Dr. C. W. Newton (University of Chicago): Streakiness of the wind field and vorticity transformations in the free atmosphere.
Prof. V. Väisälä (University of Helsinki): Lag of the hair hygrometer.
Prof. V. Väisälä (University of Helsinki): Demonstration of the Väisälä-radio-theodolite.

May 21

Prof. R. Scherhag (Universität Berlin): Untersuchungen der troposphärischen Kältepole der höheren Breiten.
Dr. V. Rossi (Finnish Met. Institute): Über die Temperaturschwankungen in der Stratosphäre.
Dr. L. Vuorela (University of Helsinki): On the upper wind distribution connected with invasion of upper tropical air.
Fil. lic. L. Raab (Swedish Met. Institute): A case of strong convection in the stratosphere.
Mr. M. Jensen and Mr. W. Nielsen (Danish Met. Institute): The motion of surface pressure centers in relation to upper mean maps.
Mr. A. H. Belmont (UCLA): A preliminary study of apparent diurnal and seasonal variations of upper-air temperature at Narsarssuak, Greenland.
Fil. lic. G. Arnason (University of Stockholm): Time-lapse photographs of clouds.

May 22

Prof. J. Keränen (Finnish Met. Institute): On frost formation in soil.
Dr. A. Nyberg (Swedish Met. Institute): A note on the eddy conductivity in statically unstable conditions.
Fil. lic. Aili Nurminen (Finnish Met. Institute): Fog in relation to discontinuities in the pressure field.

It is becoming increasingly evident that informal meetings of a small number of scientists interested in the same problems represent an extremely effective method of scientific cooperation, the more so since the large international meetings of WMO UGGI more and more tend to concentrate their efforts on organizational, administrative or technical problems. Through the concentration on a few problems of common interest and through restrictions on the size of the group participating in the meeting it is possible to stimulate effective discussions of each paper presented. It is to be hoped that this series of Scandinavian meetings will be continued and that similar group meetings will be arranged in other parts of Europe. For European science it is of course essential to maintain close contact with our colleagues in North America. Economic factors and U.S. immigration restrictions alike indicate that this contact for the time being must be based predominantly on assignments of or visits by American scientists to European institutions and meetings.

B. Bolin

Note on "A further study on the relation between the jet stream and cyclone formation"

Dear Sir,

In a very interesting article by Riehl and Teweles in Tellus no. 1 1953 some problems of vertical motion in relation to jetstreams have been discussed. The authors among other things state, that in a centre of a cold dome the cold air has ascended and that therfore Margules' theory of the conversion of potential energy into kinetic energy does not hold. They also have the opinion that there is an ascending motion on the left hand side and descending motion on the right hand side of the decelerating jet stream. I should like to make a few comments as regards these statements.

It does not appear from the charts that the cold "dome" is cut off, on the contrary there are strong indications of cold air advection in the Northwestern part of the cold "dome". The rising of isentropes is thus due to advection and not to upward motion. The cold "dome" progresses with only about half the speed of the wind in the center of the "dome" according to the authors' charts, which fact must be a result of descending motion. Anyway no physical reason for an upward motion in this part of the jet system can be seen.

Margules in 1903 did probably not know of cold domes and could not state that the whole cold dome was sinking. He just computed the release of potential energy when cold air sank and warmer air was lifted and these computations are all right. In principle this process must be the normal in the atmosphere. That this is so when jetstreams form has been

shown, see for instance Nyberg: Tellus 1949 or Nyberg: Centenary Proceedings of the Roy. Met. Soc. 1950. Riehl and Teweles now have the opinion that when jetstreams decline the cold air on the northern side of the jet ascend and that at the same time the warm air south of the jet descends. However, if one considers that part of the cold dome which is just west of the rain area and the pressure fall area. e. g. on 13 Nov. 1951 0400. it is easily seen that this cold air is sinking. Most synoptic meteorologists would use the front symbols as there is a well developed warm sector with ascending motion along a "warm front". The cold air behind the cold front is thus sinking. The rising air at the "warm front" is much warmer and potential energy is thus used to increase the circulation in the lower levels and later on even in upper levels. A new maximum of the jet is formed at the same time (Fig. 10).

It is possible that in connection with the deceleration of the jet there is a tendency to start indirect circulation but such circulation does not take place along the whole decelerating jet as may be seen from the figures. The cold air behind the cold front is sinking north of the jet. Using Riehl and Teweles' reasoning one should expect a rain area parallel to the jet and on its nothern side, but the rain area does cut the jet axis at right angles and there is also rain on the southern side of the jet. This is a normal feature.

The dynamical effect of the jet might be to start the cyclogenesis, in the process of cyclogenesis potential energy is converted into kinetic energy. The advection of vorticity may be important but the value of the absolute vorticity of between 2 f and 3 f shows clearly enough that other processes are dominating. These processes are sinking motion and convergence in the cold air north of the jet stream.

I think that Riehl and Teweles have taken up for discussion a problem which is a basic one and it really deserves continued and careful study.

Stockholm, June 1, 1953.

Yours sincerely

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Swedish Meteorological
and Hydrological Institute.

Reply

I was pleased to receive Dr Nyberg's comments; they give me an opportunity to mention some thoughts I had considered too speculative for inclusion in the formal paper.

First, however, I must dispute Dr Nyberg's comment on the motion of the cold dome. This was checked carefully with numerous winds in the central area all of which were 25—30 mph, exactly the rate of propagation of the dome. The winds on the 500-mb charts show this (fig. 2 of article) as well as other levels not reproduced. Propagation of cold centers with the speed of the wind has since been observed by us with considerable frequency; the published case is not unusual.

To pass on to the main body of Dr Nyberg's comments we had suggested (p. 71) "that the role of the dome in the cyclogenetic mechanism may be other, at least initially, than the simple sinking usually visualized". This is not to deny that most of the kinetic energy of cyclones is gained through release of potential energy. But we know that only a small fraction of potential energy released is so used, and only under special circumstances. It has been our aim to try to penetrate beyond

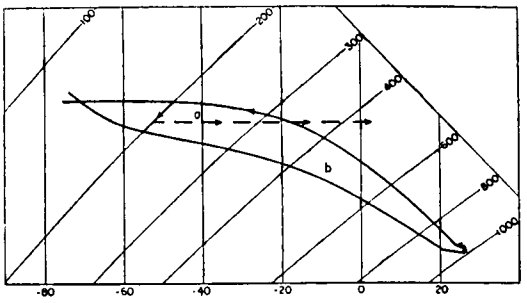

Fig. 1. Tephigram showing isothermal expansion of air at sea level to 960 mb, dry-adiabatic ascent to condensation level, then moist-adiabatic ascent to 200 mb and horizontal mixing at 200 mb. Dashed horizontal line indicates descent of mixture outside the circulation. Lower curve represents mean tropical atmosphere. The whole area (a + b) represents total heat gained by hurricane circulation, and area (a) represents maximum amount of energy available for conversion into kinetic energy.