evidence and the statistical analysis, we may conclude that the negatively charged particles make a great contribution to the E_s ionization at the auroral latitude. A bay-type magnetic disturbance often appears at about midnight in the auroral zone, where the abnormal increases of f_{min} and f_0E_s are observed simultaneously. I am not sure what mechanism is operating there, but I feel that some of the features of the *F2* layer storm in the middle latitudes mentioned above may be explicable on the basis of the auroral zone phenomena.

References

1) Sinno, K.: Rep. Ionos. Res. Japan **7** (1953) 7.
2) Obayashi, T.: Rep. Ionos. Res. Japan **6** (1952) 79.
3) Obayashi, T.: J. Radio Res. Lab. Japan **6** (1959) 373.
4) Kamiyama, H.: Sci. Rep. Tohoku Univ. Ser. 5 **12** (1960) 11.
5) Sato, T.: Rep. Ionos. Res. Japan **10** (1956) 35.
6) Sato, T.: J. Geomag. Geoelect. **9** (1957) 1.
7) Sato, T.: Rep. Ionos. Space Res. Japan **13** (1959) 91.
8) Obayashi, T.: Rep. Ionos. Res. Japan **8** (1954) 135.
9) Maeda, K. and Sato, T.: Proc. I.R.E. **47** (1959) 232.
10) Sato, T.: J. Geomag. Geoelec. **9** (1957) 57.
11) Hakura, Y.: J. Radio Res. Lab. **7** (1960) 583.

Discussion

Obayashi, T.: Is there any height change associated with an increase of f_0F2 near the geomagnetic pole?

Kamiyama, H.: In general, we found a slight decrease in $h'F$, but I think it is not of great significance.

JOURNAL OF THE PHYSICAL SOCIETY OF JAPAN Vol. 17, SUPPLEMENT A-I, 1962
INTERNATIONAL CONFERENCE ON COSMIC RAYS AND THE EARTH STORM Part I

I-5-P4. Ionospheric Disturbances at Auroral Latitudes

C. O. HINES

DRB Theoretical Studies Group, Defence Research Board, Ottawa, Canada

It is emphasized that the storm-time electric fields associated with high-latitude currents and motions must be of major concern to the particles that precipitate into the ionosphere. Detailed development of this theme is not pursued, it having been initiated elsewhere by W. I. Axford and the present author (Canadian Journal of Physics, October 1961).

§ 1. Introductions

A year or so ago, there were too few theoretical mechanisms available for describing observed disturbance effects, whereas at this meeting it has been said that there are now too many. I think in fact that we have reached about the same degree of complexity in theory as in observation, and this suggests hopefully that we might soon be able to pair off the theoretical mechanisms one by one with the observed characteristics. It is the purpose of this present paper to indicate a starting point for such a process.

As a first step towards this end, I shall present a limited and extremely over-simplified picture of the observations at auroral latitudes. From a part of this picture I shall make a theoretical inference, and then develop certain theoretical 'facts' on the basis of that inference. I term as 'facts' those conclusions on which, I believe, one could obtain virtually unanimous agreement from the theorists here present, given the initial empirical information. This approach will avoid the complication which has beclouded the theoretical discussions at this meeting, where different theorists have begun from different *a piriori* assumptions, and will emphasize instead the common point of agreement to which almost all converge. Further theoretical consequences of the agreed-on point have been developed in some detail in the theory presented by W. I. Axford and myself, although they were there developed from an *a priori* theoretical viewpoint rather than an empirical one. Some of these consequences fall into the realm of theoretical facts in the sense already indicated, and it is these that I wish to emphasize now.

This done, it will be possible to link together the overly-simplified observational pattern of disturbances and the more basic theoretical 'facts' in a highly suggestive manner. My excuse for this is my belief that the theorist's first function is to help organize the observations into meaningful groupings, even if only on a trial-and-error basis. While limiting myself to the theoretical 'facts,' I happen to believe that there is little room for error.

Finally, because the theorist's second function is to 'explain,' I will go on to express an opinion on the various mechanisms that have been advanced in the detailed pairing-off of mechanisms and observations.

§2. The observations

The observations I wish to call upon, and to over-simplify, fall into two general categories. First I would recall the analysis of Montalbetti, reported at this meeting in a paper by Montalbetti and McEwen, and then I will call upon the pattern of auroral motions related by Davis and supported to some extent by separated F-region measurements.

Montalbetti's analysis of hydrogen emissions at Churchill, combined with data from College, suggest that these emissions are strongest along a loop which descends from high to low auroral latitudes in the pre-midnight hours, and rises again to high auroral latitudes in the post-midnight hours, with near-symmetry about the midnight meridian. The occurrence of "r" type E-sporadic ionization maximized similarly, and the loop could be extended well above the main auroral zone by data on E_s from Baker Lake. It was suggested then that hydrogen emissions and "r" type E_s are physically linked, and separate doppler measurements of the hydrogen emissions indicate that protons of 20–80 kev energies are important to the combined process. It might be added that the loop defined by these measurements corresponds reasonably to the morning and afternoon 'spirals' found by Nikolsky and Burdo in data on geomagnetic agitation.

Montalbetti's analysis went on to link the occurrence of auroral emissions — which he distinguishes quite clearly from the major hydrogen emissions — with the occurence of "f" type E-sporadic ionization. He suggests the diurnal maximum here lies on a separate are which descends from high to low auroral latitudes during the hours around midnight. (Data analysed by L.E. Montbriand exhibit a maximizing of auroral-type ionospheric absorption along this same arc, with a return to higher latitudes in the later morning hours. Further data pertinent to this loop are contained in the final form of the paper by Montalbetti and McEwen, printed in these Proceedings, and in the theoretical paper by Axford and myself in the Canadian Journal of Physics, October 1961.) The auroral emissions are normally associated with electrons of energies 10–20 kev predominantly. These lower energies, relative to the proton energies previously cited, are likely to be associated with different source mechanisms in a manner which will shortly be indicated.

Turning now to the auroral motions discussed by Davis, we note that these are roughly westwards before midnight and eastwards after, at low auroral latitudes, and reversed at high auroral latitudes. The higher and lower latitude motions appear to be linked by motions towards the equator near midnight, such that two circulatory loops may be envisaged: one rotates clockwise (as seen from above, in the northern hemisphere) about a centre at about 67° geomagnetic latitude near the sunset meridian, while the other rotates oppositely about a centre at the same latitude but near the dawn meridian. The pattern of circulation, where observed, is similar to that of the classical *Ds* current system oriented along the sun-earth line, but the motion is in the sense opposite to the direction of conventional current flow. The low-latitude portion of the auroral motions is duplicated by the available evidence on F-region motions at the pertinent latitudes during storm time.

§ 3. The theoretical inference

The theoretical inference that I would draw from these latter conclusions is that the motions and currents are to be associated with polarization electric fields, having a minimum potential at the aforementioned sunset centre of circulation and a maximum potential at the dawn centre. This is a conclusion which is difficult to avoid theoretically, and which is in fact incorporated in all *a priori* theories of the *Ds* current system presented at this meeting. The orientation of the current system in practice introduces further complications, which Axford and I happen to believe are associated with inhomogeneous dynamo and conductivity effects, and these might lead to some disagreement on the *orientation* of the polarization field. Further disagreement might arise on the latitudinal variation of the primary polarization field. But these are details which should not at the moment obscure my principal point, which is that a widespread agreement exists amongst the theorists that polarization fields are present.

The magnitude of the fields can be estimated on the one hand from the auroral and *F*-region motions, and is found to be of the order 10^{-2} volts/meter in the region of the auroral electrojet. If this field persists across the electrojet, for a horizantal distance of some 10^6 meters say, then the potential drop across one of the circulating systems is found to be of the order 10 kilovolts. The potential can be estimated independently from the *Ds* current system, treated as predominantly a Hall flow, and again a value of 10 kilovolts is found to be representative for each polar loop. While each of these calculations separately is open to some suspicion, their general agreement lends support to the inference that a total potential drop of the order of 20 kilovolts is indeed to be found between the centres of the two circulation systems during typical storm conditions, and this value will be adopted for present purposes.

§ 4. The theoretical facts

Now, it is a theoretical 'fact' that such potential drops, existing at ionospheric levels on the scale discussed, must also be found at higher levels in the magnetosphere. The geomagnetic field lines will be essentially equipotential lines, and the ionospheric distribution of potential simply maps along them through the magnetosphere. Thus, in the equatorial plane at some 5 or so earth radii distance, there is a potential difference of some 20 kilovolts between the dawn and sunset meridians.

Consider now a trapped proton or electron of, say 100 kev energy, drifting longitudinally round the earth through this field. We may first compute its drift motion neglecting the presence of the electric field, and enquire afterwards whether this process is valid. On doing so, we note simply that the particle will have an energy varying in the range 100 ± 10 kev, and that this variation is not very severe. The motion would be perturbed somewhat in latitude and longitude, but not very much. The height of the mirror point would also be perturbed, perhaps even to the extent of producing precipitation and loss to the lower atmosphere. This could be important.

Even more important, or certainly more obvious, are the consequences to particles of lower energy. If we consider a 1 kev proton or electron drifting longitudinally round the earth, and ignore once again the effects of the electric field on the motion, we see that it can acquire 20 kev of energy. This would be a major event in the life of a 1 kev particle, and an event which we could ignore only at our peril. The most important single conclusion to be drawn from this present paper is that the electric fields that are associated with the *Ds* system *must* be taken into account if we are to have any hope of understanding the storm-time behaviour of particles with energy less than (say) 20 kev. In fact, when account is taken of these fields, it is found that they introduce velocities that dominate over the drift motions and produce instead large-scale convective motions that may be described approximately by the hydromagnetic 'frozen field' extrapolation of the auroral-level circulation. Compression and energization result, of a nature discussed along general lines initially by Gold and in detailed application to disturbance conditions by Axford and myself elsewhere. It is not appropriate to repeat the full implications here—some of which are more debatable than the 'facts' so far introduced—but it is appropriate to re-emphasize that storm processes involving particles of energy less than or of the order of 20 kev are unlikely to be assessed properly unless the polarization fields are taken into account.

§ 5. The theoretical implications

To return now to the picture of precipitation arcs described earlier, it is clear that the electric polarization field or equivalent magnetospheric circulation *must* play a major rôle in the interpretation of the aurorally-associated 10–20 kev phenomena, and indeed is likely to determine the location and many of the features of those phenomena. It is also clear that this field *may* play an important part in the precipitation of 20–80 kev protons, although the geographic location of such precipitation and the precipitation itself may be more strongly affected by the dominating drifts associated with the inhomogeniety of the geomagnetic field.

It will be a long process to investigate these implications in full. Axford and I believe we have made a sound start (albeit on an *a priori* rather than an empirical basis). But whether we have or not, at least the necessity for such a start should be clear.

§ 6. A theoretical assessment

To change now to the more nebulous region of theoretical mechanisms, it should be noted that those proposed at this meeting fall into two essentially distinct categories: those that are capable of producing steady-state conditions and those that are inherently time-varying. The first group embraces mechanisms that can provide a continuous source of *current* at some fixed point, and the other is capable of producing only *charge* at a fixed point of *current* at some varying point. The first is then capable of maintaining the *Ds* polarization field against losses due to the (small) Pedersen currents in the ionosphere, while the second is rapidly rendered inoperative by the accumulation of neutralizing charge, or at best produces a continuously changing pattern or orientation of the *Ds* system. In the first category I would include the mechanism of Dungey and that tentatively adopted by Axford and myself. (Alfven's description, if extended by the inclusion of magnetospheric iononization, would be analogous to Dungey's, except for the sense of the interplanetary field, and would fall in this same category. The Chapman-Ferraro mechanism of the initial phase is similarly capable of steady-state effects, and, for completeness, I should add the dynamo mechanisms that look to atmospheric motions as the generator of the currents.) The second category would include mechanisms dependent on the '*grad B*' or '*grad p*' type of drifts, such as those proposed at this meeting separately by Chamberlain, Fejer, and Kern. (The charge separation that arises at the walls of the Chapman-Ferraro cavity provides a further mechanism of this type, one which has been employed by Piddington in treating the *Ds* system.)

It is my opinion that quasi-steady characteristics, such as the ring-current, the *Ds* system, and the occurence patterns of auroral and related phenomena, will emerge as consequences of the first type of mechanism. Transient phases of this same type might be responsible for shorter-lived phenomena, particularly in the initial variations. The second type of mechanism might also play a rôle in these initial variations, and suitable combinations of the two (plus other effects discussed at this meeting by Singer in particular) are likely to account for the complexity of the commencement phase. Small-scale irregular variations during the main course of the storm, such as evidenced in active auroral displays, seem likely to be associated with the second type of mechanism and/or instability processes engendered within the first.

Discussion

Dungey, J. W.: In view of Neil Davis' analysis I think your plot of aurora is an *over*-simplification.

Hines, C.O.: I quite agree, and I admitted that I was making an *over*-simplification at the time. I did so in order to make a point. I would add, however, that my *over*-simplification is supported by the distribution of auroral maximum intensities given by Malville (J.G.R. 1959), and of course by other non-auroral phenomena. Moreover, Davis' diagrams of incidence are based on a complicated system of assignment of numbers, and does not particularly relate to the *irregular* forms which are to be associated with the midnight spiral. Finally, that spiral referred theoretically to the turbulence effect, and neglected the superimposed effects of the general compression. These would operate to produce a maximum intensity at a given latitude, nearly, and should in some sense be multiplied by the turbulence effect before a general *incidence* pattern could be deduced. This process would give a much closer agreement with Davis' *incidence* pattern.

Davis, T.N.: In his diagram Dr. Hines has shown a single curve representing the local times of maximum auroral incidence at and just inside the auroral zone. My observations show a single peak of incidence at the auroral zone as shown here. At higher latitude (70–75°) two additional maxima appear; one in early evening and one in morning. Above 75° these two maxima are much stronger than the midnight maxima, which in fact disappears. The two high latitude maxima appear to converge to a single peak near the geomagnetic pole. Whereas those two maxima predominate over the midnight maximum at latitudes just inside the auroral zone, it appears likely to me that the midnight maxima may represent the locus of the active rayed aurora occurring near auroral breakyes(?).

Hines: The high-latitude noon maximum is, I believe, related to the fact that the outermost field lines in the magnestospheric cavity pass through the ionosphere on the daylight hemisphere at high latitudes. I did not wish to confuse the picture today by reference to this, since it lies very largely in the realm of opinion on magnetospheric models — not just mechanisms. The region in question is mentioned in the full paper by Dr. Axford and myself (Can. J. Phys., 1961), and may be identified there as Region I.

Hultqvist, B.K.G.: One of the main objections which were raised against Alfvén's theory many years ago was that the charge separation in the vicinity of the earth being a part of his theory could not occur because of the high conductivity around the earth. Do you mean that that objection is not relevant, or if it is relevant to Alfvén's theory, why is it not relevant to your theory?

Hines: I have perhaps given the wrong impression. So long as Alfvén's theory is developed in terms of the motion of individual solar particles without collective interaction (except through the large-scale polarization field) it will provide only a source of space charge, and this can indeed be neutralized by terrestrial ionization. Once it is neutralized, it can play no effective part in continuing the disturbance. But the description in terms of individual particle motions, under the influence only of the large-scale electric and magnetic fields, seems inappropriate since we know that the stream densities are sufficiently strong to make collective interactions important. These collective interactions are expressible by hydromagnetics, approximately in terms of frozen fields. Dungey has considered the consequences of hydromagnetic interactions for a model which includes as an essential ingredient an interplanetary magnetic field such as Alfvén introduced (albeit in the opposite direction). Dungey's results, which take into account magnetospheric ionization, show that the circulation in the magnetosphere will proceed even in a steady state, which implies in turn that the local electric potential can be maintained despite the terrestrial ionization's attempt to discharge it. The hydromagnetic interactions must be introduced to achive this end, but I believe they must be introduced on *a priori* grounds in any event. What I really wished to say, then, is the Alfvén's *model* was capable of maintaining a local polarization field, even though his *development* of the model neglected the collective interactions which would have revealed this. [I have not in fact examined the situation to see whether the change of sense of the interplanetary magnetic fields, as between Dungey and Alfvén, invalidates this conclusion in principle; it certainly would alter the distribution of potential.]

Harang, L.: How are Dr. Hines' opinion about the drift results derived from scintillation recordings? According to recent drift measurements in Tromsö we obtain a uniform drift towards W of about 400 m/s.

Hines: The motions observed at E-region heights I believe to be unambiguous. I thought that the motions at F-region heights were also unambiguous, although the only source I can quote at the moment is Dr. Briggs' review paper for the 1960 International URSI. I believe that indicates the same pattern as I quoted, if attention is paid to the behaviour at high (but sub-auroral) latitudes during disturbed periods, when the Ds polarization field would dominate over the Sq field. In any event, I still feel theorists would be agreed on attributing the motion to polarization fields and potentials of the order I have suggested. I hope to learn more of your observations, and consider them further.