6.1 OPERATIONAL NWP POSTPROCESSING—THE EARLY YEARS

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1. INTRODUCTION

The term "postprocessing" was not in use when the activity to which it now refers came into being. Rather "statistical forecasting" or some form of it was used. The less inclusive terms perfect prog and Model Output Statistics predated postprocessing by three or more decades. For a time in North America and parts of Europe, the term "model interpretation" was in vogue. The European Center for Medium Range Weather Forecasts (ECMWF) held a workshop in 1982, "Interpretation of Numerical Weather Prediction Products." The World Meteorological Organization (WMO) sponsored a workshop in 1991 (Glahn et al. 1991), "WMO Workshop on the Interpretation of NWP Products in Terms of Local Weather Phenomena and Their Verification." Also, the term "downscaling" is being used that can usually fall under the umbrella of postprocessing. While postprocessing was used at least as early as 1980 (Finizio 1982), its widespread use did not come until about 2000.

Postprocessing has come to mean the manipulation or transformation of output from numerical weather prediction (NWP) models to produce some meteorologically-related product. The transformation can take many forms and be a multi-step process. The generated product may be a simple bias correction of a variable the model produces or may be quite different from the model outputs. In the early days, the only variables forecast by NWP were geopotential heights, wind, and maybe moisture, and a statistical technique produced, for example, daily maximum 2-m temperature. Much later, 2-m temperature was forecast directly by a model, and much less processing was required to modify it into something better.

But operational postprocessing, which started in the 1960's, was built on statistical methods and techniques developed years before. The investigation of statistical methods to produce forecasts started before NWP, and early methods involved scatter diagrams or tabulations of observed data in categories.

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2. THE CLASSICAL PERIOD

Certainly there were statistical methods before numerical weather prediction, the latter dating back only to the mid 1900's. Klein (1969, 1982) defined these as "classical" methods, and others have used this terminology [e.g., "A Classical-REEP Short Range Forecast Procedure" (Wilson and Sarrazin 1989). It is also described in Wilks (2011). As early as 1905, Besson (1905)] studied the rainfall at Paris as a function of pairs of meteorological observations. He stated, ". . . .If one has at one's disposal a sufficient number of observations, the problem can be resolved by statistics, which will furnish, for each case, not only the most probable forecast, but also the degree of probability of the event forecast." He composed diagrams, based on 21 winters of data, with the relative frequency of precipitation as a function of one variable (e.g., pressure) and two variables (e.g., pressure and wind direction). He found only marginal success by using two variables rather than just one, and stated it might not be beneficial to compute relative frequencies of rain as a function of three variables. He also noted to do so would require about 10 times the amount of data to achieve the same precision. This is puzzling, because later others using essentially the same procedure gained benefit in using more than one or two variables. However, perhaps Besson required more "improvement" than other authors, and it is not clear to what extent other authors actually tested for improvement as each predictor was added.

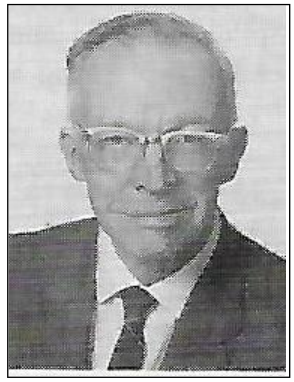
Glenn Brier greatly influenced the work in the United States Weather Bureau (WB) in the late 1940's and 1950's with his hierarchical, multi-diagram approach (Brier 1946). Jack Thompson, using Brier's approach, was one of the earliest developers of an operational statistical forecast system. Like others in the WB in that day, it was for a single station, Los Angeles. The study and results were published in the *Monthly Weather Review* (Thompson 1950), and was quite influential, especially within the WB.

The WB had a Research Forecaster Program in which there was a designated research position at about a dozen offices. The research forecaster's job was, in part, to do such statistical studies for sites in his immediate area. This program was coordinated by Roger Allen who headed the Short Range Forecast Development Section (SRFDS), part of the Weather Bureau's Office of Meteorological Research (OMR). I was hired into Allen's branch in 1958.

Glenn Brier as he received an Outstanding Achievement Award at the International Meeting on Statistical Climatology held in Toronto, Canada, in June 1993 (Murphy and Zwiers, 1993). (Picture printed in *BAMS* **74,** 1993, p 1723.)

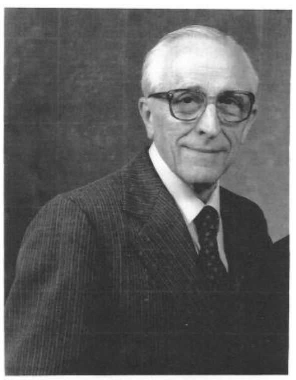
One of the earliest studies that made use of regression was done in SFRDS by Conrad Mook and Saul Price (Mook and Price 1947) who derived regression equations for forecasting the minimum temperature at Washington, D.C. This early work followed WB sponsored contract work at New York University done by J. E. Miller and A. E. Burgtorf. Physical reasoning was used to select temperature predictors upstream of Washington. The equations were developed on 9 months of data. Results on new data were mixed, and it was noted much more research was needed.

The Air Force Cambridge Research Laboratory (AFCRL) had an active program in studying and deriving statistical methods of forecasting. In particular, Irving Gringorten (1949) and Iver Lund (1955) were leaders in this pre-computer era. Irving's 1949 paper, "A Study in Objective Forecasting," was especially significant because he defined the terms "predictor" and "predictand." These terms soon became widespread in relation to statistical forecasting.



Jack Thompson had a big influence on the evolution of statistical studies in the WB. Jack was given a special award by the American Meteorological Society (AMS) in 1988 "for major contributions as an operational weather forecaster, teacher, and meteorological consultant over almost fifty years." (Picture from BAMS, 69, 1988, p 657.)

There were two "centers of activity" for statistical studies in the WB in the 1940's, 50's, and early 60's. One was headquartered in OMR, of which the SFRDS was a part, and the other in the Extended Forecast Division (EFD) located at Suitland, Maryland. Each group was trying to assist the forecaster. Many studies were for local sites, as an individual forecaster doing the studies was responsible for short range forecasts over a limited area. However, the Extended Forecast Division was responsible for nationwide forecasts of mean values of temperature a few days in advance. This drove these researchers to think more in terms of circulation patterns and their forecastability than those who were supporting "next day" forecasting.



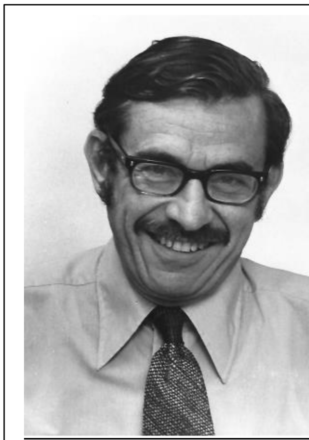
Roger Allen was Chief of the Short Range Forecast Development Section of the Weather Bureau's Office of Meteorological Research. This was the group most involved with statistical weather forecasting when I joined them in 1958. Roger coordinated the statistical activities for the Bureau for many years. (Picture furnished by Rogers's family.)

William (Bill) H. Klein, a leader in statistical development in the EFD, studied, as had Brier (1946), wintertime precipitation in the Tennessee Valley (Klein 1948). He related 5-day average precipitation to concurrent, hand prepared "perfect-prognostic," 5-day mean, 700-mb maps. He states,

". . . it is the writer's belief that, in the long run, both our understanding of the weather and our ability to forecast it will be improved most by separate considerations of the two fundamental forecast problems, the prognosis of the circulation and its interpretation in terms of weather."

He contrasts his study with others, some of which, for example Brier (1946), have been

Glenn Brier, Jack Thompson, Roger Allen, and Bill Klein had a great influence on the way statistical forecasting developed in the Weather Bureau.



William H. Klein was likely the first to apply the output of NWP in a statistical forecasting process. He championed the "perfect prog" technique and was later an avid supporter of MOS. He was the first director of the Techniques Development Laboratory. (Photo from TDL/MDL archives.)

mentioned above. He found that when he verified on independent data and used the observed 700-mb maps, the results gave the correct anomaly class about two-thirds of the time and were definitely superior to the forecasts made by an official forecaster of the Extended Forecast Section. However, when the inputs were prognostic maps prepared by forecasters, the forecasts were correct only about one-fourth of the time and were inferior to subjective forecasts made by an official forecaster at the Extended Forecast Section. He concluded: "The objective method is potentially of great forecast value, *but this value will not be realized until the quality of the prognostic maps is first considerably improved*." (Klein 1948).

This line of thinking dominated Klein's thoughts and his championing this method—a method which came to be called the perfect prog (PP)--for two decades. While PP has largely faded out for day-to-day forecasting, Bill's idea on separating the problem into forecasting the circulation and then the interpretation of weather still remains. NWP has concentrated on the circulation, and the tougher problem of "weather" forecasting has come more slowly and has been largely in the purview of statistical methods until quite recently. Bill said "circulation," because at that time geopotential heights and winds dominated the upper atmospheric forecasts. It was likely beyond anyone's ken to think about useful forecasts of temperature and moisture above the surface. But Bill, if asked, would have undoubtedly extended division of the problem to "upper atmosphere" and "surface."

Klein had related weather to upper air variables and applied the results to subjectively-prepared forecasts of those variables. In a similar manner, following some work by the U.S. Navy, Sassman and Allen (1958) related precipitation occurrence at three stations (St. Louis, Missouri;[1] Washington, D. C.; and Albany, New York) to upper air variables, and applied the results to vertical velocity forecasts produced from the thermotropic model being run at Joint Numerical Weather Prediction Unit (JNWPU) (Thompson and Gates 1956; Shuman 1989). Sassman and Allen must have shared Klein's view that the key to predicting "surface weather" was in predicting the upper atmosphere, and relating the surface weather to those upper atmospheric predictions. Although Allen's SRFDS and the EFD where Klein worked were both in the WB, they were across the city and there is no evidence that they collaborated at any time in their development of forecasting techniques.[2] However, Klein referenced Brier's and Thompson's work, so he was aware of earlier statistical studies.

It would be hard to overestimate the influence of The Travelers Research Center (TRC) on the development of statistical forecasting. They secured funding from the U.S. Air Force, the Federal Aviation Administration (FAA), and to a lesser extent the WB to improve forecasting for aviation, especially ceiling height and visibility. Their approach was to make use of current, and sometimes past, observations to forecast into the future. This approach, except for the first hour or two, cannot compete well with other systems that have an advective or more dynamic approach. Rather, TRC's contribution was in the techniques they used and importantly documented, even if mainly in contract reports. The series of reports included one by Robert (Bob) G. Miller (1958), "The Screening Procedure." This was not the first use of regression, nor even of stepwise selection, as Wherry et al. (1940) and Lubin and Summerfield (1951) had discussed it earlier, but without the term "screening."[3] But Bob brought it forth into the meteorological community, and he had a watchful audience in the WB. Soon staff` in WB headquarters and in the EFD were writing screening programs. Miller devised and promoted a stopping procedure for predictor selection, essentially a significance test, based on the additional reduction of variance of a variable. It was the F-test adjusted for the number of predictors screened.[4]


Robert G. (Bob) Miller of the Travelers Research Center. He later became a branch chief at TDL. (Photo via Allan Murphy and Ed Epstein.)

Miller and others at TRC, including Isadore Enger, Jim MacMonogle, Eugene Aubert, Duane Cooley, and Russ Harris, made copious use of binary variables—as predictands to yield probability forecasts, but also as predictors. This was the way they treated the highly non-normal distributions of ceiling and visibility.

[1] Interesting choice of stations. Allen had been stationed in St. Louis for a short time before coming to Washington.

[2] SRFDS was at 24th and M Streets in downtown D.C.; EFD was in Suitland, Maryland.

[3] There is no evidence members of TRC were aware of Wherry et al.'s (1940) or Lubin and Summerfield's (1951) work.

[4] Although the adjusted F-test was a step in the right direction, there are other issues associated with the usual non-normality of variables related to ceiling and visibility, especially binary variables. Also, the adjustment did not address temporal correlation. As a result, no specific level of significance can be attached to the test, and some other stopping criterion may be just as good.

Again, binary variables in regression was not new,[5] but Miller coined the term REEP for Regression Estimation of Event Probabilities (Miller 1964). This applied to using binary variables to represent a series of categories of a predictand, and the term REEP has persisted to today.

Another TRC report of renown was Miller's AMS monograph (Miller 1962) "Statistical Prediction by Discriminant Analysis." He essentially codified Joe Bryan's[6] work on multiple discriminant analysis (MDA) and used a method of determining probabilities from the discriminant functions due to Fix and Hodges (1951).

> Bob Miller, Joe Bryan, and others at the Traveler's Research Center were very influential in statistical weather research in the 1950's and 1960's.

While some use of NWP models was finding its way into the statistical methods, it was minor, and this period of purely statistical forecasting without the benefit of NWP can be called the classical period.

3. THE SUBSYNOPTIC ADVECTION MODEL — PREPARING FOR MOS

By the mid 1960's, there were no statistical forecasts being prepared centrally and communicated for use by field forecasters In fact, there were no statistical forecasts ready for distribution except possibly Bill Klein's mean temperature forecasts for a few stations which were being used internally at the National Meteorological Center (NMC). Statistical forecasting was not really being taken seriously by WB higher management. But foundational techniques and software had been developed, experience gained, and statistical work was spreading.

One of the organizational elements in the Meteorological Research Projects Branch, OMR, besides Allen's Short Range Forecast Research Project (SRFRP)[7], was the Aviation Forecasting Research Project (AFRP). It was newly formed and was headed by Charles F. Roberts, recently from the U.S. Air Force. He asked me to transfer into the branch to develop a short-range mesoscale model. The transfer was effective May 10, 1964 (WB 1964c).

On October 1, 1963, Dr. Robert White became chief of the WB, replacing Dr. Francis Reicheldorfer (WB 1963). He soon brought change. OMR, of which we were a part, was abolished, and the new Systems Development Office (SDO) headed by Merritt Techter inherited us (WB 1964a; 1964b). The Techniques Development Laboratory was formed in 1964 as part of SDO.

The statistical use of numerical model output was beginning, but no distribution of products to the field forecasters was even being planned. Bill Klein and associates in the EFD were using the PP technique to produce guidance to be used internally in their division. But the relationships developed between near concurrent upper air *observations* and surface variables did not hold well when applied to upper air *forecasts*, even though the results were useful. It was obvious to me that the relationships should be developed between actual NWP upper air *forecasts* and surface variables at the desired projections.[8] However, building such relationships was not possible because a lengthy sample of an operational model would be needed, and the models were undergoing rapid change. Moreover, there was no upper level management interest in developing a process whereby a suitable sample could be collected and used for this purpose.

At this time, NMC's operational model had a grid spacing of 381 km at 60° N on a polar stereographic map, which is about 340 km at the mid latitudes of the CONUS. Certainly, weather processes occur on a much smaller scale, and surface observations would support a smaller grid length. Roberts wanted me to build a smaller-scale model, a tall order for someone without modelling experience. But fortunately, a couple of models had been developed that seemed suitable. After visiting Fred Sanders at MIT, and George Platzman at the University of Chicago, I embarked on the task. Dale Lowry soon joined the project in 1965, transferring from the Analysis Division in NMC. George Hollenbaugh also joined as a programmer and that exactly tripled my null experience in such matters. Jackie Hughes and Elizabeth Booth also joined the project as meteorological technician support for the many processes being carried out by hand, such as tabulating and plotting data, drafting figures, and punching data and FORTRAN statements onto cards. George and I did all the programming for the project.

[5] Use of binary variables had been published by Lund (1955) and Suits (1957). Binary variables used early-on were of the "discrete" type; later some researchers preferred "cumulative" binaries (Glahn (1965).

[6] Joe Bryan had laid out the method in his 1950 Harvard University Ed. D. Dissertation, "A Method for the Exact Determination of the Characteristic Equation and Latent Vectors of a Matrix with Applications to the Discriminant Function for More Than Two Groups" (Bryan 1950).

[7] There had been a name change from Short Range Forecast Development Section to Short Range Forecast Research Project.

[8] The term "projection" to mean "time into the future" was becoming well entrenched. The term likely came from the WB headquarters group. Certainly, Roger Allen supported it. Bill Klein used "into the future," and projection was not being used in the early TRC reports nor the Irv Gringorten and Iver Lund papers.

The Environmental Science Services Administration (ESSA) was formed in 1965 with Dr. Robert White as Administrator. The Weather Bureau retained its name with Dr. George P. Cressman as Director (ESSA 1965). Within a few months, he brought Bill Klein over from the EFD to head TDL as its first permanent director. This was a good move. Bill was aggressive, had experience, knew Cressman well, and with his interest in statistics such work now had more status than previously. The project we had started under Roberts continued.

Quoted from Glahn and Lowry (1972):

> "The system [at NMC] then in operation (Fawcett 1962) was geared to the upper air observation times of 0000 and 1200 GMT. No hourly data (Teletype Service A) and little if any surface synoptic data (Teletype service C) were input to the numerical models. The grid length was 381 km at 60° latitude, which may be adequate to describe and project to 36 h most features at 500 mb. However, some detail is lost, and certainly the small-scale features of the sea level pressure field defined by the relatively dense hourly surface reports cannot be captured with so coarse a mesh.
>
> "Therefore, we wanted the new system to have the following characteristics:
>
> - The forecast cycle would be determined by the needs of the field forecasters rather than upper air observation times.
> - All data routinely available, including hourly, would be used.
> - A mesh length commensurate with the spacing of observation stations would be employed.
> - Numerical and statistical models would be combined to forecast actual weather variables such as cloudiness, surface winds, probability of precipitation, and maximum temperature.
> - The numerical model portion of the system had to be rather simple so that computer time would not be excessive.
>
> "In addition to requiring the system to have the above characteristics, we wanted to adapt existing models, rather than develop completely new ones, so that implementation could be achieved more quickly. With these things in mind, we chose to adapt two existing numerical models—the Reed (1963) Sea Level Pressure Model and the SLYH precipitation Model (Younkin et al. 1965). The combination and modification of these two models we call the Subsynoptic Advection Model or SAM."

Richard Reed had spent a year at NMC and developed the sea level pressure model. This is a bit of a misnomer; it was really to predict the 1,000-mb height. Reed had tested it in the usual, at the time, Eulerian framework, and also in a Lagrangian framework, mimicking graphical methods he (Reed 1960) and others had previously used (e.g., Fjortoft 1952; Okland 1962). By using the 500-mb height and a rather smooth "equivalent advecting wind" from the operational barotropic model, he found the characteristic errors in the Eulerian framework to be reduced in the Lagrangian. There is no indication this model was ever run on a grid finer than 381 km. Fred Shuman (1989) was later to say about accuracy of forecasts at NMC: "The error at sea level continued to decline, and for the 5 years from 1962 to 1966 the decline was attributed largely to Reed's model." Quoted from Glahn and Lowry (1972) concerning Reed's model:

> "This model has been in continuous use at NMC since about 1963 on the hemispheric, 1977-point grid. Since the advent of NMC's Primitive Equation (PE) Model (Shuman and Hovermale 1968) in June 1966, the Reed model has been used for a 'preliminary' forecast package for extended range guidance."

Essentially the downstream (forecast) 1,000-mb height was the upstream 1,000-mb height modified by (1) the change in 500-mb height (a deepening term) over the trajectory, (2) the change in latitude over the trajectory, and (3) the terrain change over the trajectory, each of these with an appropriate coefficient.

The 381-km distance between gridpoints came to be called a "Bedient" after Art Bedient a technological genius at NMC. This term was probably coined by John Stackpole (1978, p. 2), a denizen of NMC for many years. This exact value was used because it was ½ inch on a 1:30 million polar stereographic map projection true at 60°N. The one-half inch was exactly the distance of 5 print wheels on the IBM 1401 line printer used for gridprinting zebra maps (Hoke et al. 1981, p 42).

Besides the 500-mb forecast from NMC, we were going to use the surface observations of pressure converted to sea level (SLP). It seemed that the spacing of stations reporting SLP would support a ¼- Bedient grid length, so we chose that scale such that every fourth gridpoint was an NMC gridpoint. This was a lot of gridpoints in those days, so we concentrated on the eastern United States. The 35 X 35 gridpoint area covered is shown in Fig. 1. Our intent was to develop a model, run it, and build up a history so that we could relate weather to its forecasts.

One of the weaknesses noted by Reed (1963) was the over-intensification of anticyclones, and under certain conditions, these high pressure areas would develop into a "tear drop" shape. To try to solve this problem, we constructed trajectories with the model's equivalent advecting wind, and then constructed trajectories that would give a perfect forecast. Analysis of these trajectories indicated that an advective wind with a smaller meridional component than the equivalent wind we were using would give a better result. After experimentation, we substituted a heavily smoothed advecting wind, and got significantly better results.

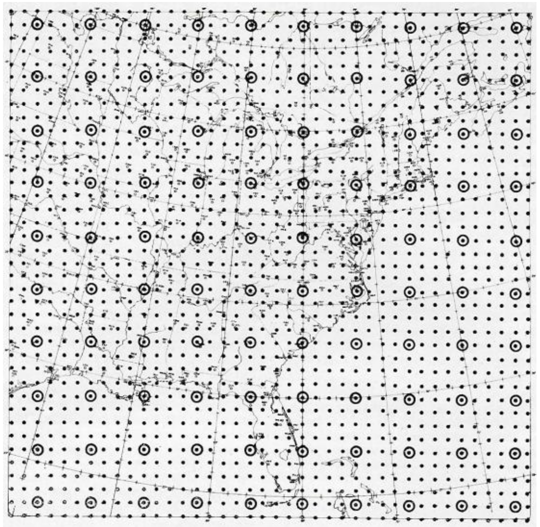


Fig. 1. The SAM grid shown by dots at gridpoints. The NMC gridpoints are at the circles. (From Glahn and Lowry 1972**.**)

Several weather variables we wanted to forecast, such as clouds, precipitation, and visibility, require some measure of moisture to be forecast, such as relative humidity. Quoted from Glahn and Lowry (1972):

> "The first 'wet' numerical model used routinely at NMC was initially developed for graphical use by Russell Younkin and Jerry LaRue. Later, Fred Sanders presented theoretical justification for its success. John Hovermale programmed the model for computer use and it was put into operation in September 1964. The name SLYH derives from the last name initials of the four persons mentioned above."

This model (Younkin et al. 1965) was solved in a Lagrangian manner, being similar in that respect to the Reed SLP model. Our use of it would be similar to its use in NMC, except we would use a mesh length ¼ that used by NMC. The moisture parameter in the model was saturation deficit (Sd). For our purposes, saturation deficit is the thickness between 1,000 and 500 mb that would have to be reduced (cooled) to produce precipitation, given the amount of moisture in the column. The downstream (forecast) Sd was equal to the upstream Sd modified by the change in thickness over the trajectory and the change in terrain height, each with an appropriate coefficient.

Plans for our model were reported in Glahn and Lowry (1969a). For the model, we needed a SLP and an Sd analysis at that scale, and none existed. Shared databases were not established, so we wrote software to decode hourly observations (Hollenbaugh et al. 1969) and to analyze them (Glahn et al. 1969b). By this time, the computer being used was the CDC 6600, a 60-bit word-length machine. The data to be decoded came from magnetic tapes collected from the communication circuits on the IBM 360-40 by NMC.[9]

George Cressman, Director of JNWPU (WB 1954) and later of NMC, recognized the power of an analysis method put forth by Bergthorssen and Doos (1955), made a few enhancements, and implemented it at 500 mb (Cressman 1959). We adopted this method and refined it for analyzing sea level pressure and saturation deficit. Essentially, the process is to start with some "first guess" value at each gridpoint, then modify the gridpoints in the vicinity of each observation based on the difference between the observed value and the grid interpolated to the observation point. This is done for more than one pass through the data, each time reducing the radius over which the observations modify the gridpoints. For sea level pressure, which is spatially continuous, a very good analysis could be made with the available observations and the gridlength being used. An example is shown in Fig. 2. Good visualization techniques were not available, and the isobars, or contours at 500-mb, were depicted by "zebra maps." These charts had alternating bands of letters and blanks between neighboring isobars; an example is shown in Fig. 3. Fig. 2 was hand drawn by tracing from a zebra map.

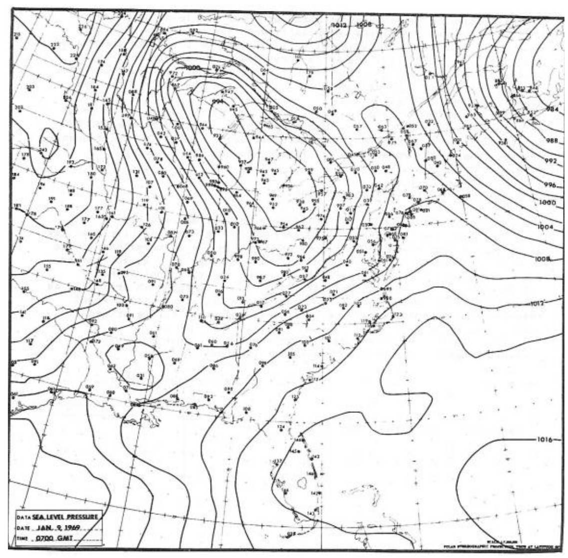


Fig. 2. Sea level pressure map analyzed at ¼ Bedient on the SAM grid for 0700 UTC January 9, 1969. (From Glahn et al. 1969b.)

Sd is not observed, so we estimated it from other surface weather variables that were observed. Total column water can be calculated from upper air reports, but we needed an estimate on smaller time and space scales. Regression equations were derived which

[9] Glahn et al. (1969b) state these were IBM 360-40s. However, Fenix (circa 1998**)** states that IBM 360-30s were purchased in 1966 and used until IBM 360-40s were purchased in 1970.

specified the natural logarithm (ln) of total column precipitable water as a function of surface dew point, weather, and clouds (Lowry and Glahn 1969). Data were gathered for 1200 UTC for 56 stations in the eastern CONUS over 2 years from the Service A teletype reports. Precipitable water values were those

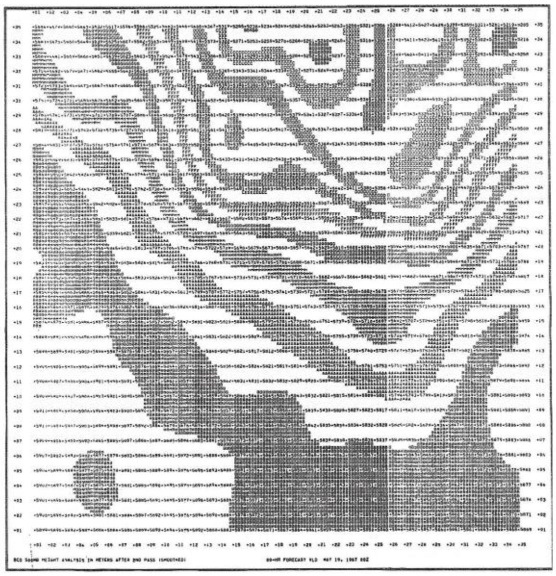


Fig. 3. An example zebra map depicting the analysis of 500-mb height for 0000 UTC May 18, 1967. (From Glahn and Hollenbaugh 1969.)

computed at NMC from radiosonde reports. Numeric code values for weather and clouds were devised for use in the regression. Approximately 86% of the variance of the ln of precipitable water could be explained by the equations. Regional and seasonal stratification added only a small improvement. Further analysis allowed the saturation thickness at stations to be specified from the estimate of precipitable water and elevation (Lowry 1972). This regression estimate of the saturation thickness could be made each day. The Sd could be computed as the difference between the saturation thickness and actual thickness and then needed to be analyzed.

Analysis of Sd is a bit trickier than sea level pressure, primarily because the values are spatially discontinuous. The values of Sd are zero by definition when precipitation is occurring, and never go negative. The analysis process tends to spread the positive values into the zero areas. Therefore the Sd values were coded to get a good demarcation between the zero and non-zero areas (see Glahn et al. 1969b for details). After coding, the Sd could be analyzed essentially the same way as sea level pressure (see Fig. 4).

We found, to our surprise after looking closely, the PE model contained gravity waves at 500 mb that needed to be filtered out before input to SAM (Glahn 1970a). Some variables output from the PE model had been time smoothed, but the heights at constant pressure surfaces had not. Fig. 5 shows the hourly values of 500-mb height for three PE gridpoints at projections 1 through 36 h. At each of the gridpoints, 3^{rd}, 4^{th}, and 5^{th} order polynomials fitted to the data are plotted. After examining plots at several gridpoints, we concluded (Glahn 1970a):

> "The heights are very noisy. The forecast change in 1 hour (due to gravity waves) may be greater than the 'real meteorological' change in 36 hours.
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> "The larger amplitude gravity waves have a period of about 6 hours. This checks roughly with previous studies. There is also a higher frequency wave indicated with a period of about 3 hours."

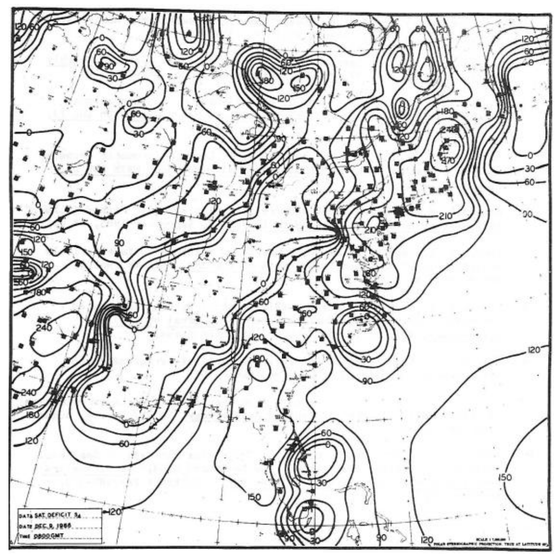


Fig. 4. An example saturation deficit analysis for 0800 GMT, December 9, 1966. The dots are stations with precipitation and the squares are stations without precipitation. The areas with no contours, along a northeast to southwest oriented frontal boundary and to the far northwest, are the areas with precipitation and zero saturation deficit. (From Glahn and Hollenbaugh 1969**.)**

For most gridpoints studied, there was not a lot of difference in the 3^{rd}, 4^{th}, and 5^{th} degree fits. What also became clear (diagrams not shown) was that to get reasonable results, the PE model output needed to be at hourly intervals, instead of the 3-hourly being produced. Eventually, the PE output was furnished hourly, and we used a 3^{rd} degree fit to obtain the values to go into SAM.

The decoding of observations, estimation of Sd, analyses, and SLP and moisture models were put together in a package we called the Subsynoptic Advection Model (Glahn et al. 1969a; SDO 1969). SAM was run for nearly 30 cases and extensive verification carried out. Quoted from Technical Procedures Bulletin No. 6 (WB 1967) concerning the tests:

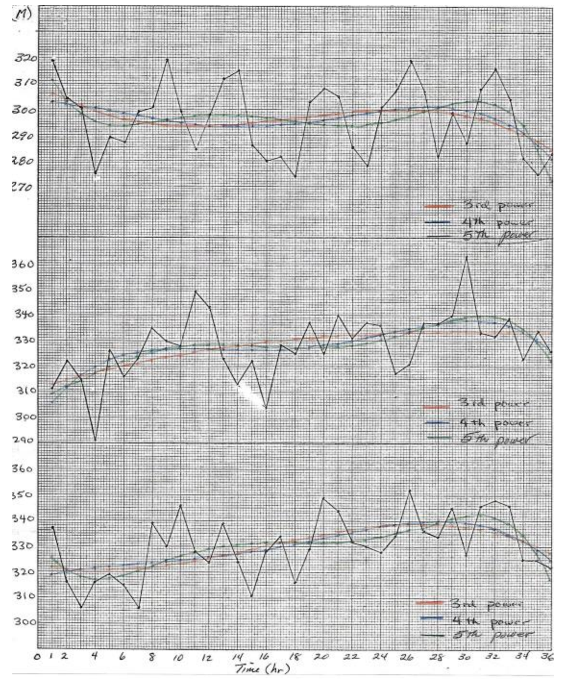


Fig. 5. PE 500-mb heights at three gridpoints, with 3rd (red), 4th (blue), and 5th (green) degree polynomials plotted. (From Glahn 1970a.)

"The results of these tests indicate that SAM apparently has a capability of predicting the occurrence of precipitation during the twelve-hour 'Today' period beginning four hours after initial data time with a degree of skill which is equivalent to that of the subjective forecasts now issued by NMC. The forecasts appear to be slightly better than those derived from the 6-layer model predictions. This apparent increase in skill is probably due to the use of a smaller grid length (and the accompanying greater detail in the initial moisture and sea level pressure fields), and the use of surface data several hours after the initial data of the PE model."

The Technical Procedures Bulletin (TPB) series mentioned above was started by Charlie Roberts in July 1967, who initially acted as TDL Director when it was first formed but was now chief of the Technical Procedures Branch, Weather Analysis and Prediction Division, Office of Meteorological Operations (MO). The purpose of the TPB series was to inform the WB forecasters, and others using WB products, of the changes occurring in the centrally produced and distributed product suite. The series started in July 1967, and lasted until around 2000, a better than average run for almost anything, some organization names having changed multiple times during that period. While the TPBs were not under the purview of the Committee on Analysis and Forecast Technique Implementation (CAFTI), they were closely tied, because for CAFTI to recommend implementation of a product, it mandated that a TPB had been written covering the product. CAFTI was formed in 1966 when Merritt Techter, Director of the Systems Development Office (SDO), parent of TDL, saw a need for a mechanism that would facilitate the implementation at NMC of techniques developed within SDO. This foresight by Techter undoubtedly contributed heavily to TDL's success in getting products implemented at NMC; before CAFTI, there had been resistance. The first members of CAFTI were Bill Klein of SDO, chair; Charlie Roberts of OM; and Harlan Saylor of NMC. When CAFTI was disestablished in 2000, the TPBs soon stopped, as no one had both the desire and the organizational clout to mandate their continuance.

A 6-month implementation test of SAM started September 6, 1967 WB (1967). Forecasts of saturation deficit and 1000-mb geostrophic wind were furnished for 25 stations for projections of 3, 6, 9, and 12 h. The Sd forecasts covered the 3-h periods ending at the projection times, and were derived from 1-h values.

The test was completed, and the results led to the recommendation (WB 1968a) for operational implementation in June 1968 twice daily starting from 0700 and 1900 UTC data. Quoted from WB (1968a):

> "The operational forecasts will be relayed to each of the four RAWARC (radar report and warning coordination) Circuits (23420, 23421, 23422, and 23423) on an unscheduled basis in the first available time following 0820Z and 2020Z. The bulletin heading will be FOUS WBC, and the format will be nearly the same as that used in the test program except that a statistically derived estimate of the surface wind direction and speed will be provided in addition to the geostrophic wind direction and speed."

SAM, implemented in June 1968, was the first numerical model to run at NMC with a grid spacing of less than 381 km. The surface wind was the first operational product forecast statistically derived from model data.

Note the addition of the statistically-derived wind. This was the first operational distribution of statistical forecasts to field offices and the first MOS (Model Output Statistics) product, although it was not yet called MOS; it occurred on or about June 10, 1968 (see Fig. 6). A rather inauspicious start, but a start. In about a 4-year period, we had decoded and collected hourly surface observations; written an objective analysis program that could analyze SLP, saturation deficit, and upper air heights; coded, improved, implemented, and tested two advective models; written verification routines; derived wind equations by regression, coordinated with CAFTI; and implemented the system at NMC.

The NWS headquarters including TDL had moved from downtown D.C. to the Gramax building in Silver Spring, Maryland in 1966. There was in the Gramax building a computer for communicating with Suitland; we could feed in cards and get printout there. This was a considerable step up in development capability; we could usually get more than one turn-around per day. Up until this time, punched cards were transported to the computer in the late afternoon and printout returned by shuttle bus in the early morning. But we were still very limited in core (memory), and jobs greater than 100K bytes would get a lower priority in the queue. A job requiring 600K was a big job. This is not meg or gig, but K!

SAM FORECASTS

	15Z			18Z			21Z			24Z		
III	$h_dh_dh_d$	ddvv	DDVV	$h_dh_dh_d$	ddvv	DDVV	$h_dh_dh_d$	ddvv	DDVV	$h_dh_dh_d$	ddvv	DDVV

SAM: Subsynoptic Advection Model

III: STATION CALL LETTERS

$h_dh_dh_d$: Saturation Thickness deficit in meters

Fig. 6. Format of SAM bulletin with saturation deficit, geostrophic wind, and surface wind. The surface wind forecasts DDVV were the first product where the statistical relationships were derived from a numerical model. (Copied from WB 1968a.)

4. THE STATISTICAL FORECASTING FLOODGATES WERE OPENED

The estimates of surface wind, as well as for geostrophic wind, in the SAM bulletin implemented twice a day on or about June 10, 1968, were for 105 stations in the eastern U.S. and were distributed on four RAWARC teletype circuits. The forecasts were based on 0700 and 1900 UTC data, the times being chosen to be helpful to the forecasters in producing their official forecasts. The surface wind estimates were extremely simply derived. The U (eastward) and V (northward) components were each computed from a regression equation of three terms: A constant and the U and V geostrophic winds from the SLP model of SAM (see Glahn 1970b for a discussion of wind prediction models). Because only a few months of developmental data were available, the equations were derived by pooling all stations together to get a large enough sample to be meaningful. In addition, the relationships were based on data valid at only 1200 UTC, but were applied to all projections. Nevertheless, it was a start, and proved that a group outside of NMC could develop a product, write the implementation software, and get it run regularly by NMC. This implementation was the culmination of a project directed by Charles Roberts 4 years previous. Also, Roberts, through his role in CAFTI, was also helpful in the implementation process, and incidentally, he as Chief of the Technical Procedures Branch of the Weather Analysis and Prediction Division, Office of Meteorology, signed TPB 14 announcing the implementation.

As stated earlier, TDL had been formed in 1964 and within a few months Dr. Cressman, now director of the Weather Bureau, brought Bill Klein over from EFD of NMC to head TDL. In the interim, either Charlie Roberts or Roger Allen acted in the capacity of director. The formation of TDL and naming of Klein, with his interest and history of statistical analysis, had a tremendous impact on the future of statistical weather forecasting. This brought together, among others, the groups formerly headed by Allen and Roberts with a laboratory director who cared about the work. Previously, Harry Wexler, the director of the Office of Meteorological Research, was much more interested in NWP than statistical forecasting and was influential in bringing operational NWP into existence.

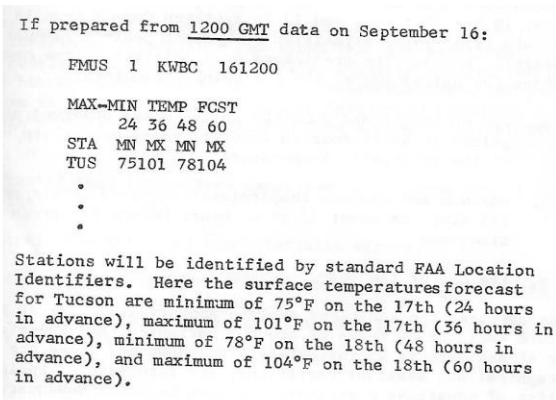
Soon after arriving at TDL, Bill authored Weather Bureau Research Paper No. 46 (Klein 1965) which was essentially work he and others had done at EFD. It gives a quite comprehensive summary of the research applications of PP, including cloudiness and precipitation as a predictand. This work is also reported in Klein et al. (1965). Bill and a small group in TDL were diligently working on temperature prediction in the same manner as Bill had been doing in EFD. Through this work, twice daily Perfect Prog (PP) forecasts of maximum (max) and minimum (min) temperature for projections 24 to 60 hours were implemented on or about September 19, 1968. These forecasts were for 131 cities over the CONUS and were distributed over national teletype, Service C (WB 1968b). The developmental process followed that described in Klein et al. (1967; 1969) and Klein and Lewis (1970). This was the first CONUS-wide (Alaskan and Hawaiian stations were not included) statistical product to be widely distributed. The product format is shown in Fig. 7.

These "forecasts" were made by applying what are essentially specification equations relating surface temperature to upper air variables and to previous observed values of temperature, the word "specification" having been used in previous studies (Klein 1963).[10] The predictors were from the barotropic and Reed SLP models. The equations were applied iteratively, where the NWP forecasts were used at the appropriate projections, and the surface temperatures were those forecast in the previous iteration. Another difference between Klein's work and that in SAM was that in SAM

[10] Klein (1963, p. 527) states that the word "specification" was introduced in 1956 by Malone and colleagues (Malone 1956).

the model predictors were from the exact location of the forecast, but Klein's screening regression could select gridpoints from essentially anywhere over the CONUS, and interpolation of the predictors to the station location was not done. This reflected Klein's extended range forecasting experience and techniques in use in EFD.

Harry R. Glahn, co-developer of SAM and MOS.



If prepared from 1200 GMT data on September 16:

FMUS 1 KWBC 161200

MAX-MIN TEMP FCST
24 36 48 60
STA MN MX MN MX
TUS 75101 78104
•
•
•

Stations will be identified by standard FAA Location Identifiers. Here the surface temperatures forecast for Tucson are minimum of 75°F on the 17th (24 hours in advance), maximum of 101°F on the 17th (36 hours in advance), minimum of 78°F on the 18th (48 hours in advance), and maximum of 104°F on the 18th (60 hours in advance).

Fig. 7. Format of the max/min temperature bulletins issued. Note the "in advance" terminology. (From WB 1968b.)

Not to be outdone, TDL's marine group provided for implementation, a system that forecasted 24- and 48-h wind waves, swell, and combined wave based on the NMC six-layer PE model (Fig. 8). The prediction equations were based on the PE 1000-mb wind, and according to WB (1968c), "Studies have indicated that the surface wind is represented best by taking 86% of the 1000-mb wind speed and backing the direction 20°." Consideration of fetch and the relationship of waves and swell to surface wind makes this a rather involved, physically-based PP technique. This followed work by the U.S. Navy and at JNWPU [see Pore and Richardson (1967) for background and details]. The system was implemented on or about October 1, 1968 (WB 1968c), and is attributed to N. A. Pore and W. S. Richardson. This technique was applied to the Atlantic and Pacific oceans, and NMC's "curve follower" was used to generate contours for a product that was distributed by facsimile [see Fawcett (1962), Fig. 3, for a picture of the curve-follower].

Dale A. Lowry, co-developer of SAM and MOS.

Also on October 1, 1968, wind forecasting equations were changed in the SAM product from those based on summer data to those based on winter data,[11] and more importantly, perhaps, was the addition of 3-hourly precipitation forecasts for four consecutive periods. These forecasts were based on areas of negative Sd in SAM and were depicted by X's on a map (see Fig. 9). Quoted from WB 1968d, "The edge of the X-covered area can be considered as the 50% probability of 0.01 inch or more of precipitation line." The wind equations were different from those initially implemented in that each regression equation had no constant term. When the geostrophic wind is very light, a regression equation with a constant term in the equations for u and v may indicate a direction which is unrealistic when compared to the direction of the geostrophic wind (see Glahn 1970b).

The first CONUS-wide statistical product was the PP max and min temperatures in 1968.

[11] TDL has primarily used two seasons for deriving statistical relationships: April through September and October through March. Initially, these were called summer and winter seasons, respectively. Later a name change was made to warm and cool seasons, respectively.

In approximately one year, between the time the first SAM test bulletin was released in late 1967 until late 1968, we had seen three different methods of postprocessing: Forecasts from SAM, which would later be called MOS, Klein's method of specification/PP, and the more physically based marine PP product. Then came another type of postprocessing. Danielsen (1961) and others had emphasized that cloud patterns and convection evolve in a Lagrangian manner, and a cloud forecast model based on this concept had been under development since 1962 by the Air Weather Service. Edson et al. (1967) had achieved significant improvements in accuracy of temperature and moisture forecasts using such a model. Following the Air Force's lead and a suggestion from the WB director (Cressman 1966), Ron Reap (1968; 1971, 1972) developed a trajectory model based on the horizontal and vertical wind forecasts from the six-layer primitive equation model (Shuman and Hovermale 1968). This NMC model was running at 1 Bedient mesh length and that is the resolution Reap used except he used topography at ½ Bedient to improve trajectory accuracy. Backward trajectories gave parcel starting points, and the initial values of temperature and dew point were estimated by a method of interpolation from radiosonde data originally developed by Endlich and Mancuso (1968). Reap found the trajectory model gave better 24-h forecasts of temperature at gridpoints than did the PE model (the PE model did not forecast dew point for comparison). This model was developed primarily to aid in severe weather forecasting, and it was implemented on or about December 17, 1968 (WB 1968e). Temperature and dew point were displayed together on one chart on FOFAX (Forecast Office Facsimile Circuit) and the trajectory 24-h net displacement and relative humidity on another chart. Like the wave chart, the NMC curve follower was used to draw the lines (see Fig. 10).

N. Arthur Pore, Marine Branch Chief, 1972.

SAM was running daily and we were collecting the forecasts. TDL's attention had now turned to developing forecasts of specific surface weather elements. Simple generalized wind equations had been developed and implemented earlier. Of prime importance was the probability of precipitation (PoP)[12] and the conditional

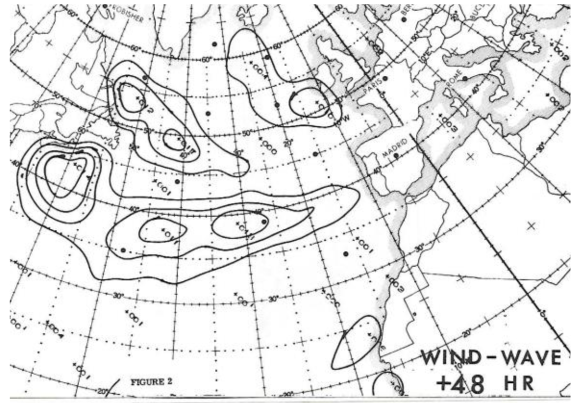


Fig. 8. Example wind wave chart. Contours are at 3-ft intervals, and the maximum is printed in the center of closed contours. (From WB 1968c.)

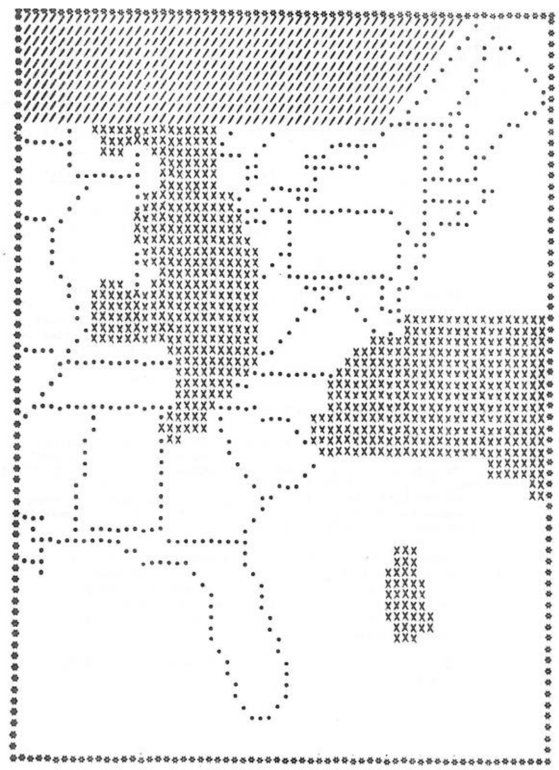


Fig. 9. The precipitation chart produced by SAM starting October 1, 1968. (From WB 1968d.)

[12] The Weather Bureau definition of probability of precipitation is "The probability of ≥ 0.01 inch of precipitation at a point over a stated period of time." The acronym PoP was not used initially when the probability forecasting program was started on an experimental basis in 1965 (Cressman 1965). The earliest use of the acronym may have been in in WB (1969a**)** dated Feb. 3, 1969 based on material supplied by Dale Lowry**.** It also appeared in ESSA News (1969) on February 14 and Glahn and Lowry (1969b) in October that same year. After that, it slowly became more common.

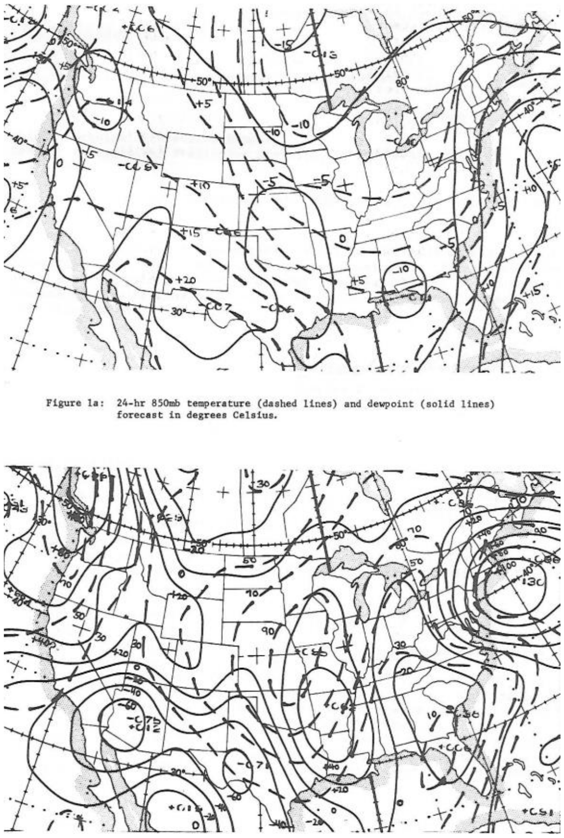


Fig. 10. Example temperature (dashed) and dew point (top) and 24-h net vertical displacement (solid) and relative humidity (bottom) from the trajectory model. (From WB 1968e.)

(on precipitation occurring) probability of frozen precipitation [PoFP(P)]. The yes/no precipitation product shown in Fig. 9 was not statistically derived, but was a representation of Sd directly out of the model. Other studies had related precipitation occurrence to (only) observed variables and those predictors were used in making the forecasts (the classical method), or to observed variables, and forecasts of those variables were used in making the forecasts (the PP method).

By this time, output of the PE model was being archived and a sample collected, so we developed regression equations to predict PoP based on variables forecast by both SAM and the PE model (Glahn and Lowry 1969b). This was the first use of the acronym MOS. Equations were developed for winter and for summer, but one of the sets was used for both the morning and afternoon runs.

The output from SAM was modified a number of times over the following couple of years. On February 12, 1969, the content and format of the transmission was revised to include sea level pressure and 1000-500 mb thickness, probability of precipitation, and conditional probability of frozen precipitation forecasts (see WB 1969a**)**. The sea level pressures were direct output from the SAM model. The probability of precipitation was provided by a new set of REEP regression equations derived from the output of both the SAM and the PE model. The conditional probability of frozen precipitation equations were also derived from PE and SAM output.

The probability of precipitation forecasts were based on regression where the predictors were picked by screening from a large set. The predictors were cumulative binary from SAM and the PE model. Climatology as categories of the relative frequency of precipitation in 6- and 12-h periods was also included. The first 6-h equation is shown in Fig. 11. Data from 80 stations were combined into generalized equations for one 12-h and two 6-h periods. Only one set of equations was derived that was used for both cycles.[13] Noticeably, no climatological variables were selected. It was clear that moisture relating to this specific time was more important than some broad brush climatological value.

The conditional probability of frozen precipitation equations (conditional on precipitation occurring) were also derived by screening regression. One equation was for the beginning of the first 6-h period (1200 UTC for the 0700 UTC run, shown in Fig. 12), and the other for the end of the 12-h period (0000 UTC). The cases in the developmental sample included only those when precipitation occurred. The climatology predictor was replaced by a predictor based on the work of Wagner (1957) which related probability of frozen precipitation to 1000-500 mb thickness. This derived predictor was chosen first, and there was only slight improvement by including temperature binaries.

The forecasts were transmitted in graphical form as a 4-panel chart on FOFAX, as shown in Fig. 13) for the 0700 UTC start time:

<u>Upper left panel</u>—Isopleths of PoP for the 12-h period 1200-0000 UTC as solid lines and sea level pressure as dashed lines valid at 1200 UTC.
<u>Upper right panel</u>—Isopleths of 1000-500 mb thickness as solid lines with sea level pressure as dashed lines valid at 1800 UTC.
<u>Lower left panel</u>—Isopleths of PoP for the first 6-h period as solid lines with PoFP(P) depicted as dashed lines valid at 1200 UTC.
<u>Lower right panel</u>—Isopleths of PoP for the second 6-h period as solid lines with PoFP(P) depicted as dashed lines valid at 0000 UTC.

[13] WB (1969a) gives one equation for 6-h Pop 1200-1800 UTC, another for 1800-0000 UTC, and another for the 12-h period 1200-0000 UTC, each with coefficients. The statement is made that infers each equation can be used for the other cycle. Evidently, the developmental system was not yet efficient enough to develop a different set of equations for each cycle.

A similar chart was transmitted for the 1900 UTC start time. As with the initial implementation, one set of regression equations was used for both start (cycle) times.

a. 6-hr PoP 1200-1800Z:

	Predictor	Contribution to PoP (%)	Cumulative Reduction of Variance
1)	Constant	34.68	
2)	SAM $S_d \leq 0$ at 15Z	15.85	.3388
3)	PE 12 hr precipitation $\leq$ .10 at 00Z	-13.71	.3999
4)	SAM $S_d \leq -7$ at 15Z	13.54	.4155
5)	SAM $S_d \leq 60$ at 18Z	6.652	.4254
6)	SAM SLP $\leq$ 1010 at 18Z	6.061	.4305
7)	PE 6 hr precipitation $\leq$ .20 at 18Z	-12.12	.4343
8)	PE mean relative humidity $\leq$ 70% at 18Z	-3.478	.4377
9)	SAM $S_d \leq -15$ at 15Z	8.743	.4390
10)	PE 12 hr precipitation = 0 at 00Z	-5.017	.4402
11)	SAM $S_d \leq 105$ at 15Z	3.851	.4414

(S_d is saturation deficit in meters and SLP is sea level pressure in millibars.) The range of forecast probabilities is 0% to 89%. The equation may be applied for the 0000-0600Z period by using the appropriate predictors at the corresponding times during that period.

Fig. 11. The PoP prediction equation for the beginning of the first 6-h period used for both run times. Each predictor was cumulative binary and derived from SAM saturation deficit (Sd), prior 12-h observed precipitation, SAM SLP, PE precipitation amount, or PE mean relative humidity. (From WB 1969a.)

This fax depiction lasted nearly a year until December 8, 1970, when some changes were instituted (see NWS 1970c for details).

The wintertime PoP equations described above were replaced by summertime equations on April 1, 1969, but the PoFP(P) equations for the winter remained in use. WB 1969b contains the caveat, “Most of the time the isopleths of PoFP(P) will be well to the north of the forecast area. When they appear, the forecasts may be less reliable than they were during the period for which they were derived.” At this time, the estimates of surface wind were dropped from the SAM teletype bulletin. Wind equations had now been derived for both seasons, and were given in TPB 23 (WB 1969b) so that they could be used on station applied to the SAM geostrophic wind, which was still transmitted.

PoP and PoFP(P) equations were rederived with another year of data. Also, seasonal PoFP(P) was added to the service “C” teletype bulletin (see Fig. 14). Wind equations were not rederived, and it was suggested the previous ones be continued for use on-station (WB 1969c).

On approximately March 18, 1970, the input to the PP max/min equations was switched to the PE model from the previously used barotropic and Reed models. Also, reported max and min temperatures in the equations were now 6 h later than previous; this was possible because waiting for the PE delayed the run by about an hour. The equations were not changed (WB 1970b). This is an advantage of the PP technique—a better model comes along, use it and the max/min forecasts should improve. And verification showed that they did improve to about the accuracy of the subjectively produced temperatures by NMC’s Analysis and Forecast Division (AFD). Following that verification, the objective max/min temperatures replaced the previous NAFAX product produced by AFD on April 1, 1970. The fax chart had values plotted for each of 131 U.S. cities and seven cities in Canada. The product went directly from the CDC computer to the facsimile circuit, thereby saving staff hours (WB 1970c). This was the first time a statistically derived product replaced a subjectively produced one at NMC. Isotherms were not included but were added on October 19, 1970 (NWS 1970b), and at that same time changes were made in the teletype bulletin. A scheme was devised and implemented to indicate missing or likely erroneous forecasts. The isotherms were formed by first finding values at gridpoints by the Bergthorssen and Doos (1955) analysis scheme,[14] although it was not identified as such, then the contours were drawn by interpolating biquadratically between the

a. Beginning of forecast period (1200Z or 0000Z)

	Predictor	Contribution to PoFP(P)(%)	Cumulative Reduction of Variance
1)	Constant	-4.434	
2)	Wagner index-SAM/PE 1000-500 mb thickness	0 to 71.10	.7315
3)	PE $T_o \leq 2°C$	16.49	.7598
4)	PE $T_o \leq 6°C$	11.08	.7636
5)	PE $T_o \leq -1°C$	9.167	.7656

(T_o is the forecast 1000 mb temperature.)
The range of possible forecast probabilities is -4% to 103%.

Fig. 12. The PoFP(P) equation for the beginning of the forecast period. The predictors are the Wagner Index applied to the SAM/PE thickness and the PE 1000-mb temperature. (From WB 1969a.)

The PP max/min temperatures replaced the NMC subjective product in April 1970.

[14] This analysis process had been used in NMC for years (Cressman 1959). It is not stated whether this was a new coding of the process, or whether NMC’s code was used. It is likely the code was new because the interpolation routine was identified as NMC’s, while no such attribution was made for the analysis code.

gridpoints**.** Monitoring of the forecasts showed that record breaking temperatures were sometimes forecast because of bad input data, so a process was put in

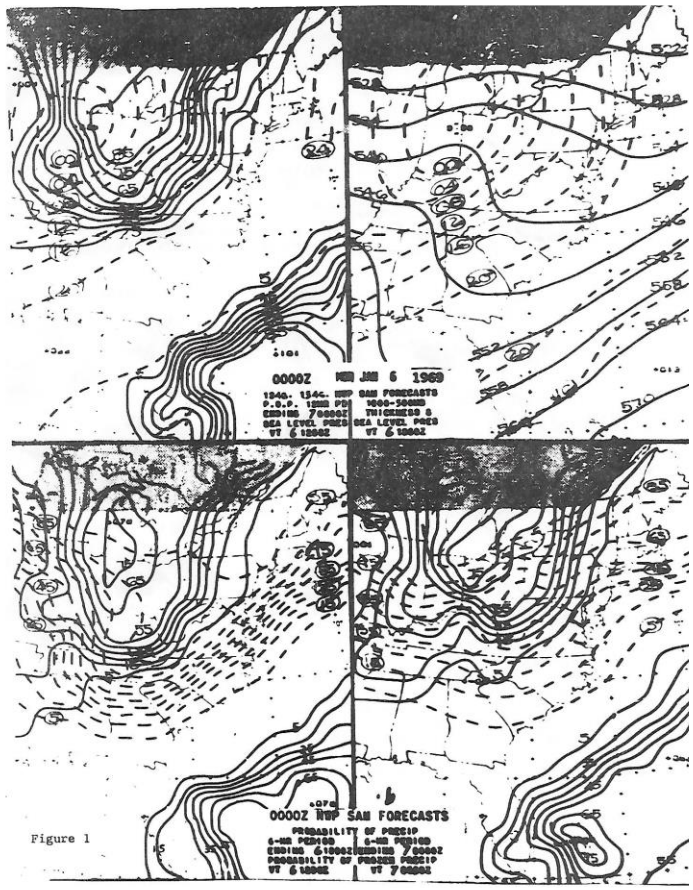


Fig. 13. Four panel fax chart transmitted with sea level pressure, 1000-500 mb thickness, PoP, and PoFP(P) (see text for details). (From WB 1969a.)

place on approximately March 8, 1971, to constrain the forecasts to near the daily record values (see NWS (1970d) for the exact procedure[15]). A list of stations having truncated forecasts was provided as part of the teletype bulletin.

On October 29, 1969, a "laminated moisture feature" was introduced into the PE model. From WB (1970a):

> "Verification figures through September 1, 1969, from TDL and NMC show the mean relative humidities and precipitation amounts forecast by the laminated PE model to show (sic) a strong bias on the dry side over the eastern United States. This strong bias may or may not hold true for other areas.
>
> . "The effect of the laminated moisture PE predictors on the machine produced PoP forecasts, of course, is to make them drier than desired. NMC is continuing to verify the products and this may or may not lead to a future adjustment in the PE model moisture. In the meantime, we feel it will be advantageous to revise the program by dropping the PE predictors from the objective forecast procedure and carrying only SAM predictors. . . They were introduced into the operational program at 1200 GMT on December 5, 1969."

It is noted that the new equations have lower reductions of variance and lower range of forecasts than the ones that included the PE model, showing the PE model was initially important before the change to the way the moisture was handled.

This hurried change indicates that changes were made in the primary NWP model being run at NMC without testing what affect they would have on a final statistical product. It also indicates the TDL statistical system was now efficient enough that new equations could be generated for both cycle times rather quickly and put into operations.

Changes were made to the PE model on March 19, 1970, but a half month of verification still showed a pronounced bias, so PoP and PoFP(P) equations for the summer continued to not contain PE model predictors (WB 1970d). PoFP(P) forecasts were removed from the teletype bulletin on May 15 to return on October 1.

Other changes to the PE were made that it was thought would eliminate the PE dryness, so equations were implemented on September 30, 1970, that contained both SAM and PE predictors (NWS 1970a). Then on April 1, 1971, summertime equations based on 3 years of data (1967-1969) were implemented. The 1970 data were not used because of the PE model dryness (NWS 1971a). The sample had now grown to respectable size.

WB (1970a) also indicates that the SAM (generalized) statistical forecasts were made on the grid, for the curve follower to use, and then the station values were arrived at by interpolation. It was recognized that the interpolated values might not be exactly what would be produced if the equations were applied directly to station locations, but it was believed ". . . .the interpolation procedure neither helps nor hurts the forecasts, on the average" (op. cit.).

[15] TPB 59 (NWS 1970d) indicates the large amount of work the EFD did to make this adjustment possible. This shows the tight connection of Klein's max/min forecasts to his previous work at EFD.

```
FOUS KWBC 240800
SAM FORECASTS
     15Z        18Z        21Z        00Z      POP 12 6 6 POFP B E
CAR 202 2421  174 2418  149 2513  129 2709   030 13 26  030 035

In the example, the probability of  frozen precipitation at Caribou, Maine
if it does precipitate at all, is 30% at 1200Z and 35% at 2400Z.  Caribou,
of course, is only the first station in a 79 station bulletin.  The number
of stations and the order of transmission remain unchanged.
```

Fig. 14. Format of SAM bulletin, explaining the new conditional frozen precipitation probabilities. Before them are the 12-h and two 6-h PoPs, the geostrophic wind ddss (e.g., 2421) and the saturation deficit (e.g., 202). (From WB 1969c.)

Both PoP and PoFP(P) equations for the next winter were again rederived. The PoP equations were based on 3 years of data for the daytime run and 2 years for the nighttime run. The PoFP(P) equations were based on 4 years of data for the daytime run and 3 years for the nighttime run. The wind equations were the same as used the previous winter (NWS 1971c).

In the meantime, the geostrophic winds were replaced with surface winds in the teletype bulletin. Previously, the surface winds were computed by very simple generalized operator equations. We thought that enough data had been collected that robust single station equations could be developed, so we did a test on 10 stations. Equations were developed for each component of the wind and for wind speed. The predictors screened were the SAM geostrophic winds and the initial observed winds on summertime data of 1967 and 1968. Forecasts were made for each day of April and May 1969, and compared to wind forecasts in the NWS terminal forecasts (FT). The accuracy of the MOS equations was as good as or better than the FTs

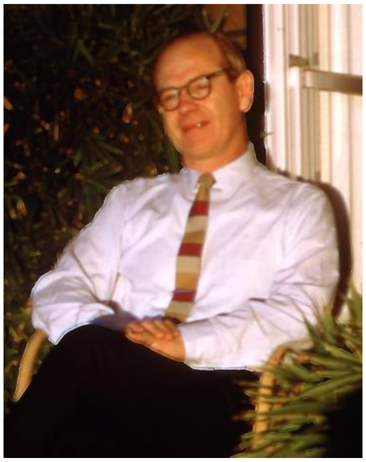
> The first implementation of single station MOS equations was surface wind direction and speed in 1970.

(Glahn 1970b). Therefore, single station equations were implemented on or about July 1, 1970 (WB 1970b).

The NMC models were still running at 1 Bedient. We experimented with a ½-Bedient barotropic model and 500-mb analysis, thinking the combination at that resolution might improve SAM. However, testing indicated little or no reason to implement this higher resolution option (Bermowitz, 1971). Also, in that regard, Jim Howcroft (1971) was in the process of tailoring the PE model to run on a limited area at ½-Bedient mesh length.

The 3-dimensional trajectory model implemented in 1968 was improved with the addition of the effects of air-sea interactions within the oceanic boundary layer (Reap 1971). This change became operational on or about June 1, 1971 (NWS 1971b).

Throughout the period 1968 to 1971, the statistical products consisted of the nationwide PP max/min temperatures (for years thereafter and continuing today, called "the Klein Temperatures"),[16] the trajectory forecasts of temperature and dew point, ocean wind waves and swell, and SAM forecasts of wind, PoP, and PoFP(P). The PP temperatures, designed, fostered, and documented by Bill Klein, were developed primarily by Frank Lewis, Fred Marshall, George Casely, and Gordon Hammons located at FOB4 in Suitland, Maryland. The trajectory forecasts were primarily the work of Ron Reap. The waves and swell were developed and implemented by the marine group; contributors were Art Pore, William Richardson, and Herman Perrotti. The SAM team consisted primarily of myself, Dale Lowry, George Hollenbaugh, Elizabeth Booth, Jackie Hughes, and Evelyn Boston.



Frank Lewis, developer and branch chief at TDL.

These products were updated either as improvements to the process of producing the forecasts, improving or augmenting the dissemination media or formats, or redeveloping equations as more data accumulated.

This was a productive period, TDL having gone from no statistically derived products in 1968 to several in 1971. Just as importantly, the process of implemen-

16 Klein temperatures are still being used in the Climate Prediction Center. They were run by TDL for many years. At some point, the "leapfrogging temperature input (using the previous forecast as input) was changed from PP forecasts to MOS forecasts. This reduced the variance of the longer range forecasts and increased accuracy. Interestingly, the PP forecasts had MOS input! Later, the running was turned over to the Climate Prediction Center (Paul Dallavalle, email dated 1/17/18).

tation had been established with the introduction of the Technical Procedures Bulletins to announce changes of dissemination of products from NMC and the formation of CAFTI to recommend changes and to insist on verification and documentation before implementation. Charlie Roberts was the moving force behind the TPBs. Merritt Techter instigated CAFTI, and Bill Klein bulldogged its formation and operation at Techter's behest. NMC was responsible for the daily running of the products, but the software was written by the developers, members of TDL.[17]

It was also a stable period. The CDC 6600 was being used the whole time, so no expensive computer conversions were necessary. We were building and documenting our development system along with developing and implementing products. It became clear the implementation and development software needed to be coordinated and actually be the same insofar as possible, and we began working toward that concept.

During this period, I also experimented with another form of postprocessing: The computer worded forecast (Glahn 1970c, 1970d). Because the final surface weather forecasts provided to the public were usually in a worded message, why should we not provide a stab at what that would be? Of course, the input should be the official NWS forecasts, but these were not handily available in the quantity and form needed, so we used statistically developed forecasts as input. I also wanted to demonstrate that it was possible to turn out a forecast in essentially the form being currently issued completely by computer. With the data we had in the SAM project, we developed regression equations for four stations for estimating surface wind, cloudiness, maximum temperature, PoP, and PoFP(P). The predictors were from SAM (0700 UTC cycle) and the PE model (0000 UTC cycle). SAM supported only the today period, so that is what we demonstrated.

The format I chose to emulate was what we could hear on the telephone. The weather element deemed most important was put first in the forecast; otherwise, the order of the elements depended somewhat on the forecasts themselves and how they best fit together. An "important" or "significant" element was defined to be: Wind of 20 mph or greater, probability of precipitation of 35% or greater, maximum temperature 10°F above or below yesterday's maximum, or maximum temperature near yesterday's maximum but 8°F or more below the climatological maximum.

The ordering of the elements was the most challenging. The actual words, phrases, and punctuation that were arranged into the forecast are shown in Figs. 15 and 16. Figure 17 shows three examples. The lead-in is, of course, arbitrary and redundant. George Cressman (1970), in discussing the published examples, said, ". . . they may prove useful to the forecaster after further improvement." Obviously, he was thinking of guidance, not a final product. Yet, essentially all such WB forecasts are today produced by computer from digital forecasts.

If a forecast could be produced for the today period, it could also be produced for tonight and tomorrow, the extent of the public forecast at the time. Later, official forecasts were in a format where periods could be combined, which was even more of a challenge (Glahn 1978a, 1978b).

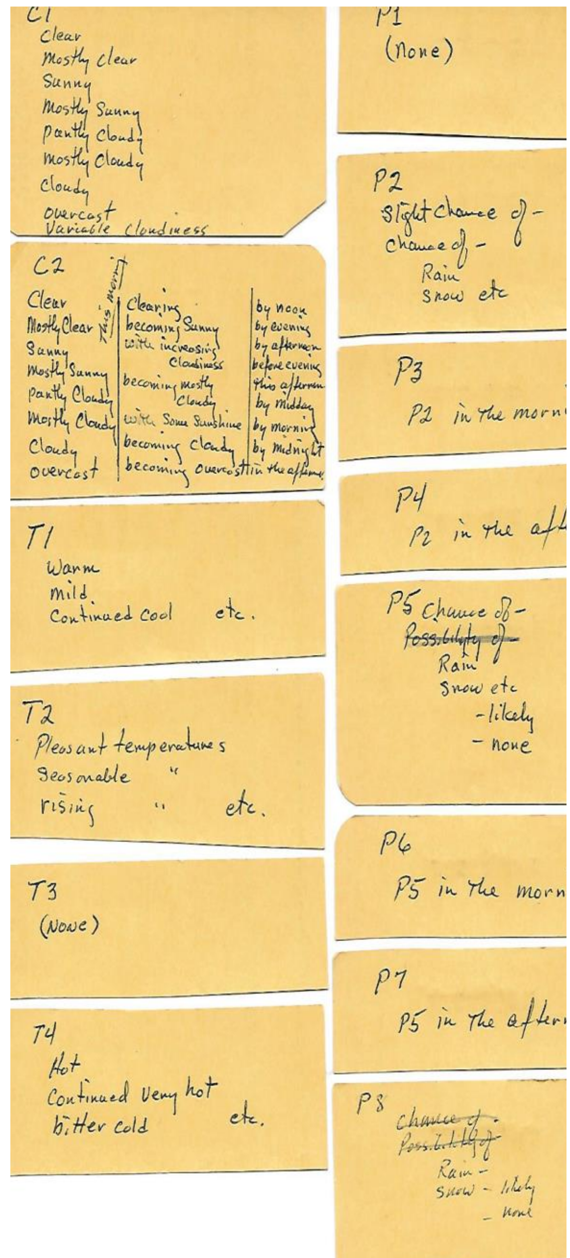


Fig. 15. Cloud and temperature (left) and precipitation words and phrases used in building computer worded forecasts.

[17] The programs made use of NMC data and "system" routines.

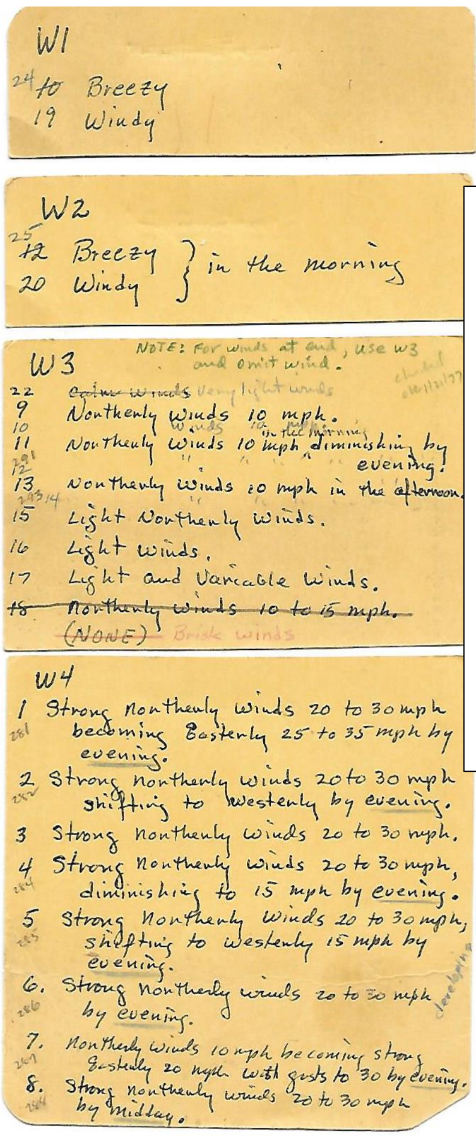
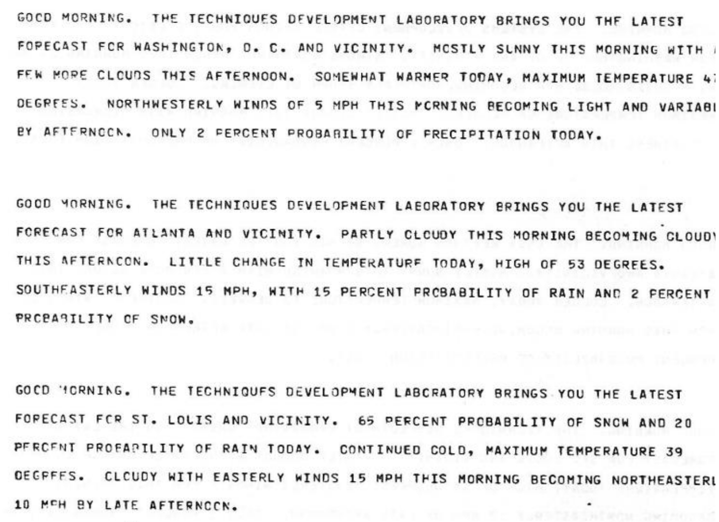


Fig. 16. Wind words and phrases used in building computer worded forecasts.

There were many improvements that could be made to our products. The max/min temperatures were for only 130 specific sites, albeit there was a graphic from which forecasts for other points could be found. More importantly, SAM MOS forecasts were for only the eastern part of the United States. Some of us believed MOS was the way of the future, and had been collecting data and forecasts from the PE and trajectory models over the CONUS since October 1969. So, on January 1, 1972, the first CONUS MOS product was implemented, and took the place of the formerly subjectively prepared product (NWS 1971d). It was based on both the PE and trajectory models.

` As stated earlier, a breakthrough had occurred when a statistical product replaced one that had been previously prepared by forecasters. This occurred on April 1, 1970, when the PP temperatures replaced the NAFAX NMC product. Now, a similar breakthrough occurred on January 1, 1972, when the MOS PoP forecasts replaced the manual product on NAFAX (NWS 1971d). Four panels, each of 12-h periods, covered the periods 12-24, 24-36, 36-48 h, and 48-60 h (Fig. 18).


GOOD MORNING. THE TECHNIQUES DEVELOPMENT LABORATORY BRINGS YOU THE LATEST FORECAST FOR WASHINGTON, D. C. AND VICINITY. MOSTLY SUNNY THIS MORNING WITH FEW MORE CLOUDS THIS AFTERNOON. SOMEWHAT WARMER TODAY, MAXIMUM TEMPERATURE 4 DEGREES. NORTHWESTERLY WINDS OF 5 MPH THIS MORNING BECOMING LIGHT AND VARIAB BY AFTERNOON. ONLY 2 PERCENT PROBABILITY OF PRECIPITATION TODAY.

GOOD MORNING. THE TECHNIQUES DEVELOPMENT LABORATORY BRINGS YOU THE LATEST FORECAST FOR ATLANTA AND VICINITY. PARTLY CLOUDY THIS MORNING BECOMING CLOUDY THIS AFTERNOON. LITTLE CHANGE IN TEMPERATURE TODAY, HIGH OF 53 DEGREES. SOUTHEASTERLY WINDS 15 MPH, WITH 15 PERCENT PROBABILITY OF RAIN AND 2 PERCENT PROBABILITY OF SNOW.

GOOD MORNING. THE TECHNIQUES DEVELOPMENT LABORATORY BRINGS YOU THE LATEST FORECAST FOR ST. LOUIS AND VICINITY. 65 PERCENT PROBABILITY OF SNOW AND 20 PERCENT PROBABILITY OF RAIN TODAY. CONTINUED COLD, MAXIMUM TEMPERATURE 39 DEGREES. CLOUDY WITH EASTERLY WINDS 15 MPH THIS MORNING BECOMING NORTHEASTERL 10 MPH BY LATE AFTERNOON.

Fig. 17. Examples of the computer worded forecast.

The PoP 2-season (October-March and April-September) equations were each based on one season of developmental data ending October 1970. We developed generalized equations over regions. The regions were determined by combining stations that had similar relative frequencies of precipitation observed when the forecast PE model mean relative humidity was $\geq$ 75%. Over a 1-year test period, the PEATMOS[18] PoPs were compared to local forecasts and to those produced by NMC. The measure of skill was the Brier skill score, where the baseline climatology was relative frequency by month and station determined over a 15-year sample. PEATMOS beat NMC, and except for the first period was about as good as the local forecasts. I credit Harlan Saylor, a prominent forecaster and manager at NMC, for recognizing the quality of MOS and the potential saving of NMC resources in replacing the manually-produced product. Three years of operations of this product are detailed in Lowry and Glahn (1976).

MOS PoP forecasts replaced the NMC subjective product in January 1972.

[18] The acronym PEATMOS stood for PE (model) and trajectory (model) MOS. This is the only time we distinguished a MOS product by the predictors used.

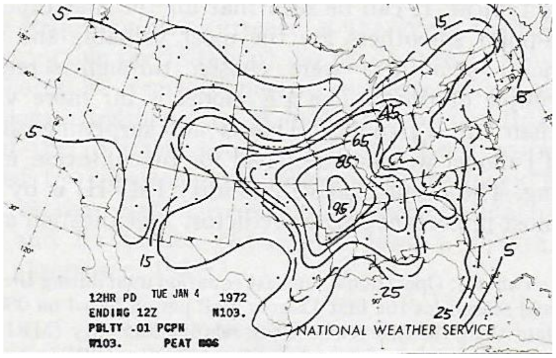


Fig. 18. One panel from the 4-panel PoP fax chart for Jan. 4, 1972. (From Lowry and Glahn 1976.)

`. SUMMARY

The term "postprocessing" we use today to apply to any processing of NWP output did not come into widespread use until somewhere around 2000. Prior to that it was called statistical forecasting, interpretation of NWP products, or some similar term. Whatever the name, the relevant statistical techniques were put in place by a couple of decades of work where NWP was not involved—the so called "classical period." Major contributors to this period were Glenn Brier, Jack Thompson, and Roger Allen. Computers were not in use, so the methods were generally some form of scatter diagrams. Iver Lund and Irv Gringorten of the Air Force Cambridge Research Laboratory started to bridge the gap to computers, and were strong contributors to technique development. The group at The Travelers Research center, foremost being Bob Miller, made full use of computers as early as the late 1950's, and brought into meteorology screening regression, REEP, and multiple discriminant analysis. These had a profound influence on the way PP and MOS developed in the WB.

Bill Klein, working for EFD of the WB had been studying the relationship of temperature to upper air geopotential heights, so it was logical that he apply those relationships to output of NWP, which he did about as soon as useful NWP forecasts were available. This came to be called the perfect prog technique, because in implementation it assumed the progs (prognoses) were perfect. This was a useful product, and was able to be brought to market quickly. It worked reasonably well for temperature, because forecasters and users liked to see the variability of forecasts at the longer projections. However, the method did not work well for probability forecasting, where there is emphasis on reliability.

It's impossible to know when building the relationships between a variable to be forecast and actual NWP forecasts was conceived, but it was not feasible to do so in the early years of NWP because (1) the operational model was undergoing rapid change, and (2) there was no infrastructure to support such an endeavor, either from the management, development, or implementation perspectives. Faced with that situation, we built a model so that the concept could be tested. The resulting SAM model ran at the times to be helpful to forecasters in the eastern U.S. The model was run for a number of years, a stable sample archived, a statistical system built, relationships derived, and the forecasts made and disseminated. The skill was favorable when compared to existing operational forecasts. This proved the MOS concept and that TDL could produce a software system that could run reliably at NMC. By 1972, two of TDL's statistical forecasts replaced NMC's forecaster-produced graphic forecasts of the same variables.

During this period, largely due to TDL's products being developed and needing to be implemented, the process was put into place for review of products before implementing and for documenting them for users. For review, CAFTI was formed with representation by the development (TDL), implementation (NMC), and management (Office of Meteorology) arms of the WB. For documentation, the Technical Procedures Bulletin series was put into place, and CAFTI assured each system implemented had a TPB. Merritt Techter, Director of the Office of Systems Development, the parent of TDL, was instrumental in bringing CAFTI about. Charlie Roberts was the prime mover in establishing the TPB series.

By 1972, TDL was interpreting the PE model, as well as SAM, in terms of local weather and had built and documented a software system so that the process could continue.

Acknowledgments. Thanks to all those who painstakingly documented their work in the early years of TDL's statistical forecasting activities. I am also indebted to those who wrote and maintained the Technical Procedures Bulletins from which considerable of this history was taken. This paper is the responsibility of the author and does not necessarily represent the views of the NWS or any other governmental agency.

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