Dec. 18, 1968

TO: National Academy of Sciences Committee on the General Atmospheric Research Program

FROM: H. Riehl

RE: 1968 Report of Panel on the Structure of the Tropical Atmosphere

The panel held one meeting on 6-7 June 1968 at Fort Collins: the minutes of this meeting are attached as part of the present report. After the meeting insufficient communication took place within the panel to submit a report to the NAS Committee in September as originally planned. The chairman also went overseas for several weeks in late September and October. Thus, the present document must be regarded as representing the chairman's viewpoint alone. However, reference is made to other documents, already communicated directly to the NAS Committee:

1. A letter and attachments on tropical wind analysis using ATS satellite data from Fujita to Charney dated 12 November 1968
2. A report entitled "Some Comments to the Tropical Meteorology Panel of GARP, by T.N. Krishnamurti, dated 20 November 1968
3. A proposal by Charney and Riehl to ESSA entitled "Exploration of the planetary boundary layer in the rain areas of tropical disturbances."
4. Comments on the above proposal by Pearce and Riehl dated 19 November 1968

The Scientific Problems in the Context of GARP

Quoting from the attached minutes: "The purpose of the meeting was to discuss the question of the structure of the tropical atmosphere and arrive at a procedure, for the GARP U.S. National Program, as to what experiments

other subjects mentioned initially could be solved with past and present data programs, plus commensurate theoretical development, there still would be a need to field-test the results. In all probability, there will be certain aspects of tropical meso-structure and its interaction with the large-scale environment that can be tested. But it would be far too optimistic to hope for more than than, and we must plan on the assumption that a portion of the parameterization problems remains to be solved.

With these thoughts in mind -- and assuming no other national or international effort will preempt the situation -- there is room for a renewed U. S. national effort in 1972 or 1973 and subsequent years. Planning for such an effort must be started immediately, it is understood, in view of the lead time necessary to secure the necessary funding and make equipment preparations and other arrangements.

The various planning documents at the author's disposal all suggest an experiment in the Marshall Islands area. The sketchy comments indicate that this effort again will not really solve the meso-scale questions -- however, fortunately, we are only in the beginning of the planning stage. From various quarters, especially on the side of operations, the question has been raised: why the Marshall Islands, which are so far away, where logistic support is most difficult and the expense of a large-scale undertaking extreme. This author does not see a good answer to these inquiries. In his opinion, the Marshalls had been talked about at Stockholm for an

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international program, not a U. S. national one. Even at the international level, questions may be raised about the Marshall Islands site. From the U. S. national viewpoint there certainly are areas much closer to home which would be suitable for experimentation.

1. For parameterization of meso-scale features the whole Gulf of Mexico or its eastern or western half surely would be ideal. The relatively high latitude does not matter for this particular purpose. In a three-month season from 15 June to 15 September one can expect at least 45-60 days with really "tropical" broadscale situations there, quite enough to test any theoretical interaction model that will be available in 2-3 years.

2. It is always assumed that only cumulonimbus situations pose a problem. Of this, the author is by no means convinced. Enough quasi-steady state situations occur around the Americas, so that it will be possible to test simple quantitative models observationally

   (a) for the trades in the Caribbean

   (b) for the fog regime south of Panama, enclosing an area that would reach to Lima and to the Galapagos.

It would be most useful to know whether accurate prediction within GARP specifications really is so easily achieved in these areas. In the author's opinion, such limited area programs are a definite "must" as pre-GLOMEX experiments.

3. A cumulonimbus interaction program surely can be undertaken in the Bahamas - Caribbean area as well as anywhere. The land effects are not that confounding. Besides, land stations yield precipitation values; as stated earlier, another experiment without good control of precipitation hardly makes any sense.

It is suggested to try for a cb experiment in that general area in 1972 or 1973 -- the two other experiments above could take place even earlier if desired. The experiment should be placed into a compact area, certainly not larger than the BOMEX area. Although initial plans should be drawn up strictly to test theoretical models, nevertheless a massive concentration of all types of data gathering facilities -- especially including radar -- should be provided, in case the analysts find that the initial models fail and we are once more driven off into fishing expeditions. Such concentration cannot be achieved in large areas -- only in a small area not far from home base, so that all instrumental emergencies can be covered. Further, enough time should be allowed for the actual experiment, i.e., it should run through an entire rainy season, not just a month or so. The author has the impression that at times huge preparations have not paid off to complete satisfaction because (a) not enough time was allowed for the actual operations and (b) not enough manpower and funding was set aside for evaluation. In this latter respect, it may be said generally that a real-time program of research

during field measurements is of the greatest essence. If this is not done, research results will always fall short of the maximum in our type of geophysical effort.

Operational and financial plans for the three types of experiments mentioned above can be worked out in principle by June 1969 assuming that enough aid is provided from the theoretical side in order to understand clearly the models to be tested.