

The ENIAC Computations of 1950— Gateway to Numerical Weather Prediction¹

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Abstract

The first numerical weather prediction was made on the ENIAC computer in 1950. This lecture gives some of the historical background of that event and a partially narrative account of it.

Victor P. Starr

Victor Starr began his academic career in the autumn of 1940 as an instructor and charter member of the University of Chicago's Institute of Meteorology. He had arrived in Chicago less than a year before as a Weather Bureau employee, in company with Horace Byers who had been deputized by Carl Rossby to test the fertility of the midwestern academic soil. Rossby was then on leave from M.I.T. to the Weather Bureau as an assistant chief for research, and was casting about for a new base of operations. The destiny of the new Institute was altered precipitously by the attack on Pearl Harbor. My own destiny was also suddenly changed by that cataclysmic event which in combination with Rossby's powerful magnetism propelled me into the study of meteorology.

On a fine day in the Spring of 1942 a course in weather-map analysis was in progress in one of the musty classrooms of the University's Ryerson Laboratory. As that was not one of my favorite subjects, I suppose I was thinking more about the fair weather outside than of the weather on the map. Suddenly I became aware that someone had quietly approached my desk, had pulled up a chair, and had started to draw something, obviously for my edification. His actions were slow and deliberate, and when he finished, he turned the sketch for me to see. I was enchanted! To me, the Laplacian had been a differential operator. Here was a geometrical—and obviously computational—way to look at it (Fig. 1). The implication intended was clear, as I had already been introduced to the idea of geostrophic vorticity. I looked up to express my delight only to find that the visitor had vanished as silently as he had come.

Thus began my friendship with Victor Paul Starr (Fig. 2), whose memory and whose widespread influence on the world of meteorology we honor today. Many in this

room have been inspired, as I was, by his intense absorption with the natural world and the sheer intellectual joy of man's understanding of it. Above all Victor Starr had an unerring intuition for the workings of seemingly complex phenomena and the capacity to analyze such phenomena with simple yet powerful methods. Those who came under the spell of this gentle man could not fail to find through him a deeper understanding of the meaning of science and a lasting appreciation of its importance in the development of the human intellect.

Prologue

Dr. Lorenz, Mrs. Rose Starr: it is a particular satisfaction for me to have this opportunity not only to memorialize Victor Starr but also to join you in observing the 50th anniversary of academic meteorology at M.I.T. Your department and mine at the University of Chicago have many affinities. Both were founded by Rossby—ours about a decade after yours—and both were graced with the presence of Starr as well as Rossby. Your department and mine too take pride in distinguished alumni who are leaders of American meteorology. Many of them are here today. It is truly a pleasure to be with you on this occasion.

On August 29 and 30, 1946, at the Institute for Advanced Study in Princeton, New Jersey, a notable conference took place—attended I should add by both Rossby and Starr. The conference was titled simply "Conference on Meteorology", but it may be considered the first conference on numerical weather prediction. It was organized by John von Neumann, probably with assistance from Rossby, and from Harry Wexler of the Weather Bureau. The purpose of the conference—as far as can be inferred from a terse, four-page summary²—was to enlist the support of the meteorological community for a bold project. This undertaking had already been proposed to the Navy by the Institute for Advanced Study—that is to say, by von Neumann. In the words of that proposal³:

The objective of the project is an investigation of the theory of dynamic meteorology in order to make it ac-

¹ Victor P. Starr Memorial Lecture, Massachusetts Institute of Technology, Cambridge, Massachusetts, 27 October 1978. The verbatim text of the lecture has been modified for publication in the BULLETIN by the addition of citations, section headings, and an abstract.

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² Typewritten resumé of "Conference on Meteorology, August 29–30, 1946". Institute for Advanced Study, Princeton, N.J., 4 pp.

³ Typewritten proposal to Office of Research and Inventions (Navy Department) 8 May 1946. Submitted by Frank Aydelotte, director, Institute for Advanced Study, Princeton, N.J., 7 pp.

$$\nabla^2 Z \approx \frac{Z_1 + Z_2 + Z_3 + Z_4 - 4Z_0}{d^2}$$

FIG. 1. Finite-difference Laplacian.

cessible to high speed, electronic, digital, automatic computing. . .

Among the small group of about 20 at this elite gathering of August 1946 were Bernhard Haurwitz and Jerome Namias, both of whom I am happy to say will address us later in these anniversary proceedings. Also present then as well as here now was the man who, with von Neumann, plays the leading role in the story I want to tell this afternoon, namely Jule Charney. It is, I think, amusing that the minutes of the conference refer to his contribution on that occasion in one brief sentence: "Some rather abstract problems were suggested by Dr. Charney."

Participants in some events of great moment are not at first fully aware of the significance of the event for the future course of human affairs. An example is the experimental detection of electromagnetic radiation by Heinrich Hertz, or indeed of the discovery of the laws of such radiation by Maxwell 20 years before that. I think it can be fairly said, however, that the significance of the ENIAC calculations—the first numerical weather forecasts—was at the time rather well understood by the participants and bystanders. Perhaps it would be more accurate to say that there was a full appreciation of the symbolic importance of these calculations as the gateway to numerical weather prediction. What I think was not anticipated was the enormous practical value of the barotropic model and the leading role it was to play in operational prediction for many years to come.

Public knowledge about the ENIAC calculations is confined to the paper⁴ published in 1950 by Charney, Fjørtoft, and von Neumann on "Numerical integration of the barotropic vorticity equation." What I aim to do in my remarks this afternoon is to look at the ENIAC calculations as an event with institutional and technological, as well as scientific elements, and also to add some fragments of narrative so that you will sense its human dimension. In my efforts to put this story together I was greatly helped by Dr. Charney, who provided en-

couragement as well as access to his archival material. You will understand, I am sure, that the story cannot be told without making frequent reference to Dr. Charney, but I want to assure you that you have no cause to feel uneasy about these references to a member of the audience, as he has read what I am going to say. I do not have any revelations that will take him by surprise, nor will I risk embarrassing him by dwelling on his achievements, which have long been well known.

There were two ENIAC expeditions. In this lecture I will talk only about the first, which took place in the Spring of 1950 and produced four 24 h forecasts from actual meteorological data. A description of this work was given the same year in the paper I have already mentioned. The second series of ENIAC calculations was made about a year later in order to carry out a plan originally intended for the first series but aborted then for lack of time. It was devoted to forecasts from a theoretical model of stable and unstable waves on zonal jet-like flows in a channel bounded by streamlines on two latitude circles. The results of this second expedition have not been published. They have some features that were interesting at the time and might still be historically noteworthy, but perhaps it was inevitable that a report about them was inhibited by a sense of anticlimax. Nevertheless, I hope that a description of ENIAC 1951 will some day be given.

An eyewitness account, even from the perspective of 30 years, cannot by its nature be accepted as impartial historical analysis. It can, however, provide source material for historians of future generations, and it is in this spirit that I have approached the subject of today's lecture. The degree of detachment I can claim is impaired by the fact that I am personally acquainted with many of the principals about whom I shall speak. On the other hand, my detachment is enhanced by the fact that I played only a relatively minor role in the whole enterprise.

Institutional elements

A complex event such as the ENIAC weather calculations can be seen as the confluence of several independent streams of human activity. Of the streams that converged at Aberdeen, Maryland, in March 1950, we can discern three conspicuous institutional elements: the Moore School of Electrical Engineering of the University of Pennsylvania in Philadelphia, the Ballistic Research Laboratory of the U.S. Army Ordnance Department at Aberdeen Proving Ground, and the Electronic Computer Project of the Institute for Advanced Study in Princeton. For this institutional part of the story, much of what I shall say I have taken from Herman Goldstine's book, *The Computer from Pascal to von Neumann*.⁵

The Electronic Numerical Integrator and Computer

⁴ Charney, J. G., R. Fjørtoft and J. von Neumann, 1950: Numerical Integration of the barotropic vorticity equation. *Tellus*, 2, 237–254.

⁵ Goldstine, H. H., 1972: *The Computer from Pascal to von Neumann*. Princeton University Press, 378 pp.

(that is, the ENIAC) was built in about two and one-half years 1943–45 at the Moore School, for the Ballistic Research Laboratory. Collaboration between the Moore School and the Ballistic Research Laboratory began in 1933 with the design of a modified form of Bush Differential Analyzer, the famous analog computer constructed by Vannevar Bush of M.I.T. and put into operation in 1930. Bush's work, incidentally, was promoted by M.I.T. President Samuel Stratton, who should be mentioned today as having presided over inception of M.I.T.'s first course in meteorology 50 years ago.

The Moore School may rightly be regarded as the birthplace of the electronic computer. There the ENIAC was created, and also its successor the EDVAC (Electronic Discrete Variable Computer). After 1945 the emergence of computer technology swiftly became a flood tide in this country and abroad, and the Moore School's dominance came to an end.

Even before the ENIAC was formally dedicated at the Moore School early in 1946, the idea of a computer project at the Institute for Advanced Study was germinating. The source of these activities was John von Neuman (Fig. 3), Professor of Mathematics at the Institute since 1933. In 1937 von Neumann began his association with the Ballistic Research Laboratory as a consultant, and in 1940 he was appointed to the Laboratory's scientific advisory committee. His connection with the ENIAC began in 1944, about midway in its construction, when he became a consultant to the EDVAC project. Before the end of 1945, with the strong backing of Oswald Veblen, von Neumann had succeeded in convincing the Institute's Director and Board of Trustees to make a moral commitment to an Electronic Computer Project and—equally important—a financial commitment of \$100 000. RCA—the Radio Corporation of America—pledged a like amount. The central goal of the project was construction of a state-of-the-art electronic digital computer—a highly anomalous activity for the cloistered confines of the Institute for Advanced Study. The project, directed by von Neumann, got under way early in 1946, with Herman Goldstine—von Neumann's deputy—in charge of the logical and mathematical aspects and Julian Bigelow in charge of engineering. The Institute computer eventually became operational in 1952.

From the very beginning von Neumann was determined to make a major effort toward numerical weather prediction. In May 1946, only a few months after the start of the Electronic Computer Project, he submitted a proposal to the Navy for creation of a meteorology group within the Project. This proposal³ was perhaps the most visionary prospectus for numerical weather prediction since the publication of Richardson's book a quarter-century earlier. At the outset von Neumann says:

The objective of this project is an investigation of the theory of dynamic meteorology in order to make it accessible to high-speed, electronic, digital, automatic computing, of a type which is beginning to be available, and which is likely to be increasingly available in the future. It is also expected that these investigations will give indications as to what further observations are necessary



FIG. 2. Victor Paul Starr (1908–76).

—both of the laboratory type and of the field type—in order to make theoretical work, that is supported by such high speed computing, more fully effective.

After outlining the design specifications of the Institute computer, von Neumann's proposal continues as follows:

A careful analysis of the present status of meteorological theory, carried out in particular by Dr. John von Neumann of the Institute for Advanced Study and Dr. C. G. A. Rossby of the University of Chicago, indicates that even if computing equipment of the type indicated above were immediately available, we would not be able to use it at once. This is even valid for some more limited, but nevertheless very interesting and important, problems, which might be solved by ENIAC. Indeed, the possibilities that are opened up by these devices are so radically new and unexpected, that the theory is entirely unprepared for them. There was no practical motivation in the past to work out those parts of meteorological theory on a mathematical and analytical level, which in order to become really effective, would require calculational methods that are 1000 to 100 000 times faster than what seemed possible at the time! A complete reassessment or reevaluation of the theory is therefore an absolute prerequisite.

The Navy wisely funded this proposal, starting 1 July 1946. In the following month von Neumann sought to enlist the support of a wide segment of the meteorological community through the conference I mentioned

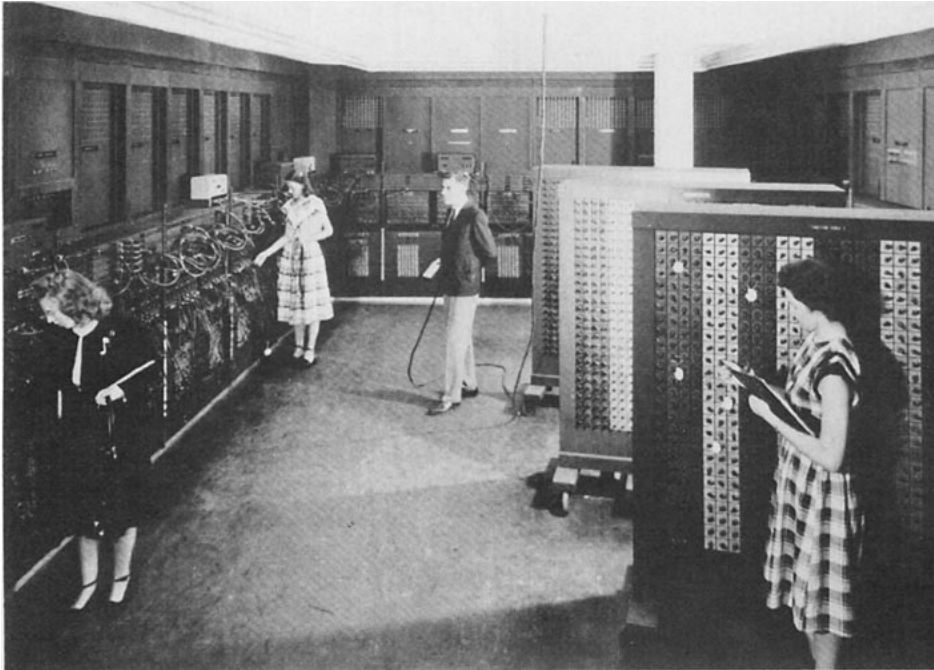


FIG. 4. Electronic Numerical Integrator and Computer (ENIAC), 7 July 1948. Ballistic Research Laboratory, Aberdeen Proving Ground, Aberdeen, Maryland.

together consumed about 140 kW. Of the 42 panels, 20 held registers called accumulators, of which all but about seven were used for various control operations. Thus the programmer had available only about a half-dozen or at most 10 words of high-speed read/write memory. An intermediate direct-access but read-only memory of 624 six-digit words was provided by three so-called "function tables", on which decimal numbers were set manually by means of 10-pole rotary switches, a tedious and lengthy procedure. The function tables are the free-standing units on the right in this photograph (Fig. 4). An essential adjunct to these facilities was punch-card equipment that served as a large-capacity read/write memory—reading done by card reader and writing by card punch. As for speed, ENIAC's add time was 0.2 ms, multiply time between 2 and 3 ms, and access time to the function table 1 ms. Access time for the punch-card equipment I leave to your imagination!

The interplay between electronic and mechanical components is nicely illustrated by what in the weather-forecast problem was called Operation 4, calculation of the Jacobian—in other words of the vorticity advection. At each grid point a Jacobian was calculated after reading three cards. In this process the card reader made three quite audible clicking noises and then a slightly longer interval followed during which ENIAC did the Jacobian multiplications and additions. The speed of this rhythmic operation was such that one could easily do a three-step jig to the clicking noises of the card reader. Indeed, for some now-forgotten reason we were greatly relieved when at each time step Operation 4 finally made its presence known audibly through the

card reader, and I do not doubt that on more than one occasion a lively jig was actually seen to be performed.

Somewhere in the borderland between technology and science lies another important element of our picture: the concept of the stored program. When first constructed, the ENIAC was a hard-wired machine; that is, its actions were controlled in a manner somewhat analogous to the way a plugboard controls a punch-card machine. This arrangement made programming very abstruse and prevented the ENIAC from being a general-purpose computer. The concept of a stored program, which is basic to computer design as we now know it, made its first appearance in 1944–45 in the design of the EDVAC, successor to the ENIAC. A stored program is one in which the individual commands are expressed as numbers and stored as such in the direct-access memory. It is constructed from a repertoire of commands that is universal in the sense that the solution of any problem, no matter how complex, can be programmed at least in principle, if indeed the mathematicians are able to tell us how to solve it.

Historians of these early days apparently find it a little difficult to decide how to apportion credit for the first clear-cut exposition of the stored-program concept, but there seems to be unanimous agreement that von Neumann played an important role. It was also von Neumann who suggested that ENIAC could be converted to a rudimentary stored-program computer by means of not very extensive engineering modifications and a suitable list of commands. With a roster of about 60 instructions, this conversion was made in 1948, soon after ENIAC's arrival at Aberdeen. Code for the ENIAC was



FIG. 3. John von Neumann (1903–57).

earlier in this lecture. By the time of his first report⁶ to the Office of Naval Research, in November 1946, von Neumann was able to give a preliminary account of work underway by a meteorological staff that included two full-time scientists, Paul Queney and Albert Cahn, several on a part-time basis, such as C. L. Pekeris and Hans Panofsky, and consultants such as Harry Wexler and Gilbert Hunt. Crucial for the whole undertaking was development of the hydrodynamical equations in a form suitable for numerical prediction—the “reassessment . . . of . . . theory” in the words of von Neumann’s proposal to the Navy. This task was assigned to Cahn, who, however, left soon after in December 1946, at which time Philip Thompson joined the project and took over Cahn’s assignment. He stayed for about two years. In July 1948, a few months before Thompson’s departure, Jule Charney arrived in Princeton. He remained at the Institute as leader of the meteorology group for eight years to the end of the project.

The other institutional element about which I shall speak briefly is the Ballistic Research Laboratory. Here once more I must flaunt my alma mater, for at the turn of the century the fountainhead of American mathematicians was the University of Chicago, where Eliakim H. Moore was one of the stars of the intellectual galaxy gathered by President William Rainey Harper. Out of Moore’s Department of Mathematics came George Birkhoff, Gilbert Bliss, Oswald Veblen, to name a few.

⁶ Typewritten “Progress Report for the Period of July 1, 1946 to November 15, 1946, on Contract N6ori-139, Task I (Meteorology Project).” Submitted by John von Neumann, Project Supervisor, 3 pp.

Veblen went to Princeton University shortly before World War I and created there the second great American academic center of mathematical excellence. In 1930 he brought von Neumann to Princeton and in 1932 he joined the newly-established Institute for Advanced Study, whence he was followed a year later by von Neumann. I have already mentioned Veblen’s role in supporting the establishment of the Electronic Computer Project at the Institute. During the first world war Veblen was called upon to take charge of a group set up to work on problems of exterior ballistics. In 1918 this activity was moved to Aberdeen Proving Ground, and in 1935 it became the locus of a research division which in 1938 was named the Ballistic Research Laboratory. In World War II Veblen was again called upon to guide the work of the Laboratory, and throughout the war was its chief scientist. The Laboratory’s interest in computing machines goes back at least to 1935 with its acquisition of a Bush differential analyzer. Soon after, it moved in the direction of digital computation with punch-card equipment. Its involvement in the ENIAC project was a natural outgrowth of these activities.

The ENIAC

I have come to the technological part of my story: the history of computing machines, and especially of the electronic computer. This is a history full of fascination but to dwell on it here would take me too far from my central theme. I must, however, remind you of where the ENIAC enters this history. That is quite simple: the ENIAC was the first multipurpose, electronic, digital computing machine, both in design and construction. It came at a time—1943 to 1945—when the electromechanical computer, pioneered at Harvard, M.I.T., Bell Telephone Laboratories, and IBM, had reached its zenith. As I said before, the ENIAC was built at the Moore School under contract with the Ordnance Department of the U.S. Army. The chief engineer was J. Presper Eckert, acting initially on basic concepts developed by John W. Mauchly, who served as principal engineering consultant. Herman H. Goldstine, representing the Ordnance Department, actively promoted the project.

The ENIAC became operational in December 1945 with an urgent problem from Los Alamos, and was formally dedicated a few months later. In 1947 it was moved to Aberdeen and there it continued to serve until 1955 when, overwhelmed by obsolescence this historic machine finally was put to rest. It has been in the custody of the Smithsonian Institution since 1962.

The ENIAC was a behemoth (Fig. 4). A contemporary description^{7,8} lists 18 000 vacuum tubes, 70 000 resistors, 10 000 capacitors, 6000 switches. Its 42 main panels, each about 2 ft wide, were arranged along three walls of a large room. They were about 10 ft high, 3 ft deep, and

⁷ Goldstine, H. H. and A. Goldstine, 1946: The Electronic Numerical Integrator and Computer (ENIAC). *Mathematical Tables and other Aids to Computation*, 2, 97–110.

⁸ Berkeley, E. C., 1949: *Giant Brains, or Machines that Think*. New York: John Wiley & Sons, 270 pp. (see Chapter 7).

stored on the Function Tables and therefore could not be changed internally, but this was not a fatal deficiency. By the time the meteorology group appeared on the scene in 1950, ENIAC had been operating successfully in the stored-program mode for more than a year, a fact that greatly simplified our work.

Quasigeostrophic prediction

Having concluded these brief glances at the institutional and technological background for ENIAC 1950, and crossed the borderland between technology and science, we have come to what I believe to be the central historical problem of our topic. That problem is to trace the origin of the quasi-geostrophic prediction equations, and in particular, the quasi-geostrophic barotropic vorticity equation, the scientific basis for the ENIAC calculations of 1950 and 1951. The quasi-geostrophic prediction equations made their first authentic appearance in 1948 in a monograph by Jule Charney⁹ and independently a few months later in a monograph by Arnt Eliassen.¹⁰ The objectives of these papers are quite different. Charney directly attacked and solved a fundamental and long-standing problem of dynamic meteorology: how can the general equations of hydrodynamics, formulated more than a century before, be adapted to give a self-consistent and mathematically-tractable description of large-scale motions of the atmosphere? Eliassen, on the other hand, laid out the first comprehensive treatment of isobaric coordinates in meteorology, illustrated by applications to the theory of gravity waves in a stationary atmosphere, to vorticity waves on a zonal flow, and to quasi-geostrophic prediction equations.

The difficulties in the way of a physical theory of large-scale motions of the atmosphere were partly seen as early as 1904 by Margules, who noted the great sensitivity of surface pressure tendency to small changes in horizontal wind velocity, and by Dines a decade later, who described the nearly compensating juxtaposition of layers of convergence and divergence of mass, as well as of warm troposphere, cold stratosphere and vice versa. These peculiarities of large-scale air motions are manifestations of a pervasive state of near balance between the dominant forces, namely pressure gradient, gravity, and Coriolis force. Starting with Guldberg and Mohn in the 1870s, atmospheric dynamicists gradually perceived the fundamental importance of this state of quasi-balance, and explored its consequences in various ways. But until the work of Charney and of Eliassen, they had not succeeded in describing it in a predictive mathematical way.

Why did it take almost three-quarters of a century to arrive at the quasi-geostrophic prediction equations? Surely the length of that gestation was not for want of great analytical and creative minds, such as those of the

Norwegian school—V. and J. Bjerknes and Solberg, of the English school—Richardson and Jeffreys, and above all of C.-G. Rossby. This is a problem of historical analysis that is of great interest and doubtless much complexity, involving the multiplicity of technical, economic, social, and human factors that together make up the dynamics of scientific progress. It would be a worthy study for an historian of science, and I hope it will some day be done. I shall not presume to venture in that direction today.

Interlude

Ladies and gentlemen, I have a confession to make. We live in an age when electronic miracles have become commonplace and we have grown immune to any sense of awe and astonishment at what 30 years ago would have been literally unbelievable. I confess that I want to try to dramatize for you the incredible achievements of computer technology since the days of ENIAC, and I have asked the IBM Corporation to help me do so. They graciously consented and have arranged for us what should be an impressive demonstration, for which I am most grateful to Sheryl Tutaj of IBM Chicago and Ron Frank of IBM Cambridge.

On this table is an IBM 5110 Portable Computer, which as you see is not much larger than a typewriter. Alongside this desk-top computer is a small printer on which hard-copy output can be taken, and there is also a television monitor on which output can be viewed more readily. Mr. Ron Frank of IBM Cambridge has taken the weather-forecast program exactly as it was designed for the ENIAC and has coded it to run on this small machine. He has just now entered some control data and has started the computation of one of the forecasts made on the ENIAC in 1950. Before the end of my lecture, barring accidents, the machine will have completed the 24 h forecast. Meanwhile, we can go about our business without paying any attention to it.

Narrative

On the first Sunday of March 1950 an eager band of five meteorologists assembled in Aberdeen, Maryland, to play their roles in a remarkable exploit. On a contracted time scale the groundwork for this event had been laid in Princeton in a mere two to three years, but in another sense what took place was the enactment of a vision foretold by L. F. Richardson 50 years before. The proceedings in Aberdeen began at 12 p.m. Sunday, March 5, 1950 and continued 24 hours a day for 33 days and nights, with only brief interruptions. The script for this lengthy performance was written by John von Neumann and by Jule Charney, who also was one of the five actors on the scene. The other players at Aberdeen were Ragnar Fjørtoft, John Freeman, Joseph Smagorinsky, and I.

In trying to set out a narrative of the five weeks at Aberdeen I am hampered by a fading and capricious memory. I can recall such moments of crisis as when a thumb of one of the operators got caught in a punch-card machine, but the sustained continuity of our actual

⁹ Charney, J. G., 1948: On the scale of atmospheric motions. *Geofysiske Publikasjoner*, 17(2), 17 pp.

¹⁰ Eliassen, Arnt, 1949: The quasi-static equations of motion with pressure as independent variable. *Geofysiske Publikasjoner*, 17(3), 44 pp.

work is now beyond my powers of recall. Fortunately, Dr. Charney has preserved the log book in which are recorded, day by day, the stages of progress and regress in the ENIAC operations, interlaced with occasional lapses of frivolity or anguish. While groping for a less clinical alternative to the log book, it occurred to me that my wife might have saved my letters to her from that period and I was delighted to find that indeed she had.

Before giving you the flavor these contemporary relics provide, I want to draw your attention briefly to what for any future historian of this period will be an interesting and pertinent question: Where does Carl Rossby (Fig. 5) fit into the ENIAC picture? I have mentioned his presence at von Neumann's conference of August 1946, and it was to Rossby—the leading theoretical meteorologist of the country—that von Neumann naturally turned in the formative stages of his meteorology project. However, in terms of specific acts or documented scientific input Rossby did not play a direct role in the ENIAC project. Was he disinterested? Did he misjudge the significance of the project?

By no means! We must recall that in 1947 Rossby began the process of disengagement from Chicago and establishment of his final base of operations in Stockholm. During this transition period I received several letters from him that bear on our subject. In March 1948 from Stockholm he wrote about dispersion in barotropic planetary waves on a uniform zonal current and the fact that in a model with free surface the group velocity has both an upper and a lower limit. He had already alluded to this in a remarkable paper published three years earlier, entitled "On the propagation of frequencies and energy in certain types of atmospheric and oceanic waves". He returned to that subject in February 1949 as his contribution to volume 1, number 1, of the new journal *Tellus* that he had just then launched in Stockholm. In this brief note, entitled "On the dispersion of planetary waves in the atmosphere", he treats what he considered a fundamental problem for the as yet unborn science of numerical weather prediction, namely:

... the maximum speed at which the "influence" of a given source point is propagated and dispersed into the environment. . . .

His intense interest in the dawning of numerical weather prediction is evident in a letter dated 9 October 1948, written a few months after Charney had arrived in Princeton. He urged me then:

By all means get a statement from Charney on the present status of the computing project. I am by now completely convinced that the pilot project discussed last summer and carried further by Charney in a recent memo to von Neumann, namely the computation of the state of motion in an "equivalent" barotropic atmosphere (with enough convergence and divergence to keep signal velocities finite), is not only psychologically important to the Princeton group but could also lead to highly significant practical results.

My final reference to Rossby comes from a letter written a few months later, on May 8, 1949, about a year



FIG. 5. Carl-Gustaf Rossby (1898–1957).

before the first numerical weather forecast on the ENIAC. In this letter he describes the linear one-dimensional model of Charney and Eliassen, then in course of publication in *Tellus*. With typical Rossby enthusiasm he characterizes their method, designed for hand computation, as "extraordinarily promising". Continuing, he writes:

The following questions now arise: Is it not urgently required to push this method to the limit and to put a solid, physical and objective method, though it be based on coarse approximations, in the hands of forecasters? Namias, who is here, got extremely excited (about) the whole thing and wrote to Harry Wexler to have him interview Charney with the idea of introducing the method in the Weather Bureau. . . .

Rossby then suggested an extension of the Charney/Eliassen procedure, and concludes with these comments:

It seems to me that we now must go on . . . to a systematic test and extension of Charney's method so as to get rid of the horrible subjectivity which still characterizes all, or almost all forecast efforts. . . .

I must confess that I have an extremely strong feeling that we are standing at the threshold of a new era in applied meteorology and that we must push this line to the point where it can be put in general operation. . . .

Clearly, Rossby was not aloof to the auguries that were foreshadowing a revolution in the science of meteorology. Those of you who knew him personally will remember that an abiding concern for deductive advancement of the practical art of weather forecasting was characteristic of Rossby, notwithstanding his natural inclination toward theory. Lord Kelvin, in whom resided an extraordinary

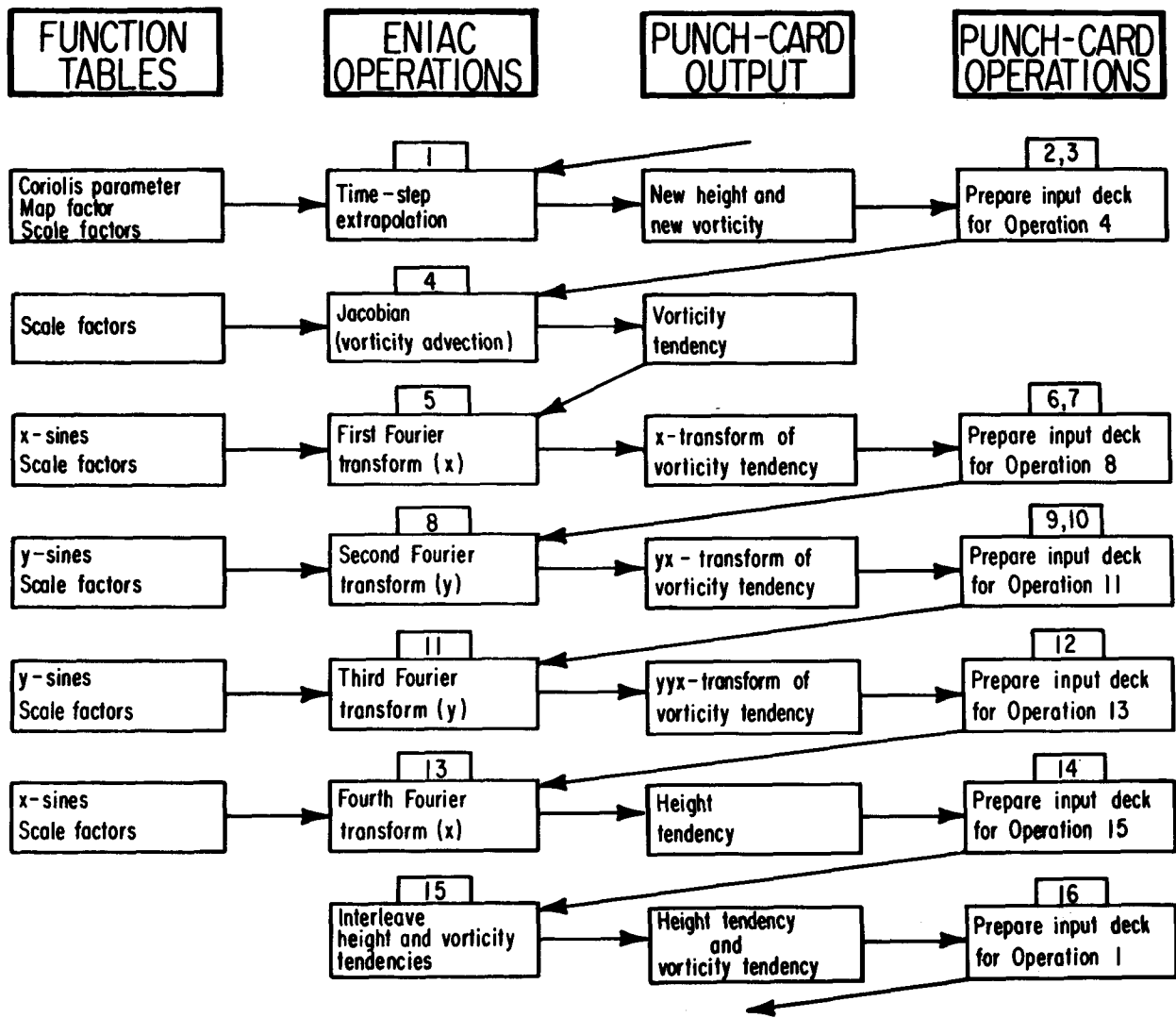


FIG. 6. The 16 operations in each time step of the first numerical weather forecast.

genius for harmonizing the contrasting motivations for pure and applied science had this to say:¹¹

There cannot be a greater mistake than that of looking superciliously upon practical applications of science. The life and soul of science is its practical application, and just as the great advances in mathematics have been made through the desire of discovering the solution of problems which were of a highly practical kind in mathematical science, so in physical science many of the greatest advances that have been made from the beginning of the world to the present time have been made in the earnest desire to turn the properties of matter to some purpose useful to mankind.

A few moments ago I mentioned letters to my wife. I hope you will excuse their homely style—clearly not designed for publication. On Friday, March 3, 1950, I wrote from Princeton

... there will be 5 of us—and we will take 8-hr shifts.

¹¹ Thomson, W. (Lord Kelvin), 1891: *Popular Lectures and Addresses. Volume I. Constitution of Matter*. Second Edition. London, Macmillan, 470 pp. (see pp. 86–87)

Also, 3 ENIAC people will be assigned to the project. The operations are due to start at midnight. . . .

At 3:45 a.m. the following Monday I wrote from Aberdeen

We came down here by train Sunday from Trenton. . . . So far it's just preparatory work for a couple days—testing, etc. The machine is a miracle.

Two days later the report was:

The ENIAC work is moving rather smoothly. . . . We have almost finished the main part of the various checking routines . . . and it looks now like we may be able to begin actual operations tomorrow, or the next day at the latest. . . . Please tell Prof. Rossby . . . we are expecting him on Tuesday, and . . . would like to know his estimated arrival time.

After another two days, on Friday, March 10, I wrote:

At last I have a break. The actual computation began about 36 hours ago. . . . However, we find that the ENIAC is a little slower than was anticipated so that some re-

coding has to be done. We worked through the night last night planning for this and are now about ready. . . .

About a week later, on March 16, I said

While I'm . . . waiting for the ENIAC to behave properly, I'll write a few lines. . . . Ragnar is . . . with me on this shift, but there hasn't been much for us to do because the machine has been making errors since about 0300. Rossby came and left on Tuesday. First he called from Washington and said he would arrive on a 1:00 PM bus from Baltimore. I met the bus but he didn't show up. Then he called and said he would come in on a 2:45 train which I met. He said he discovered when he got to Baltimore, that he left his wallet in a ticket office in Washington, and found only two pennies in his pocket. He was rescued by Traveler's Aid and had to go back to Washington that night to pick up the wallet, which was found. . . .

Finally, on March 18, the 13th day, I wrote:

We have completed a 12-hr forecast and although I have not seen the . . . results, the others seem to feel definitely encouraged.

A few days later on March 22, I was obliged to leave Aberdeen to return to my duties in Chicago for the start of the Spring Quarter.

The computer program

To this point in my lecture I have spared you the technical details of what actually was done during those five weeks in Aberdeen. I hope you will agree that a general account of ENIAC 1950 should give some attention to the operational procedure, and it is with such matters that I approach the end of my story today. The official description of the calculations is, of course, the paper by Charney, Fjørtoft, and von Neumann. This contains all that is essential for scientific posterity to know, but does not dwell on the intimate details that give a more vivid picture of the experience. That is what I shall try to convey to you.

Please recall now that ENIAC's direct-access read/write memory consisted of fewer than 10 registers available to the programmer, and that the Function Tables provided a direct-access but read-only memory of about 600 words. Thus, it devolved upon punch cards to serve as the large-capacity read-write memory, and this mandated an intimate coupling of punch-card operations with ENIAC operations, ingeniously contrived by von Neumann. Here you see a flow diagram (Fig. 6) of the 16 successive operations that had to be performed in each time step. Six of these were ENIAC operations, which did all the arithmetic, and 10 were punch-card operations, which carried out the various rearrangements and preparations of data required by the ENIAC operations. Each ENIAC operation produced an output deck through the card punch, and this was used to create other decks by means of reproducing, collating, and sorting operations.

I can illustrate this process by referring to Operations 2 and 3. These are card operations designed to prepare the input deck for Operation 4—the ENIAC operation in which vorticity advection is computed. Operation 1 is the ENIAC operation that does the time extrapolation

to new values of pressure height and geostrophic vorticity. The output deck from Operation 1 is the input for Operation 2, where it is first duplicated, then altered manually, then triplicated, and then altered manually again. In Operation 3 the result of these manipulations is subject to two interfiling procedures on the collator. After all of these steps, each fraught with the peril of human error, we have a deck ready for the ENIAC in Operation 4.

Of the arithmetic operations performed by ENIAC, the only nontrivial ones were the four in which the Laplacian was inverted to solve Poisson's equation, namely Operations 5, 8, 11, and 13. The procedure adopted by von Neumann for this purpose was expansion in eigenfunctions of the Laplace operator. Since the domain was a rectangle and the boundary values zero, each eigenfunction is simply the product of a sine function of x and a sine function of y . The discrete solution of Laplace's equation obtained in this way is a quadruple summation, each of the four sums being equivalent to a discrete Fourier transform. Two of the sums correspond to the x - and y -transforms of the vorticity advection, and these are carried out in Operations 5 and 8. The other two, equivalent to x - and y -transforms, correspond to eigenfunction summations. These are done in Operations 11 and 13. Numerous card-reproducing and strictly manual operations intervene between the first and fourth Fourier transforms.

In the terminology of numerical analysis, the method I have just described for inversion of the Laplacian is one of a class of direct methods in which an exact solution of the discrete problem is obtained in a finite number of steps. Von Neumann chose this method because of the peculiar limitations of the ENIAC. Later, however, on the Institute for Advanced Study computer, with a direct-access read/write memory of 1024 words, iterative methods were used exclusively and indeed, were dominant in numerical weather prediction throughout the 1950s and 1960s. In the past decade the pendulum has been swinging back to direct methods, which are much improved since the days of ENIAC.

You will have a better appreciation for the somewhat cryptic entries of the log book if I tell you first about the difficulties we had to contend with. These were of two kinds: operational problems and programming problems. Probably the main source of operational trouble was the ENIAC itself, which had a mean error-free path of only a few hours and often took many hours to repair. Each of the 20 accumulators was dependent on 550 vacuum tubes! Malfunction of the punch-card equipment, while not so time-consuming, was nevertheless a constant background of interruption to our progress. Human error was not a major problem because, fortunately, the operators were highly skilled, but it did inevitably happen somewhere in the 14 separate punch-card procedures required in each time step. What of programming problems? Yes, of course we made coding errors. Most of these were the usual blunders that every programmer lives with; some were mistakes arising from

subtle idiosyncrasies in the command structure of ENIAC. Of all the difficulties that plagued us, however, by far the most baffling and certainly the most disconcerting was the assignment of scale factors for the individual ENIAC operations. These factors intruded because ENIAC was strictly a fixed-point machine, each register holding a fixed-point decimal number with 10 digits and sign. The purpose of the scale factors was to prevent overflow or underflow or simply excessive loss of significance in each of the various ENIAC operations. This was accomplished by normalizing every array to the interval -1 to $+1$. Some of the scale factors needed to do this had to be found by trial and error.

The log book

Permit me finally to take you on a brief excursion through the log book¹². All of the five who I previously mentioned made entries in the log, but in reading these extracts I shall not stop to identify the individual writers.

- 5 March 1530 Arr. Aberdeen. Conference . . . at BRL. Checked Op. 15 code and scale factor for Op. 8.
- 6 March 0000 Set F Tables and tested. ENIAC mostly inoperative.
- . . .
- 7 March 0000 ENIAC tested. Op. 1 . . . begun.
- . . .
- 8 March 0000 Began checking inputs and outputs of Op. 1 . . . Op. 1 output duplicated . . . and interfiled.
- 1600 . . . Tabulated Op. 4 output . . . —9 errors. Corrected input of Ops. 1 and 4. Retabulated Op. 4 . . .
- 9 March 0000 Ran Op. 5 with modified output Op. 4. Found errors in scaling of . . . (Jacobians) which contributed to disagreement with hand computed . . . (Fourier transforms.) Corrected errors and prepared new input to Op. 5 . . .
- 0500 Finished Op. 5 on ENIAC . . . Op. 5 takes 23 minutes. Operation 15 takes 7 minutes . . .
- . . .
- 0930 Finished operation 8. Apparently, the scale factor .28 is not yet on ENIAC . . .
- . . .
- 1110 Von Neumann called— . . . “Where are you now?” . . . Testing code. . . How much more testing? About three more hours . . . Are you going to start today? Will be cutting cards . . . Are you going to start tomorrow? Yes . . . What is wrong with projection? Too many grid points. . . It is unstable too? It might be.
- . . .
- 10 March 0045 . . . Output of Op. 8 disagreed with hand-computed . . . (values). All . . . (in first row) disagreed; all . . . (in first column) disagreed; 2 other . . . (values) disagreed. The last were found to be errors in the scaling of hand computed . . . (values). The

second set was tracked down to an incorrect setting on the F.T. The first set . . . was found to be due to a great loss of significance . . . Nothing can be done about this. The final output was duplicated and is now regarded as correct. Began Op. 8 for zeroth time step.

- 11 March 1145 Von Neumann called. I said progress rather good—3rd time step . . . He asked how is Mercator. I said ok but takes too long (about 4 hr ideally)—we will finish 12-hr Mercator forecast sometime Wednesday, then switch to stereographic which we think will be about twice as fast. He said we should try a 2-hr time step and compare with two 1-hr steps. He asked about operating efficiency. I said I guess about 60% (mostly because of IBM equipment but also due to ENIAC) . . .

. . .

1745 . . . Noted that the boundary . . . (Jacobians) are growing at an uncomfortable rate. Discussed possibility of revising method of extrapolation.

- 12 March 0130 Spent a restless night and morning worrying about the growth of the tendencies at the lower left hand boundary of the forecast area. Thought up countless explanations.
- 1400 Found that growth was not fault of basic mathematics but was a concatenation of two errors of computation . . . (a) ENIAC errors . . . (b) coding errors: boundary . . . (tendencies) all wrong because they should be printed 0.50000 but are coded to print 0.00000. Sought for a method to revise code for Op. 15 with minimum change . . .

Thus ended the first of five weeks—an initiation into the harsh realities of an undeveloped technology that exacted much toil and torment but ultimately yielded rich rewards.

Epilogue

I can think of no better way to close this narrative than to let Jule Charney do so in his own words, taken from a letter he wrote to me from Princeton on April 10, 1950, a few days after the expedition had completed its work in Aberdeen. This letter reflects the mood at the end of that historic marathon experience.

We returned from Aberdeen only Saturday, after having been given a week's extension on the ENIAC. The extra week made all the difference in the world. At the end of four weeks we had made two different 24-hr forecasts. The first, as you know, was not remarkable for accuracy, although it had some good points. . . . The second was made from the Jan. 31, 1949 map. . . . It turned out to be surprisingly good. Even the turning of the wind over western Europe and the extension of the trough, which Ragnar thought to be a baroclinic phenomenon, was correctly forecast. . . . Thus at the end of 4 weeks of work it appeared that the barotropic model gives good results for large-scale systems, but there was too little evidence to back up this conclusion. . . . If we had had to stop there, we would still have been up in the air. But in the next week we . . . made a 24-hour forecast for Jan. 31 . . . 1949 . . . and a 24-hr forecast for Feb. 14,

¹² Handwritten log in small memo book, 41 pp. (in possession of Jule Charney).

1949, in which two cutoffs occur. The results showed . . . that with certain well marked exceptions, the large scale features of the 500 mb flow can be forecast barotropically. The exception was the case where . . . a marked cyclonic development took place in the lower part of the eastern american trough. There is no doubt in our minds that this was a truly baroclinic phenomenon. . . . All in all I think we have enough evidence now to bear out most of Rossby's prophecy. Of course we shall want more. . . .

Ladies and gentlemen, this concludes the substance of my lecture, but bear with me a moment longer. In his letter Dr. Charney referred to a 24 h forecast for February 14, 1949. IBM's disarmingly petite computer has now finished its work and produced this same forecast. Here you see the initial and forecast maps it created in a test run made before my lecture. You are welcome to come up and examine the machine and its output, and

also inspect the original ENIAC log book that Dr. Charney has allowed me to put on display.

Dr. Lorenz, may I thank you and your colleagues again most cordially for having given me this opportunity to join you in memorializing Victor Starr by sharing with you the recollection of a significant event. May I also congratulate you on the golden anniversary of your distinguished Department of Meteorology.

Acknowledgments. I am grateful to Jule Charney for advice and for access to archival materials; to the IBM Corporation for the computational demonstration that accompanied the lecture—in particular to Ron Frank for programming and conducting the demonstration and Sheryl Tutaj and Richard MacKinnon for administrative arrangements; to Jane McNabb for Fig. 2; to Herman Goldstine for Fig. 3; to Michael Romanelli for Fig. 4; and to Edward Lorenz for inviting me to give this lecture. ●

announcements¹

Conference on Ozone/Oxidants—Call for papers

Papers are being invited for a Symposium on Ozone and Oxidants sponsored by the Air Pollution Control Association, TT-5 (Interactions with the Total Environment), and TT-2 (Chemistry). The symposium, which will be held 15–17 October 1979 at the Galleria Plaza Hotel in Houston, Tex., will focus on recent developments, activities, and policy concerning sources, generation, transport, and effects of ozone and oxidants. The subject matter will concentrate on new information developed since the last Ozone Specialty Conference held in Dallas, Tex. Topics will include, but are not restricted to: measurement techniques, transport and modeling methods, technology, health effects, standards and justification for changes, effect on socioeconomic and permitting activities, laboratory studies, and natural versus anthropogenic

sources. Some solicited papers will be presented. Recent studies in the Houston and northeastern regions are desirable. If sufficient numbers of qualified papers are received, double sessions may result.

Titles with short abstracts (approximately 200–300 words), typed and double spaced, should be sent no later than 1 July 1979 to: Dr. Paul R. Harrison, Technical Chairman, MRI, 464 West Woodbury Rd., Altadena, Calif. 91001. Notification as to acceptance will occur prior to 30 July 1979. Papers must be submitted in final form by 1 September 1979. Due to the shortness of time, it will be necessary to adhere closely to the deadlines. Contributors are advised not to submit abstracts for work not already completed at the time of writing the abstract.

Questions concerning advertising, display booths, and local arrangements should be addressed to: Mr. Kenneth W. MacKenzie, Ozone/Oxidant Local Chairman, ERT, 6630 Harwin Drive, Suite 175, Houston, Tex. 77036 (tel: 713-791-1901). Questions concerning technical matters should be addressed to Harrison, at the address given for him above.

¹ Notice of registration deadlines for meetings, workshops, and seminars, deadlines for submittal of abstracts or papers to be presented at meetings, and deadlines for grants, proposals, awards, nominations, and fellowships must be received at least three months prior to deadline dates.—*News Ed.*