apparent that stress balance gives a requirement on the product of ion current and terminal ion velocity. Again neither seems more intrinsic than the other. If one is making probe measurements, then, as you suggest, it would be preferable to talk about ion current. If one is concerned with ion wave instabilities in the sheath, as I was, then it makes sense to talk about ion velocity.

4) Finally, I would like to state what I think is the problem that should be solved. Two objections to (C) were that the electron density used was not exact, since electrons lost to the wall were neglected, and that ion velocity spreads were neglected. The first is true also of (A), and if I am right in (1)

above, so is the second. Thus a new theory should take these effects into account. Also, since the collisionless approximation is so restrictive, the theory should cover the transition from a collision dominated region to the collisionless sheath region. One would like to have the one-dimensional analog of probe theory, in which the probe current is related to the plasma density and temperatures at infinity. Unfortunately, in the one-dimensional case one has to specify a production function; moreover, one cannot have an infinite plasma because the density will diverge at infinity. Even so, given a production function and a large but finite size, one should be able to predict, in terms of N_o, kT_e, and kT_i, the current to the walls and the ion velocity distribution everywhere. One would expect this distribution to be small and isotropic in the body of the plasma, to become more and more skewed in the transition region, and to be an almost monoenergetic stream with nearly the sound velocity by the time the sheath is reached. Once this problem is solved, all kinds of arcs could be understood, as well as the behavior of negatively biased probes in a strong magnetic field. In the latter case the production function is merely replaced by a transverse diffusion coefficient.

So far we have assumed a steady-state theory. Actually, I know of no experimental evidence that sheaths can be formed without oscillations. On the contrary, there is evidence that sheaths have oscillations (c.f. Gabor, Ash, and Dracott and the Munich paper of Von Gierke, Ott, and Schwirzke). The next step would be to determine the stability of sheaths when $kT_i < kT_e$, their structure if oscillations must arise, and how oscillations will affect the ion velocity distribution.

Sincerely,

Francis F. Chen

FFC:lw

P.S.

I have just read the excellent paper of Harrison and Thompson, which you brought to my attention. This has further cleared up the situation for me, and I may have to take back some of my remarks. It is now clear that in a collisionless discharge the ion velocity distribution is invariable at any given η, since the first and second moments of $f(v)$ have been shown to depend only on η, and all higher moments can be shown to depend only on η. Thus the production function $j_p(\eta)$ is not an experimental variable, whereas $J_p(x)$ is. You must have been well aware of this, but I just now caught on. This being the case, you did take the velocity distribution into account, there being only one possible distribution. If fact, this means that it is not possible to impose a production cut-off η_c as suggested in the last section of (A). As the spatial production function is changed, the potential η will change to foil any attempt to create a prescribed ion velocity distribution.

Since the ion velocity is essentially fixed in this model, it does not make any sense to talk about an upper or lower bound to the velocity. The ion current is also essentially fixed; the only uncertainty being the deviation from strict neutrality in the plasma region. This is just your $I_2(\eta)$ and is of course why you have an inequality for J_p rather than an equality. The deviation from equality must be very slight.

Harrison and Thompson also make clear the difference between this sheath problem and the problem of the Bohm sheath criterion. In the latter the ion velocity is not fixed but can be under the experimentalists' control. Only then is it meaningful to talk about a bound on either J_p or v_p. I am gratified that Harrison and Thompson agree with (C) in predicting an increase in critical ion energy when the ions are not monoenergetic. In this paper, as in all previous papers, the problem of the boundary conditions which was treated in (C) was glossed over. I do not, however, understand H & T's derivation of a minimum η for the sheath boundary. It would seem that they set $\frac{d\eta}{dz} = 0$ at this boundary, which is not true to the order of approximation they were talking about.

December 14, 1961

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Dear Frank:

I was delighted to receive your letter of December 8th. I have a very deep and continuing interest in the subject matter contained in your letter, and I find your comments extremely interesting.

Without going into great detail at this time, I believe I am in agreement with most of your remarks and philosophy concerning the sheath problem. Let me just recapitualte briefly. In the mathematically idealized problem I treat in the Munich paper, the ions are treated exactly. Equation (3) of what you call paper A shows that the ion distribution is uniquely specified in terms of the rate of ion production and potential distribution. Harrison and Thompson have already pointed out that if the rate of production can be expressed as a function of the potential, then the ion distribution becomes a unique function solely of the potential. Similarly, if the electron density can be related to the potential, one can, in principle, solve Poisson's equation and obtain a potential distribution corresponding to a specific ion production model.

The key point in this model is that the ions are produced at rest and fall through a one-dimensional potential drop as free particles. As you pointed out, such an assumption has many shortcomings, but the model is mathematically attractive in that it allows one to treat the ions exactly. The inconsistency in my analysis is in the way I treat the electrons. I consider only those electrons which are in equilibrium with the potential. In addition to these electrons, there are at least two other important groups of electrons, that is, the electron group responsible for ionization, plus the electron group which contributes to the ambipolar current flowing to an insulating wall. In the event one treats a biased probe, the contribution of the latter group of electrons becomes less and less important the more negatively one biases the probe. I have neglected these two groups of electrons in my analysis for the sake of simplicity. Although I believe they represent small correction terms, the problem is still open.

Thus there is much room for improvement, namely, an improved theory would look more critically at the contribution of the high energy electron groups I have neglected and, perhaps more important, would examine the effect of collisions in the ion terms. In addition, as you point out, departures from strict stationary conditions and the presence of sheath oscillations is a very important and challenging problem.

I, myself, have been playing on and off with another phase of the sheath problem, namely, the effect of confining magnetic fields. In this, one is concerned with the effect of the self-magnetic field due to the discharge. For weak currents and weak magnetic fields the sheath configuration between the plasma and an insulating wall is still pretty much the way we picture it. However, as the current and the magnetic field increase, the electrons can become more and more effectively confined to the plasma region and somewhere the sheath configuration must become such that the ions are retarded rather than the electrons, that is, presumably the potential may actually go positive in the sheath. I would like to understand how and when this transition occurs.

I have found the principle of stress balance to be very helpful in understanding the general sheath problem. Loosely speaking, I feel there are two factors that are important to the description of confining sheaths. In one part we have a space charge potential distribution which arises from the disparity in the mass of electrons and positive ions. In the second part the velocity distribution of ions and electrons must adjust themselves so that the total stress on the system vanishes. The field contribution to the stress arises from the electric and magnetic field through the Maxwell stress tensor. For the particular geometry we are concerned with the electric stress and the self-magnetic stress due to the discharge current are oppositely directed. The particle contribution to the stress comes from the electron pressure, the ion pressure, and the inertial motion of the ions. The contribution from the inertial motion of the electrons is usually vanishingly small compared to that of the ions. Now, it turns out if you look at stress balance, that the electron pressure drop in the plasma can never be balanced by the electric field and must be balanced by the combined action of the magnetic field plus the ion stress. In general, this is how one finds that the formation of a sheath results in critical ion requirements. This matter was not particularly emphasized in paper A, but is mentioned more prominently in the forthcoming Nuovo Cimento article. Unfortunately, I have no preprints or reprints at this time.

Sincerely yours,

P. L. Auer