Turing's Cathedral

The Origins of the Digital Universe

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working life. I was one of a small band of eight- to ten-year-olds who spent our free time exploring the Institute Woods, and occasionally visited the barn. A few beams of sunlight perforated the roof through dust raised by pigeons that fluttered away from us overhead.

Julian's cache of war surplus electronics had already been scavenged for needed parts. We had no idea what most of it was—but that did not stop us from dismantling anything that would come apart. We knew that Julian Bigelow had built a computer, housed in a building off-limits to children, just as we knew that Robert Oppenheimer, who lived in the manor house belonging to the barn, had built an atomic bomb. On our expeditions into the woods, we ignored birds and mammals, hunting for frogs and turtles that we could capture with our bare hands. It was still the age of reptiles to us. The dinosaurs of computing, in contrast, were warm-blooded, but the relays and vacuum tubes we extracted from their remains had already given up their vital warmth.

I was left with an inextinguishable curiosity over the relics that had been abandoned in the barn. "Institutes like nations are perhaps happiest if they have no history," declared Abraham Flexner, the founding director of the Institute for Advanced Study, in 1936. It was thanks to this policy of Dr. Flexner's, maintained by his successors, including Oppenheimer, regarding the history of the Institute in general, and the history of the Electronic Computer Project in particular, that much of the documentary material behind this book lay secreted for so many years. "I am reasonably confident that there is nothing here that would interest him," Carl Kaysen, Oppenheimer's successor, noted in response to an inquiry, in 1968, about records concerning the von Neumann computer project from a professor of electrical engineering at MIT.[2]

Thanks to former director Phillip Griffiths, with support from trustees Charles Simonyi and Marina von Neumann Whitman, I was invited to spend the 2002–2003 academic year as Director's Visitor at the Institute for Advanced Study, and granted access to files that in some cases had not seen the light of day since 1946. Historical Studies–Social Science Librarian Marcia Tucker and Archivist Lisa Coats began working to preserve and organize the surviving records of the Electronic Computer Project, and Kimberly Jacobsen transcribed thousands of pages of documents that are only sparsely sampled here. Through the efforts of current director Peter Goddard, and a gift from Shelby White and the Leon Levy Foundation, a permanent Archives Center has now been established at the IAS. Archivists Christine Di Bella, Erica Mosner, and all the staff at the Institute, especially Linda Cooper, helped in every capacity, and the current trustees, especially Jeffrey Bezos, have lent continuing encouragement and support.

Many of the surviving eyewitnesses—including Alice Bigelow, Julian Bigelow, Andrew and Kathleen Booth, Raoul Bott, Martin and Virginia Davis, Akrevoe Kondopria Emmanouilides, Gerald and Thelma Estrin, Benoît Mandelbrot, Harris Mayer, Jack Rosenberg, Atle Selberg, Joseph and Margaret Smagorinsky, Françoise Ulam, Nicholas Vonneumann, Willis Ware, and Marina von Neumann Whitman—took time to speak with me. "You're within about five years of not having a testifiable witness," Joseph Smagorinsky warned me in 2004.

In 2003 the Bigelow family allowed me to go through the boxes of papers that Julian had saved. In one box, amid Office of Naval Research technical reports, World War II vacuum tube specification sheets, Bureau of Standards newsletters, and even a maintenance manual for the ENIAC, stamped RESTRICTED, was a sheet of lined paper that had evidently been crumpled up and thrown away, then uncrumpled and saved. It had been turned sideways, and had one line of handwriting across the top of the page, as follows:

> Orders: Let a word (40bd) be 2 orders, each order = C(A) = Command (1–10, 21–30) • Address (11–20, 31–40)

The use of *bd* for *binary digit* dates this piece of paper from the beginning of the von Neumann project, before the abbreviation of *binary digit* to *bit*.

"In the beginning," according to Neal Stephenson, "was the command line." Thanks to Neal, and many other supporters, especially those individuals and institutions who allowed me into their basements, I spent an inordinate amount of time, over the past eight years, immersed in the layers of documents that were deposited when the digital universe was taking form. From Alex Magoun at RCA to Willis Ware at RAND, and many other keepers of institutional memory in between—including the *Annals of the History of Computing* and the Charles Babbage Institute's oral history collection—I am indebted to those who saved records that otherwise might not have been preserved. To a long list of historians and biographers—including William Aspray, Armand Borel, Alice Burks, Flo Conway, Jack Copeland, James Cortada, Martin Davis, Peter Galison, David

With the war approaching, Charney, who was scraping by as a teaching assistant in mathematics and physics, had to decide whether to pursue meteorology, which interested him, or aeronautics, which promised to be more useful for the war. He went to see aeronautical pioneer Theodore von Kármán at Caltech, who advised meteorology, explaining that aeronautics had matured to where future progress would be made through engineering, not mathematics, whereas meteorology was ripe for a mathematical approach. Charney never looked back. Jacob Bjerknes had recently come to UCLA from Norway to launch a training program for meteorologists, and Charney joined the new department as a teaching assistant in July of 1941 at sixty-five dollars a month.

Charney had a remarkable ability to condense the entire atmosphere, from planetary to molecular scale, into equations that captured what was important and discarded what was not. "Voltaire's giant, Micromegas, who stood in the same relation to the atmosphere as we do to a rotating dishpan, would describe the atmosphere as a highly turbulent, heterogeneous fluid subject to strong thermal influences and moving over a rough, rotating surface," he would later write. "He would discern a mean zonal circulation with westerly surface winds increasing with height in middle latitudes in both hemispheres and easterly surface winds near the Equator and poles." Closer examination would reveal perturbations related to the uneven distribution of the continents and oceans, and superimposed on these quasi-permanent features "he would find a whole collection of migratory vortices, varying in scale from thousands of kilometers to centimeters and less, but with the bulk of the energy in the thousand-kilometer class."[33]

By the end of the war Charney was in the middle of his PhD thesis, "The Dynamics of Long Waves in a Baroclinic Westerly Current," completed in 1946. Newly married, he and Elinor Charney (née Frye) then left Los Angeles for Chicago and his first postdoctoral appointment, with Rossby, who invited him to the Princeton conference in August, where he met von Neumann, learned of his ambitions, and sensed that there was a bit too much mathematics, and too little meteorology, at the IAS. He and Elinor sailed for Bergen and Oslo in the spring of 1947, where he worked among the Norwegians until returning to Princeton in the early spring of 1948.

Charney had arrived at the right place at the right time. The computer was undergoing initial testing, and the first problems were being coded in anticipation of there being a machine on which they could run. A rotating contingent of Norwegian meteorologists, led by Arnt Eliassen and Ragnar Fjørtoft, joined the group. Charney became the liaison between the hands-on experience of the Norwegian forecasters and the mathematical world of von Neumann. "Although I had a fairly clear idea of what I wanted to do in the physical sense, I had only the vaguest notion of how to do it mathematically," Charney explains.[34] Von Neumann's skills were exactly the reverse. Charney also attracted and nurtured a number of extraordinary American meteorologists, notably Joseph Smagorinsky and Norman Phillips, who played a leading role in the realization of numerical weather prediction over the next ten years.

Smagorinsky was still a graduate student after the war when Charney came to give a talk at the Weather Bureau headquarters at Twenty-fourth and M streets in Washington, D.C., at a time when numerical forecasting was almost entirely alien to the way the Weather Bureau worked. "During the war, when I was a student, a cadet at MIT, I had been told by one of the eminent professors there, Bernhard Haurwitz, that numerical forecasting can't be done," says Smagorinsky. "And the reason given was not a very good one. But it was easier to say that it can't be done than it can be. And I carried this notion of impossibility in my mind." After Charney's talk, Smagorinsky asked the only question that showed any real thinking about the problem, and Charney invited him to join the new computing group.

"The primary reason for Richardson's failure," Charney noted in the first progress report he prepared for the Office of Naval Research, "may be attributed to his attempt to do too much too soon."[35] It was the meteorological community as a whole who solved Richardson's first problem: gathering sufficient data to establish initial conditions—it soon being recognized that within days the notion of "boundary conditions" disintegrated and it was necessary to have hemispheric knowledge, the boundary between Northern and Southern hemispheres being the only one that held up over time. It was von Neumann, Goldstine, and Bigelow who solved Richardson's second problem: supplying enough computing power to do the job. And it was Charney who did the most to solve Richardson's third problem: formulating equations whose solutions did not quickly become more unstable than the weather itself. The key was to filter out noise.

"The atmosphere is a musical instrument on which one can play many tunes," Charney explained to Thompson in February of 1947. "High notes are sound waves, low notes are long inertial waves, and nature is a musician more of the Beethoven than the Chopin type. He much prefers the low notes and only occasionally plays arpeggios in the treble and then only with a light hand. The oceans and the continents are the elephants in Saint-Saëns' animal suite."[36]

Thompson was listening, and reported to the Office of Naval Research that "the hydrodynamic equations cover the entire spectrum of events, sonic waves, gravity waves, slow inertial waves, et cetera, and it might simplify matters considerably if those equations were somehow informed that we are interested in only certain kinds of atmospheric behavior—i.e., the propagation of large-scale disturbances."[37] With Charney's help, numerical filters were soon constructed, and incorporated into codes that, with many hundreds of hours of hand-computing by Margaret Smagorinsky, Norma Gilbarg, and Ellen-Kristine Eliassen, were taken for trial runs. "The system that they were going to use on the big computer, we were doing manually," says Margaret Smagorinsky. "It was a very tedious job. The three of us worked in a very small room, and we worked hard. It was a small room with three people and three Monroe calculating machines."[38]

With the delays in the completion of the computer—and the priority given to the hydrogen bomb problems—it was decided to run a full-scale trial calculation on the ENIAC instead. In March of 1950, Charney was joined by George Platzman, Ragnar Fjørtoft, John Freeman, and Joseph Smagorinsky on an expedition to Aberdeen. They were guided by Klári von Neumann, who helped code their problem and initiated them into the ways of the ENIAC and its peripheral card-processing machines.

"The enactment of a vision foretold by L. F. Richardson 50 years before . . . began at 12 p.m. Sunday, March 5, 1950, and continued 24 hours a day for 33 days and nights, with only brief interruptions," Platzman reported.[39] "We have completed a 12-hour forecast," he noted, thirteen days later, in his journal for March 18. "At the end of four weeks we had made two different 24-hour forecasts," Charney reported on April 10. "The first . . . was not remarkable for accuracy, though it had some good points. . . . The second . . . 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."[40] Over the course of the next week they made two more twenty-four-hour forecasts, for January 31 and February 14, 1949.

Because of the limited internal storage, "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." Each step of the calculation required sixteen successive operations: six for the internal ENIAC arithmetic, and ten for the external punch-card operations to process the results and prepare for the next step. "In the course of the four 24-hour forecasts about 100,000 standard I.B.M. punch cards were produced and 1,000,000 multiplications and divisions were performed," Charney, von Neumann, and Fjørtoft reported. Once the bugs were worked out, "the computation time for a 24-hour forecast was about 24 hours, that is, we were just able to keep pace with the weather."[41]

Charney and his colleagues returned to Princeton in triumph. "It mattered little that the twenty-four hour prediction was twenty-four-hours in the making," he explained. "That was purely a technological problem. Two years later we were able to make the same prediction on our own machine in five minutes."[42] Emboldened by their success, they developed a series of increasingly detailed models of the atmosphere over the Northern Hemisphere by day, while livening up the atmosphere at the housing project by night.

"Oh, we loved him," says Thelma Estrin, of Charney. "He was warm and friendly and loved parties and was always the last one to go home." The engineers and the meteorologists, living together in the close-knit Mineville barracks, drew the other academic visitors into their fold. "All the meteorologists were great fun, hard drinkers," says Hungarian topologist Raoul Bott, who arrived, with a degree in engineering, as a protégé of von Neumann's in 1949. "We had tremendous wild parties," he remembers. "It was a high point in my life."

Bott singles out one evening, just after the first ENIAC expedition, when the poet Dylan Thomas was in town. "And at about ten thirty in the evening, or maybe eleven, we were having a great party in one of those shacks there, and I thought, 'Well, wouldn't it be great to bring Dylan Thomas here now?' So I called the hotel—I was a brash young man—and got Dylan Thomas, and he had been in bed. And he said, 'Oh, I'd love to be woken up, by all means,' you know, and he was ready for partying. And so I drove there—we had this 1935 convertible

Buick—with my wife, she of course all excited, and right away when he got in the car I could see that there would be a little bit of a problem, because obviously that was going to be his girl for the night."[43]

As they refined their models, Charney's group needed a benchmark by which to gauge their predictions. For his test case, Richardson had used the otherwise uneventful morning of May 20, 1910. Charney's group chose Thanksgiving 1950, when a severe storm struck the central and eastern United States. The weather system, whose development was missed by the forecasts available at the time, caused three hundred deaths, unprecedented property damage, and even blew part of the roof off the Palmer Physical Laboratory at Princeton University. It was the perfect storm.

"Because of the spontaneity and intensity of its development this storm was selected as an ideal test case for the prediction of cyclogenesis," Charney explains. Despite the unpredictability of turbulence, he believed that "the inception and development of cyclones are determinate and predictable events." Although cyclogenesis might appear random, "the initial perturbation will have a preferred location in space and time, and its amplitude, though it may be small initially, will be entirely determined by the basic flow. It is like an automobile being pushed slowly but inexorably over a cliff."[44]

"The storm of November 25–27 was first noted on the surface weather map of 1230 GMT, November 24 as a small Low developing over North Carolina and western Virginia," began the summary in the November 1950 *Monthly Weather Review*.[45] Over the next forty-eight hours the disturbance grew to become the worst storm ever recorded over the United States. Coburn Creek, West Virginia, received sixty-two inches of snow. Records of minus 1 degree Fahrenheit were set in Louisville, Kentucky, and Nashville, Tennessee, and thirty inches of snow fell in Pittsburgh, bringing the steel industry to a halt.

"The two-and-a-half-dimensional model did not catch the cyclogenesis, [although] there was some vague indication of something going on," Charney later reported. "And so we went to a three-level model, that is, a two-and-two-thirds dimensional model, and we did catch the cyclogenesis. It wasn't terribly accurate, but there was no question that [we did]. And I always thought that this was a terribly important thing. . . . I wanted the world to know about that!"[46]

The successful 24-hour prediction, following a 16-by-16 grid of 300-kilometer cells through 48 half-hour time steps, required 48 minutes of computing time. According to Charney, "during this time the

machine performed approximately 750,000 multiplications and divisions, 10,000,000 additions and subtractions, and executed 30,000,000 distinct orders."[47] While trying to simulate weather *inside* the computer, the meteorologists were plagued by the weather *outside* the computer. The "York" refrigeration units continued to become overloaded in the sultry Princeton heat, and during thunderstorms the Williams tube memory often failed. On one very hot day in May there was trouble with the IBM card equipment, and the machine log records: "IBM machine putting a tar-like substance on cards." The next log entry explains: "Tar is tar from roof."[48]

Charney's plan to develop a hierarchy of models, incrementally more complete, now picked up steam. On August 5, 1952, von Neumann chaired a meeting at the Institute "to explore the possibilities of routine preparation of numerical forecasts by the Weather Bureau and the Air Force and Navy meteorological services." In September of 1953 the Weather Bureau, air force, and navy agreed to establish a Joint Numerical Weather Prediction Unit, and in January of 1954 a Technical Advisory Group chaired by von Neumann recommended the use of an IBM 701, the rental of which had been budgeted at $175,000 to $300,000 per year.[49] IBM delivered the computer early in 1955, and the first operational forecast was made on April 18.

By 1958, numerical forecasts were running neck and neck with manual ones, and by 1960 they had pulled ahead. Starting with twenty-four-hour forecasts, there was an improvement of about twenty-four hours per decade in extending the forecast range. The question for von Neumann and Charney was: What next? "Von Neumann seemed to feel that the problem of short range forecasting was pretty well in hand," remembers Thompson. "Well, I think he had a somewhat naive view of how far we had come and how far we still had to go, but he was looking ahead." Von Neumann divided the problem into three regimes. In the first, short-range regime, what happens depends more on the initial conditions than on the subsequent energy inputs and dissipation. With sufficient observations, and enough computing, short-range predictions, on the order of a few days to one week, can be made. In the second, medium-range regime, beyond a week, the influences become increasingly divided between those produced by the initial conditions and those introduced by the energy inputs and dissipation, and the behavior becomes very difficult, perhaps impossible, to predict. In the third, long-range regime, "the atmosphere very quickly forgets what it looked like in the beginning," as Thompson put it, "and its behavior is dominated almost entirely by the integrated day-to-day effects of the energy inputs and by the dissipation."[50] With sufficient knowledge of those inputs and dissipations, the task of predicting not weather but climate should be computationally tractable, and von Neumann and Charney, now joined by Norman Phillips and Joseph Smagorinsky, decided to tackle this next.

In September 1954, Norman Phillips began running a primitive general circulation model (the ancestor of all climate models in use today) that remained stable for up to forty days of simulated time. As Smagorinsky described it, "despite the simplicity of the formulation of energy sources and sinks, the results were remarkable in their ability to reproduce the salient features of the general circulation." Although the results became irregular and nonlinear when run beyond forty days, Phillips and Charney felt this was due to numerical instabilities, not the underlying model, and that with better coding and a more powerful computer, a true prediction of climate might be reached. "The code almost completely exhausts the resources of the present machine," they reported, "there being only about a dozen 'words' in the combined Williams-Drum memory of 3072 words which are not used." In February and March 1954, the model was run to thirty-one days and appeared "surprisingly realistic." Even "features similar to the cold and warm fronts of the classical Norwegian wave cyclone" were produced.[51]

"Our object was to establish a purely physical theory of climate, that is to say, to make the infinite forecast," Charney later explained to Stan Ulam. "Johnny foresaw that this would be a simpler problem than the problem of long-range prediction, since the statistical properties of the motion were likely to be more correct than the individual motions." We now know this is not as easy as it seemed at the time. "He always had, in the back of his mind, of course, large-scale weather modification," Charney adds. "We spent many pleasant Sunday afternoons together inventing theories of climate, and it became clear that nothing could be done to explain past climates or to lay the groundwork for climatic modification until we were able to understand our own climate in purely physical terms."[52]

To launch the new project, von Neumann and Charney hosted a conference on the dynamics of climate at the Institute on October 26–28, 1955. Oppenheimer gave the opening address, drawing "a parallel between the present conference dealing with problems of the general circulation of the earth's atmosphere and the conference held at Los Alamos, New Mexico, in preparation for work on the atomic bomb," and noting that "the problem which faces the participants of the present conference—a problem dealing with the complicated dynamics of atmospheric motions—is a much more difficult one."[53]

Von Neumann, while a master of simplifying assumptions, was realistic about the obstacles. "Even if we were adequately informed, the inclusion of turbulence and radiation in the prediction equations would be quite involved," he announced. Nearly all the phenomena under consideration were unstable, and minute differences could be amplified into large effects. "For example, only about $1/100{,}000$ of all the water on earth occurs in vapour form in the atmosphere; yet the presence of water vapour makes a difference of 40°C in the average temperature of the earth," he observed. "This is more than twice the difference between the temperature at the time of maximum glaciation and that at the time of total deglaciation of the earth."[54]

The twenty-nine attendees, although hopeful about modeling the climate, acknowledged the problem to be highly complex. "Consideration was given to the theory that the carbon dioxide content of the atmosphere has been increasing since the beginning of the industrial revolution, and that this increase has resulted in a warming of the atmosphere since that time," the proceedings report. "Von Neumann questioned the validity of this theory, stating that there is reason to believe that most of the industrial carbon dioxide introduced into the atmosphere must already have been absorbed by the ocean." The debate was on.[55]

Sigmund Fritz, of the U.S. Weather Bureau, added that "the effects of plant life must also be taken into consideration." William von Arx, from Woods Hole Oceanographic Institution, stressed that "the balance depends on the buffer capacity of the seawater," and noted that "there is a significant amount of carbon dioxide locked up in the plankton cycle." Charney asked about "the statistical significance of Wexler's result that substantially more blocking activity takes place in Januaries which occur during periods of sunspot maxima than in those which occur during minima of the sunspot cycle." Von Neumann "felt that there must be a minimum size of the ice field that would be self perpetuating" and "called attention to the fact that the processes which led to periods of glaciation and deglaciation must have been relatively constant over many centuries." He asked "whether there is any evidence to suggest that high volcanic activity had been sustained over such long periods of time."[56]

"It is not necessary," von Neumann and Charney argued, "to resort to explanations of climatic change which require external mechanisms such as solar and volcanic activity." Richard Pfeffer, of Columbia University, noting that "the radiation absorbed by a unit mass of air is measured as the small difference between two large fluxes," asked "whether present-day measurements of the distribution of water vapour and temperature (the chief variables which determine the radiational characteristics of the atmosphere) are sufficiently accurate to determine this difference." Edward Lorenz, of MIT, noted, concerning the effects of clouds, that "it would be necessary to specify, in addition to the mean cloud cover, the diurnal range . . . whether the clouds appear at night or during the day." Von Neumann, in conclusion, advised that they should "first attempt to determine the extent to which climate can be changed by internal mechanisms through non-linear feedback processes," and "stressed that the problem is a complicated one."[57]

Weather prediction remains divided into the three regimes established in 1955. The first, short-term regime is predictable. The second, medium-term regime is now known to be unpredictable, much as many suspected, despite von Neumann's hopes. The third regime is still under debate. "I think in those days we were very optimistic," says Charney. "I remember at that time receiving reports that Norbert Wiener had regarded von Neumann and [me] as practically *gonifs*—thieves. That we were trying to mislead the whole world in thinking that one could make weather predictions as a deterministic problem. And I think in some fundamental way Wiener was probably right."[58]

It was Edward Lorenz, a consultant to the IAS meteorology project, who would establish the unpredictability of the atmosphere—shortly after von Neumann's death. Approaching the question from another direction, Charney asks, "if Laplace's mathematical intelligence were replaced by a computing machine of unlimited speed and capacity, and if the atmosphere below 100 km were spanned by a computational lattice whose mesh size were less than the scale of the smallest turbulent eddy, say one millimeter . . . would the problems of meteorology then have been solved?"[59] He answers that all predictability would vanish in less than one month, "not because of quantum indeterminacy, or even because of macroscopic errors of observation, but because the errors introduced into the smallest turbulent eddies by random fluctuations on the scale of the mean free path (ca 10–5 mm at sea level), although very small initially, would grow exponentially. . . . The error progresses from 1 mm to 10 km in less than one day, and from 100 km to the planetary scales in a week or two."[60]

As to whether climate—the "infinite forecast"—is predictable, the jury is still out. Von Neumann expected that not only would climate become predictable, but it would also be controlled. The balance points, once identified, would be too easy to tip. The real climate change crisis, according to von Neumann, was not whether we can control climate, but how to decide who sits at the controls. "After global climate control becomes possible," he warned in 1955, "this will merge each nation's affairs with those of every other, more thoroughly than the threat of a nuclear or any other war may already have done."[61]

Von Neumann and Wiener could both be right. Wiener may well be as correct about climate as he was about medium-term weather prediction: that the atmosphere can no longer be treated as a deterministic system beyond thirty days or so. Von Neumann could be right in the sense that even if climate cannot be predicted, that does not mean it cannot be controlled.

Imagine a future, combining the visions of Lewis Fry Richardson with those of von Neumann, where the Earth (including much of its oceans) is covered by wind turbines immersed in the momentum flux of the atmosphere, and photovoltaics immersed in the radiation flux from the sun. Eventually enough of these energy-absorbing and energy-dissipating surfaces will be connected to the integrated global computing and power grid, to form, in effect, the great Laplacian lattice of which Charney and Richardson dreamed. Every cell in this system would account for its relations with its neighbors, keeping track of whether it was dark, or sunny, or windy, or calm, and how those conditions may be expected to change. Coupled directly to the real, physical energy flux would be a computational network that was no longer a model—or rather, was a model, in Charney and Richardson's sense of the atmosphere constituting a model of itself.

Any such distributed planetary system, however, once it is sufficiently fine-grained, will itself become unpredictable—just like the atmosphere in which it is immersed. Whether the photovoltaic landscape is absorbing or reflecting, and whether the wind farms are under full load, freewheeling, or nudging the atmosphere here and there, may, in the course of time, indeed control the climate, but the workings of the model, and how it is going to behave a week from Thursday, will remain as mysterious as a partly cloudy day still is to us.

"Sometime in the early 1950s, von Neumann, I, and several others were standing outside of the Electronic Computer Project Building in Princeton," remembers Joseph Smagorinsky, "and Johnny looked up at a partially cloudy sky and said, 'Do you think we will ever be able to predict that?' "[62]

TEN

Monte Carlo

Between 1946 and 1955 we crossed the country twenty-eight times by car.

—Klári von Neumann, 1963

"WE WERE on the Riviera, in Monte Carlo, at the center of gravity for incurable gamblers," remembers Klári von Neumann, of a gambling expedition with Francis, her first husband, midway between World War I and World War II. "When we walked into the Casino, the first person we saw was Johnny; he was seated at one of the more modestly priced roulette tables with a large piece of paper and a not-too-large mound of chips before him. He had a 'system' and was delighted to explain it to us: this 'system' was, of course, not foolproof, but it did involve lengthy and complicated probability calculations which even made allowance for the wheel not being 'true' (which means in simple terms that it might be rigged)."[1]

"Francis went on to another table," Klári continues. "For a while I wandered around watching the lunatic pleasure of people destroying themselves, then I went to the bar and sat down, wishing I had company with my drink. As I was sipping my cocktail, Johnny appeared." The game theorist had run out of luck at the roulette table, and Klári, who was running out of luck with her first marriage—"an absolute disaster"—had to pay for his drink. "I was a rich girl, my father was very wealthy and Francis was an incurable gambler—this just about sums up my sex-appeal to him. After four years of all kinds of troubles, we divorced—my father bought it for me."[2]

Klára Dán was born on August 18, 1911, into a wealthy Jewish family in Budapest. "The most pampered, spoiled brat in a very large closely knit clan," she remembers herself as "a beautiful and absolutely obnoxious child, who squealed, yelled and howled her way through the first formative years of life." Her father, Charles Dán, an industrialist and financier, served as an officer in the Austro-Hungarian Army during World War I, surviving the war in relative comfort, but with

James Pomerene joined IBM in 1956, where he led IBM's early efforts to develop cached, high-speed memory architectures and parallel, multiple-core processors, and was appointed an IBM Fellow in 1976—with complete freedom to pursue any avenue of research, equivalent to a permanent membership at IAS. Herman Goldstine left the IAS in 1958 to supervise IBM's mathematical research center, housed temporarily at the Lamb Estate in the Hudson Valley while awaiting the completion of the Thomas J. Watson Research Center at Yorktown Heights, where he continued the tradition of scientific computing he had established at IAS, becoming an IBM Fellow in 1969. "At the Lamb Estate we thought of ourselves as princes of the earth because of our computing support," remembers Ralph Gomory, who joined Goldstine's group in 1959. "Every day a station wagon left the Lamb Estate and went up to Poughkeepsie. It carried our programs and returned the next day with results."[26]

Jack Rosenberg left the IAS for a position at General Electric in Syracuse in 1951, moved to Los Angeles in 1954, and, after von Neumann's death ended his plans to work for the proposed new computing laboratory at UCLA, joined the Los Angeles Scientific Center of IBM. "A long time IBM engineer showed me some circuit diagrams of IBM's first electronic computer, the 701," he remembers. "It was a copy of the Von Neumann computer, which I had developed in 1947–51." Rosenberg was offered an IBM Fellowship in 1969, which he declined, explaining that "the company was too large and corrupt."[27] He still lives in Pacific Palisades, listening to music through a set of the same phase-synchronized "coherent sound" loudspeakers that he had installed in Einstein's house in 1949. Einstein, in appreciation, granted Rosenberg a wide-ranging and candid interview, which Rosenberg recorded on high-fidelity equipment, but will not release. "Einstein said it must never be made public," he explains.[28]

Gerald and Thelma Estrin moved to Israel in 1954 to supervise the construction of the WEIZAC at the Weizmann Institute of Science in Rehovot, returned to the IAS in 1955, and moved to UCLA in 1956, where they helped establish the new Department of Computer Science, nurturing a new generation of entrepreneurial computer scientists, including two of their own daughters, Deborah and Judy Estrin, and Paul Baran of RAND. "It was a marvelous accident, wonderful for everything that happened afterwards, that things didn't get classified," say the Estrins, looking back at the Electronic Computer Project at IAS.[29]

Andrew and Kathleen Booth returned to England, where they remained instrumental in the continued development of digital computing and X-ray crystallography, before moving to Canada in 1962. "Kathleen and I were amused at the concern for our moral wellbeing!" Andrew responded, when shown a copy of the February 1947 discussion between Goldstine and von Neumann concerning their housing arrangements at the IAS.[30]

Joseph and Margaret Smagorinsky helped found the Princeton Geophysical Fluid Dynamics Laboratory, where climate modeling continued from where the IAS meteorology project had left off. Jule Charney and Norman Phillips settled at MIT, forming the nucleus of a computational meteorology group that resolved some of the differences between John von Neumann's reasons for believing that weather could be made predictable and Norbert Wiener's reasons for believing that it could not. Hedi Selberg transferred her expertise to the Princeton Plasma Physics Laboratory, and Ralph Slutz became director of computing at the National Center for Atmospheric Research, in Boulder, Colorado. Richard Melville and Hewitt Crane went to the Stanford Research Institute, developing, among other things, the ERMA system for electronic clearing of machine-readable checks between banks. Dick Snyder returned to RCA, working on magnetic-core memory but unable to persuade RCA, as Zworykin had managed to with television, to take the lead. Morris Rubinoff returned to the University of Pennsylvania, and for an interval to Philco, where he supervised the design of the Philco 2000, the first fully transistorized computer, with asynchronous arithmetic, a feature that had been developed at IAS. Arthur Burks settled at the University of Michigan, where he founded the Logic of Computers Group in 1949, edited von Neumann's *Theory of Self-Reproducing Automata* (1966), and, with Alice Burks, published the definitive *Who Invented the Computer?* in 2003.

Robert Oppenheimer was stripped of his security clearance in 1954, one day before it would have expired on its own, in a deliberate act of public humiliation that brought postwar dreams of civilian control of nuclear weapons to an end. "The military wanted the whole deal: the laboratories, the computers, the whole future, in nuclear weapons, from A to Z," explains Harris Mayer. "When we set up the AEC, the military was shut out of what was their major firepower, and they never forgot that, and they wanted it back. They got back the command of the nuclear weapons and the computers, and a part of this, a minor part, actually, was to discredit Oppenheimer."[31] His AEC