SUMMARY

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I've been given the horrendous job of summarizing in 25 words or less everything that has been said at this conference. Actually, I don't even remember accepting this job. This conference is concerned with small scale experiments, in universities, on anomalous diffusion of steady state devices. We must consider these talks in reference to the objective of these experiments. I think the primary objective of such small scale research is to simplify the situation which exists in fusion reactors. Usually fusion reactors have such complicated geometries that it would be almost impossible to make careful analysis of what is going on. This is the rationale for having small, simple experiments of anomalous diffusion. The main purpose of these experiments should be, to be extremely careful, and to understand the details of what is happening, rather than to try to cure the anomalous diffusion. In this conference it has become apparent that we are getting better and better at this. People are being more and more careful and are beginning to pick out the physics from the jumble of experimental data that is collected.

There have been several types of experiments discussed. First is the experiment on the positive column. I mentioned the first day that experiments on the helical instability in a positive column are the best understood. This conference has confirmed this. Johnson's non-linear calculation of the development of this instability has explained the experimentally observed hysteresis effects. Expositions of the effects of slight inclination of the B field and hysteresis observed has thrown great light upon this instability. We understand these positive column experiments best of all and they set a good example for all the other experiments.

Experiments of the second type are those done in cesium plasmas. We have heard talks about these experiments by Decker, Rynn, Rogers, Eastlund, Chang, Hartman, and Hendel. This conference seems to be dominated by experiments with the Q-type machines. People are beginning to realize the difficulties of the experiments in Q machines and are trying to straighten out the small details so that the understanding is becoming clearer and clearer. The cesium plasma is entirely dominated by end plates. This is being realized more and more and people are taking these effects into account. For instance, the temperature gradient of the end plates is extremely important as shown by Hartman. One can have d.c. drifts due to the electric field embedded in the plasma because of temperature gradients in the end plate. Also, the frequencies of instability which are observed in cesium plasmas are greatly affected by temperature distributions in the end plates. Once people recognize this, more and more accurate identifications of the instabilities and the waves which are observed can be made.

The third type of experiment is in arc discharges. This work is on a less firm ground and on a still less sophisticated level than the first two categories simply because the arcs are more complicated devices; not because the people who do the work are any less complicated. In an arc, in addition to the plasma density gradient, we have neutral gas and we have currents. To describe the situation is much more difficult. Experiments in arcs have tended to be less fruitful in bringing out the basic physics of the situation. However, people have made very good progress in observing anomalous diffusion in arcs and showing its onset and the relation between the onset and the outset of noise. Among these experiments are those described by Robertson and Decker. The arc experiments of Lazar at Oak Ridge are really not arc experiments because the phenomenon he was describing occurred outside the arc and it doesn't really fall into any one of these categories. Thomassen has reported on experiments in a pig discharge, which is a sort of arc, where the current is in the perpendicular direction rather than the parallel direction, and Ingraham has described some experiments at M.I.T., which have not been performed, that also fall into this category.

The fourth category is experiments in r.f. plasmas. This is on even less firm ground because there has been much less extensive work done on r.f. plasmas prior to this. Powers and Lindman have described experiments in r.f. plasmas of two different types, and Robertson has described some theoretical work he and Hawkins did.

In an r.f. plasma, which seems to be very simple, you have this glass tube and excite the plasma with r.f. at a frequency which does not really interact with the plasma. You observe then the anomalous loss to the walls at some other frequency which is characteristic of the plasma itself, which seems simple. On the other hand, these experiments are less advanced then those other ones. The phenomena are less well understood and I think again the reason is the end effects. There is really no difference between an r.f. column in a glass tube and the positive column or an arc discharge in a glass tube except that in an r.f. plasma you have a glass end to the tube and in an arc you have a conducting end to the tube. Therefore, in an arc you have a longitudinal electric field but not very much transverse field. If you have eliminated the conducting end plates, and you have shown you have no longitudinal currents, then the electric field is in a perpendicular direction. In these r.f. experiments it has been observed that the instability occurs sooner and more readily than in arc discharges. I think that is simply because there are no end-plate short-circuit effects in the r.f. discharges. It will be very interesting to see if the d.c. and r.f.

experiments can be brought into agreement by treating the ends properly and more carefully.

Finally, there are several miscellaneous talks. We have heard Lashinsky talk about the fine parts of non-linear theory, mode locking and so forth, which eventually we'll get to. Most of us are not at the point of sophistication where our experiment is so cleaned up that we have to worry about non-linear effects the way people on the positive column do, but it is good to hear that these people are already thinking about this problem and how to attack it. We heard an exposition by McLane on the techniques of correlation measurements; the talk didn't strictly concern an unstable situation, but the technique was a very useful one. The development of this type of electronics will be useful in studying turbulence. Finally, we remember the very nice movie shown by Hockney of the computer experiment of anomalous diffusion. This is really a revelation because in that experiment there were neither collisions nor K parallel, nothing except plasma pressure, yet the type of drift instability which we have been considering at this conference seemed to have occurred. For the study of non-linear situations I think this technique would be very fruitful.

What are the general conclusions that one can draw from this conference? First, I think one can say that there is always anomalous diffusion, that is, almost everybody reports anomalous diffusion. There was only one discrepant report in this conference. That was by Rogers in a cesium plasma in which he thinks the diffusion was classical. The results from Rynn could be interpreted either way and the results of Eastlund showed that the diffusion was anomalous. All of these incidently were done in cesium plasmas. I think I can make a good case for the following. You always have anomalous diffusion unless there is end plate stabilization---very strong line tying at the ends of the machine or the situation described by Buneman earlier today in which beta is very small. Beta is just the square of the ratio of the Larmor radius to the Debye length. The reason we don't have anomalous diffusion in this case is that we have finite Debye length stabilization. Some people define a plasma as a gas in which the Debye length is the shortest natural distance. If the Debye length is not the shortest distance of the system it does not exhibit the cooperative effects that a plasma exhibits. I think that when this condition exists as in the magnetron, one does not really face up to the problems of thermonuclear diffusion.

Second, we have shown that the ends are very important. Our ability to treat the end effects is becoming better and better. I think that this is a direction for improvement of these involved experiments. Since in the small experiments one necessarily has ends, one must really have a good feel for what is happening at the ends, and what is tying the perturbation down at the ends of the discharge in order to make these small experiments work.

Third, many of these experiments have explored the possibility of stabilization by means of minimum B fields. Unfortunately, one canno draw any clear conclusions about what is reported here. In some experiments there was stabilization. In some experiments there was stabilization of the hash, but no decrease in the perpendicular diffusion and in other experiments there was some change in which one could not really say whether the situation was improved or not. Now I think trying to study minimum B stabilization in finite devices is rather difficult. Perhaps the end effects are so strong that the effect of the minimum B field is not immediately clear. But I think this is a direction in which one can work to try to make some sense out of what is really happening when one puts quadr pole or bumpy fields on a plasma.

Finally, in the correlation of the low frequency noise with perpendicular diffusion, I think that many people have shown that the onset of anomalous loss is correlated with the onset of low frequency noise. For instance, the experiment of Thomassen, the experiment of Powers and the remarks made by Hendel yesterday, and the 4 kilocycle oscillation reported by Eastlund, all showed a correlation of the enhanced loss with the low frequency oscillation. I think this is a consequence of the fact - this is my personal opinion - that anomalous diffusion is due to oscillations of a plasma which are associated with the density gradient. Oscillations which are closely aligned with the density gradient have these low characteristic frequencies, and I think as experiments are recorded, there is more and more data piling up in favor of the fact that it is these low frequencies due to the density gradient which are responsible for enhanced loss, and that all other types of instabilities, which theoreticians might think of, are probably not as important. There is still some question as to whether among the low frequencies it is the incoherent part of the hash which is doing the diffusing or whether a single sine wave or a wave which has a discrete frequency spectrum can cause anomalous transport across a magnetic field just as efficiently as hash or vice versa. I think this point is not yet clear and deserves investigation.

In summary I believe we have come a long way in cleaning up our experiments in small devices but we have not yet achieved the objective of the definitive experiment which clearly ties down a known instability to the Bohm diffusion coefficient.