Ablaufgeschwindigkeit der Vorgänge in der Natur beträgt dann ungefähr 2 000. Bedenkt man, dass die Verwendung eines nichtgeostrophischen Prognosenmodelles, in dem die internen Gravitationswellen nicht eliminiert sind, Zeitschritte von 15 oder sogar 7½ Minuten erfordert, so bedeutet die Rechnung mit Zeitschritten von 3¾ Minuten nur noch eine Vervierfachung oder Verdoppelung des Aufwandes an Rechenarbeit. Der jeweilige Filmvorschub kann einprogrammiert werden.

Einer praktischen Anwendung steht jedoch zunächst noch eine zu lange Erstellungszeit für einen derartigen Film entgegen. Die zum Ablauf einer 72-stündigen vorausberechneten Entwicklung erforderlichen 1152 Kartenbilder benötigen alleine 13,87 Stunden lediglich zur Darstellung auf dem Bildschirm (pro Bild 45 sec gerechnet).

4. Schlussbemerkung

An der Ausprüfung des Programmes hat Frl. CH. HÜBNER entscheidenden Anteil gehabt, wofür ihr an dieser Stelle gedankt sei.

SCHRIFTTUM

DÖÖS, B. R., EATON, M. A., 1957: Upper-air analysis over ocean areas. *Tellus* 9, pp. 184—194.

MÜGGE, R., 1953: Zyklonenbildung auf der Wetterkarte. Institut für Film und Bild in Wissenschaft und Unterricht, Hochschulfilm c 581/1951.

STAFF MEMBERS INSTITUTE OF METEOROLOGY, UNIVERSITY OF STOCKHOLM, 1954: Results of Forecasting with the Barotropic Model on an Electronic Computer (BESK), *Tellus* 6, pp. 139—149.

LETTERS TO THE EDITOR

Statistical Forecasting

"*Statistical Forecasting Operators Based on Dynamical Equations*" by Duane S. Cooley, *Tellus*, **10**, pp. 331—341 (1958).

Dear Sir,

In this valuable article Dr. Cooley reports that the non-linear terms as predictors of 24-hour height change in a linear regression scheme add insignificantly to the reduction of variance obtained by the linear terms alone. He states (p. 339) that "one should not infer directly that their value is small in dynamical forecasting. However, it is not believed that statistical and dynamical forecasting techniques are so greatly different that there are not some serious implications about the relative importance of these non-linear terms for dynamical forecasting also."

It seems evident to me that there are no implications whatever about any such relative importance. Dr. Cooley has already pointed out (pp. 337—338) that the dynamical forecasts are made in one-half-hour time steps. If they were made with a 24-hour forward time-difference, the forecasts would undoubtedly achieve on the average an insignificant reduction of variance. Everyone who has worked with maps in the laboratory will testify that a mechanical forecast from a single tendency computation is without value. Dr. Cooley's statistical method should be compared in this regard not with the JNWP forecasts, but with the graphical technique originated by R. FJÖRTOFT (*Tellus*, 4, No. 3, 1952). Here again the instantaneous velocity field is never used for advection, but a space-mean velocity field, and for best results even this field is modified in accordance with the intuitive expectation of the forecaster. It would be interesting to know the contribution to reduction of variance in Dr. Cooley's method of Fjörtoft's advection term $\overline{V}\frac{\delta\zeta}{\delta s}$ It would also be interesting to see the results of applying the operators to independent data.

In conclusion, therefore, the relatively small importance of advective terms in the statistical scheme described is a measure not of their importance in the dynamic equations, but rather of some average rate of change of the wind field during the forecast period.

Yours very truly,
MORTON G. WURTELE
University of California
Department of Meteorology
Los Angeles 24, California

Reply

Dear Sir:

I wish to thank Professor Wurtele for his comments on my paper.

The remarks quoted from the final portion of my paper should have been phrased as a suggestion rather than a conclusion. Certainly Professor Wurtele has a firm theoretical basis for his point of view. However, empirical evidence from my paper and others indicates that the non-linear vorticity advection terms are not being incorporated effectively in either the dynamical or statistical forecasting procedures.

In reply to Professor Wurtele's comments, I wish to point out the results of the forecasts made by THOMPSON and GATES (1956), by numerical solution of both the barotropic and thermotropic equations using the initial data for the same 30 days (60 cases in January, 1953) and nearly the same area (North America) as used in my study. Table I of their paper states that the barotropic 500 mb forecast height changes had a mean correlation with the observed changes of 0.74 and had a mean RMS error of 229 feet per day. The 500 mb thermotropic forecast height changes also correlated 0.74 with the observed changes and had a mean RMS error of 231 feet per day.

Thus, the vorticity and thickness advection terms

$$J\left[(Z_5 - Z_{10}), \nabla^2 (Z_5 - Z_{10})\right]$$
$$J(Z_{10}, \nabla^2 Z_{10}), \quad J(Z_5, Z_5 - Z_{10})$$

which appear in the finite difference form of the thermotropic equations but not in the barotropic equations added nothing to the 500 mb height change prediction when dynamical methods were used.

The statistical operators have not been applied to the study of the reduction of variance of the advection term $\overline{V}\dfrac{\delta\zeta}{\delta s}$ nor to independent data.

Sincerely,
DUANE S. COOLEY
Meteorological Development Laboratory
Geophysics Research Directorate
11 Leon Street
Boston, Massachusetts

REFERENCE:

THOMPSON, P. D., and GATES, W. L., 1956: A test of numerical prediction methods based on the barotropic and two-parameter baroclinic models. *J. Meteor.* **13**, pp. 127—141.

BOOK REVIEW

C. F. RICHTER, *Elementary Seismology*, W. H. Freeman and Co., San Francisco, 1958, 768 pp.

In order to appreciate the importance of this book, not equal to anything earlier in print, it is necessary to start with a few words about the author. Dr Charles F. Richter is Professor of Seismology at the California Institute of Technology in Pasadena, California. He took his Ph. D. in 1928 with a thesis in quantum physics and started his work at the Seismological Laboratory in Pasadena already in 1927, the year of its foundation. The intention was that he should work on theoretical problems, but he soon found out the necessity of observational data for a fruitful research. Since Professor B. Gutenberg came to the Laboratory in 1930, there appeared a long series of joint papers by Richter and Gutenberg, culminating in the famous book "Seismicity of the Earth". Richter is the father of the magnitude scale, first published in 1935, and later very much extended; this scale has placed earthquake statistics on a sound basis and is indispensable for simple energy computations etc. Richter has worked extensively on the seismicity in southern California both by seismogram studies and field investigations; he is famous for his detailed analysis of the large number of aftershocks of the Kern County earthquake in 1952. Few, if any, seismologists have an experience in interpretation of seismic records, comparable with Richter's. It is of extremely great