LOUIS J. BATTAN: THOUGHTS ON RADAR METEOROLOGY

October 8, 1986

Today is October 8, 1986 and I am dictating some thoughts on radar meteorology at the request of Dave Atlas who is writing a book on the history of meteorology.

Dave, I have gone over your letter of October 4th, 1986 and you ask an awful lot of things in this letter, some of which I can deal with and some which I think I might as well say very little about because I know very little about it.

As you know, we both got into this radar business at about the same time and I must admit my early thoughts while still in the Air Force were that radar would be a very valuable device for observing precipitating phenomena, but I didn't visualize in the middle and late '40s that radar would become as important an observational instrument as it has become; that is to say, I didn't visualize the quantitative aspects of it at that time.

Let me go on then, on some of the more specific points that you raised. I arrived on the Thunderstorm Project in the summer of 1947 at which time most of the decisions on instrumentation had already been made and, as you know, essentially all of it was qualitative. We used a TPS-10 and then we had the 584 radar sets and then we had that big traffic control radar, the number of which I can't recall. The analysis was largely of dimensional aspects and of echoes. I would suggest you review the final report of the Thunderstorm Project, "The Thunderstorm." It more or less tells what we did in terms of the analysis. The analysis at the time was obviously somewhat limited by the nature of the observations. We didn't understand the importance of such

things as beam patterns and the radars were not calibrated; therefore, the analysis was somewhat limited.

You asked about using radar for getting quantitative measurements of rainfall rate and I do recall something about Byers and Coons relating the depth of penetration of echoes over the horizon to something, but I'm not sure it was rainfall rate. But there was a paper which was published in the Transactions of the AGU, about 1949 or so, in which the total volume of rainfall which fell at the ground was related to some integrated volume of radar echoes in integrated areas. Similar work has been done in the last few years by Paul Smith, but there was a paper by Byers and collaborators. I got involved in the later stages of that process, pointing out the limitations, but you should check the Transactions of the AGU on it.

As far as microbursts: again, the Thunderstorm Project book spends a lot of time talking about downdrafts and thunderstorm outflows and divergents and so on. A lot of that work was done by Harry Moses, so the concept of the microbursts is not really a new one; the name is a new one. The Thunderstorm Project realized there were small downdrafts and large ones; they just called them outflows. The threat to aviation also was not entirely overlooked; I recall Roscoe Braham making some analyses of turbulence in low level outflows. We didn't recognize, as I recall, nobody recognized the problems that Fujita has highlighted recently, namely, the effect on an airplane suddenly when it moves from a region of headwind to a region of tailwinds. So the phenomenon I think was recognized but its consequences were not.

On the Puerto Rican experience: that was during the early '50s in the Cloud Physics Project and we went to Puerto Rico largely because we wanted warm clouds. We were studying the effects of waterspray seeding on warm

clouds so we went to the Caribbean. The radar--you called it the tail radar in your letter--wasn't in the tail; it was in the nose of the B-17. That idea, which was generated early in the Cloud Physics Project, incidentally is discussed in great detail in a meteorological monograph of the American Meteorological Society which came out, I suppose, in the middle 1950s. We were concerned in the seeding project whether or not a cloud had an echo in it and we wanted to scale scan through it vertically so we decided to turn an APQ-13 radar set over on its side. We put it in the nose of the airplane and it scanned vertically, a corkscrew type scan, and it worked pretty well.

In the letter you asked about our interest in the pulsed-Doppler radar and our reactions to it. You're absolutely right. The Boyenval paper certainly did get us excited, which is why we brought him to Tucson to have him give a seminar right away, or shortly after the conference, because of the fact that we did see that Doppler radar offered all kinds of possibilities for studying motions, adding a totally new dimension to the measurements we were making. This is why we--when I say we, I mean Dick Kassander and I--put together a proposal as quickly as we could and shot it off to NSF as quickly as possible, recognizing the fact that other people would also have the same kind of interest. Again, our imaginations didn't extend as far as reality has taken us, but we certainly recognized that it had fantastic potential.

You asked about the Herman and Browning work and I must say that was a very exciting time. The history of that work on the backscattering from hail started this way. Ben Herman, who was then a graduate student, was working on the scattering of infrared radiation by cloud droplets. He had programmed the Mie scattering equations and came walking into my office one day and said that he had this program and did I see any useful application. That was the

time you were working in England with Frank Ludlam and I had gotten a letter from you saying that you had gotten some exciting results, that the backscattering cross sections were much larger than you expected. So I said to Ben, well yeah, let's run off some scattering, backscattering calculations for microwaves and for particles, spherical ice particles, which simulated hailstones. Shortly thereafter, back came the calculations showing the curves that I sent to you. And I must say, it was one of the big thrills of my scientific experience when I got your letter on which you had plotted your actual measurements against the curve. I remember thinking to myself, isn't this astounding; theory and measurements are in agreement.

Incidentally Dave, I'm dictating this thing at home and so I don't have access at the moment to all my files so some of this stuff seems to be a little disjointed.

I should mention that John Theiss played an important role in this Doppler development. Dick Kassander and I set up essentially the specifications, and it was John Theiss with one or two assistants at various times who designed and constructed, maintained and operated it. There never would have been a Doppler program here at Arizona without John Theiss. For the most part, he did the engineering and I did the meteorology.

You asked about accomplishments, things that I was involved with which I think were worthwhile. Some of them are fairly easy to identify, and some not so easy. I should send you a copy of my publications and you can look through them. There are a few things though that I think are worth mentioning.

I think the early work I did with the Thunderstorm Project, although mostly qualitative in nature, was worthwhile. Most of the radar analyses

which appear in that book called "The Thunderstorm" was done by me or under my supervision, and I think it stimulated interest in this whole field and supplied some information about the nature of thunderstorms.

I think the work on initial echoes and how it relates to precipitation in convective clouds which I did in the '50s was of some value.

The technique we talked about earlier--the use of a conical-scan radar on an airplane for meteorological purposes--had some value and I played a role in bringing that into being.

I think probably the construction and the manufacture and operation of the Doppler radar starting in the early '60s was a big step forward, and I think our work stimulated a lot of other people and led to some valuable information.

Some of the things we did which got little attention I think were of some interest. Working with some graduate students, as you may know, we made some of the early measurements of the vertical motions of angel echoes showing that some were essentially convective in nature and others demonstrated some kind of wave motion. We also made measurements of the polarization properties of angel echoes. As far as I know, we were the first ones to make polarization measurements of angel echoes outside the U.S.S.R.

I think some of the work we started experimenting with at the end of the Doppler project had and still has some promise and, that is to say, to measure the Doppler spectra in two planes of polarization. I think the work we did on the internal structure of thunderstorms by means of Doppler radar has had some effect on how we think of thunderstorms. I don't think it's had enough effect.

I think there still is a great value in the use of vertically pointing Doppler radar because of the fact that it gives better height resolution and, in some cases, horizontal resolution than does multi-Doppler radar. I've said a number of times and I still believe that one of the problems with multi-Doppler radar is that the spatial resolution isn't large enough. There is no question that multi-Doppler radar is extremely valuable and we'll see more and more of it in the future obviously. Nevertheless, it doesn't tell you very much over spatial scales of the order of 100 to 1,000 meters, and when you measure reflectivity and vertical motion over those time scales in thunderstorms, whether they're relatively small thunderstorms or big ones (supercell ones), what you find is there is tremendous variability; and my intuition continues to tell me that that variability has got to have an important role to play on the dynamics and on the precipitation formation processes in thunderstorms. I think that point has not been given adequate attention by the theoreticians and the model builders in this country and elsewhere.

I think some of the theoretical work that I have been involved with, with Ben Herman and Bob Browning and more recently with Craig Bohren, has been quite important. We discussed a little earlier the work on hail scattering attenuation. You may recall that many years ago Ben Herman and I published a paper on the scattering properties of spongy hail and that we were bothered, of course, by the fact that we didn't know how to handle the problem of dialectric constant of mixture of water and ice. I am very pleased that Craig Bohren and I got together and collaborated on that problem. He did the theoretical work and, again, I did what you might call the meteorological work; that is to say, I set the problem up and he was able to dig through the theory and extend it

enough so that I think we have a much better understanding now of the techniques for calculating dialectric constant of mixtures of two substances. I think that was an important step forward.

Incidentally, in connection with that work, you may be curious to know that Ben Herman and I also published an article on the backscattering properties of bubbles; and about three or four years later I got a letter from some doctors at Billings Hospital in Chicago who were interested in the technique and asked us to make calculations of the backscattering at, as I recall, some optical wavelengths of some properties of the blood.

Well, as far as I can recollect at the moment, those are some of the things that I have worked on over the years which I think are worthy of note. Maybe if you look over my list of publications you'll see something else that interests you.

On the final matter, Dave, I'm afraid I can't accept your invitation to prepare a paper with Ed Kessler for the 40th Anniversary Radar Conference. I'd like to but I'm really not in shape to do it, but there are others who can do it and it should be done.

I think you're right in saying that the number of universities and other groups with radar development programs these days is too small. There really is too much centralization, or so it appears, and the problem there is, of course, that not enough people are using their imagination to conceive new and better ideas on new Doppler techniques and new analysis techniques, etc. I think it needs to be examined, and if the pendulum is swung from not enough centralization to too much, then the effort should be made to swing the pendulum back in the other direction.

I'm sure the Conference will be a good one. I think it's time to look back to see where we've been and to look forward and see where we're going, and the sooner something like that is done the better we'll be.

I wish you good luck on your project. I know you'll do a great job as you always do.