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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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

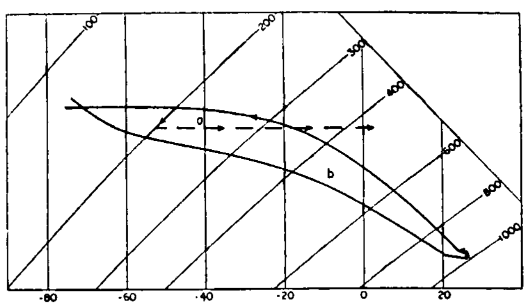

Fig. 1. Tephigram showing isothermal expansion of air at sea level to 960 mb, dry-adiabatic ascent to condensation level, then moist-adiabatic ascent to 200 mb and horizontal mixing at 200 mb. Dashed horizontal line indicates descent of mixture outside the circulation. Lower curve represents mean tropical 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.

generalities and ascertain more precisely the circumstances that will make conversion of potential to kinetic energy possible.

In a sense, the paper represents a continuation of previous studies on the energy release in tropical cyclones (1). There it is a question of conversion of latent heat released during ascent to kinetic energy. Fig. 1 shows a typical ascent path. If the air that has risen descends dry-adiabatically after reaching the upper troposphere, it will everywhere be warmer during the descent than during the ascent, and no kinetic energy can be gained from the rainfall. If however, as is suggested, high level mixing takes place between the air inside the circulation and its surroundings, then the descent of the mixture will yield a positive area (a) on the tephigram capable of conversion to kinetic energy. The maximum amount of energy that can be gained through this process is far more than that needed to maintain the circulation against surface friction.

Concerning the cold dome, the question arises whether a special mechanism can be found that will serve a purpose analogous to the mixing of inside and outside air in the high tropospere of tropical storms. It is well known that simple sinking of a whole dome, shown in fig. 2 a, decreases the horizontal pressure and temperature gradients, thus is not at all suitable for the generation of wind. If the surface of the dome, however, is deformed as indicated in fig. 2 b, then a partial conversion of potential to kinetic energy can take place.

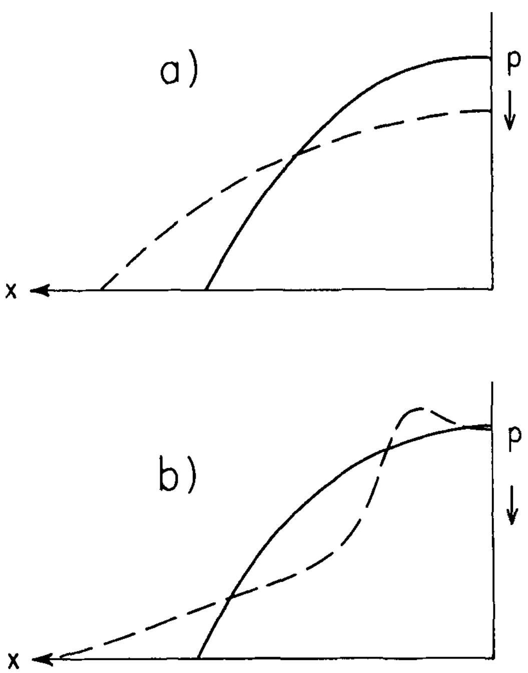

Fig. 2. Sinking of cold dome with boundary shown initially by solid line, after sinking by dashed line. (a) Simple sinking of whole dome; (b) net sinking couple with ascent of cold dome center.

Our study suggests that fig. 2b is realistic, and work is continuing along that approach. We are far from disputing that a large part of the cold air in the west is sinking as Dr Nyberg seems to suggest; low level spreading of the cold air is quite obvious from figs. 1 a—d. Our reference was to the center of the dome, the generation of a stronger jet stream as the slope of the dome steepened, and the downstream effects as the strong winds move forward. Extension of this approach may succeed in overcoming the obstacles of getting beyond generalities concerning the energy cycle.

Dr Nyberg also seems concerned with the "indirect" vertical circulation at the forward edge of the jet maximum. Such indirect circulations were not discovered by us; their association with the jet stream was first brought out in an article in 1947 in which Dr Nyberg participated (2). I feel that we can let the case speak for itself regarding the vertical cell; moreover, the comments on the frontal analysis are not clear to me. The frontal picture is a simplified version of official analyses made at the time; it was extremely difficult to maintain good continuity due to weak low level airmass differences, a frequent feature. Concerning the intersection of jet stream axis and rain area (not at right angles) I must refer Dr Nyberg to p. 76 where this intersection has been discussed explicitly. Noting that the evidence showed low-level convergence north of the jet axis but did not reveal subsidence to the south we concluded that any indirect cell was not complete and that the region where subsidence compensating for the upward mass transport through the 200-mb surface takes place, is not completely delineated by the charts presented here.

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