DEPARTMENT OF THE ARMY CORPS OF ENGINEERS

BEACH EROSION BOARD
OFFICE OF THE CHIEF OF ENGINEERS

DISCUSSION OF TECHNICAL MEMORANDUM NO. 120

"THE PREDICTION OF HURRICANE STORM-TIDES IN NEW YORK BAY"

(AND CLOSURE BY AUTHOR)

TECHNICAL MEMORANDUM NO.120-A

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A D D E N D UUM

TECHNICAL MEMORANDUM NO. 120-A "Discussion of Technical Memorandum

No. 120 'The Prediction of Hurricane Storm-Tides in New York Bay' "

BEACH EROSION BOARD, U. S. ARMY CORPS OF ENGINEERS

Page 9, Figure 3 -- The dotted curves in diagrams (d) and (e) of this figure result from surge calculations by Harris' modified version of Wilson's prediction formula.

TECHNICAL MEMORANDUM NO. 120-A

APRIL 1961

FOREWORD

In view of Public Law 71, 84th Congress which requires the Corps of Engineers to study possible hurricane protection measures for coastal areas susceptible to damages from hurricanes, the Corps has supported a number of research studies to enable more accurate estimation of water levels associated with particular design storms. One of the more recent of these studies was directed primarily toward determination of hurricane water levels for a variety of possible hurricane occurrences for the New York Harbor area. A portion of the results of this work has been published as Technical Memorandum No. 120 of the Beach Erosion Board "The Prediction of Hurricane Storm Tides in New York Bay" by Dr. Basil W. Wilson of the Agricultural and Mechanical College of Texas.

The Corps of Engineers' interest in hurricane water levels is shared by the U. S. Weather Bureau, although with a slightly different end in view. Where the Corps' primary interest is in determining water levels for particular locations which may be used for design criteria for hurricane protection, the Weather Bureau has the responsibility of providing prediction of water levels associated with all current storms in order to provide daily (or more frequent) warning to areas which may be affected by an on-coming hurricane, enabling precautionary measures to be taken or evacuation if such should be indicated. As a part of their research in this field, leading to better methods of predicting actual hurricane surge levels, the method derived by Dr. Wilson for New York Bay was reviewed by Mr. D. Lee Harris of the U. S. Weather Bureau with an eye to its possible use in a general prediction scheme. This review has led to a series of comments by Mr. Harris, who is Chief of the Hurricane Surge Unit of the Weather Bureau. Mr. Harris, in his comments, suggests a slightly alternative approach to Dr. Wilson's which he believes might make the method of more general application for Weather Bureau purposes. Dr. Wilson reviewed Mr. Harris' comments and in turn prepared a reply thereto.

Because of the obvious pertinence of these comments and reply to the work discussed in the earlier Beach Erosion Board publication (Technical Memorandum No. 120) this supplement to that report is now being published as Technical Memorandum No. 120A.

Views and conclusions in this report are not necessarily those of the Beach Erosion Board.

This report is published under authority of Public Law 166, 79th Congress, approved July 31, 1945.

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COMMENTS ON PAPER BY Basil W. Wilson, "The Prediction of Hurricane Storm-Tides in New York Bay".

by

D. Lee Harris
U. S. Weather Bureau
September 14, 1960

Professor Wilson's summary of the various effects which may contribute to the abnormal elevation of the sea surface during storms and his summary of wind stress studies are welcome additions to the literature on the subject. The agreement between the observations and predictions shown in his figure 20 supports the notion that the storm tide at the coast can be determined to a useful degree of accuracy from a limited knowledge of the wind and pressure fields offshore. Furthermore these results lend credence to the particular meteorological analysis utilized, and strongly suggest that the statistical analysis included the most important parameters in something near the optimum manner. They do not show that all of the data considered are really necessary or even useful.

Because Wilson's model or a similar one might be useful in preparing operational forecasts, the underlying assumptions of his model have been carefully examined with a view toward their simplification and extension to other areas. Independent data from Hurricane Hazel, October 14-16, 1954 have been used to test Wilson's prediction equation and a simplified equation derived from the data used by Wilson. As a result of this comparison it is concluded that Wilson's model does have a useful degree of predictive skill during the first few hours of the predictions, but considerable doubt is cast on the choice of a period of seven hours and dampening factor. It is shown that predictions of comparable quality can be obtained for the four storms studied by Wilson without making any assumptions about periodicities and that a prediction model which does not require the assumption of a natural period can be expected to produce more satisfactory results for future storms for which periodicity is not known a priori.

WILSON'S MODEL

In articles 8 and 9, Appendices B and C and the Supplementary note on the correlation procedure, Wilson discusses the theory of forced motions of a damped harmonic oscillator and techniques for applying this theory to storm surge predictions. He develops an expression for the storm surge at any time t in the form

$$\eta(t) = a\ \eta(t-\Delta t) - b\ \eta(t - 2\Delta t) + F(t) \qquad (1)$$

where F(t) represents all of the applied forces and the coefficients a and b are given as functions of the period of the oscillations and the dampening coefficients. Wilson chose a period of seven hours and a dampening rate of fifty percent of the amplitude each seven hours.

In article 16 he selects one nearshore and five offshore locations at which the meteorological forces are to be evaluated. He evaluates the time period which will be required for the effects of the meteorological forces at four of the offshore locations to reach the observation point on the assumption that the impulse will travel with the speed of a long gravity wave. At the remaining offshore point he assumes that the impulse will travel as an edge wave. However, in the last paragraph of article 17 he expresses some doubt about the validity of this assumption.

In article 9, he introduces a time lag, denoted by T_0, which according to article 23 "for each storm were ultimately determined largely by trial and error on the basis of optimum correlations." Thus he alters the computed lag time by an amount which will give the best agreement between the observed and computed values for each of the four storms used in this investigation, and appears to have lost the significance of the computation of the lag time on dynamic principles. This procedure led to the remarkable result that for both of the hurricanes considered, the effects of the atmospheric forces near the shore were experienced at the shore more than one hour before the forces were applied.

One of the variables used in his regression analysis is the sum of the atmospheric forces directed parallel to the shore at the nearshore station from the time $t = 0$ to the time for which the prediction is wanted. Since his figure 18 shows that these forces were not identically zero at $t = 0$ for each storm, the choice of the initial time, $t = 0$, will influence all of the subsequent calculations through this summation process.

Figure 20 of Wilson's paper shows the results of two calculations. In one, the observed storm surge elevations at 20 minutes and 40 minutes before prediction time were considered as known values. The results of this calculation for the four storms examined by Wilson and for hurricane Hazel were truly phenomenal. This should have been expected because of the high autocorrelation coefficient cited below.

There is some uncertainty concerning the second calculation. In the legend of the figure he states that it is "based on two initial values of $\eta(t)$." In the text he says that "The more stringent test of the success of the correlation lay in the use of Eq. (45) (the statistical equation derived in his paper to predict the time-histories of water surface elevation for each storm from just two starting values of $\eta_A(t - 1/3)$ and $\eta_A(t - 2/3)$."

These statements imply that two distinct starting values were used to provide data on the water level and its rate of change at the beginning of the computation period. In the next sentence he says, "These starting values were taken identical with the actual storm tide elevation at $t = 0$ for each storm."

The differential equation for the forced motion of a damped harmonic oscillation may be stated in the form:

$$\frac{d^2y}{dt^2} + K\frac{dy}{dt} + S^2y = F(t), \qquad (2)$$

and the general solution may be given in the form:

$$Y = Ae^{-Kt/2}\cos\ \sigma(t - t^*) + \frac{1}{\sigma}\int_0^t F(t')e^{-K/2(t-t')}\sin\sigma(t-t')dt' \qquad (3)$$

$$\sigma^2 = S^2 - (K/2)^2 ,$$

where A and t* are the amplitude and phase of any oscillation which exists at the time $t = 0$, and the integral gives the effects of the disturbing force after the time $t = 0$.

A and t* may be evaluated from two starting values. The starting values taken at $t - 1/3$ and $t - 2/3$ will be equal only if $t^* = 3/6$, or $A = 0$. Setting the two starting values identical is equivalent to adding another subjective restraint, making five in all, the period, dampening factor, log (T), choice of time $t = 0$, and the phase of the disturbance at the time $t = 0$. This latter constraint does not receive any support from the empirical data included in Wilson's figure 20. However this constraint does have one virtue; during the early part of the forecast period it minimizes the error which might result from an incorrect determination of the natural period, as any pair of distinct starting values would result in assigning a larger amplitude to the initial disturbance. The error resulting from this constraint will vanish after a sufficiently long period to allow the term $e^{-Kt/2}$ to become sufficiently near zero. The assumptions made by Wilson set $e^{-Kt/2} = .5$ after 7 hours; .25 after 14 hours and so forth. Most of the verification data shown by Wilson were for the first 14 hours in which this error is present.

If one could choose $t = 0$ at a time when one or both initial values are identically zero, A would be zero and the entire solution would be given by the integral. A detailed analysis of the integral in its general form does not appear to be justified. However, when one remembers that in practice $F(t)$ will always be small at $t = 0$, will rise to a maximum value and then decline to small values once again, it is easy to see that errors resulting from an incorrect value of σ will not be readily apparent for small values of t, however for later values of t the solution obtained for an improper σ, that is an improper period, will ultimately get out of phase with the true solution.

AN ALTERNATE APPROACH

The coefficients a and b which define the period and dampening factor as well as the coefficients used in describing the forcing function can be determined from the regression analysis. This procedure has the advantage of not requiring a subjective evaluation of the period and dampening factor. Further, it is known that any observed periods result from the interaction between the natural periods of the water bodies involved and the periods of the forcing functions. It is not self-evident that the period most readily seen in the short time series of data examined by Wilson is the true natural period, even for those cases examined. The natural period, if determined empirically, should be the one which appears after the elimination of the direct meteorological effects.

Since the computed time lag between the application of the meteorological forces and their observation at the shore is rendered somewhat uncertain by the introduction of the term T_o, and the uncertainty as to whether the impulse from Wilson's location 2 would travel as a long gravity wave or an edge wave, there appears to be some advantage in testing various lags empirically to determine which is most applicable for each point. Such a procedure would permit maximizing the resulting correlation without the strange situation of having the effects of certain forces observed before the forces are applied.

A statistical technique known as "screening" is widely used by meteorologists. A multiple correlation problem, which may involve more than 100 independent variables, is fed to an electronic computer. Several forms of the program, designed to answer slightly different questions are available for use on the IBM 704. In the one most familiar to the writer the output of the program includes the means and standard deviations of all the variables used, the complete correlation matrix and a sequence of regression equations. The first regression equation is the one which gives the highest correlation coefficient when only one independent variable is used. The second is the equation for two independent variables in which the second variable chosen is the one having the highest correlation to the residual which results from using the first equation. The third involves three variables, the first two and the one which correlates most highly with the residual resulting from the use of the first two, and so on, until all variables have been used or some cut-off point is reached at which insufficient improvement results from using additional variables. This basic program has been modified by the writer and his co-workers for a basically different approach to the harmonic oscillator problem. Professor Wilson has been kind enough to supply a tabulation of the forcing functions and storm surge values used in his analysis for re-analysis by the screening program.

If the a and b of equation (1) are determined as the coefficient in a regression equation expressing η as a function of the observed η for the two preceding time increments, and the period is computed from the resulting values of a and b, a period of approximately 6 hours is obtained. If the regression equation is expanded so that the coefficient of several of the forcing functions described by Wilson as well as the a and b of equation (1) are determined from the same regression analysis, the period increases with the amount of meteorological data used. A period of 9 hours was obtained with the maximum amount of meteorological data used in this analysis. Thus the length of the period determined statistically depends on the amount of meteorological data employed in the analysis - a wholly untenable conclusion. The correlation obtained with no meteorological data was .997. This increased to .998 when meteorological data and periodicity effects were both considered. A value of .993 could be obtained from meteorological data alone. Actually a correlation of .894 was obtained between the surge and one of the forcing functions.

The screening analyses also provide data on the optimum time lag to be applied to the observations for each of the locations specified by Wilson. The lags derived by Wilson before applying the correction T_o are the best that could be obtained for δP_A and $\sum F_t$. The optimum lags are from 20 minutes to an hour longer than assumed by Wilson for $[F_r]_4$, $[F_n]_A$, $[F_r]_5$, and $[F_r]_7$. This increase in the lag time is believed to reflect the time required for the water to come into equilibrium with the applied force. Wilson's analysis implied an instantaneous response to the applied forces at the point of application. The screening analysis showed that a lag of no more than 1 hour would have been best for $[F_n]_2$. Wilson used 3 - 2/3 hours. This is believed to imply that the impulse from this location traveled as a long gravity wave rather than as an edge wave. A lag of 1 - 2/3 hours would have been better than the 3 hours suggested by Wilson for $[F_r]_3$. The interpretation of this is more uncertain than for the preceding terms. However, a reference to Wilson's figure 5 suggests that the travel time from location 3 to the observation point may have been shorter over a path somewhat to the east of the radial line used by Wilson and therefore leading over considerably deeper water with much higher (around a factor of 1.4) gravity wave velocities.

A correlation of .954 was obtained between the surge, and the functions δP_A, with a lag of one time period, and $[F_r]_5$ with lags of four and twelve time periods. The standard error of the estimate with this formula is 0.6 ft. The standard error arising from Wilson's formula is not stated in his report. This equation is

$$\eta_A(t) = 2.323114 - 0.012743\, P_A(t-\Upsilon) + 0.000828 \left[F_r\right]_5 (t-12\Upsilon) + 0.000326 \left[F_r\right]_5 (t-4\Upsilon). \quad (4)$$

Υ = 20 minutes

COMPARISON WITH OBSERVED DATA

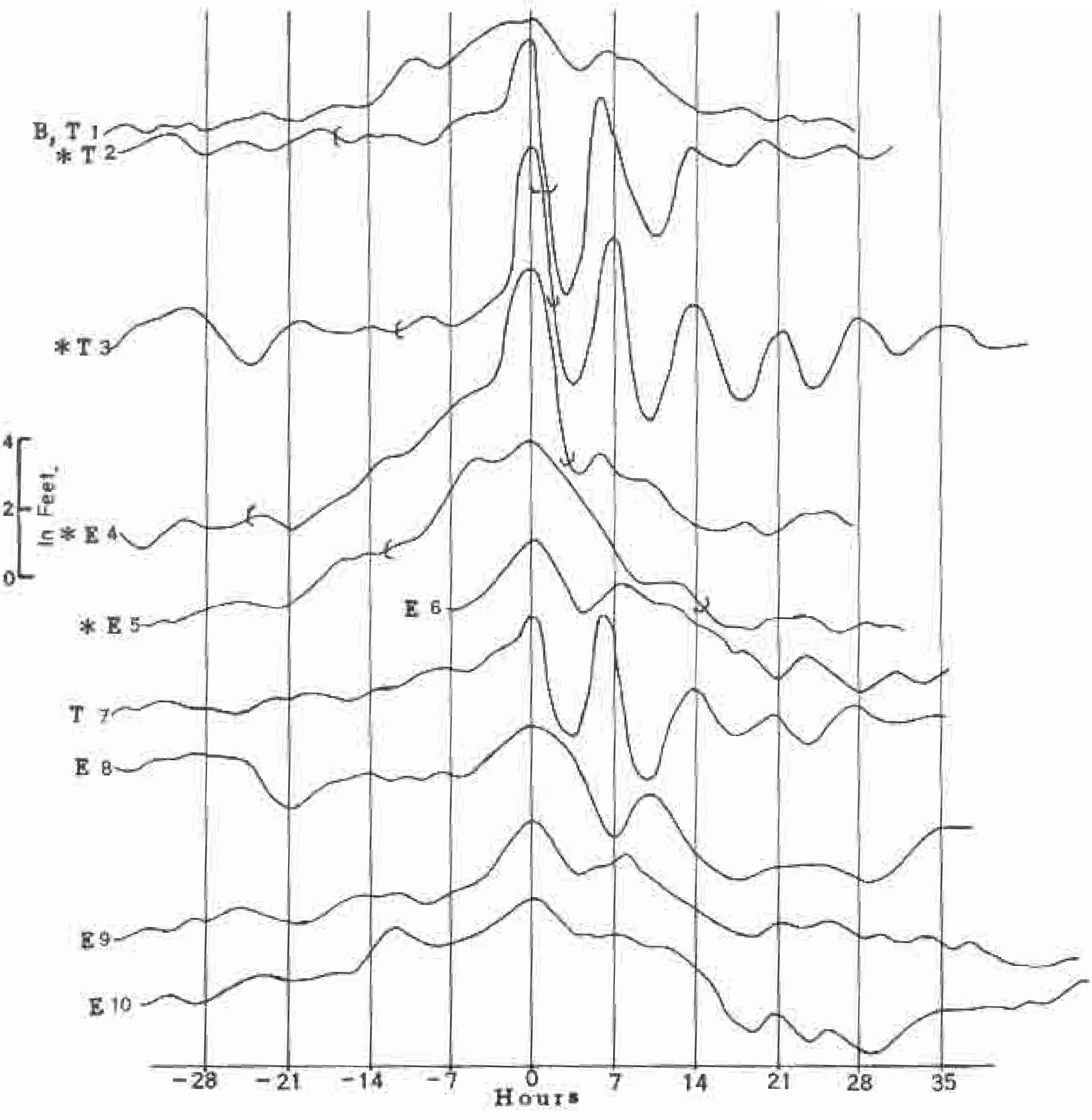
The writer of this note has compiled a large number of storm surge curves for Atlantic and Gulf Coast storms. The storm surge is defined as the difference between the observed tide and that predicted from astronomical considerations and is equivalent to the "storm tide" as used by Wilson. Copies of the storm surge graph for ten of the most prominent storm surges in New York harbor are shown in figure 1. The less prominent surges, not reproduced here give even less support to the idea of a seven hour period and a dampening coefficient of fifty percent each seven hours than the data in this figure.

The observed surge, the surge as computed by Wilson and as computed by the revised model are shown in figure 2. This figure is based on Wilson's data. It appears that both models reproduce the dependent data about equally well.

Computations for Hurricane Hazel, are shown in figure 3. The observed curve is reproduced several times for comparison with various prediction models. The first prediction curve (a) is based on the Wilson model with two distinct starting values. Notice the immediately visible effects of the enforced seven hour periodicity. The second prediction curve (b) is based on two identical starting values, beginning at a time in which the initial value is identically zero. In this case the verification is remarkably good for the first 10 to 12 hours. The third curve (c) was started on a time at which the two starting values were approximately equal but not zero to show the importance of a correct choice for t = 0. The effect of this initial error appears to vanish within two periods as expected.

The fourth curve (d) is based on the revised equation. The revised equation does not contain any term, which provides information about the stage of mean sea level at the beginning of the computation period. Such data are needed for it is known that significant sea level anomalies may exist for several days or weeks in advance of a hurricane (Harris, 1959). These cannot be blamed on the hurricane. This information can be provided by making the mean value of the observations and predictions agree during the first few hours of the prediction period. This is done in the final curve (e).

All of the predictions appear to give useful information from the



LEGENDS

Harbor T indicates storm of tropical origin.
E indicates storm of extra tropical origin.
B indicates data from the Battery, other data are from Sandy Hook.
* indicates storm used by Wilson, the period studied by Wilson is shown by brackets; numbers indicate date of records as given below:

1	19 Sept. 1928	6	14 Dec. 1953
2	21 Sept. 1938*	7	31 Aug. 1954
3	13 Sept, 1944*	8	28 Nov. 1954
4	25 Nov. 1950*	9	14 Dec. 1954
5	7 Nov. 1953*	10	22 Mar. 1955

Figure 1. Storm Surge Curves for New York

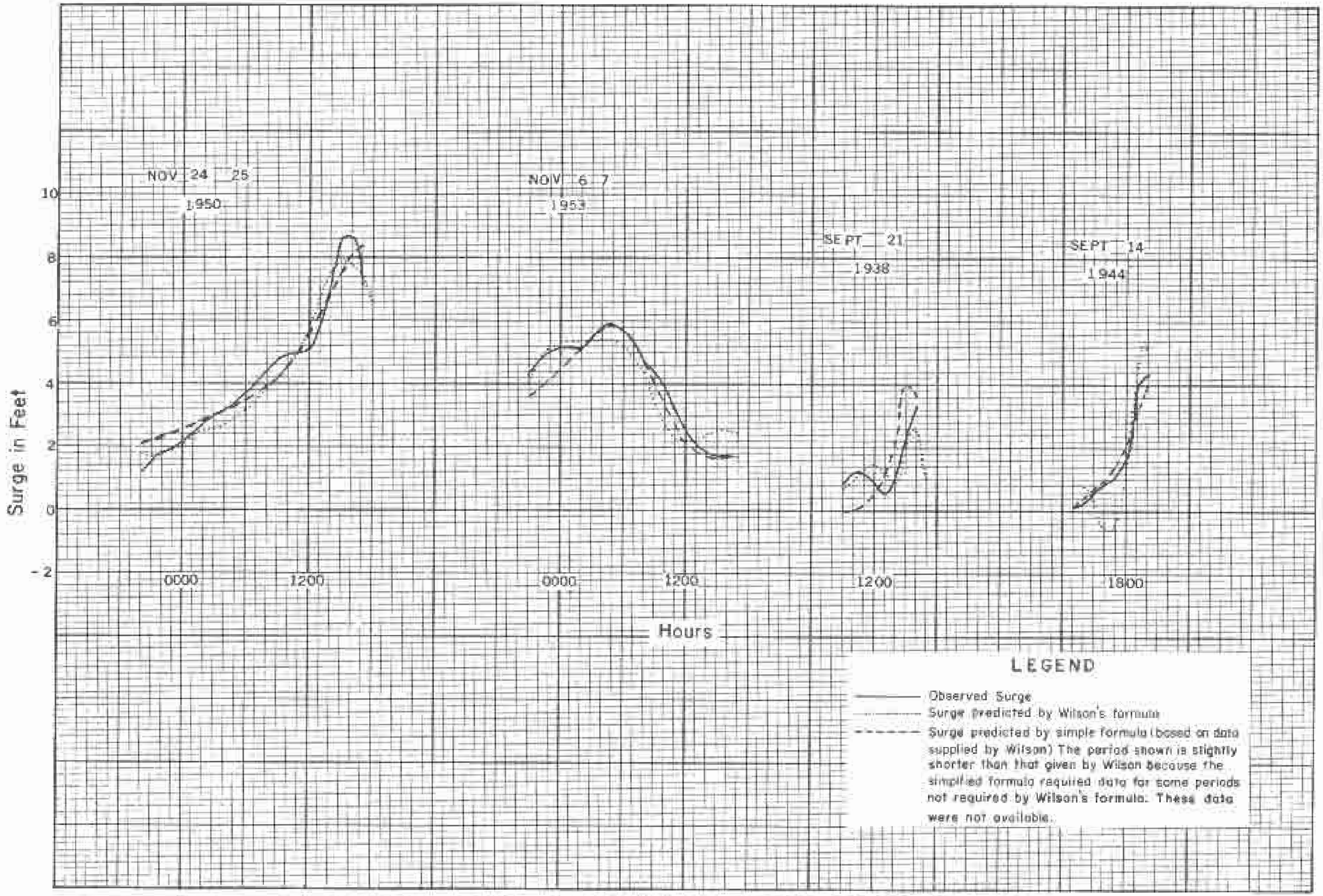


FIGURE 2. OBSERVED & COMPUTED SURGES, SELECTED STORMS

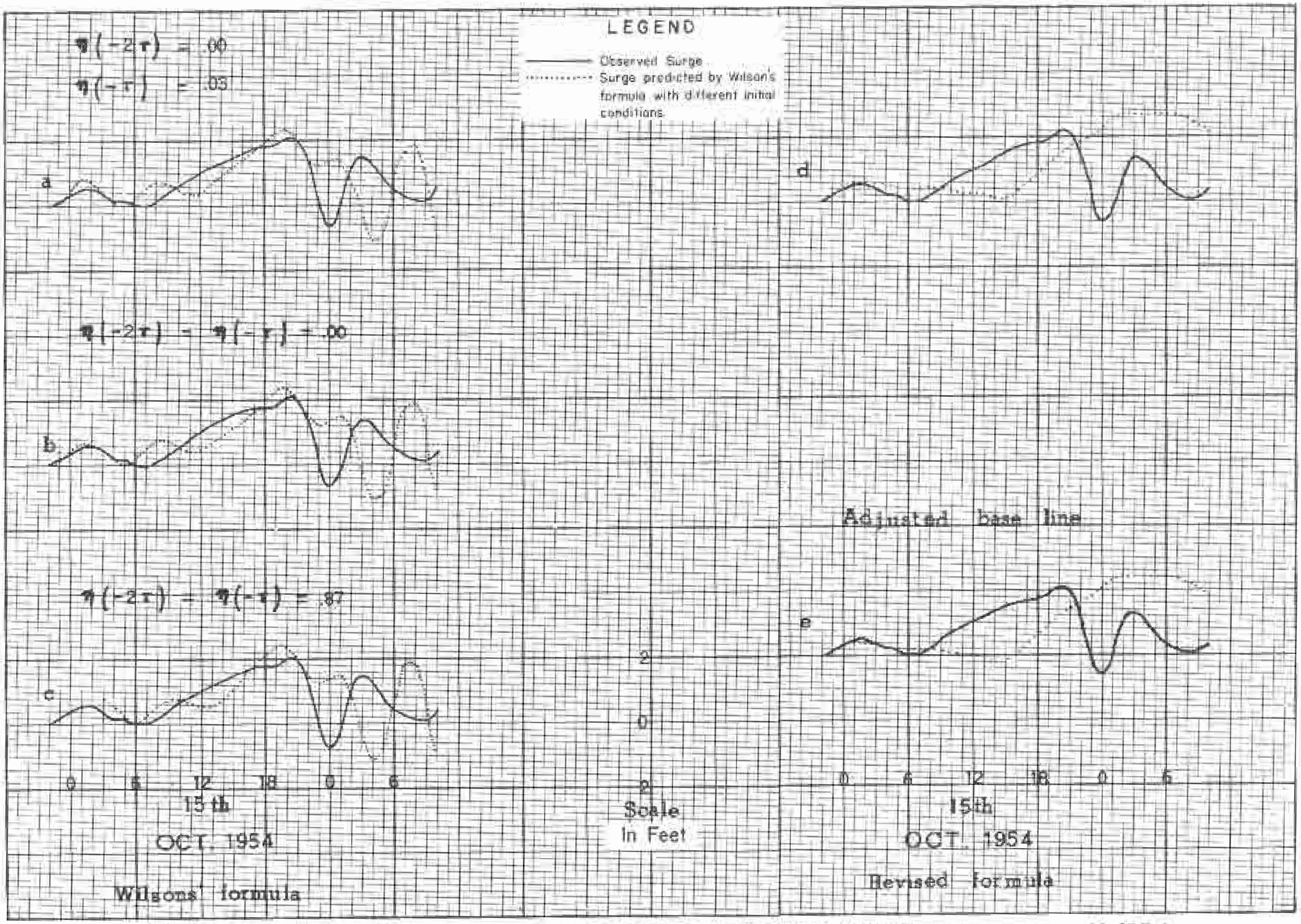


FIGURE 3. OBSERVED & COMPUTED SURGE FOR HURRICANE HAZEL OCTOBER 14-16,1954

beginning of the calculations to a little past the first surge peak. All of them tend to deteriorate shortly after the first peak. The reason for this deterioriation cannot be definitely established. In three of the four cases examined in the empirical analysis, little or no data was considered during the period after the first peak surge, and this deterioriation may result from an attempt to extrapolate the data into a period of the storm not covered in the original determination of the coefficients. In all of the data used in the original analysis, the storm track was over or to the east of New York City so that the highest winds were well to the east of the city. In Hazel, the storm track crossed central New York state and the highest winds were to the west of the city. It will be shown below that this is an important difference. The assumption of an incorrect periodicity and dampening factor may also have contributed to the deterioriation of the Wilson model.

APPLICATION TO HYPOTHETICAL MODELS.

As stated above, the maximum strength of the atmospheric forces in all of the storms analyzed by Wilson was well to the east of the harbor entrance. In the hypothetical storms examined, the storm tracks were altered so as to produce the peak atmospheric forces near the harbor entrance. It is reasonable to expect this distribution of winds to produce the maximum storm surge in the harbor, but the application of the empirical forecast model, derived from data in which the maximum winds were near the eastern end of Long Island needs to be examined rather critically. In studying the independent data, Wilson altered the time lag for each storm in the way which would produce the best fit for the observations. If one chooses the best lag for each station rather than each storm, a more objective procedure, it is found that most of the predictive information furnished by any of the data is furnished by Wilson's location 5. The prediction model described by equation (4) predicts a peak surge of only 2.0 feet for the maximum possible hurricane. This is obviously incorrect and results from the accidental stratification of cases. The wind conditions at location 5 were the most representative of the entire fetch area in the storm for all four of the storms considered by Wilson. Wind conditions at this location are not representative of the entire fetch area for any of the hypothetical storms. The calculated peak surge for the hypothetical storms can be increased by including wind data from points nearer the shore, but only at the expense or a decreased correlation with the original data for equations of a given complexity. The relative importance of the near shore locations can be further increased by taking less efficient lag times at location 5 or omitting this point entirely, but only at the expense of a further decrease in correlation with the original data.

In summary, it is found that the coefficients in the regression equation are very sensitive to the assumed time lags for the various stations and any assumed periods and dampening factor. Slight changes in these factors, which produce only minor changes in the resulting correlations between computed and observed surges may produce major changes in the estimated peak surges for any hypothetical storm, particularly if the wind field, relative to the location for which predictions are desired is significantly different from those used in deriving the coefficients.

STATISTICAL STABILITY

It is difficult to assess the number of degrees of freedom represented by any time series. It is reasonably certain that this is not increased by interpolating between observation points. Distinct storm surge values are available no more frequently than hourly. Therefore not more than one-third of the individual sets of data analyzed by Wilson and reanalyzed here can be regarded as independent. The wind fields were constructed by compositing observations through time and space. A theoretical model depending on only three parameters was used in compositing the hurricane data. Therefore it appears that the number of independent observations for each hurricane cannot be much greater than three. The major tool available for compositing the data for the extratropical storms is the synoptic chart which is prepared each six hours, and therefore not more than five such charts were available for each extratropical storm. This would imply a total of about 16 degrees of freedom for the entire set of data. Of course, one may expect the number of degrees of freedom on each chart to exceed one, because of the areal representation of data, but then the entire science of weather forecasting is based on the inter-correlation between successive synoptic charts and the strong spatial correlation in most weather elements. One can be sure of only four degrees of freedom available for this analysis, and it appears that the maximum cannot be much, if any, larger than 16. With Wilson's analysis two degrees of freedom were lost for each storm by the subjective determination of the starting time and the lag T_o, leaving no more than 8 for determining the period and dampening factor and the ten least squares coefficients. Even with the simplified analysis presented here, too few degrees of freedom remain for the determination of stable coefficients.

CONCLUSIONS

The general method of attack employed by Wilson or the alternative procedure described above shows much promise of producing useful estimates of the storm surge from very limited amounts of meteorological data. However if the dynamic principles involved are compromised by the use of highly idealized mathematical models, assumptions whose validity cannot be established, or empirical constants which must be evaluated from the data, it is essential that one use enough independent data to insure statistical stability in the results.

The results achieved in this study appear to justify a continuation of this method of attack, at least until a statistically stable set of coefficients has been determined and tested. However, it must be realized that the peak surge computed for the maximum probable hurricane by either Wilson's Model or the alternative presented here is a first estimate only, and may well require revision later.

I would like to express my appreciation to Dr. Wilson for furnishing me with tabulations of the forcing functions used in both his original analysis and in computing the surges to be expected from the hypothetical storms.

REFERENCES

1. Harris, D. L., (1959) "An Interim Hurricane Storm Surge Forecasting Guide". National Hurricane Research Project Report No. 32.

2. Wilson, B. W. (1960), "The Prediction of Hurricane Storm-Tides in New York Bay".

THE PREDICTION OF HURRICANE STORM-TIDES IN NEW YORK BAY

* Author's reply to the discussion of D. L. Harris.

The author is gratified to find that his study of hurricane storm-tides in New York Bay has evinced such interest from one who has been researching in a similar field. He is particularly grateful to Mr. Harris for his incisive reading of the author's paper and his shrewd comments and constructive criticism, which, as the author will endeavor to show, have actually helped the development of what (he believes) is now a reasonably reliable procedure for storm tide prediction.

Harris concludes from his examination of the author's method that, although it does have some measure of predictive skill, all the data that it utilizes are really neither necessary nor useful, even when sound physical reasoning is the basis for their inclusion. Harris has shown a reluctance to accept the idea of the existence of any inherent damped oscillation in the New York storm tides, apparently on grounds that its appearance in some records is not very obvious. He is further concerned over the apparent departure from dynamic principles when the effects allegedly precede the causes at the mouth of the bay. These and other apparent blemishes seem to have led Harris to a search for an alternative prediction method. The outcome relies almost entirely on statistics and thereby loses physical significance, though retaining some shadow of resemblance to the author's system. The fact that Harris' prediction formula fails to show a practical result for the design hurricane would seem to be clear evidence of its general unreliability, resulting from abandonment of sound physical structure. But of this more will be said later. It seems necessary first to examine more closely, and dispel, where needed, the objections arrayed by Harris against the author's approach.

The Dynamical Model

The author has shown in his paper that a moving system, comprising the driving forces of wind stress and pressure gradient which activate the water surface in a hurricane, can be expected to generate something in the nature of a linear damped oscillation of the water body at the coast. The period of the free component of this oscillation would be some function of the length and depth of the continental shelf. As in all dynamic systems, the prevalence of the free oscillation in the general disturbance would depend on its rate of damping and its dynamic augmentation by the driving forces, which in virtue of their own distinctive frequency would give rise to a characteristic part of the disturbance.

If a storm is slow-moving, its forcing frequency will be small compared to the natural frequency of the shelf area, in consequence of which the free oscillations it induces at the coast will receive little or no magnification and thus may not be

*Contribution from the Department of Oceanography and Meteorology of the Agricultural and Mechanical College of Texas, Oceanography and Meteorology Series.

very evident in the water level records. This could explain why Harris has not been too successful in discerning such oscillations in the numerous storm records to which he has had access. Where, however, resurgences are clearly in evidence they are undoubtedly the result of dynamic magnification both of the free and the forced oscillations and would suggest a close concurrence in values of their corresponding frequencies. While it is true that the interplay of the free and the forced frequencies would tend to beget oscillations of an apparent period different from either, the difference would not be large under conditions of near-resonance. It must be supposed that very prominent resurgences in storm tide records are a manifestation of resonance or near-resonance conditions and therefore the periodicity measurable should conform closely to the natural period of the shelf oscillation. It is therefore a valid procedure to conclude from such storm tide records as those pertaining to the 1938 and 1944 hurricanes that the resurgence period of 7 hours is very close to being the natural period of the dynamical system.

We may pursue this question rather more objectively and ask ourselves whether there is any other physical justification for believing that a period of about 7 hrs is representative of the natural period of the water body over the continental shelf near New York. On the basis that the water depth is constant, Kajiura [1959] finds that a triangular shaped shelf, such as that fronting New York Bay, should have modes of free oscillations with periods approximating 7 and 4.2 hrs. Kajiura's estimate could easily fluctuate considerably round these values owing to different possible interpretations of the shelf dimensions to be fitted to this model, but his period values are probably median. Even by such a crude approximation as that of likening the shelf to a broad rectangular open-mouthed basin of uniform slope, we find [cf. Lamb, 1932 Edn., p. 276] that the natural periods T would conform to a sequence:

$$\frac{\pi L}{T\sqrt{gd_o}} = 0.61, \quad 1.36, \quad 2.16, \ldots \qquad (1)$$

where L is the length of the shelf and d_o its depth at the edge of the continental slope. For values of $L = 100$ nautical miles and $d_o = 405$ ft (shelf slope 1 in 1500), the periods of the first three modes according to Eq. (1) conform to:

$$T = 7.62, \quad 3.42, \quad 2.15 \quad \text{hours} \qquad (2)$$

Neither of these models is really adequate for computing the natural periods accurately, but both give the trends of what is to be expected.

The next question arising is whether any such periods as those of (2) are discernible in the records of meteorological tide at Sandy Hook. In answer of this the author recently instituted a search by residuation analysis [Chrystal, 1906] of the records furnished by Harris. It was assumed that 7.0 hours was the fundamental period of the shelf oscillations, and this periodicity was therefore extracted first from each of the records. The residuals were then inspected for remaining periodicities and were residuated successively by Chrystal's procedure. The results of this study are shown in Table I below:

Table I: Apparent Periodicities in Storm Tide Records for Sandy Hook.

Storm		Apparent Periodicities (hrs)					
Date	Type						
2 Oct. 1929	TF	7.0	4.1	-	13.3	-	-
21 Sept. 1938	T	7.0	3.8	1.85	12.4	6.3	3.1
14 Sept. 1944	T	7.0	4.1	1.80	12.4	-	-
25 Nov. 1950	ET	7.0	3.9	1.80	-	-	-
15 Feb. 1953	ET	7.0	4.1	1.80	-	-	-
21 Aug. 1954	T	7.0	3.9	1.85	12.4	5.6	-
15 Oct. 1954	TF	7.0	3.8	-	12.6	-	-
3 Nov. 1954	ET	7.0	3.8	-	12.3	-	-
14 Dec. 1954	ET	7.0	4.0	-	12.4	-	-
11 Feb. 1955	ET	7.0	4.0	1.80	12.4	-	-
14 Oct. 1955	ET	7.0	3.8	1.85	11.4	-	-
30 Oct. 1955	ET	7.0	4.0	1.85	11.5	-	-
27 Sept. 1956	TF	7.0	3.8	1.85	13.1	-	-
26 Dec. 1957	ET	7.0	3.8	1.85	-	-	-

* T = Tropical TF = Tropical with fronts ET = Extra-Tropical

The analyses shown in Table I represent the efforts of two persons working quite independently and reveal a remarkably consistent pattern. It is inferred from Kajiura's results and the indications of Eq. (2) that the first three columns of periods in Table I give the first three, and presumably the only important, modal periods of the free shelf-oscillations. The fourth column of periods probably represents traces of the astronomical semi-diurnal tide that have survived the subtraction process of removing ordinary tides from the water level records. The remaining periods of 5.6, 6.3 and 3.1 hrs, discovered in only two cases of tropical storms, are unaccounted for, but need not concern us here, since the evidence otherwise strongly favors shelf oscillation periods of 7.0, 3.8-4.1, and 1.80-1.85 hrs.

Insofar as New York Bay is concerned, it seems, then, that all points of view converge to the conclusion that the prominent resurgences which often accompany hurricane inundations are manifestations of the shelf oscillations and of a degree of near-resonance between the free and the forced disturbances. The validity of the author's dynamic approach is therefore justified.

The next matter concerns the rate at which the oscillations evanesce, or the inherent damping effects within the system. Here Harris casts doubt on the author's conclusion that the records reveal an amplitude decay ratio, per cycle of the oscillations, of 0.5. This value, however, was originally arrived at jointly by R. O. Reid and the author in consultation and was later found to check the independent observation of Redfield and Miller [1956]. The values of K and of S adopted in the author's paper would thus seem to be well validated and the coefficient values a and b, dependent thereon, in the author's prediction formula, are justified quantitatively as well as qualitatively by the physical structure of the formula.

The most penetrating part of Harris' discussion concerns the question of the lag-time T_0. It must here be admitted that the evolution of the author's prediction formula was, in the first instance, largely intuitive; it became clothed with physical reasoning at a comparatively late stage of its development. The use of lag times T_N ($N = 1$ to 7 in Eq. (38) of the author's paper), based on the travel times of long period waves in water, propagating from the stations N to the bay-mouth A, was part of this intuitive procedure which has not yet been adequately explained by physical reasoning. In pointing up the anomaly concerning the overall lag T_0, Harris has in effect exposed this weakness and revealed the inadequacy of the author's explanation. As it turns out the intuitive procedure was not at fault, merely the author's justification of it on physical grounds. The necessary modifications to remove the defect have been made in the shortened version of the author's paper presented to the Seventh Coastal Engineering Conference [Wilson, 1960 (ii)]. They will be repeated here as the necessary reply to Harris's discerning criticism.

In endeavoring to give the prediction formula a two-dimensional capacity to converge the storm influences over a wide area to a point on the coast, the author caused the excitation effect, F_r , at an offshore station to be translated to the coast A at the speed of a long free gravity wave in water. This however translated the effect (waves) and not the cause (wind stress and pressure gradient) from the station to the bay-mouth. The travel times T_N appearing in Eqs. (38) should strictly have been based on the times that the meteorological forcing functions, F_r , directed radially to the bay-mouth, would have taken to travel the intervening distances, if their identities could be preserved. The value of T_N , in other words, should be some function of the speed of the storm and not of the speed of the waves. Herein lies the difficulty, however, for if T_N were to be determined on this basis, it would have required evaluation of the forcing functions F_r , as continuous functions of t and of radial distance r . The manual labor in determining $F_r(t,r)$ for each radial line would have been prohibitive, so that this approach would not have been practical.

It turns out that the intuitive use of T_N , based on the travel times of waves in water, in association with an overall lag time T_V , which we shall discuss shortly, can be justified as a physically sound procedure. It is a well known dynamical principle that if ω/S , the forced-free frequency ratio of Eq. (28), is less than unity, the phase difference between cause and effect tends to be very small. For slow moving storms at least, then, T_N , based on wave travel time, should be correct. Only when ω/S approaches or exceeds unity, as with fast-moving hurricanes, in the linear oscillating system we are considering, will T_N become unreliable if founded on wave travel time. The dynamics of the system have suggested [cf. Eq. (30)] that a lag T_o will result between cause and effect. Since T_N has translated effect to the bay-mouth, it must be suitably corrected by an amount T_V in order to ensure that the cause will precede the effect by the necessary lead T_o . Thus it becomes necessary to rewrite Eq. (38) as

$$\eta_A(t) = a\eta_A(t-\tau) - b\eta_A(t-2\tau) + \sum_{1}^{N=7} c_N [F_r]_N (t - \tau - T_N + T_V) \qquad (3)$$

This differs from Eq. (38) of the author's paper insofar as T_V replaces T_o with an opposite sign. Although Eqs. (38) and (45) were thus incorrect in the sign and the use of T_o , the actual use of a phase correction followed Eq. (3) above.

The value of T_V , it must be noted, will be constant for a particular storm. In the correlation procedure involving the data for the four reference storms, T_V was taken zero for both the slow-moving storms of 1950 and 1953.

The values of T_V for the fast moving hurricanes of 1938 and 1944 had to be determined by successive approximations based initally on intelligent guesses. Thus specific values of T_V had to be assumed in order to perform the least squares determination of best-fit values of the coefficients c in the prediction formula.

We now come to the objection of Harris that in introducing T_V to those forcing functions at the bay-mouth, for which T_N is zero, the effect $\eta_A(t)$, then precedes these causes, $[F]_A(t - \tau + T_V)$. For the 1938 and 1944 hurricanes T_V was found to be 1-1/3 and 1-2/3 hrs respectively so that the effective leads involved, $(T_V - \tau)$, are 1 and 1-1/3 hrs. This leading of the causes by the effect, it must be remarked, is only true of the station A and not of the forcing functions of the offshore stations. The author's concept here, was that all the forcing functions, translated to the bay-mouth by the phases T_N, were to be considered amalgamated there, so as to yield the general result, in conformity with theory, that

$$\eta_A(t) = a\eta_A(t - \tau) - b\eta_A(t - 2\tau) + cF_A(t - \tau - T_o) \qquad (4)$$

On this basis they were therefore all subject to the correction T_V, regardless of position. If the author were to revise his method at all, it would be in favor perhaps of voiding the correction T_V in the case of those forcing functions determined at the shore station A, but retaining it for all terms applicable to the offshore stations.

Harris has raised another objection that the inclusion of the geostrophic summation term in the prediction formula makes the latter subject to the errors resulting from the forcing functions for this term not having been identically zero for $t = 0$ in all of the four reference storms. Fig. 18 of the author's paper [1960(i)], however, shows that the initial values of $[F(t)]_A$ were sizable only in the case of the 1953 storm. The overall influence of this error, then,may be expected to be rather small since the data of the four storms were pooled in the correlation procedure.

Proof-Tests of the Prediction Method: Hurricane "Hazel" of 1954

The author's prediction formula, as given in his two papers [Wilson 1960(i); 1960(ii)], was applied to three hypothetical design hurricanes, considered to be moving along tracks that would subject New York Bay to the worst possible storm tide effects. The predictions of the maximum surge heights are recorded in Table II along with rough estimates based upon central pressure, p_o, according to various empirical relationships [Wilson 1960 (i),(Fig. 1); Kajiura, 1959].

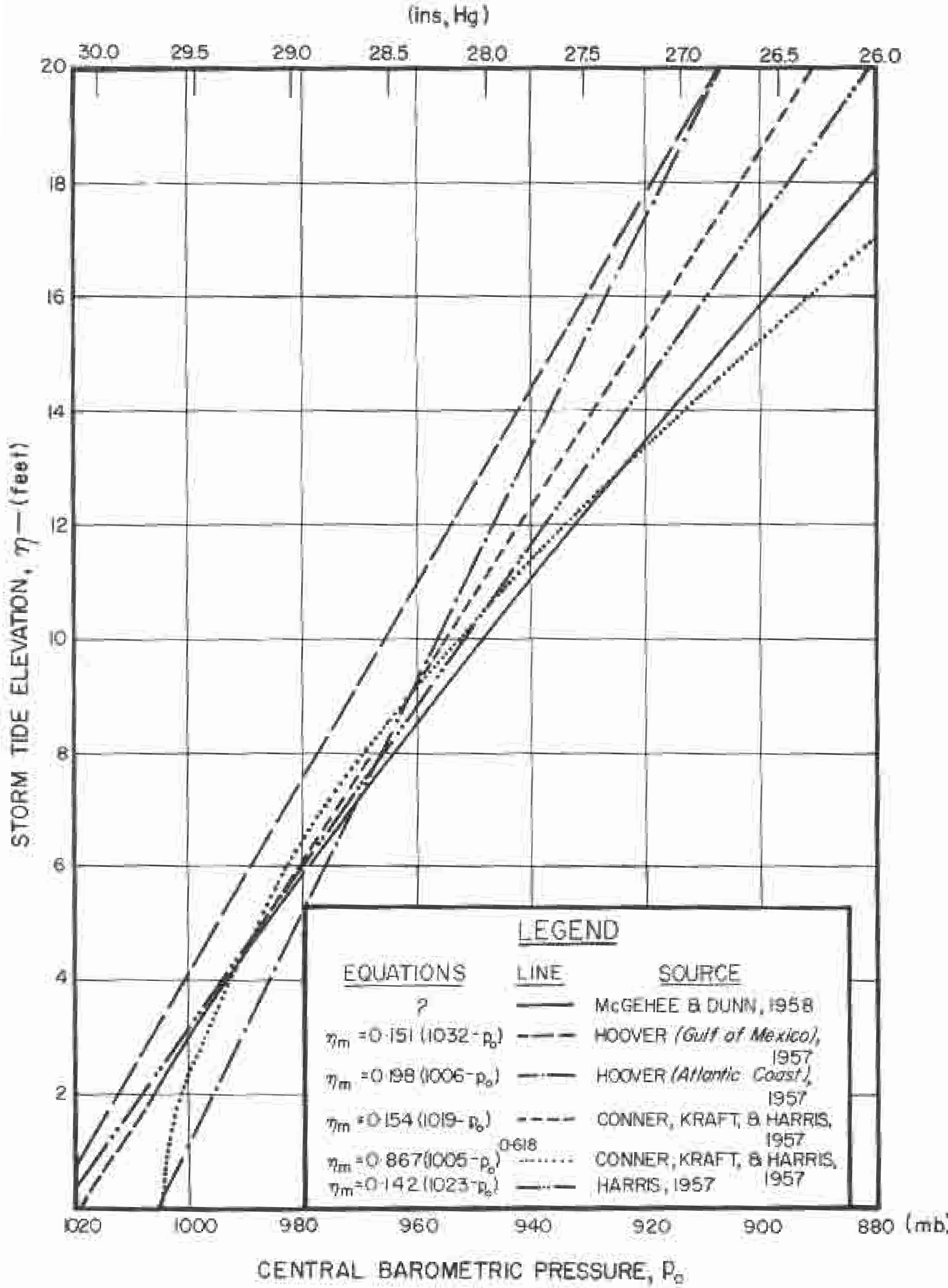


FIG 1: Empirical correlations between maximum storm tide elevation, η, and central pressure, p_o, for U.S. Atlantic and Gulf Coast hurricanes.

Table II : Design Hurricane Storm Tides

Design Hurricane Features				Max. Storm Tide Elevation (ft) above mean sea level			
				Author's Formula		Empirical Curves	
Type (1)	Speed of Advance V (knots) (2)	Radius to Max Winds R (n mi) (3)	Central Pressure p_o (ins Hg) (4)	Sandy Hook (5)	Perth Amboy (6)	Fig. 1 (7)	Kajiura [1959] (8)
"1938"	20 25 30 35 40 50	50 50 50 50 50 50	27.8 to 27.9	6.0 7.0 8.3 8.9 8.8 8.8	9.5-11.5	10.8-14.5	11.7-12.8
"1944"	30 40 30	30 30 40	27.6 to 27.7	9.3 12.3 9.8	13.5-15.5	11.5-16.0	13.0-13.7
Probable Max.	40	30	27.0 to 27.5	15.3	17.1-19.1	12.8-19.0	14.2-16.7

It is evident that the prediction formula gives results which are generally consistent with what might be expected according to columns (7) and (8) of the above table. The latter statistical estimates record, in general, the maximum storm tide elevation as it might occur anywhere, usually inland from the actual coast, whereas the predictions in column (5) apply at the mouth of New York Bay. At Perth Amboy the surge height will always be higher than at Sandy Hook and the predicted values in column (6) are indicative of what might be expected there, and are probably more in accord with columns (7) and (8).

It would appear then that the prediction formula has shown itself versatile and capable of rendering rational results in three test cases, but the real proof-test must lie in its capacity to hindcast the storm tide that some actual storm generated, other than any of the four reference storms used in its compilation. Harris has undertaken just such a test in respect of hurricane 'Hazel' of October, 1954. The test is considered to be severe because hurricane 'Hazel' followed a track which crossed the eastern seaboard south of Cape Hatteras, well outside the ambit of paths followed by the four reference storms. Hurricane 'Hazel', moreover, apparently merged with a frontal weather system after crossing the coast and assumed some of the characteristics of an extra-tropical storm. Harris' Fig. 3 shows, if anything, that the author's formula has survived this crucial test with flying colors, but the criticism of it that it fails to predict the resurgence accurately calls for special comment.

Harris has kindly furnished the author with tabulations of data relative to the forcing functions, as evaluated at the Weather Bureau for the storm in question. The nature of these forcing functions (derived from their products with the coefficients c , as supplied by Harris) is shown in Fig. 2 by the full-line curves. From his experience in deriving the equivalent functions for the four reference storms, the author is at once inclined to doubt the reality of the numerous waves in these curves. Fictitious effects such as these can easily result from imprecise graphical determinations or inadequate interpolations of weather patterns between available synoptic charts. Two smoothings of the data have therefore been introduced in Fig. 2 as likely to be more representative of the realities of the case.

Straight application of the forcing functions of Fig. 2 in the author's formula then results in the predictions A, B and C shown in Fig. 3 (b), (c) and (d) respectively. We note first of all in Fig. 3 (a) that no less than three versions of the storm tide recorded at Sandy Hook have been culled from data supplied by Harris. It is not known which of these is correct, though it would seem now that the dash-dot version accords with that given by Harris in his Fig. 3. In the absence of this knowledge the full line version in the author's Fig. 3 was taken as a standard of comparison.

In applying the prediction formula the two initial starting values $\eta(t - \tau)$ and $\eta(t - 2\tau)$ were each taken as 0.15 ft and the lag correction T_V as zero initially (in the case of Fig. 3). Prediction (A) in Fig. 3 (b) based on Harris' forcing functions, is seen to have very much stronger secondary oscillations than predictions (B) and (C) in Figs. 3 (c) and (d) which are based on the smoothed curves of Fig. 2. It is thus evident that the suspected fictitious fluctuations in the forcing functions are largely responsible for the large amplitudes of the oscillations in the predicted surge. In prediction (C), (Fig. 3 d), the precursing surges are reduced to about the right proportions and both the major trough and first resurgence after the principal surge are of magnitudes which agree quite closely with the dash-dot (Harris' Fig. 3) version of the storm tide (Fig. 3 a), though their phasing is poor.

Because of this unsatisfactory phase positioning of the resurgence, Harris has concluded rather prematurely that the dynamical approach involving the concept of shelf oscillations has here merely shown its inadequacy. However it must be remembered that hurricane 'Hazel' was a remarkably fast-moving storm with a speed fully comparable to , or even in excess of, that of the 1938 and 1944 reference hurricanes, for which lag corrections, T_V, of 1-1/3 and 1-2/3 hrs respectively had to be used. Accordingly it may be assumed, initially, that a comparable lag correction, T_V = 1-2/3 hrs, is required in any prediction for hurricane 'Hazel'. Introducing such a correction in combination with the same conditions which yielded curve (C) of Fig. 3 (d), the new result (A) in Fig. 4 is found to be distinctly better in respect of the phasing of the resurgence. For comparison two versions of the recorded storm tide are shown in Fig. 4.

Prediction (B) of Fig. 4 investigates the consequences of applying the lag correction, T_V = 1-2/3 hrs, to the forcing functions of the offshore stations only, and here we find that the resurgence has now been brought into quite close phase congruency with the resurgences of the actual records. The overall agreement here is indeed very satisfactory. The fact that the absolute peak of the main surge is now out of phase with the actual is really no reflection of the inaccuracy of the prediction since it is almost certain that the pinnacle oscillation in the true record is attributable to the peaking effect of a second mode shelf oscillation having the natural period of 3.8 to 4.1 hrs, allowance for which has not been incorporated in the prediction formula.

Prediction (C) in Fig. 4 determines the effect of a larger lag correction, T_V = 3 hrs, applied to all the forcing functions, but the result is not as satisfactory as prediction (B). This lends credence to the view, already expressed, that in the original correlations for the four reference storms it would have been better to have applied the lag corrections T_V to the forcing functions of the offshore stations only.

It would appear that an unreal feature of the predictions which must still be explained is the large negative value of $\eta(t)$ occurring after 0600 of October 16, 1954 (Fig. 4). It is not easy to comment on this except to note in Fig. 2 that the forcing functions $[F_n]_A$ and $[F_n]_2$ have a suspiciously sharp decline in value just prior to this time. Inaccuracies here might well explain the final mal-functioning of the prediction. There is also the possibility, however, that the trough of the true storm tide, after the first resurgence, was filled to some extent by a second-mode shelf-oscillation of period 3.8 to 4.1 hrs, which of course, has not been embodied in the prediction.

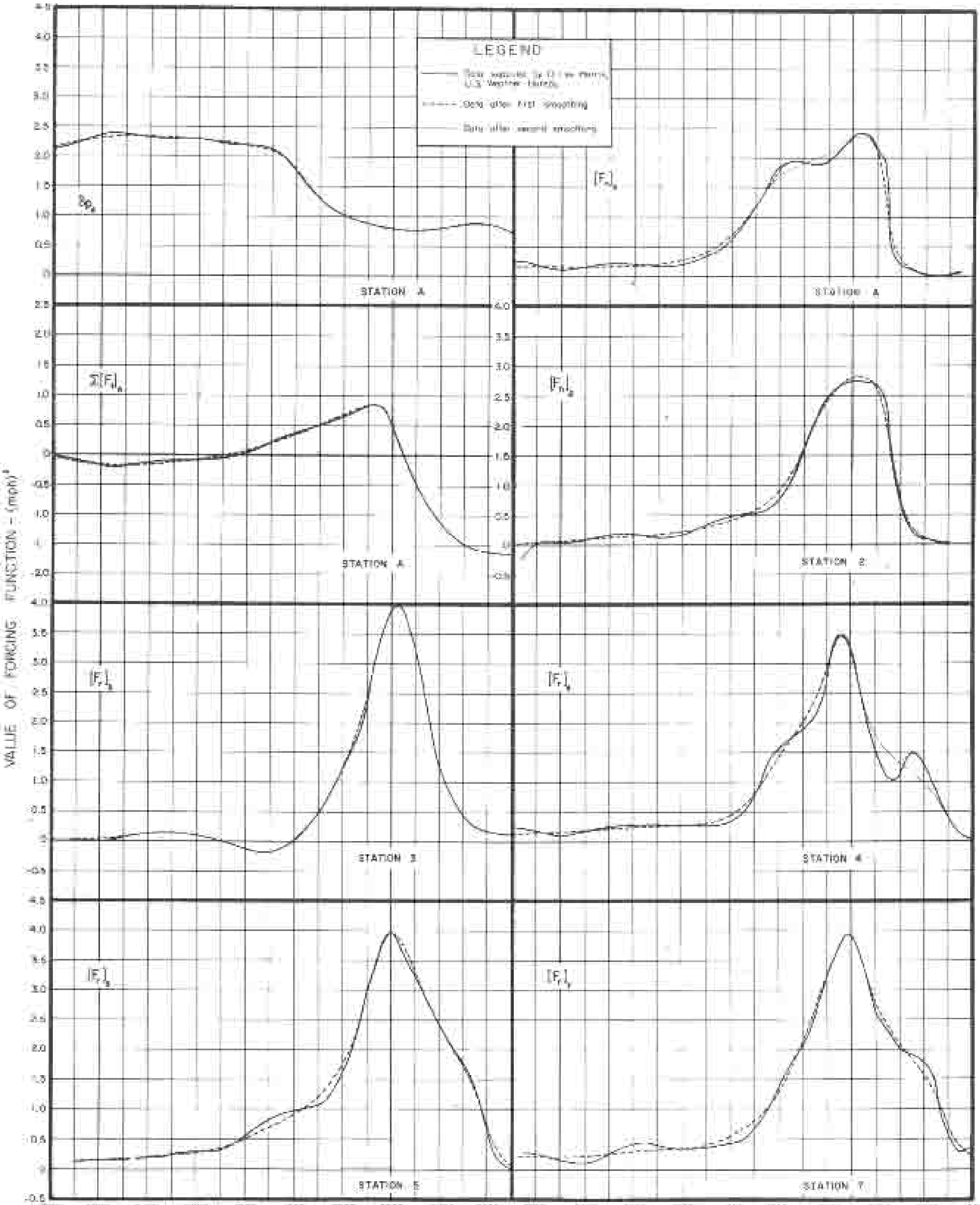


FIG 2. Values of forcing functions at different stations for hurricane 'Hazel' of October 15-16, 1954.

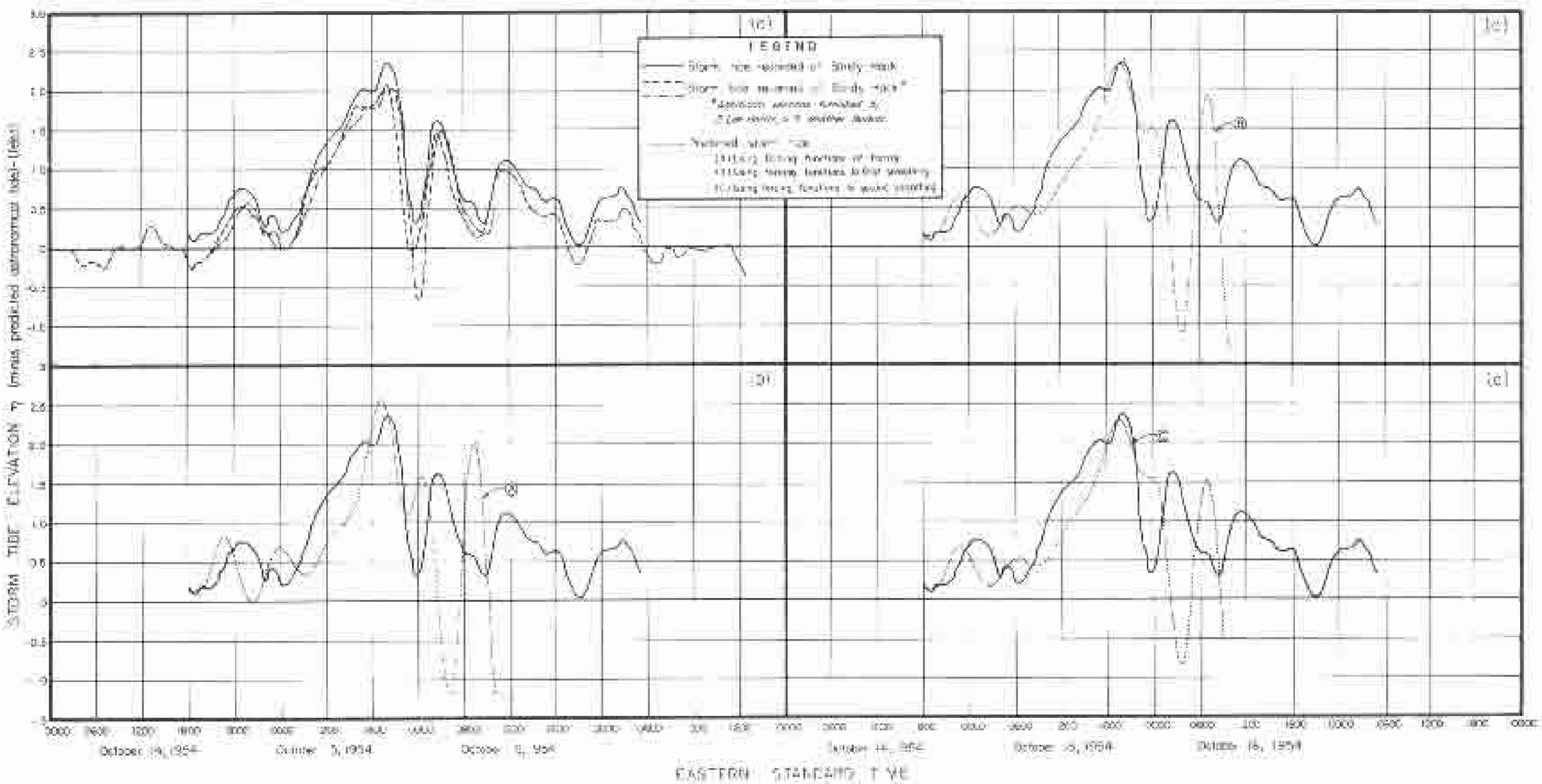


FIG 3. Comparison of actual and predicted storm tides at Sandy Hook for hurricane 'Hazel' of October 15-16, 1954. Predictions are based on initial starting values $\eta(t-\tau) = \eta(t-2\tau) = 0.15$ ft and a lag correction $T_V = 0$.

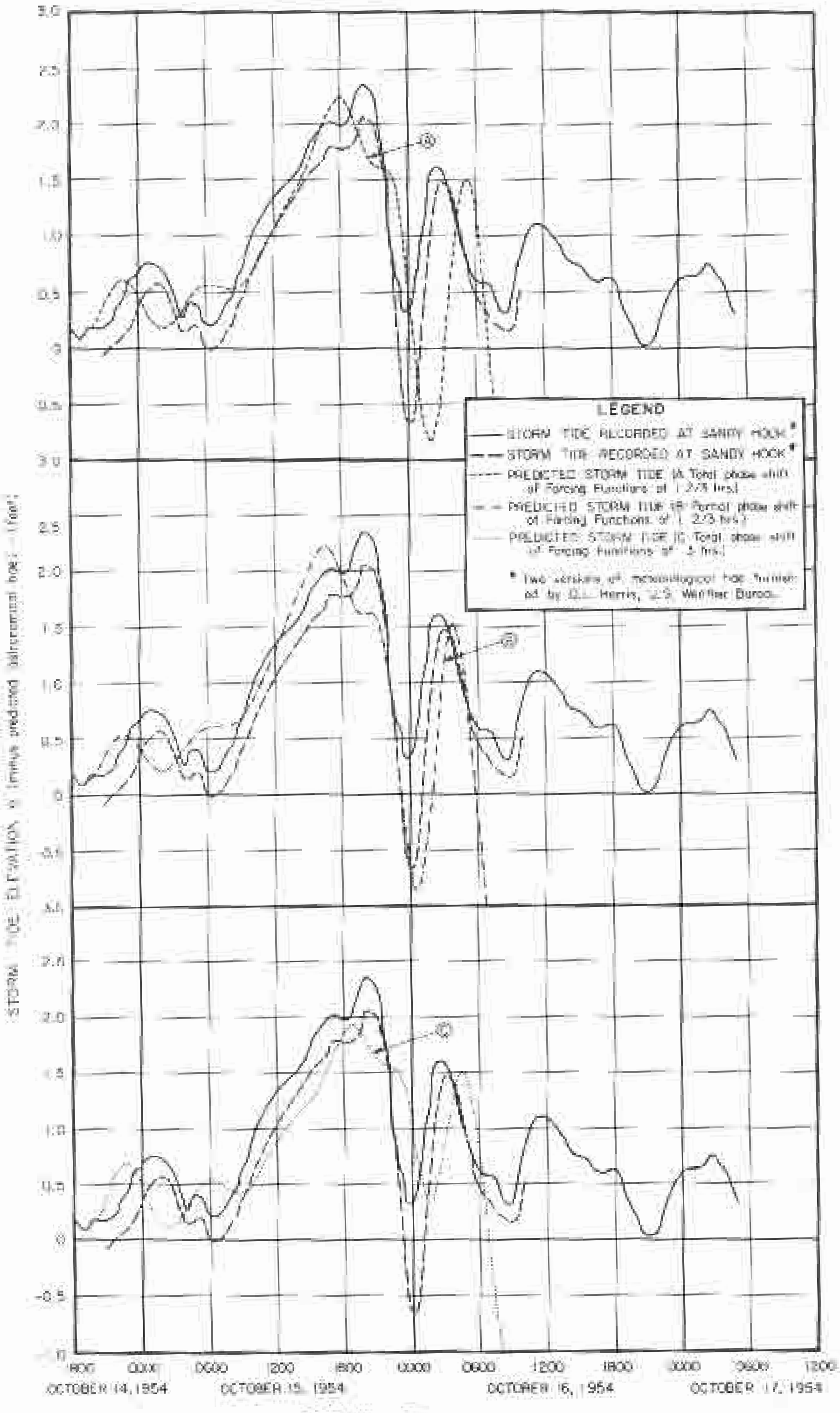


FIG 4. Comparison of actual and predicted storm tides at Sandy Hook for hurricane 'Hazel' of October 15-16, 1954. Predictions are based on initial starting values $\eta(t - \tau) = \eta(t - 2\tau) = 0.15$ ft and lag corrections T_V recorded in the legend.

Harris' Model

The author will not venture into any detailed discussion of Harris' statistical manipulations of the data which led him ultimately to an alternative prediction formula. One step calls for comment, however, insofar as it led Harris to the abandonment of the author's approach.

Harris apparently applied the author's Eq. (18) [Wilson, 1960 (i)] to the data for a statistical determination of the coefficients a and b and thus, from the relationships of Eq. (19) presumably, found the apparent free period to be 6 hrs. He then expanded the regression equation by introduction of forcing function terms and found that the inherent period increased up to 9 hrs, when the maximum amount of meteorological data were used. Harris' major point here is that this result of an increase in period with amount of meteorological data used, is untenable. In the author's opinion, however, it is quite logical.

In the first place the 6-hr period evaluated from the simple regression formula cannot have too great a physical significance because the statistical procedure is forcibly compelling the data to fit a system of pure oscillations. It is clearly only the predominance of a near-resonance of free and forced oscillations in the records of the 1938 and 1944 hurricanes that brings the result so close to the author's free-oscillation period of 7 hrs. Closer concurrence is marred, however, by the non-conformity of the 1950 and 1953 storms to the condition of near-resonance.

When forcing functions are added to the regression formula, the procedure compels the data to fit a system of combined free and forced oscillations. The low frequencies ω of the slow storms, inherent in the meteorological data, would naturally go to work in modifying the output frequency S of the free oscillations, and since differing amounts of meteorological data would be introducing differing influences, related to the two-dimensional area of the continental shelf, it is quite inevitable that S should reflect these differences. The remarkable thing to the author is that the value of 9 hrs found by Harris is still so close to the observational value of 7 hrs.

Harris' Eq. (4), which he offers as an alternative to the author's prediction formula, attempts to allow for dynamical effects of wind stress and pressure gradient essentially by just two terms, applicable at station 5. These terms, considered separately, are of the form

$$\eta(t) = A\, f(t-\alpha) + B\, f(t-\beta) \tag{5}$$

It will be of interest to enquire what the response of this equation will be to a simple harmonic input of the form

$$f(t) = C \sin \omega t \tag{6}$$

Upon inserting Eq. (6) in (5) we find

$$\eta(t) = RC \sin(\omega t - \epsilon) \tag{7}$$

in which the response $R(\omega)$ and phase $\epsilon(\omega)$ are

$$\begin{aligned} &\text{(i)} \quad R(\omega) = [A^2 + B^2 + 2AB \cos \omega(\alpha - \beta)]^{\frac{1}{2}} \\ &\text{(ii)} \quad \tan \epsilon = \frac{A \sin \omega\alpha + B \sin \omega\beta}{A \cos \omega\alpha + B \cos \omega\beta} \end{aligned} \tag{8}$$

Harris'[3] values of A, B, α and β are

$A = 0.0828$	$\alpha = 12\tau = 4$ hrs.
$B = 0.0326$	$\beta = 4\tau = 1\text{-}1/3$ hrs.

and Eq. (8, i) thus reduces to

$$R(\omega) = 0.089\, [1 + 0.682 \cos \frac{7\omega}{3}]^{\frac{1}{2}} \tag{9}$$

For a very slow moving storm then in which ω might approach zero, the response of the formula will be $R = 0.089 \times 1.682 = 0.115$ or $(A + B)$. For a fast moving storm in which ω might have a value approximating $\frac{2\pi}{7}$ radians/hr (forcing period 7 hrs), the response is found to be

$$\begin{aligned} R &= 0.089\, [1 + 0.682 \cos \frac{2\pi}{3}]^{\frac{1}{2}} \\ &= 0.059 \end{aligned} \tag{10}$$

Thus we find the anomalous result that Harris' formula will yield a dynamic effect completely opposite to what should be expected. Undoubtedly it is this feature that causes it to fail in predicting rational results for the design hurricanes.

An inevitable weakness of Harris' formula, apart from the above, is its limitation of the use of meteorological data to those prevailing at just one off-shore station (No. 5). If a very small, tight hurricane should follow a track parallelling the east coast, only a short distance offshore, it is conceivable that the forcing functions at station 5 would be almost negligible until a very late stage, and Harris' formula could then be expected to fail almost completely. The author's formula, on the otherhand, was designed to be versatile in just these circumstances by including forcing functions representative of the entire approach area to New York Bay.

Conclusions

It is felt that the evidence of this closure to the discussion of Harris establishes the following:

(1) The meteorological tide induced by a storm incident on a coastline is composite of a steady state superelevation and a combination of free and forced oscillations. The free oscillations are dictated by the topography of the coastline and the continental shelf. The forced oscillations are a function of the size and speed of the storm. The combined effects approximate those of a linear damped oscillating system.

(2) In the case of New York Bay the periods of very prominent resurgences are an index of the principal mode of the free shelf-oscillations, in keeping with the concept of near-resonance between the excitation and the free oscillations. There is evidence in all water level records examined of the existence of periodicities of 7.0, 3.8 to 4.1 and 1.80 to 1.85 hrs and there is theoretical support for believing that these are connected with the first three modes of the free shelf oscillations off New York Bay.

(3) A formula of the type of Eq. (3) is consonant with the dynamical system of (1) and (2). The two-dimensionality of the formula is assured by determining the meteorological forcing functions $[F_r]_N$ at a suitable number (N) of offshore stations covering the area of approach to the coastal station.

(4) The transference of the forcing functions from the offshore stations to the coastal station can be accomplished effectively by time adjustments T_N, expressing the time taken by a long wave in water to travel from any station N to the coastal station. Suitable correction must be made to this lag by reducing it in the amount of T_V, a time whose value will be constant for a particular storm and will depend on the speed V of the storm.

(5) The value of T_V for a particular storm can be determined fairly readily by intelligent guessing and comparison of the speeds of progression of different storms affecting the area of consideration, in combination with the correlation procedure of determining the coefficients c_N in Eq. (3).

(6) The prediction formula based on Eq. (3) or the more detailed Eq. (38) [Wilson, 1960(ii)], as applicable to New York Bay, not only gives rational results for a series of 10 design hurricanes but has survived a stringent proof-test in predicting the effects of hurricane 'Hazel' of October 1954, with an error generally less than ± 0.5 ft. [Fig. 4(B)]

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3. Tides - New York Harbor

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II. Wilson, B. W.
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