Remembrance of Things Past

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In preparing a paper for this symposium on the topic of scientific and numeric computation, I have been forced once again to think over what these terms mean now and what they have meant throughout time. In my case this is a worthwhile task requiring me to reappraise the subject and ask myself if this is what was intended by the fathers of the field.

I think that with very many branches of mathematics we can well ask the perfectly proper questions: What is the purpose of this subject? Why did its creators choose to go in one direction rather than another? After all, even though mathematics is a magnificent creation of the human intellect, it is not merely a collection of complicated but arbitrary topics lumped together in an inchoate whole. We know that there are remarkable threads and themes that run through many of the topics and that many others are there to provide us with the tools needed to make yet other studies. The unities present are remarkably abundant, and the sense of arbitrariness that people sometimes mention seems to me often a reflection of their lack of understanding of the topics in question.

At this point it is perhaps relevant to quote some of von Neumann's views on mathematics and mathematicians. He said, "Most people, mathematicians and others, will agree that mathematics is not an empirical science, or at least that it is practiced in a manner which differs in several decisive respects from the techniques of the empirical sciences. And, yet, its development is very closely linked with the natural sciences. One of its main branches, geometry, actually started as a natural, empirical science. Some of the best inspirations of modern mathematics (I believe, the best ones) clearly originated in the natural sciences."

The subject of mathematics is very different, however, from, say, theoretical physics, and it is perhaps worth pausing for a bit to understand just how this is so. As we know, mathematics falls naturally into a large number of more or less distinct fields, and almost no one today has any reasonable grasp of the whole. On the contrary, physics seems to be a very different sort of topic. A crucial difficulty is met in the experimental area: Whatever anomaly this presents must be cleared up before the practitioners of the field can go forward. It is not possible for them to do what we very often do: drop the problem

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as being intractable and proceed to an entirely different challenge. As we can appreciate, certain critical experiments in the real world cannot be ignored if their results contradict existing theories. All the best scientists in the field are forced to face up to the challenge and to make whatever modifications are necessary to reestablish equilibrium in their science. Thus, experiments such as Michelson's led to the introduction of special relativity, and the conflict between that subject and classical celestial mechanics led to general relativity.

Let us look back at the beginnings of our subject, at the works of Hipparchus and Ptolemy, who worked in the period from about 150 B.C. to A.D. 150. Obviously they were not the first men to make significant use of mathematics. The great geometers—many of whose names have been lost to us because of Euclid's remarkable efforts to pull together all the empirical, semiempirical, and pure mathematical efforts in geometry—certainly developed one of the most noteworthy structures in the ancient world. We need not concern ourselves here with how much was empirical and how much purely mathematical. All that we need to know is that Hipparchus and later Ptolemy used the Euclidean apparatus to explain the motions of the heavenly bodies with excellent accuracy. I believe that it was they (and perhaps especially Ptolemy) who were mainly responsible for the initiation of our subject. Ptolemy was faced at the beginning with the problem of explaining the motions of the visible planets, the sun, and our moon with sufficient accuracy so that an observer armed with the astronomical instruments of that day could locate the body in question. The paper construct that Ptolemy created in his *Almagest* is in some ways like an elaborate mechanical device or rather a series of these devices, one for each of the visible planets, the sun, and the moon. These devices are made out of circles with smaller circles mounted on their perimeters. Each of these was, so to speak, handmade so that the particular body moved in accordance with observational data that in many cases went far back in time and enabled Ptolemy and his colleagues to determine many parameters with considerable exactitude.

Ptolemy did not, of course, develop the basic mathematics that he used to explain or rather to predict the locations and times of various celestial events. He obviously decided that he would accept the mathematics available at that time, Euclidean geometry, and went on to develop a means for using it in a practical way to give results in numerical form. The apparatus that he and Hipparchus put together is what we call trigonometry. Its utility has been so great that it has survived as a standard topic in school curricula for almost two millenia. Let me hasten to point out that very few things in our magnificent western culture have such survival times; therefore, let us not sneer at this subject. Ptolemy made two essential observations to establish his computational tool. He saw that a table of the sines—actually the subtended chords—of a series of equally spaced angles was just what was needed. This is quite clear, but what I think is remarkable is that he did not measure these chords or sines by physical means. Instead, he developed the lovely relations of trigonometry and coupled these with the knowledge of the number of degrees in the angles of certain regular polygons. By these means he was able to build up virtually all the needed entries in a table of sines with a half degree spacing. He needed, however, one more thing: the sine of $1/2°$. To obtain this he developed a neat scheme for interpolation based on an elegant inequality of Archimedes that says that if $A > B$ then $A/B > \sin A/\sin B$. He applied this to obtain the result

$$(2/3)\sin(3/4) < \sin(1/2) < (4/3)\sin(3/4)$$

This gave him $\sin(1/2)$ with a relative error of 2×10^{-6}.[1]

In addition to giving Ptolemy his table, this study gave us a whole way of viewing mathematics. It meant that the scientist who wants to explain the world need not go off into an experimental study, but can seek out a mathematical tool to use instead. This has reduced the need for experimentation to the determination of physical fundamentals such as physical constants whose values are very properly the subject for experimentation. If Ptolemy had not seen how to use mathematics to fill in his table but had constructed various-sized angles and actually measured chords, heaven knows what applied mathematics in general and computation in particular would have become.

In any case, so great was this success—through its remarkable predictive powers—that to try to emulate applied mathematics by becoming more mathematical in nature became and has continued to be a desideratum of virtually all sciences. So, for example, we see that some of the very great advances in theoretical physics have been made possible at least in part by the highly mathematical form that the subject has assumed. That the model stems from Ptolemy and his great predecessor Hipparchus also reminds us that our particular subject has been for a long time very much a handmaiden of mathematical astronomers.

It is perhaps not without some interest to note what the distinguished Arab astronomer al-Kashi, who lived during the time of Tamerlane in Samarkand (1400), did in his observatory. He was concerned with seeking a more elegant way to find the sine of $1/2°$ than Ptolemy had produced. To this end he noticed that there is a simple cubical relation between the sine of $3A$ and the sine of A:

$$\sin 3A = 3\sin A - 4\sin^3 A$$

so that if he had the sine of 3° he could then find the sine of 1°. This led al-Kashi to develop an iterative scheme for solving the cubic equation and very likely led to the subject that was known as the Theory of

Equations. This was a field that was often taught at an elementary level in a number of universities. One of the most noteworthy topics in that field, at least for me when I was a student, was the so-called Newton-Raphson method for iteratively solving functional equations.

Let us now leave this ancient history and move forward into more modern times, and let us discuss my doings. Back in the days before World War II, Gilbert Bliss at the University of Chicago was interested in exterior ballistics and announced a course in the topic. He also was planning to write a book on the subject, which he in fact did. But the teaching of his graduate courses had fallen to me in those days because his health was uncertain. In this I was very fortunate. In the course of teaching the students at Chicago, I had to take them through a certain amount of numerical analysis so that they could learn how to solve the differential equations of motion for a projectile-fuse combination. This was a skill that I had acquired more or less painfully from an astronomer at Chicago named Walter Bartky. We had tables of logarithms and little else besides a method first named after Adams and Moulton.

This and similar methods played a major role at the Ballistic Research Laboratory at the Aberdeen Proving Ground in Maryland. They are characterized by the calculation and recording of many differences, since linear operations are cheap to perform by hand and paper for storage of partial results is inexpensive. These methods make use of as few nonlinear operations, such as multiplications and divisions, as possible, because these involve the use of log tables and entail many table look-ups and interpolations.

Therefore, when I arrived at Aberdeen and was assigned to the department that had to produce all the firing and bombing tables for the Army and the Air Force, I found myself back home again with the techniques that I had been teaching young people at Chicago. Fortunately from my point of view, I was put in charge of a substation of the laboratory at the University of Pennsylvania's Moore School of Electrical Engineering. I was in touch with several men who were keen on the problem of automating dull tasks that could be done better by machine than by human. In fact the staff included a number of faculty and at least one graduate student who had been involved in precisely this topic for some years in connection with a differential analyzer built at the school in the mid 1930s, with a copy made for Aberdeen. This was one of the reasons why Aberdeen and the Moore School were contractually related during the war.

The differential analyzer was an electromechanical device invented by Vannevar Bush in the early 1930s to integrate the differential equations arising in the field of electrical engineering. The equations for the motion of a projectile were readily adaptable to these machines, which afforded a fast way to solve them. The machines' accuracy was not high; about 5 parts in 10,000 was the best one could get. It took about 10 to 20 minutes to integrate the average trajectory. To illustrate, let me remark that such a trajectory involved about 750 multiplications and would take a human at least seven hours. Our main aim was to reduce this 10- to 20-minute time by an order of magnitude and to provide at the same time a nonhuman way to perform all the interpolations and other numerical steps that were needed to produce a firing table.

Fortunately for me, Grist Brainerd, then a young professor at the Moore School, proposed a solution to the problem first raised by a colleague of Brainerd named John Mauchly. His idea was to build an electronic digital computer to replace the differential analyzer and bring two enormous advantages to us: the speed of electronics and the accuracy of the digital principle. The Army accepted this proposal, and the Moore School actually built the device, the ENIAC, under Brainerd's aegis and with a superb young engineer named Presper Eckert. It is not my place here to spend more time on the details of this essential advance in our field. Suffice it to say that it immediately changed the face of the computational world.

Since the ENIAC and its successors had very small memories for intermediate results, the entire economy of computing changed overnight. Instead of being in a world of expensive multiplication and cheap storage, we were thrown into one in which the former was very cheap and the latter very expensive. (In fact we are only now getting into an economy in which storage is becoming exceedingly cheap.) This meant that virtually all the algorithms that humans had devised for carrying out calculations needed reexamination. In addition many areas of numerical analysis, such as the numerical solution of partial differential equations, were suddenly potentially open to us. This was the world in which we found ourselves at the end of World War II.

It was into this world that Johnny von Neumann projected himself with the gusto and élan that characterized all his activities. He went at something either with "full speed ahead and damn the torpedoes" or not at all. Nothing was ever so complete as the indifference with which Johnny could listen to a topic or paper that he did not want to hear.

At this time he was gung ho for the wonderful world that the electronic computer was opening up. We decided that we should set up at the Institute for Advanced Study a full scale effort to have a major hand in creating this brave new world. To do this we instituted what we called the electronic computer project and decided that our thrust needed to be multipronged.

We accordingly had a group devoting itself to a study that might now be called computer architecture and science. Here our main aim was to discover the right way to organize a computer so that it would be flexible and easily responsive to its users. This effort resulted in a series of papers on planning and coding of problems that had a fundamental role in shaping the architecture of the modern computer. We also pushed in a small way into topics such as merging and sorting of data and into the question of the least number of operations needed to perform a given function.

Another group was devoted to numerical methods (more on this later), and a third group was created to engineer and fabricate a computer embodying our architectural ideas. As one might suppose, the results of this third enterprise were transitory; the changes taking place in the engineering field were so great that the machine was perhaps obsolete within a year or so of its completion.

Finally we envisaged a group that would use the results of the others to solve some important problem that even the lay public could grasp to show the significance of the electronic computer to the world around us. Johnny chose the field of meteorology and set up a first-rate group of men around Jules Charney, who formulated the equations for the motion of climatic phenomena as partial differential equations. They of course had to make many simplifying assumptions, both to formulate the problem and to get it into such a size that our computer could calculate the motion of the weather at speeds in excess of the real speed so that forecasting into the future became possible.

It is not our business to discuss here the details of this project beyond remarking that the results of that effort were taken up by the weather bureaus of all the leading nations of the world. In fact, here in Princeton there is a laboratory established by our weather bureau that devotes its activities to the development of accurate long-range forecasting techniques.

Let us now take up some of the topics that engaged our attention during the period from 1946 to 1957 and that relate to our field. Obviously one of the first and most likely topics to be discussed was the solution of large systems of linear equations, since they arise almost everywhere in numerical work. V. Bargmann and D. Montgomery collaborated with von Neumann on a paper on this subject.[2] Then H. Hotelling, the well-known statistician, wrote an interesting paper in 1943 in which he studied a number of numerical procedures, including the Gaussian method for inverting matrices. He pointed out in a heuristic and as it turned out, inaccurate, way that the Gaussian method for inverting statistical correlation matrices would require about $k + 0.6n$ digits during the computation to obtain k-digit accuracy. Thus to invert a matrix of order 100 would in his terms require 70 digits be used if one wanted 10-digit accuracy.[3]

Johnny and I never quite believed that Gauss would have used a procedure so lacking in elegance, given his great love for computation. Indeed, his collected works contain a considerable amount of material on both astronomy and geodesy that shows his love for and great skill at calculation. As some partial evidence of this, we know he certainly used the so-called Cooley-Tukey method to handle Fourier transforms. Taking his skill as a given, we looked closely at the procedure and wrote a paper on the subject that we used as an elaborate introduction to errors in numerical calculation.[4] We tried in that paper to alert the practitioners in the field to a phenomenon that had not been particularly relevant in the past but was to be a constant source of anxiety in the future: numerical instability. In the course of the analysis we also brought to the fore the now obvious notion of well- and ill-conditioned matrices. Since then, of course, people such as Wilkinson have greatly simplified the very complicated analysis we went through to arrive at our final results.

In a second paper we raised a question that we thought might become more important than in fact it ever became.[5] We said, let us not worry so much about what might happen in a very small number of pathological cases; instead let us see what occurs on the average, what we can expect if we need to do this same task many times. To achieve this probabilistic result I had to develop proofs for several theorems in probability theory, which I did with considerable difficulty, only to receive a letter from a statistician named Mulholland after the paper appeared in which he showed me how to do one part with the slightest work: A mere flip of his wrist sufficed to demonstrate some obvious thing. My only consolation was that Johnny had not seen how to do it simply either. In the event, I suppose that our second paper scared practitioners of the subject away from the field of probabilistic estimates instead of bringing them in, or perhaps it simply was not a very important idea. Human egotism being what it is, I naturally hope it was the former, but honesty makes me think it was the latter.

Other things that one might reasonably want to know about a symmetric matrix are its eigenvalues or, as Veblen used teasingly to say, its proper-*Werte*. At that time Frank Murray, a mathematician from Columbia who had collaborated with von Neumann at one period on operator theory, was in Princeton for a term. The three of us set ourselves the goal of considering all reasonable ways that one might find the eigenvalues and discover which seemed the best in the sense of numerical stability. We made an extensive search and came up with one that pleased us very much. Since I seem to have had some priority on this scheme, it was agreed that I would present it at a 1951 meeting to be held at UCLA, where the National Bureau of Standards had a western numerical institute. There I presented the paper, which was very well received, and then Ostrowski got up and asked me if I knew that this method had first been worked out by Jacobi in 1846.[6] Of course the answer was no. Jacobi was interested in finding a better way to analyze some data of Leverrier in the *Connnaissance des temps* and did it by finding the eigenvalues of a symmetric matrix of order seven. His results significantly improved Leverrier's. I shall not discuss the improvements that Householder and then Givens made to our knowledge of how to find eigenvalues.

Instead I must turn now to the field of partial differential equations. There are several other papers in this volume on this topic, written by people who collaborated with Johnny during his lifetime and continued to make major thrusts after his death. One of Johnny's early interests was hydrodynamics, which he understood profoundly. I must tell you that some, indeed perhaps most, applied mathematicians know a great deal about the mathematical tools that they can use to solve problems but have little deep knowledge of the physics, chemistry, biology, or what have you that underlies their subjects. Not so Johnny. His grasp of the physics, the theory, the apparatus, and the experiments were all food for his interest. It is this that made his interest in the computer so profound. He was very concerned about the electrical characteristics of each type of vacuum tube, and about what resistors, capacitors, and inductances were made of and why. One had the impression that when he entered a field he had to encompass it all, however elaborate it might be.

In any case he was one of the very few people, outside of its three authors, who knew the 1928 paper on the solution of partial difference equations.[7] Here Courant, Friedrichs, and Lewy considered how to solve partial differential equations and in the course of their analysis based on the characteristic curves of hyperbolic difference systems, showed that certain inequalities had to be satisfied. These inequalities now go by the name of Courant conditions. In any case Johnny was a consultant to Los Alamos, where his expertise in hydrodynamics was of great value. He was a leader there for numerical calculation and gathered around himself a group of keen physicists, including Nicholas Metropolis, who became his apostles. His "object all sublime" was to replace experimentation by computation in so far as possible in fields where the equations for a problem could be unambiguously formulated. He even did this using Howard Aiken's electromechanical machine at Harvard to show the feasibility of such procedures.

His enthusiasm and vitality were so great in this connection that I agreed to let Los Alamos put what was then a huge problem on the ENIAC. The task was horrendous: People such as Metropolis and his then colleague Frankel worked like mad to get results. Whether this particular calculation was of any real use to Los Alamos I never asked, but it certainly caused that laboratory and all the other Atomic Energy Commission laboratories to take a vital interest in numerical work.

A look at von Neumann's collected works will show the most casual reader how much effort he and his collaborators (such as Goldstine, Metropolis, Richtmyer, Taub, and Ulam) put into hydrodynamic calculations. This meant, in effect, studies of hyperbolic and parabolic partial differential equations. One of the most interesting things for von Neumann in the study of hyperbolic equations was the truly anomalous and remarkable emergence of shocks—discontinuities—in otherwise thoroughly smooth situations, brought about by very slight and continuous motions. A number of his papers relate to precisely this point. One we wrote analyzed what happens if a very powerful explosion takes place at a point in a homogeneous medium.[8] The result is a spherical blast wave that emanates from the point. The shock was handled by making use of an iterative procedure originally used by Peierls for solving the Rankine-Hugoniot equations. Another intriguing method for coping with shocks was developed by Johnny and Robert Richtmyer, who conceived the idea of arbitrarily introducing some viscosity into an otherwise inviscid fluid.[9] This is the same thing as introducing artificial dissipative terms into the equations, giving the shocks a thickness roughly comparable to the mesh size of the numerical net. This changes the shocks into near discontinuities that propagate at essentially the right speeds and across which the temperatures and pressures change by nearly the right amounts. This meant that one could totally ignore the Rankine-Hugoniot equations and proceed in a simple numerical fashion.

Von Neumann's interest in hydrodynamic and related calculations arising at Los Alamos and other places where nuclear particles were under study also resulted in the development of a lovely and perhaps totally unexpected gem of a field: Monte Carlo.

This was a nice example of von Neumann's combining interests in a number of subjects. He saw here how Newton's brilliance had enabled people to express in continuous form equations relating discrete particles, so that instead of horrible systems of unmanageable equations one could write down a few elegant conservation relations and solve the equations that they embody. Now the numerical revolution caused the analyst to replace the continuous equations by systems of discrete ones. Johnny and Ulam got the idea of returning to finite systems and playing repeated games according to the rules of probability theory.

Instead of saying more on this, perhaps I can just mention some work that we did on a conjecture of Kummer.[10] This was part of an idea that we had of using the computer as a new and improved form of scratch pad to develop examples and counterexamples. Artin had mentioned to us this conjecture of Kummer which was based on a very few—in fact on 45—cases. Artin believed that it was too difficult to undertake a proof of the conjecture without more evidence of its truth. We accordingly ran a test for about 10,000 values and found that there was little evidence from our results to justify Artin or anyone else in undertaking a major effort to try to establish the result.

I often think that in addition to all the individually remarkable things that Johnny did in our field, he did something else that may almost be more important. This is a matter that I have skirted in what I have said to this point and which I find difficult to discuss without someone's thinking that I am making a pejorative remark. I believe that von Neumann's great status in the world of the physical and social sciences was sufficient so that when he told people to compute digitally and not to make analog computations by means of various sorts of physical experiments they believed him. I think that this in large measure accounted for the early acceptance of the digital computer. I do not imply by my remark that it was necessary for the ultimate use of the computer by the scientific world at large; I simply mean that he caused it all to happen at a rate that was much accelerated over what it would have been had he not influenced the field so decisively. I should like to give two examples of this. First, young Tom Watson, Jr., just back from being a pilot in the CBI (China, Burma, India) area and having heard of Johnny and his interest in electronic computing, came to the Institute for Advanced Study to see for himself what the new world was all about. I feel very certain that this had an extremely important impact on IBM and hence on the world at large. The other example arose from the fact that Johnny, after joining the Atomic Energy Commission, exerted great influence on the laboratories of the Commission to use computers and to authorize both IBM and Sperry-Rand to undertake a sort of competition that resulted in two monster machines for their era—the Stretch and the Larc computers. Out of these, many great advances in our modern world arose.

Instead of continuing I think that this is perhaps a good point to close by illustrating Johnny's expository style when applied to a technical subject that he wanted to make clear to a nontechnical audience. To do this I include some paragraphs from an address that he gave at the dedication of a large electronic computer built by IBM for the Naval Ordnance Research Laboratory.[11]

> The three main areas of geophysics are, of course, air, water, and earth. Let me begin with the air, i.e., with the phenomena in the atmosphere. I am referring to dynamical, or theoretical, meteorology. This subject has for a number of years been accessible to extensive calculations. It is therefore worthwhile to estimate what NORC could do in this area.
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> We know today, mainly due to the work of J. Charney, that we can predict by calculation the weather over an area like that of the United

States for a duration like 24 hours in a manner, which, from the hydrodynamicist's point of view may be quite primitive because one need for this purpose only consider one level in the atmosphere, i.e., the mean position of the atmosphere.

We know that this gives results which are, by and large, as good as what an experienced "subjective" forecaster can achieve, and this is very respectable. This kind of calculation, from start to finish, would take about a half minute with NORC.

We know, furthermore, that this calculation can be refined a good deal. One cannot refine the mathematical treatment *ad infinitum* because once the mathematical precision has been reached at a certain level, further improvements lose their significance, since the physical assumptions which enter into it are no longer adequate.

In our present, simple descriptions of the atmosphere, this level, as we know, is reached when one deals with approximately three or four levels in the atmosphere. This is a calculation which NORC would probably do (for 24 hours ahead) in something of the order of 5 to 60 minutes.

We know that calculations of meteorological forecasts for longer periods, like 30 to 60 days, which one would particularly want to perform, are probably possible but that one will then have to consider areas that are much larger than the United States. In a duration like 30 days—in fact in much shorter durations, like 10–15 days—influences from remote parts of the globe interact. We also know that interaction between the Northern and Southern Hemispheres is not very strong. Therefore, one can probably limit the calculation in the main to one entire hemisphere, but not to a smaller area.

Such calculations have so far only been performed in tentative and simplified ways and all those who have worked on these problems have done so in the sense of a preliminary orientation only. . . .

A calculation of this order on NORC would, I think, require something of the order of 24 hours' computing time. This can be off by a factor of perhaps two, one way or other, but in any event this order of magnitude is acceptable for research purposes.

In this area, therefore, an instrument like NORC becomes essential at about this latter level. Indeed, whether one does a simple 24-hour forecast in half an hour or in two minutes is not decisive. But in a 30 day hemispheric calculation it is very important whether one needs 24 hours or a month. If it takes a month one will probably not do it. If it takes 24 hours, one may be willing to spend several months doing it 20 times, which is just what is needed.

Notes

1 Ptolemy worked with chords, not sines, with chord $2\alpha = 2\sin\alpha$, constructing a table of chords in increments of a degree rather than of sines in increments of half a degree. In addition, it may appear odd that sin(3/4) would be easier to compute than sin(1/2). Ptolemy knew the values for 72° and 60°. Also, given the values for A and B, he could

calculate results for $A + B$, and hence for 12°. From a half-angle formula, he was able to obtain the values for 6°, 3°, $3/2$°, and finally $3/4$°. (ed.)

2 J. von Neumann, "Solution of Linear Systems of High Order," *Collected Works* V (1963): 421–77.

3 H. Hotelling, "Some New Methods in Matrix Calculation," *Ann. Math. Stat.* 14 (1943): 1–34.

4 J. von Neumann, "Numerical Inverting of Matrices of High Order I, II," *Collected Works* V (1963): 479–572.

5 J. von Neumann, "Numerical Inverting of Matrices of High Order II," *Collected Works* V (1963): 558–72.

6 C. G. J. Jacobi, "Über ein leichtes Verfahren die in der Theorie der Säcularstörungen vorkommenden Gleichungen numerisch aufzulösen," *J. Reine Angew. Math.* 30 (1846): 51–95.

7 R. Courant, K. O. Friedrichs, and H. Lewy, "Über die partiellen Differenzengleichungen der mathematischen Physik," *Math. Ann.* 100 (1927): 32–74.

8 J. von Neumann, "Blast Wave Calculation," *Collected Works* VI (1963): 386–412.

9 J. von Neumann, "A Method for the Numerical Calculation of Hydrodynamic Shocks," *Collected Works* VI (1963): 380–85.

10 J. von Neumann, "A Numerical Study of a Conjecture of Kummer," *Collected Works* V (1963): 771–72.

11 J. von Neumann, "The NORC and Problems in High Speed Computing," *Collected Works* V (1963): 241–44.