OFFICE NOTE NO. 11

NOTE ON THE PRESENT STATE OF DEVELOPMENT IN BASIC THEORY IN JNWP

Among the gross features of the atmosphere, one characteristic stands out which pertains directly to the dynamics of the system. Throughout the troposphere, which contains about 80% of the mass, the potential temperature varies by about only ± 10%. In illustration, the potential temperature in the troposphere of the U. S. Standard Atmosphere varies from 287° at 1000 mb to 312° at the tropopause, 235 mb. From long familiarity, it is easy for the meteorologist to under-emphasize this simple fact, but it appears to be the central empirical source from which the success of numerical predictions derives.

Brushing aside all considerations arising from the observed detailed vertical structure of the atmosphere, suppose that one were to directly associate the descriptively isentropic nature of the atmosphere with a dynamically autobarotropic behavior. The result would be the formulation and application of the equations for a single-layer autobarotropic gas. These equations would correctly include the effect of the free upper surface. Experience here at JNWP has tended to show that the autobarotropic equations, slightly modified to account for the stabilizing influence of the stratosphere, have greater over-all skill than any other dynamical formulation proposed to date.

On the other hand, suppose that one were to attempt to account <u>in the mechanism</u> for the observed vertical structure of the winds. The result might be, for example, the equivalent barotropic equations. The free-surface effect would be an order of magnitude too small--so small, in fact, that its neglect would have no important effect on the prediction. But omission of the free-surface effect leads to de-stabilization of the ultra-long waves, and serious prediction errors. This is a clear case of error arising from a departure from nature. The isogonality of the wind in the vertical is a more realistic description of the atmosphere than the invariance of the wind in the vertical of the autobarotropic layer. This, however, is less important in this case than the fact that the equivalent barotropic equations describe no physical system.

Suppose we now attempt to improve on the autobarotropic model, by taking into account the potential energy stored in the internal structure of the atmosphere. The goal will be the modest one of obtaining any increase at all in over-all skill over the autobarotropic model. An approach, almost certain of success on its face, would be to study the mechanics of a number of superposed autobarotropic layers. It is intuitively obvious that such a physical system would approach the atmosphere in its characteristics of behavior as the number of layers increased. It is also reasonable to expect _a priori_ that even a two-layer model would result in _some_ improvement in over-all skill over the autobarotropic , although perhaps not an easily detectable improvement. The solenoid field in the atmosphere does not appear to be particularly complicated, and should be representable in part, at least, by two layers.

The stunning fact is that in experimental runs at JNWP, not only does a two-layer model fail to predict better than the single-layer autobarotropic--it predicts measurably worse! In my opinion, this is so counter to expectations that it must constitute the main question in basic theory asked now in our research at JNWP. Until we get a firm answer to the question, why the addition of information results in deterioration in the prediction, there can be little development of a straight-forward nature. Many answers have been proposed, but none yet demonstrated to be correct. Among the more interesting are

1. Two layer models have been successful elsewhere, although not at JNWP. This might call for testing models claimed to be

successful. There appear to be no fundamental differences between our two-level model and others, only differences in detail. And experience with baroclinic models elsewhere has been limited, compared with experience here.

2. Two layers cannot adequately resolve the baroclinicity. This does not explain why a two-layer model is worse than the single-layer, which does not resolve baroclinicity at all. There is the possiblity that the two-layer resolution contains some sort of bias which would be removed or diminished with a higher degree of resolution.

3. Non-adiabatic effects, friction, turbulent mixing, or some other effect, unaccounted for in the present two-layer models somehow com pensate or balance the effects included in the model.

4. Our two-layer model contains physical inconsistencies and/or consistent numerical errors which are more important than the baroclinic effects included in the model. This should at least be more probable in the meteorologist's mind than it was, say, a year ago, since we have had a number of experiences with this type of error.

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