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THE EVOLUTION OF MODERN TIDE ANALYSIS AND PREDICTION — SOME PERSONAL MEMORIES

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ABSTRACT

The science of tide analysis and prediction reached so high a level of achievement in the early years of this century that there was little change or improvement for about fifty years. However, as electronic computers became both available and more powerful, very significant changes were introduced into virtually all aspects of tide observation, analysis and prediction. By virtue of this author's service for many years in the U.S. Coast and Geodetic Survey, the Atlantic Meteorological and Oceanographic Laboratories and, more recently, at Scripps Institution of Oceanography, he has been a participant in many aspects of the changed procedures. His memories of how these changes came about are featured in this paper.

INTRODUCTION

From time to time I have been urged to put into print some of the incidents that I have described to various audiences on topics related to tide analysis and prediction. Scientific work to which I have contributed has already been well documented in various papers, contributions to books, etc. The descriptions that follow are therefore not a comprehensive report on the subject but rather a potpourri of incidents related to items that normally would not be included in a report on the evolution of modern tide analysis and prediction.

In 1976, I participated in a symposium on geophysical predictions at a meeting of the American Geophysical Union in San Francisco (National Research Council, 1978). Speakers on one discipline after another described the state of

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the art in his field and the progress to date and then indicated the need for bigger and better computers to really do the job. I started my talk on tide predictions saying, 'It seems I am the only speaker at this symposium who will tell you that things were in pretty good shape by the end of the nineteenth century'.

The above statement was possible because the state of the art in tide analysis and prediction reached levels of achievement in the early part of the 20th century that were not significantly improved upon for about fifty years. Even the early electronic computers (IBM 650 and 1620 were the first acquired by the

FIG. 1.— Second United States mechanical tide predicting machine used for predictions beginning in 1912. Using 37 constituents, its two banks of gears permitted obtaining both times and heights of high and low waters simultaneously and, in addition, it produced an inked curve of the prediction with marked times of high and low waters and hourly heights.

U.S. Coast and Geodetic Survey) were unable to outperform the mechanical analog tide prediction machine completed by R. A. HARRIS and E.G. FISCHER in 1910 (Figure 1). This machine, described in SCHUREMAN (1958), summed 37 constituents with frequencies ranging from one cycle per year to eight cycles per day and, for the first time, provided both times and heights of high and low waters in a single run. The gearing was so accurate for the 37 incommensurate frequencies that, from an initial setting, no constituent was off more than 2° at the end of a year's prediction. The first United States mechanical tide predicting machine (Figure 2) summed nineteen constituents and was used from 1885 to 1911 (HICKS, 1967).

There was considerable concern about security for the tide prediction machine during World War II for both military and routine utilization. In an effort to offset possible damage from sabotage, considerable overtime on the machine increased the advance preparation of tide tables from two to four years. Shortly after the end of the war, the Coast and Geodetic Survey received a 12-component portable tide prediction machine taken from a German pocket battleship. While not as comprehensive nor as accurate as the regular machine, it was welcomed as at least an interim substitute, if one became necessary. At about that time (late 1940s), a high priority was given in federal agencies to microfilming all essential records and storing these with various supplies in a number of distant mountain caves just in case a nuclear bomb should level the Washington, D.C. area. The German tide-predicting machine was a perfect addition to this program. I regret to say that, until now, it has not been possible to relocate what would now make an excellent museum display.

By the early 1950s, concern rose that the predicting machine, even though safe, was wearing out despite an active maintenance program. At one point a complete set of new gears was ordered for a new machine but nothing further was done on this replacement program. Later a contract was placed with Fischer-Porter to automate the existing machine, activating a typewriter to prepare a finished page of tide tables. The program was far more difficult than first anticipated and the company lost a large sum on its fixed-price contract. As noted previously, in the same decade tide-predicting programs on the IBM 650 and 1620 took longer to run than the manually-operated mechanical machine driven by a hand crank, a source of considerable distress to the IBM personnel involved.

Electronic tide predictions and analysis

Electronic computers were rapidly improving in speed and capacity and it was obvious that eventually tide predictions would be prepared on these machines. In the early 1960s I was instrumental in arranging for Robert Cummings, Chief of the C&GS Tide Prediction Section, to be assigned to work with D. Lee Harris and N. A. Pore at the Weather Bureau for the purpose of programming tide analysis; the latter two had become interested in tides through their storm surge research. The assignment was a great success, the result of which (HARRIS *et al*, 1963) was a least-square analysis program solving for the harmonic constants of all constituents simultaneously, as opposed to the tradi-

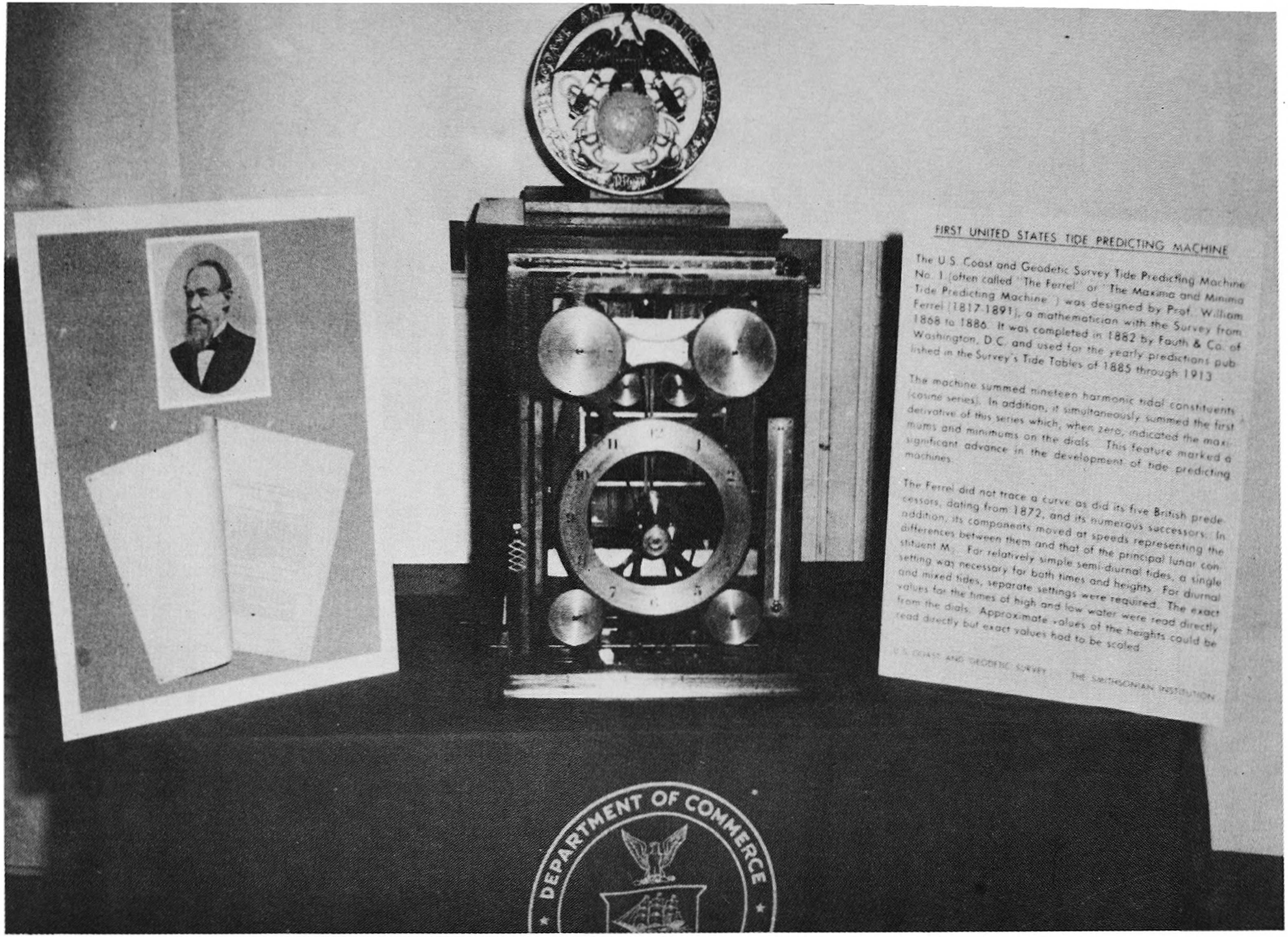

FIG. 2.— First United States mechanical tide predicting machine used for predictions beginning in 1885. First, the derivatives of nineteen cosine curves were added, fixing the times of high and low waters when the sum was zero; then the nineteen cosine curves were added to obtain the heights at these times of high and low waters.

tional method of a modified Fourier analysis for individual constituents (SCHUREMAN, 1958).

After an objective evaluation of five analytic processes (ZETLER and LENNON, 1967), the Harris, Pore and Cummings analysis program was accepted by the C&GS for series of at least one year. I attended an International Hydrographic Bureau (IHB) symposium on tides in Monaco in 1967. The French spokesman (from the Service Hydrographique) announced that they had programmed analysis as described in SCHUREMAN's manual (1958) and they now could analyze a year of tidal hourly heights in 80 hours in their computer. Admiral Franco, a leading tidal authority as well as director at the IHB, was chairing the meeting and he promptly called on me to comment on the report. I had to say that we no longer used that procedure in the C&GS and that, using the Harris *et al* method of least squares, we completed an analysis of one year of tides in three minutes on an IBM computer.

Cummings continued working with Harris and Pore, this time on computer prediction of tides (HARRIS *et al*, 1965). As a result, the 1966 tide tables were prepared on an electronic computer and the mechanical tide-predicting machine became a museum display. A similar change took place at about the same time in England, but Kelvin tide predicting machines may still be in use in some countries. During a meeting of the International Union of Geodesy and Geophysics (IUGG) in Moscow in 1971, I requested and was promised a tour of the USSR tide-predicting facility; I knew they had purchased a Kelvin machine in the mid 1950s. Each day another excuse was found for not arranging for the visit and finally I left for home without achieving this request. In retrospect, I suspect that the equipment was not classified (since Kelvin machines are well documented), but the location may well have been classified to avoid attempted sabotage of a unique instrument related to military security. At that time (and perhaps now) USSR annual tide tables were classified.

Spectral analysis

In 1962 the C&GS decided to allow me to do graduate study for one year. Walter Munk invited me to join him and Gordon Groves at Scripps Institution of Oceanography on a tide study as a visiting scientist. It was a fantastic opportunity!

Groves trained me quickly in computer programming and I had available to me BOMM computer software for time series analysis developed at IGPP (Institute of Geophysics and Planetary Physics), at that time a historic stage of development in computer methods. I recall Groves applying to the computer center for a half-hour of free time on the CDC 1604, the time ordinarily allowed for new programming trainees. Before the half-hour was used up, the remainder was cancelled by the director of the computer center on the grounds that my programs were using as many as six magnetic tapes. It did no good to protest that this was not an unusual number for a BOMM program. I quickly became involved in spectral analysis, high, low and bandpass filters, Nyquist frequencies and aliasing, all new to me except that I learned that in an earlier paper (ZETLER, 1959), I had indeed created a high-pass filter although the name was unknown to me.

I was involved in many aspects of Munk's programs including preparing long time series of tide records from all over the world from punched cards by editing and writing magnetic tapes (stored in a collection of 'geotapes'), but the major emphasis was a study of the continuum. Munk liked to use the statement, 'Noise exists everywhere except in text books on tides'. George Darwin (son of Charles Darwin), in developing the harmonic method of analyzing and predicting tides, had identified a limited set of exact frequencies important to this process (DARWIN, 1883) and tide manuals dealt only with these and ignored the energy levels elsewhere in the spectrum. MUNK and BULLARD (1963) had already identified energy levels between the tidal species (sets of constituents having the same number of cycles per day). Groves and I were to prepare an extremely high resolution analysis in the low frequencies (less than one cycle/day) using about fifty years of hourly heights (about a half-million values) at both San Francisco and Honolulu. Scanning that many values visually for errors was too great a task so an automatic digital method was devised (ZETLER and GROVES, 1964) to correct obvious errors. Proposed corrections for several years were reported to C&GS for verification on the original marigrams and the response permitted a fine tuning of the procedure. The final 'cleaned up' series were stored digitally on magnetic tape. After low-passing using filter D35 in GROVES (1955), decimating, and then prewhitening with a high-pass filter, cross spectra were obtained by the Tukey method with a resolution of 0.0005 cycle/day. We failed to find peaks that did not exist in tidal theory. The continuum peaked near zero frequency and decreased monotonically with increased frequencies, peaking only at tidal lines. The results were published in GROVES and ZETLER (1964).

MUNK *et al* (1965) extended the study by looking at the level between tidal groups (same number of cycles per month) and even between the constituents within the same group. In the vicinity of strong tidal lines, they found that the continuum rises into cusps. The paper attributes this to nonlinear interactions of the strong lines with the peak of the continuum near zero frequency. Somewhat later Groves suggested a more likely explanation, that the cusps are associated with a modulation of the internal tides by the slowly varying thermal structure. CARTWRIGHT (1982) expressed the opinion that 'the cusp-like rise is in practice more likely to be due to mediocre instrumental maintenance, notably in time-keeping'.

I recall one day listening to Walter Munk describing the progress in delineating the continuum to a class and my being so happy at being part of an effort at the very frontier of the science. Then I was struck by the thought that it was not completely new. I recalled instructions by Paul Schureman at C&GS that we not use analyzed amplitudes less than 0.03 foot because he had found empirically from successive annual analyses of San Francisco tides that the corresponding phases varied so greatly, they were unreliable. Of course, he was describing a white noise (about the same energy levels for all frequencies in the spectrum). If he had been aware of the sharp rise in the continuum in the low frequencies, perhaps the analyzed harmonic constants for the fortnightly and monthly tides would not have been included routinely in the published lists of harmonic constants.

I had been attending international meetings on earth tides that generally were dominated by Lecolazet of France and Melchior of Belgium. I began to get uneasy when Melchior obtained an IBM 1620 and began using a tide analysis program based on spectral analysis. I gathered that he believed that a more complicated calculation was therefore a better one. I prepared a paper (ZETLER, 1964) for Melchior's *Marées Terrestres, Bulletin d'Information* that opposed the power spectrum method because it gave up a very important basis of tide analysis, namely the Darwin identification of exact frequencies of tidal constituents. The paper also included noting that most spectral analysis routines included procedures that smeared nearby frequencies, again defeating exact resolution. When I met Melchior at the next earth tides meeting, he essentially warned me that a prominent French geodesist (I believe it was Robert) would tear my paper to shreds. The following morning I happened to be sitting with Robert at breakfast and outlined my paper to him. I was truly relieved that he responded, 'I couldn't agree with you more.'

There were two unexpected returns from this paper that came years later. When a super-grade rating was proposed for me in my position at the Atlantic Oceanographic and Meteorological Laboratories (then ESSA, now NOAA) in Miami, I had to submit letters of reference. Jack Rossiter, Director of UK's Tidal Institute, singled out this paper, saying that at a time when the international tide community was jumping on board the spectral analysis bandwagon, I alone stood up and asked that we take a long hard look at what we were doing. Later, when a USSR researcher, Maximov, wrote to me inquiring about tidal analysis of the monthly and fortnighly tides, I sent him a copy of the paper because in it I showed results of analysis of some long series that had been slightly truncated to make a Fourier frequency almost exactly match a required tidal frequency as a means of improving resolution of tidal lines in spectral analysis. I remember writing to Maximov that it was not an important paper but it did answer his question. His reply astonished me, 'You are wrong ; it is the most important thing you have ever done'. Later, he sent me a book including my results.

I was less proud of the same results when Henry Stommel commented that these determinations were probably the only valid determinations for the fortnightly and monthly tides and then inquired about the phases; he commented that it had always been suspected that phases for these frequencies match those for equilibrium tides. I reluctantly acknowledged that this information, readily obtainable from my analysis, had not been investigated and was no longer available.

Analysis of data in random time

I recall vividly a conversation with Capt. Laurence Swanson, then head of the Office of Photogrammetry, C&GS, in the mid-1960's. He was an ardent advocate of using photogrammetry for various geophysical programs; this particular time he was proposing a tidal current survey of Charleston Harbor using 4' × 8' plywood sheets painted various colors. He would photograph these from airplanes repeatedly during daylight hours, thus providing a time series of plywood positions at various times. When I informed him that our tidal analysis procedures required continuous equally-spaced time series, he was indignant, replying that we could put a man on the moon but couldn't do something so much simpler that he was proposing.

It must have troubled me, too, because later that evening I became convinced that what I had said was impossible, was readily possible. We never actually did the current survey that way but out of it came 'Harmonic Analysis of Tides from Data Randomly Spaced in Time' (ZETLER *et al*, 1965). The method uses the Harris *et al* method described earlier, making observation equations involving all unknowns simultaneously but the program specifies the time for each observation. A few years later, when NASA was seeking geophysical applications for spacecraft, the method was used to demonstrate feasibility for tide studies (ZETLER and MAUL, 1971). At a related NASA workshop at the University of Chicago, a decade later, the same approach was included in the proposals of the various participants.

In a similar effort, when an acoustic tomography experiment required tide analysis of intermittent acoustic observations, two methods were devised and described in ZETLER (2, 1981).

Extended harmonic analysis

The tides at Anchorage, Cook Inlet, Alaska are very large, a mean (semidaily) range of about 26 feet. Tide predictions had been published routinely by the C&GS but in the mid-1960s their accuracy was no longer acceptable. Oil had been discovered in Cook Inlet and the deep-draft tankers required a greater degree of accuracy for navigation purposes. It was obvious that the shallow water and huge ranges combined to distort the tide curve to a degree that could not be simulated using the higher frequency constituents (4, 6 and 8 cycles per day) for this purpose on the mechanical tide-predicting machine.

Because the tides in the North Sea are both large and have large shallow-water characteristics, both British and German tidal experts had long since developed methods of coping with this combination. There being no point in 'reinventing the wheel', I sent off a set of hourly data to G. W. Lennon at the U.K. Tidal Institute with a request for help. He replied quickly, confirming my opinion, and requested a full year of hourly heights, the necessary input of their procedure (DOODSON, 1957). Unfortunately, the tide gauge at Anchorage freezes in the winter and therefore a continuous year of data was not available. Since I knew that Horn's procedure in Germany used nineteen years of data, there was no possibility of help there. We were back to square one.

An effort was initiated to get a continuous record for a year by installing a pressure gauge on the sea floor but there remained an element of doubt as to whether the tidal characteristics remain unchanged during the winter freeze.

Once again, the recently-developed least-squares analysis procedure provided a mechanism for approaching the problem. Using a least-squares analysis for the standard C&GS 37 constituents and predicting for the same period, residuals were obtained by subtracting the predictions from the observed values. A high resolution spectral analysis of these residuals showed peaks of energy that occurred at frequencies that appeared to be sums and/or differences of the principal constituents. For example MNS_2 is ($M_2 + N_2 - S_2$), a semidaily compound tide. After the largest peaks were identified, a new least-squares analysis was computed, adding the frequencies of these new compound constituents to the original 37. The same process was repeated a number of times, each time lowering the levels of residual peaks. To reduce the size of the increasing matrix of unknowns (74 unknowns for the original 37 constituents), constituents were grouped by species, assuming, as did SCHUREMAN (1958), side bands from adjacent species are trivial.

To test whether the process was stationary, a long series of tides at Philadelphia, a river tide with large distortion of a cosine curve, was analyzed by the same procedure for years 1946, 1952 and 1957. The unsatisfactory constituents (large changes in phase in different years) contained, among others, sums of K_2 and N_2. When the sum of M_2 and L_2 is substituted for these, there is a difference of 1 cycle per 4.5 years. When this substitution was made, the affected constituents had satisfactory matches in phase for the three years. L_2 was not considered originally as a principal constituent in seeking appropriate combinations to match the frequencies of observed peaks in residual spectrum because its theoretical amplitude (equilibrium tides) is much smaller than K_2. However, once our attention was directed to L_2 we found L_2 to be more than twice K_2 at Philadelphia. Later we learned that HORN (1960) had determined that the L_2 frequency in shallow water ports is primarily the compound tide $2MN_2$.

As the experimental tests appeared to be working successfully, I described what we were doing in a personal letter to Lennon. His reply was somewhat embarrassed as he told me that he and Rossiter were using very much the same approach for tides at London. Our paper, ZETLER and CUMMINGS (1967), showed that, with 114 constituents, the required improvement in Anchorage predictions had been achieved. By coincidence, ROSSITER and LENNON (1968) also used 114 constituents but not all of these corresponded in frequency. I understand that Rossiter was quite chagrined that our paper was published first. The process, called extended harmonic analysis, is now used routinely at some tide-predicting agencies.

It was not a pure coincidence that British and U.S. development of extended harmonic analysis occurred at about the same time. Both countries had moved, shortly before, to computer predictions, discontinuing the previous use of mechanical tide prediction machines; this made it possible for the first time to use any constituent frequency as required, whereas before we had been limited to the finite set of frequencies incorporated into the hardware of predicting machines. Furthermore, we needed the least-square analysis procedure that also was a new development as well as new skills at spectral analysis, although the Fast Fourier Transform was not yet in general use. Most of all, I credit the availability to me of BOMM, a highly developed set of computer software for time series analysis that I had learned as a visiting scientist at IGPP, Scripps Institution of Oceanography. I recall that for the one and only time in my life, each program used in the study ran properly the first time, something unheard of with ordinary software.

An incident relating to this study had a severe impact on my ego. In reading a paper some time later, I was delighted to see Walter Munk use 'Darwin-Zetler symbols' in referring to the naming of shallow water constituents (GALLAGHER and MUNK, 1971). I was highly flattered to have my name connected with George Darwin who had developed the harmonic method of predicting tides a century earlier (DARWIN, 1883). Then I remembered a line I had heard in the play, 'Yacobovski and the Colonel', a Broadway play about an aristocratic Polish colonel and a peasant Jew, Yacobovski, joining forces in an 'odd couple' arrangement, struggling together to work their way back to Poland through German-occupied territory. The peasant, who has lived all his life by his wits, is frequently outraged by the cavalier and naive actions of his companion. At one point, he shouts, 'You represent the best thinking of the nineteenth century'. He did not mean to be flattering and I got to wondering whether Walter Munk, at that time hard at work developing the response method that he hoped would replace harmonic analysis and prediction of tides, had similar feelings about my improving a nineteenth century process that he hoped to supersede by his much more esoteric response method. I never asked him!

Comparative tests of tidal analytical processes

At a UNESCO symposium on tidal instrumentation and prediction of tides in Paris in 1965, Lennon and I presented a paper (ZETLER and LENNON, 1967) that happened to be scheduled last after a large number of papers in which tidal experts from all over the world had described their latest procedures. In giving our talk, I remember prefacing our prepared text by saying 'It was appropriate that our paper should be last; tidal experts from all over the world describe new procedures but, unless there is some international mechanism for evaluating them, it is difficult or impossible to judge their merit. Although it was a very modest beginning, for the first time tidal experts in two countries had evaluated their procedures using the same set of tide data'.

In the next decade, the idea was carried much farther in conjunction with an international intercomparison of open sea tidal pressure sensors (UNESCO, 1975) when tidal analysis experts from many countries cooperated in a similar tide analysis intercomparison workshop. Another similar study is described in ZETLER *et al*, 1979.

Response method

MUNK and CARTWRIGHT (1966) profoundly impacted traditional tide analysis and prediction by developing the response method in which the input functions are the time-variable spherical harmonics of the gravitational potential and of radiant flux on the Earth's surface. This is a major departure from the traditional solutions in which tide oscillations are described by the amplitudes and phase lags for a finite set of cosine curves of predetermined frequencies.

The response method was included in the intercomparison tests described in the previous section and, invariably, was found to be somewhat superior. Nevertheless, until now, response analysis has been a research tool that has not been introduced routinely into analysis by most tidal institutions. As described by Munk and Cartwright, the final output of response analysis were frequency-dependent admittances (amplitude ratio and phase lag on reference series). A paper by ZETLER *et al* (1969) provided a mechanism for bridging the gap by deriving harmonic constants (the output of traditional analysis) from response admittances.

Another innovation of response analysis was the separation of gravitational and radiational components of solar tides. Traditional harmonic analysis solves for the vector sum of the two. ZETLER (1971), using a relatively crude procedure on harmonic constants obtained by traditional methods, succeeded in separating S_2 into gravitational and radiational components with results rather similar to results obtained from the far more esoteric response analysis. The procedure did use the assumption, implicit in response analysis, that admittances vary smoothly over a narrow frequency band.

Just as the number of constituents that can be resolved in a harmonic analysis depends on the length of series being analyzed, the variability of admittances for a tidal species (all constituents with the same number of cycles per day) varies with the number of complex weights in the response analysis; as with harmonic analysis, the number of complex weights that can be resolved depends on the length of series being analyzed. An article, 'The optimum wiggliness of tidal admittances' (ZETLER and MUNK, 1975) explored this aspect of response analysis.

Deep-sea pressure measurements

In the mid 1960's, Walter Munk spearheaded an international program for sea-floor measurement of tides in the deep ocean. For many years tidal experts had worked at preparing regional and even global cotidal and co-phase charts for the principal tidal constituents. Most of the available observations were obtained at estuarine locations and, therefore, were not satisfactory for good estimates of the tide offshore. They valued greatly tide data for small oceanic islands, although even these observations were obtained mostly within harbors. Most of all, they wanted tide measurements in the open ocean.

In the same decade, Pekeris and Hendershott were pioneering in theoretical calculations of global tides, relying on powerful computers to make calculations that were previously impossible. MUNK and ZETLER (1967) outlined the ongoing effort to develop dependable pelagic pressure gauges and discussed a variety of scientific objectives of the program that included related geophysical phenomena in addition to global tides. CARTWRIGHT *et al* (1969) outlined an optimum analytical procedure for obtaining 'as comprehensible and meaningful as possible' a response analysis of pelagic tide measurements.

For several years, a high priority objective of the Working Group on Deep-Sea Tides was to obtain an observed series at or near an amphidrome (point of zero tide for a particular constituent). However, ZETLER and CUMMINGS (1972) showed that applicable measurements in the Caribbean brought out the fact that as one approached an amphidrome, the signal-to-noise ratio decreased, making the effort to pinpoint an amphidrome self-defeating. The Working Group then endorsed a program of obtaining observations at anti-amphidromes (places with relatively small changes in constituent amplitude and phase over a significant area).

The Working Group arranged for an international intercomparison of open sea tidal presssure sensors in the vicinity of a steep continental shelf edge off the west coast of France in 1973 (UNESCO, 1975). The exercise included gauges of the United States, United Kingdom, France and Canada. The exercise showed a strong advantage in using free-fall capsules that are later recalled to the surface using acoustic transponders as compared with those connected by cables to subsurface floats. The exercise showed that five types of sensors intercalibrated closely.

As more observations were obtained and analyzed for tides, a compendium of tidal constants for about one hundred stations was published by the IAPSO Advisory Committee on Tides and Mean Sea Level (CARTWRIGHT *et al*, 1979). The publication is described in a paper by ZETLER (1980). When about another hundred pelagic stations became available a few years later, a second volume of the IAPSO compilation was prepared by CARTWRIGHT and ZETLER (1985).

An array of pelagic pressure gauges were deployed in the western Atlantic in the MODE (Mid-Ocean Dynamics Experiment) experiment. Two independent drops at the same location gave the following M_2 amplitudes and Greenwich epochs :

32.067 cm and 2.5°
32.074 cm and 2.6°.

The agreement to four figures was a triumph of engineering achievement for Frank Snodgrass (SNODGRASS *et al*, 1975). For the entire array, the analyzed harmonic constants were in excellent accord with the traditional Atlantic cotidal charts. M_2 tidal currents, calculated from the analyzed tidal amplitudes and constants, were in rough agreement with preliminary estimates from current measurements (ZETLER *et al*, 1975).

Tidal datum planes

Mean high water is the tidal datum plane used in the United States for delineating boundaries between private ownership and state property along coastlines and in estuaries. They must be very accurate as ownership of very expensive waterfront property is involved. In my quarter-century service in C&GS, I had worked in every section in the Tides and Currents Division except the Tidal Datum Planes Section. Nevertheless, my work in other sections made me familiar with procedures used in calculating various tidal planes and with the relevant C&GS manuals.

In 1979, Admiral Alan Powell, then Director of the National Ocean Survey (NOS), the new title for C&GS, asked me to investigate a controversy with respect to one aspect of calculating tidal datum planes. I agreed to do this although I was not familiar with either side of the controversy. It turned out to be one of the most challenging assignments I ever had.

In some areas, for example the New Jersey marshes, only the upper portions of the tide are recorded because the bottoms are cut off by a sill ; this makes it impossible to compute 'mean high water' by the NOS range-ratio method. The NOS alternative is the height difference method, 'Δh'. Jack Guth, a retired NOAA officer engaged in engineering surveys, was dissatisfied with the Δh procedure and developed an alternative, modified time method (MTM) also known as EWE (extrapolated water elevation). Neither side could convince the other and there had been bitter arguments at annual meetings of the American Congress on Mapping and Surveying; a legal court battle seemed imminent.

My first attempt to achieve an acceptable resolution was to devise a field program in appropriate conditions to test both methods and I was delighted to accept several useful modifications of my plan by Guth. However, when NOS refused to fund Guth's participation in the field test, he refused to be involved. NOS completed the test for me, but, without Guth's participation in it, there was no chance to resolve the dispute based on the results.

Both sides had presented me with various statistical studies supporting their positions and both emphasized instances in which the alternative method had failed (inaccurate estimates of mean high water). The latter provided me with useful research material; if a procedure fails, why does it do so and what can be done about it?

In 1981 I addressed a meeting of the American Congress on Mapping and Surveying (ZETLER, 1, 1981) and described how I proposed to resolve the controversy. Since both methods sometimes gave inaccurate results, I would demonstrate modifications to each method to eliminate the problem. The changes to the Guth 'time' method would also eliminate a tidal height requirement that made it applicable only some of the time. If both sides would accept my modifications of their procedures, it would be unnecessary to choose between them. Needless to say, I was greatly relieved at the conclusion of discussions following my presentation when both sides agreed with me.

Predicted extreme high tides

During the winter of 1982-83, a combination of high tides, higher-than-normal sea level and storm-induced waves were devastating to the coast of California. Newspaper accounts of the damage referred to predictions of even higher astronomical tides in the early part of the next decade and led to increased public concern as to the safety of many coastal structures.

Published studies on astronomical extreme high tides concentrated on semi-daily criteria whereas California tides are 'mixed' (roughly comparable diurnal and semi-diurnal components). Predictions were prepared for four California ports for the period of 1983-2000. After monthly extreme high tides were tabulated and examined, it was found that additional criteria were indeed important. Added consideration must be given to tropic tides (diurnal tides larger than normal when the moon is near maximum declination) and to the 18.61 year period of the lunar-node cycle. Furthermore, extreme high diurnal tides tend to occur when the sun is near maximum declination (summer and winter) whereas comparable semi-diurnal tides ordinarily occur near the equinoxes (spring and fall).

The basic statistics from this study are published in a technical note (ZETLER and FLICK, 2, 1985) ; the theoretical aspects are covered more completely in ZETLER and FLICK (1, 1985).

Tide predictions using satellite constituents

Conventional harmonic tide predictions for the last century have used *f* factors to modify the amplitudes of lunar constituents and *u*'s to correct the equilibrium phases (V_0) as a means of approximating for a given period (one year or less) the effect of the 18.61 year cycle of the revolution of the moon's node. Historically, there was little choice; friction in geared mechanical tide-predicting machines imposed finite limits on the number of constituents used.

DOODSON (1921) clearly identified and evaluated satellite constituents; his study was updated using the latest astronomical constants by CARTWRIGHT and TAYLER (1971) and by CARTWRIGHT and EDDEN (1973). Nevertheless, satellite constituents, now readily usable on modern computers, have not been used for tide predictions. As a result, predictions have really been quasi-harmonic, requiring modifying amplitudes and phases periodically, at present every year for U.S. predictions, every two months for Canadian, and every 30 days for U.K. predictions. With satellite constituents, nineteen years of tide predictions for Seattle (1921-1939) were computed from initial settings for 1 January 1921.

Although the improvement in accuracy of predictions is, as expected, relatively small, nevertheless it is more satisfactory to use truly harmonic predictions and to predict for much longer periods from initial settings. Furthermore, the method removes the need for rather contrived procedures, in particular that of constituents modifying M_1 and L_2 by cycles per 8.85 years (revolution of lunar perigee) in the *f* and *u* corrections for these constituents. The experiment is described in ZETLER *et al* (1985).

Supplement to Schureman (1958)

The many changes that have been described for tide analysis and prediction procedures made obsolete many portions in SCHUREMAN's Manual (1958). I was reluctant to undertake a complete new edition of the manual but I did prepare a supplement, 'Computer Applications to Tides in the National Ocean Survey' (ZETLER, 1982). On a page-to-page basis, Schureman's procedures are compared with those in present practice. My paper, 'Tide Predictions', published in Geophysical Predictions (National Research Council, 1978) is included as an appendix.

A somewhat upgraded but less technical summary, 'State of the Art in Tide Predictions' (ZETLER, 1983), is published in *Proceedings of the 18th International Conference on Coastal Engineering*, Cape Town, South Africa, November 1982.

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Paul Schureman was reported to have been a strict martinet in his early years, for example, admonishing his staff severely if they failed to use both sides of adding machine tapes. When I met him in his senior years, he was working alone on a research assignment. He graciously put aside his work when approached for assistance in tidal theory, obviously pleased to have a young employee interested in doing more than routine data processing. He broadened my horizons one day by saying, 'Wouldn't this be a terrible world if all of us who fail to achieve the top position were to consider ourselves failures ?'

I was Walter Zerbe's assistant for about fifteen years ; he was an outstanding teacher and an innovative researcher. I recall participating in his study of water level flunctuations in the U.S. Navy's David Taylor Model Basin, tidal variations of a very small fraction of a centimeter at the ends of an enclosed basin about 850 meters long. As I checked his calculations, time and again I thought he had gone as far as he could; I was amazed and excited when he finally extracted from these data sensible harmonic constants for the earth tides in the area.

Walter Munk opened up new worlds to me at the frontiers of tidal science. I recall one incident in particular in his lecture to a class in 1963 on the utilization of computers. He noted that many people set about to program a computer to do exactly what they had already been doing by other means. Instead, he said, they should be seeking ways to have the computer do their tasks better, frequently in ways impossible in the past. I have tried to follow this advice ever since and any contributions I have made have been in the spirit of that philosophy.

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