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Professor Jule Charney
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Dear Jule:

You probably recall receiving a letter from Isadore Katz of Johns Hopkins last month in which he proposes to carry out one of the major field experiments mentioned in the GARP plan, that related to strong air mass modification off the U.S. east coast in winter. He suggested that the capabilities of the Wallops Island radar facility be used as a primary source of data for such a program. On the evening of October 16, after the NCAR reception, a meeting was held among a few of those (about 10) who were thought to be interested and informed on the subject. After a short discussion of some of the scientific problems and observational facilities and needs the meeting came hard up against an impasse. Nobody there, including myself and Owen Phillips, could state with any authority what the next move should be in the direction of developing a plan which would meet the GARP objectives and become accepted as the or a GARP observational experiment in this area. At that point the meeting went downhill rapidly, and the last significant decision was for the group present to ask me to act as a temporary chairman or representative, with the immediate task of inviting the U.S. Committee to establish some sort of machinery or guidelines to aid in this process.

My first reaction to this request was a sinking feeling. Since I drafted much of the Section 4 in which the strong air-mass modification experiment was proposed, along with several others, I could foresee an almost endless maze of committee activity like this in the future. On second thought, however, perhaps I can limit my liabilities somewhat. I think I can only admit substantial creative responsibility for at most two of the proposed observation programs, that already mentioned and the closely related trade wind observation experiment. The others were written either in direct response to very well-documented working group reports, had some other adequate constituency, or else are pretty far down the road. So, I guess I am prepared to spend some time in the next few months in further developing these proposals, in consultation with experts in these areas. If you know of conflicting or closely related plans I would appreciate the information. What to do with the product is a question also, but right now I am inclined toward submission for publication in the AMS Bulletin, as a sort of parallel to Panofsky's article.

I think that before the U.S. Committee gives its final blessing to specific proposals for any of these field programs it should consider three questions. First, what is the need for further enlightenment, so far as large scale dynamic modeling and predictability is concerned? Second, what are the theoretical foundations which set the goals of data acquisition and anslysis? Third, what are the personnel, equipment and management requirements? In the blue book and associated working group reports we sort of faked our way through the first of these questions, got a good start on the second and first two parts of the third, and avoided the management aspect almost entirely. That last item concerns me somewhat, since the structure which Homer Newell suggests seems oriented mostly toward the major global observation requirements and perhaps the tropical experiment, with other programs to be considered from a "Darwinian" viewpoint. If the smaller field observation programs are to be considered as part of his "Darwinian" structure, they may not ever get big enough to do the job required of them.

But then, what is the job required? This is the first question above, and in several respects we have never had a very satisfactory answer from the large scale modelers. For example, in Newton's appendix to my working group report he estimates that very large amounts of angular momentum are exchanged vertically by severe storm systems. We may ask what does this exchange, which is ignored in even the most sophisticated general circulation models, do to the immediate and future evolution of the synoptic scale motions. An experiment to test this could be undertaken by any of the three competent groups in the country, but it does not appear to be on their immediate agenda. In the blue book and before, Suomi stressed very hard the need for the large scale dynamicists to simulate the proposed observational systems before they are deployed. I am now thinking that we made a mistake in not similarly asking for simulation of new scientific knowledge. It sounds absurd at first, but for predictability purposes it could probably be done quite readily in several cases. We certainly know the order of magnitudes of the small scale influences and their time and space scaling, even though we are somewhat mystified about just how they develop. Of course, it is known that certain deficiencies in the treatment of cloud convection cause gross errors in climatology, and perhaps this is sufficient motivation for the tropical experiment. Nevertheless, for planning purposes I think it would be very beneficial to determine whether the uprising of an unexpected cloud cluster disturbance in the Marshalls significantly effects the large scale atmosphere in 2 or 20 days.

Sincerely,

Dave

D. K. Lilly

cc: D. S. Cooley