INTERVIEW

OF

DON FAIRFIELD

P R O C E E D I N G S

MR. CLINE: All right. There we go. Okay. If you could just tell us again your name and a little bit about who you are and then we'll start with the first question.

MR. FAIRFIELD: Uh-huh. Yeah, I'm Don Fairfield. And I've been working in the field of magnetospheric physics for many years. I was sort of in on the beginning of it.

I know when I was taking my first physics course in college in 1957, Sputnik was launched. And when I graduated from college, I had -- I was offered an assistantship at Penn State. And I went to Penn State and -- to work in the ionosphere research lab there. And this was 1960.

And the next year, they offered me the opportunity to work with Jim Dungey who was a consultant at Penn State but -- and he would come visit a couple of times a year. And I accepted their invitation to do this. And he was my thesis advisor for my five years at Penn State.

In 1961 he published, which I think most people would -- will admit was probably the -- maybe the most important paper in space sciences. And for my Master's thesis at Penn State, he had me looking at high latitude magnetograms that are from the IGY, because he had an idea of how the currents might be driven by the solar wind.

And after completing that Master's thesis, he gave me a problem for my Ph.D thesis, which in retrospect, I think was maybe the best thesis project anybody ever had.

His idea was that the magnetosphere was controlled by the interplanetary magnetic field and it would operate differently depending on whether the interplanetary field was northward or whether it was southward. And because when it was southward, then the currents would be driven and the magnetosphere aurora would be produced and it was a very significant theory.

And his idea was that I could look at the magnetic field data. And at that time the first satellite to orbit the earth and make measurements outside of the magnetosphere was Explorer 12. And he arranged for me to go to the University of New Hampshire and work with Larry Cahill who was the experimenter on the magnetic field experiment.

And I went to New Hampshire for the summer of 1964 and looked at this data. It was an interesting experience. When I arrived there, the halls were completely filled with bookshelves of printout of his data because before this time he had worked with satellite data where you have maybe just a few minutes of data on one rocket flight. And now he suddenly had day after day month after month of satellite data and his experiment measured the magnetic field three times every second.

So he was overwhelmed with this data. And he had some students with mechanical calculators computing averages of this three-second data and -- while some other people were working trying to get the data onto modern, at that time, computer.

Anyway, that was a very profitable summer. And indeed, I -- the magnetograms that I was using to detect the magnetic field activity was clearly enhanced when the interplanetary field was southward.

And it -- I remember thinking, is research always this easy. And because it did just seem very, very apparent that his ideas were correct.

After finishing my degree at Penn State, I accepted a postdoctoral position at NASA Goddard working with Norman Ness. And Norman then had the best solar wind data of anybody at that time with his data from the interplanetary monitoring platform.

So my first goal was to try to provide a statistical support for the -- my earlier work. And to do that, I needed a measure of the magnetic fact -- of round magnetic activity which was better than the three-hour KP averages.

And at that time Davies and Secura -- Sugiura had published a paper defining what they called the "AE," or auroral electrojet, Index. And this was just what I needed to -- for statistical work.

And they -- so I worked with the data center at Goddard and started computing this AE Index on a regular basis. And then I went on to compare it with the larger data set from M spacecraft. And indeed, that did confirm the association with southward field and magnetic activity.

And that work I then published in a proceedings of the Kosovar meeting I think in 1967, and I think that was the least-referenced paper I ever published because this is a rather obscure journal. I never bothered to publish it in a real journal. This was a -- just the proceedings of a meeting.

Q I think one of the questions that we had were also -- which you've already started touching on some of them -- were, what were some of the key events or turning points in space weather research that you were involved in. And again, if you can --

A (Inaudible.)

Q That's right. And just kind of maybe repeat that a little bit what the question is and then just you can move into this really awesome looking list, and I think that will be awesome.

A Yeah. Back in these early days when spacecraft were being launched quite frequently, it was really very exciting because every spacecraft went to a different region and found new things.

And so it was not like the situation is today where you make incremental advances, but you were discovering really new and unexpected things at that point.

When I first came to Goddard, Norman Ness sort of turned over the analysis of the M-2 spacecraft to me. And this spacecraft was actually called a failure by NASA headquarters because the apogee was only about 16 earth radii; whereas, they had meant for it to go out about twice as far where it would survey or -- survey the solar wind and it's magnetic field.

But in fact, 16 earth radii was an excellent orbit for studying the magnetosphere and -- for it took many, many years before there was another satellite in that very important region.

The nice thing about the M satellites was that they go out to typically 30 earth radii. They covered all regions of the magnetosphere. So I felt very, very fortunate in being able to work in all these different regions where the spacecraft traversed.

One of the early things I did was to map out the positions of the magnetopause and the bow shock. Explorer 12 had really mapped the magnetosphere pretty well. It went out to 13 earth radii. But that wasn't far enough out to see the bow shock. So a lot of the early work on -- you know, was done at the bow shock.

And I went through and determined the various crossings on each orbit and published a paper which really delineated the position of the bow shock and the magnetopause, which was of considerable interest to people who would follow because it's important to know where these basic structures were located.

I also used the early magnetic field M-2 data to study the configuration of the magnetic field and particularly how the solar wind compressed the data on the day-side and then extended the data on the night-side. Because it was just before that time that Norman Ness with his, you know, one data had discovered the -- discovered the magnetotail.

(Whereupon, there was a brief break in the recording.)

MR. CLINE: No, that's fine. Okay. We'll

continue with -- with where you were just a second ago.

UNIDENTIFIED SPEAKER: The day-side/night-side of the magnetotail.

BY MR. CLINE:

Q Okay.

A Of course, the configuration of the magnetosphere is important because that controls where the particles go. So this was of considerable interest.

At that time, the conventional wisdom was that nothing would propagate outward from the bow shock and -- well, at least any -- no magnetic field waves could propagate outward from the bow shock because the wave propagation speed was less than the incoming solar wind speed.

But in looking at the data, it was clear that there were waves upstream of the bow shock. And this was a surprise that I was able to use IMPs data to show that the location of these waves upstream from the bow shock depended on what the interplanetary direction was.

It was on one side of the magnetosphere when the magnetic field was directed one direction and on the other side if the magnetosphere were directed another direction. And this, in fact, was really the first determination of the foreshock as it was later called which has been much studied in later years.

And it became, of course, the question was, how do these waves exist upstream when they're not -- don't have high enough velocities to propagate upstream.

And I remember going over to the University of Maryland and talking to Derek Tidman. And it was his suggestion that it was particles moving upstream from the bow shock which would generate these waves in the upstream region.

And this is something that I think has held up over the years and much -- very much more data has been done on the foreshock since then.

When I was in graduate school, there was -- the field of plasma physics was only just beginning. I -- at Penn State they didn't teach a course in plasma physics because the field was so new. So I felt very fortunate over the years in learning my plasma physics by seeing it in action, you might say. Because, of course, this whole interaction of the solar wind with the magnetosphere is all plasma physics.

I remember at one point I -- when we had IMPs 1 and 2 flying at the same time, I was able to see features in the interplanetary magnetic field, first at the spacecraft further upstream and then at the spacecraft nearer to the earth.

And this was clear evidence of how the magnetic field is convected with the solar wind. And this was a -- I think a pretty well accepted idea at that point, but I don't think anybody had ever observed it. So it was interesting in being able to see something that the -- as predicted by the theory.

I remember in another case, I was doing some early work on the earth's bow shock, and I was able to determine that there were waves standing on the bow shock itself. And this was a phenomenon that was on the cover of an early book on plasma physics and shock physics. So it was very interesting to see something in real life that had been predicted by theory.

Since the magnetic field controls all particle motion, it was of interest to have a model of the field which was representative of the distortions of the magnetic field and the fact that the field was no longer a dipole field but was distorted by the solar wind.

So at some point, I collected data from a number of IMPs satellites in the magnetosphere. And then I worked with Gilbert Mead at Goddard, and we came up with the first quantitative model of the distorted magnetic field by fitting the data to magnetic field -- well, by using -- we used Gill Mead's experience in modeling fields with the data to -- to create this realistic model of the magnetic field.

And, of course, subsequently, many people went on to produce more sophisticated models, but that was useful as a start.

One of my most interesting experiences, I would say, was in work with the polar rain. The polar rain was known to be low energy electrons precipitating over the polar cap. And it had been observed that they precipitated over one polar cap and -- but not over the other, but then sometimes they would reverse and it was the other polar -- other -- well, say -- start that again.

Q Okay.

A It was observed that sometimes they would precipitate over the north polar cap but not the south polar cap, but sometimes that would reverse and they would appear to the southern hemisphere and not the northern hemisphere. And it was also not so clear where these particles came from.

But in working with Jack Scudder at Goddard, we used his knowledge of the solar wind to realize that the electrons in the solar wind are aligned with the magnetic field, at least a portion of the distribution is aligned with the magnetic field, follows the magnetic field lines.

And we realized that the -- it was these electrons which would follow magnetic field lines which connected to the magnetotail, and they would precipitate over one polar cap; whereas, the other polar cap would be going out into distant space.

But which polar cap connected back to the sun was dependent on the reconnection at the magnetopause. And when the magnetic field reversed and whether it -- when the magnetic field was pointing outward from the sun, it would connect to one lobe of the tail. And if it were pointing in the opposite direction, it would point to the other lobe of the tail. And that was why the polar rain reversed hemispheres.

In later years, I did further work on the foreshock. I realized that many of the perturbations in the upstream region were magnetic field compressions. And this is realized, that many of the perturbations in the upstream region were density compressions. And this was different than what is seen in the unperturbed solar wind.

So it was evidence that something was happening in the foreshock and then these density perturbations would be convected through the bow shock and impinge on the magnetosphere and create compressions of the magnetosphere. And this was apparent in the data and was a new phenomenon at that time.

Subsequently, much further work has been done on the foreshock and how it affects the earth. But this was one of the early evidences for it.

At a later time, I again used IMP data to notice that there were very significant waves on the magnetopause surface.

And I worked with Antonius Otto with the University of Alaska, and we realized that these were -- were Kelvin Helmholtz waves. Kelvin Helmholtz instability had been well known, but it had not been seen on the magnetopause until this time.

But with Antonius's theoretical expertise and the IMPs data, we were able to demonstrate that this was occurring at the magnetopause.

(Whereupon, the interview of DON FAIRFIELD was concluded.)

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