A Note On Early Monte Carlo Computations and Scientific Meetings

CUTHBERT C. HURD

This note describes what may have been the first formulation of a Monte Carlo calculation and reproduces the paper where the formulation is described. Speculation is given with respect to the origin of the name. Four early computer meetings at which Monte Carlo calculations were discussed are described. Anecdotes concerning Aiken, Curtiss, Metropolis, Ulam, and von Neumann are included. Finally, the present state of Monte Carlo is discussed.

*Categories and Subject Descriptors: G.3 [**Probability and Statistics**]—Monte Carlo algorithms; K.2 [**History of Computing**]—people, software*
General Terms: Algorithms, Human Factors
Additional Key Words and Phrases: J. H. Curtiss, A. S. Householder, N. Metropolis, S. M. Ulam, J. von Neumann

This note first describes some of the background leading to what I believe was the first formulation of a Monte Carlo computation. Second, an apparently overlooked report describing that formulation is reproduced. Third, speculation concerning the origin of the name is given. Fourth, information is given concerning four meetings where Monte Carlo was discussed; the computing devices to which the speakers referred are listed. Also, a copy of a letter I wrote to John H. Curtiss concerning the organization of one of those meetings is reproduced. Finally, an indication is given concerning the present extent of the use of Monte Carlo. Bernard A. Galler, editor-in-chief of the *Annals*, sent this article to a number of persons whose comments are printed, along with my responses.

I. The First Formulation of Monte Carlo?

Monte Carlo is the confluence of deterministic, stochastic, and computational methods with computer-generated random numbers an important component.

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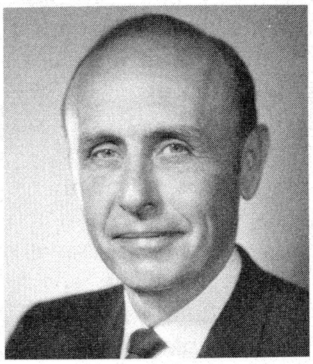
Stanislas M. Ulam (1976) states that "the idea which was later called Monte Carlo method occurred to me when I was playing solitaire during my illness," and also states that "the idea came into concrete form with its attendant rudiments of a theory after I had proposed it to Johnny [von Neumann] in 1946 during one of our conversations." Ulam and von Neumann (1947) state, "This procedure is analogous to the playing of a series of solitaire card games and is performed on a computing machine. It requires, among others, the use of random numbers with a given distribution." They give no further details in their abstract, and there is no evidence that the paper was published. Ulam, with whom I spoke several times by telephone before his death, did not remember its publication but referred me to a 1947 Los Alamos report by Richtmyer, Ulam, and von Neumann, reprinted here. The report, which describes a stochastic model of a physical process and gives a procedure for computation on the ENIAC, is apparently not well known. It was classified "secret" originally and not declassified until July 31, 1959; only eight copies were originated. The classification would explain the lack of reference to it in Metropolis and Ulam (1949) and in Householder (1951). The small distribution may explain the lack of reference to it in Kraft and Wensrich (1964).

I have found no documentation concerning the first use of the name *Monte Carlo.* Nicholas Metropolis, with whom I have also spoken by telephone, and Ulam both say that it was "used in meetings." Anyone who is familiar with the cast of characters at Los Alamos in the 1940s and who is also familiar with the pervasive and droll sense of humor that was exhibited when these characters interacted, can imagine someone coining the name *Monte Carlo*. To the best of my knowledge, it first appeared in public print in Metropolis and Ulam (1949).

II. Symposium on Modern Calculating Machinery and Numerical Methods

I spoke to von Neumann in late 1947 or early 1948 about consulting with George A. Garrett and me at Oak Ridge, Tennessee, concerning computational methods of what is now called "simulating" the gaseous diffusion process for uranium separation. I believe that I first heard of Monte Carlo at a symposium held on the University of California at Los Angeles campus at the National Bureau of Standards Institute for Numerical Analysis, July 29–31, 1948 (Cannon 1949). There were 34 speakers and session chairmen, of whom I either knew or came to know 26. Ulam presented a paper entitled, "Problems in Probability and Combinatorial Analysis." I am sure that he used the term *Monte Carlo*, and I am equally sure that his paper attracted a great deal of interest from Alston S. Householder and me because of problems we were attacking in Oak Ridge. Ulam's paper was not published, although papers by Dantzig, Hartree, and Lefschetz were (Curtiss 1951).



Cuthbert C. Hurd is a mathematician who studied at Drake University, Iowa State University, and the University of Illinois. He took postdoctoral work at Columbia University and the Massachusetts Institute of Technology. Following teaching and deaning, he was employed at the K-25 gaseous diffusion plant in Oak Ridge, Tennessee, as a technical research head. He joined IBM in 1949, formed the Applied Science Department, later a division that was responsible for introducing the IBM 701, 650, 704, and FORTRAN, as well as other products. Hurd was chairman of the board of Computer Usage Company from 1962 to 1974. He is now chairman of Picodyne Corporation, a microcomputer-based company that specializes in educational courseware. He is also president of Quintus Computer Systems Inc., a logic-programming company.

At that meeting von Neumann gave the invited address, "Electronic Methods of Computation," and Metropolis related an anecdote (confirmed by Ulam). Those were the days of the Chief, the Super Chief, and the El Capitan on the Atchison, Topeka & Santa Fe railroad. Metropolis, Ulam, and von Neumann wished to travel from Los Alamos to Los Angeles for the symposium by driving to Albuquerque, New Mexico, and taking the overnight trip by train to Los Angeles. Because von Neumann's address was to be at 11:00 on the morning of their arrival, he asked the travel bureau at Los Alamos to make a reservation for him to return immediately after the lecture. He doubted that suitable reservations would be made. Sure enough, when the party arrived at the Albuquerque train station, Johnny's return reservation had been made for a day after his arrival because, according to the ticket agent, "no one in his right mind would leave for Los Angeles one day by train and start back the next day." The intensity of Johnny's reaction is not recorded.

Another anecdote was related to me recently by George B. Dantzig. According to him, Curtiss announced at the opening session of the 1948 symposium that a prize would be given for "best" paper. Actually, he had already planned that Ida Rhodes would be given a large bouquet of roses. But things did not go exactly as planned. Apparently Howard H. Aiken took the announcement seriously and considered himself a contestant for the prize. When Aiken called for his first slide, it appeared on the screen postage-stamp size. Aiken's presentation, of course, fell into disarray. According to Dantzig, Aiken always thought that Curtiss had deliberately ordered the wrong projector to prevent him from winning the prize.

A query: Why did Householder travel from Oak Ridge to Los Angeles by train for the symposium, instead of traveling with me by DC-3, since the design of airplanes by floating point had not yet begun?

III. Symposium on the Monte Carlo Method

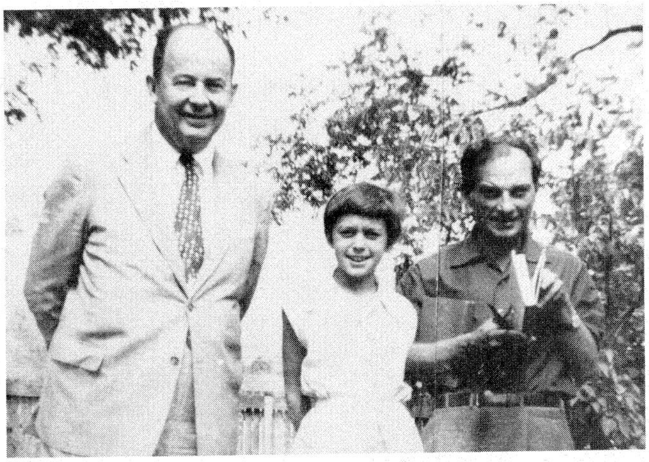
At the 1948 symposium, discussions concerning a formal Monte Carlo meeting apparently began. According to Curtiss (see Householder 1951), an organization committee was formed, of which I was a member, and such a symposium was held in Los Angeles at the Institute for Numerical Analysis on June 29, 30, and July 1, 1949. In Householder (1951), Curtiss and Householder give the background, and 14 papers are printed. Attendees were primarily mathematicians, physicists, and statisticians.

The computational devices described were varied and interesting. In order of appearance: Wilson utilized a spinning cylinder, which I believe was constructed from a Quaker Oats container; Spinrad et al. used nomographs; Householder described the use of Rand random-digit cards and IBM punched-card equipment; DeMarcus and Nelson described a Gauss and Seidel solution that was later implemented on the IBM Selective Sequence Electronic Calculator (SSEC); King described the use of an IBM 405 tabulator and 513 reproducing punch; Mayer spoke of an ENIAC calculation; Hammer gave a flowchart for the use of IBM machines, including a 602 multiplier; Brown referred to an electronic roulette wheel to be used in generating random numbers; Forsythe mentioned a 604 calculating punch; Neyman discussed dice throwing and an instrument constructed by Lehmer to carry out experiments concerning expected bomb damage; Wishart referred to the use of a hand computing machine by Karl Pearson. Shoor et al. spoke of a calculation made on the IBM "card programmed calculator," which they said had been facetiously dubbed "the poor man's ENIAC," and then they proceeded to describe "Betsy" (see the next section and also Byron E. Phelps, *Annals*, Vol. 2, No. 3, July 1980). Because this calculation might have represented a high-water mark in the use of punched-card machines in Monte Carlo calculations, it is worthwhile to quote the concluding paragraph in the Shoor paper (Householder 1951, p. 26; also Phelps 1980).

> This, in essence, describes the methods we have been using for the computation of neutron attenuation problems. We have computed to date some 10,000 particle life histories. This is the equivalent to approximately 150,000 processes and in terms of card handling alone, 2,500,000 cards through the computer. This does not cover those operations using the sorter, collator, and gang punch. We find on the average that an attenuation study is adequately represented by a thousand particles with an average particle handling in the machine of six seconds per process. We can, on the average, run approximately 400 particle life histories per 8-hour working day and accumulate the required statistical data.

This brings me to the letter I wrote to Curtiss on May 26, 1949, reprinted here. Its importance is the indication of the affection and regard Ulam and von Neumann had for each other in not wishing to upstage the other. The paradox, whose resolution is left as an exercise for the reader, is that the 14 papers mention Ulam not at all. I cannot imagine his being silent, nor can he imagine my being silent, but Germond, whom I cannot locate, makes no reference to either of us in his summary of the round-table discussion.

I am also puzzled by the misprints in the copy of the Curtiss letter and hope that he was sent a corrected



Stan Ulam (*right*) with his daughter, Claire, and John von Neumann in about 1954.

copy. At the time, however, I had been in IBM only three months, and although I had been told that I was on the executive payroll, I had neither office nor secretary. Perhaps I typed the letter myself.

IV. Seminar on Scientific Computation

The second meeting devoted to Monte Carlo computations that I attended was held in the IBM Department of Education, Endicott, New York, November 16–18, 1949. Papers by Woodbury, Kahn, Householder, Birnbaum, Hurd, King, and Curtiss (Hurd 1950) refer to Monte Carlo techniques. With one exception, all the computations discussed were performed on IBM punched-card machines. In particular, William Woodbury, in response to a question concerning the machine used, described "Betsy," the combination of an IBM 603 multiplying punch and an IBM 405 accounting machine that had been hooked up at the request of Woodbury and Gregory J. Toben at Northrop Aircraft Company and was the forerunner of the IBM Card Programmed Calculator (CPC). I spoke of the CPC and also discussed the ability of the IBM 604 calculating punch to generate random numbers following a suggestion of von Neumann (Householder 1951, pp. 36–38). In the latter connection, we had wished to demonstrate the CPC before the group as it solved a set of 10 simultaneous linear equations. We had the engineering model of the machine in the basement of the education building, and we were never certain if it would perform on short notice. As a backup we arranged to have the CPC on one elevator leading to the "classroom" and a 604 on another elevator. The procedure was that when it came my turn to speak, Donald W. Pendery, Walter H. John-

May 26, 1949

Dr. John Curtiss
Institute for Numerical Analysis
Bureau of Standards
Los Angeles, California

Dear John:

I hope that you have received the telegram in which I stated that von Neumann, Ulam, and I all agree to the change in date for the Monte Carlo Conference. The only objection which I could imagine is that there might be some who wish to go to both this conference and the Conformal Mapping Conference, and who might be inconvenienced by change. I am sure, however, that no resident of California could admit that an extra day in California could be inconvenient.

Householder and I talked last week and believe that the man responsible for each section of the program should invite the chairman for that section. Consequently, Householder is selecting chairman [chairmen]. I wish that you would serve as chairman on the opening morning. This would be appropriate for several reason[s], but, in particular, both von Neumann and Ulam have asked that the other precede him on the program. Although it is now set that Ulam will lead off with von Neumann second, I think that someone with a good sense of humor, and a good knowledge of the situation should precede [chair], and should indicate that each of these modest gentlemen wish [wishes] the other to precede [him].

Thanks again for suggesting [that] IBM's name be included on the masthead. I would prefer not to have this done simply because when we first discussed the meeting I was an employee of Carbide. If in the future there are meetings toward whose success I could contribute and if it seems helpful to have IBM's name used directly I would be glad to do this.

On a separate sheet I am listing the names and addresses of a few people whom you may not have thought of and who are known to me as having problems for whose solution the Monte Carlo method would be helpful. Will you please include them on the mailing list.

If I can give any last minute help for [from] this end please let me know.

Sincerely yours,

Cuthbert C. Hurd

CCH:co'b

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Dr. Ledyard Tucker
Educational Testing Service
Princeton, New Jersey

Prof. John Tuckey [Tukey]
Princeton University
Princeton, New Jersey

Dr. E. T. Welmers
Bell Aircraft Corporation
Buffalo, New York

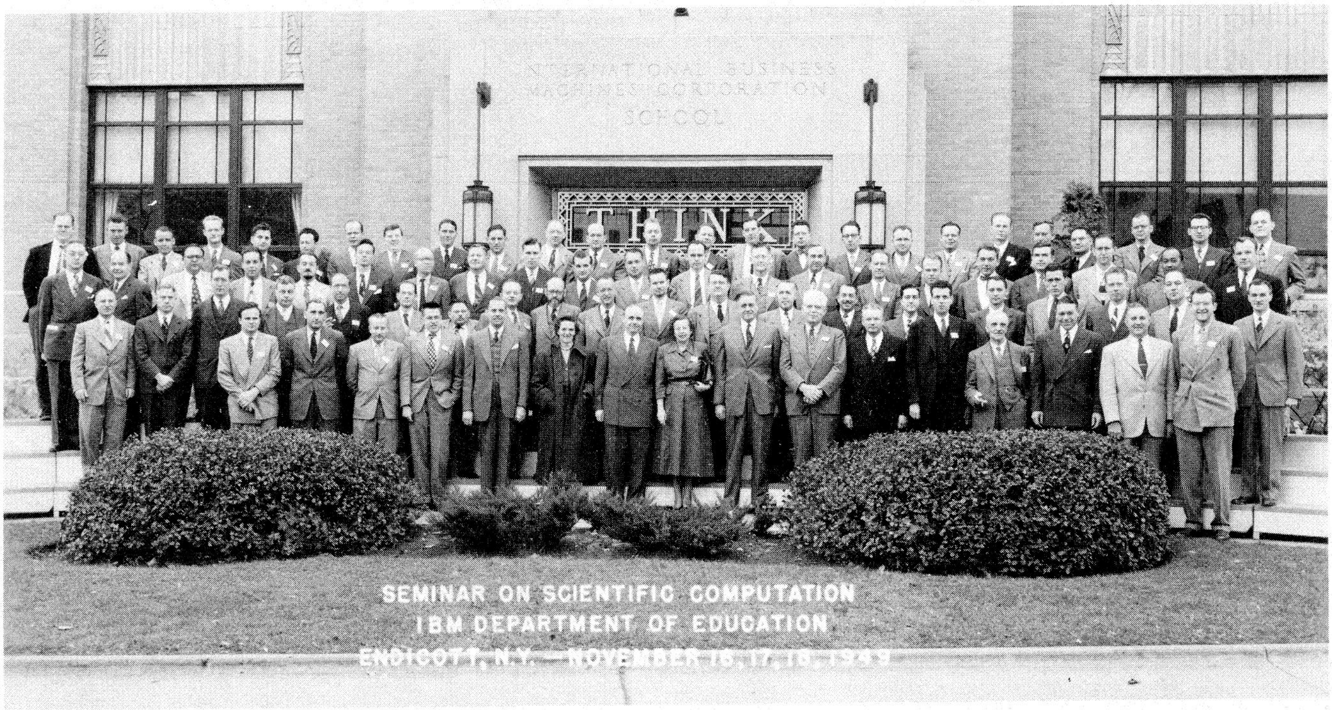
Prof. J. W. Wolfowitz
Columbia University
New York, New York

son, and a customer engineer (perhaps Jack Trout) or perhaps a development engineer (John E. Dayger), would send up the 604 if the CPC was not ready. The time came, the 604 arrived on the elevator, and Pendery and Johnson rolled it out. I then described the random-number calculation, showed the control panel ("plugboard") that I had wired, inserted the control panel, inserted a deck of punched cards on the first of which was punched the starting 10-digit number, waited for the vacuum tubes to warm up, and pressed the start button. The cards began to feed and be punched at the rate of 100 per minute. I held some of them up, and an acute observer in the front row (Bruce G. Oldfield) asked, "Dr. Hurd, why are all those cards punched with only zeros?" I stopped the 604, Pendery or Johnson came forward and made some adjustment which I don't remember, and the demonstration continued perfectly. At a later time in the week the CPC was also demonstrated.

Although he did not discuss a Monte Carlo calculation, at that meeting David L. Hill reported the use of 103 hours of computing time on the SSEC in his paper on the dynamics of nuclear fission (Hurd 1950, pp. 9–16). Von Neumann attended the meeting and gave the banquet address, "The Future of High-Speed Computing." A summary of his remarks, which referred to Monte Carlo methods, was printed (see Hurd 1951). The attendees are shown in the accompanying photograph. The presence of Tom Watson, Jr., who opened the meeting, indicates the importance he placed on the promising new field of scientific computing. I organized the meeting and served as a chairman and "class president."

V. Second Symposium on Monte Carlo Methods

A second symposium was held on the University of Florida campus in Gainesville, March 16–17, 1954.



Participants in seminar on scientific computation. *Left to right*: First row: A. J. J. Van Woerkom, D. T. Perkins, F. C. Hoyt, J. Alexander, G. P. Lovell, M. S. Rees, C. C. Hurd, M. G. Mayer, T. J. Watson, Jr., C. A. Hutchinson, D. Orton, J. R. Bowman, L. Brillouin, D. L. Bibby, J. R. DeHart, P. J. Blatz. *Second row*: Z. W. Birnbaum, W. J. Eckert, J. C. McPherson, L. H. Thomas, R. F. Brill, H. Ekstein, J. F. Eichelberger, E. Isaacson, D. A. Flanders, J. H. Curtiss, G. H. Shortley, G. E. Kimball, J. E. Dayger, O. B. Shafer, E. A. Barber, Jr., R. Plumb, R. K. Winslow, S. W. Dunwell, I. B. Johns, W. C. Davison. *Third row*: A. H. Taub, J. von Neumann, H. Kahn, W. W. Woodbury, H. R. J. Grosch, D. L. Hill, S. R. Brinkley, A. Rose, T. J. Williams, R. W. King, L. M. Haupt, Jr., M. H. Hebb, L. T. Waterman, A. J. Thieblot, A. A. Frost, C. P. Wells, G. W. King, V. Schomaker, R. H. Stark, H. R. Branson, R. L. Merrill. *Fourth row*: J. W. Tukey, B. G. Oldfield, H. Hoerlin, F. J. Martin, V. L. Parsegian, M. Yachter, G. M. Roe, R. J. Seeger, A. S. Newmeyer, M. L. Deutsch, P. Herget, D. E. Kibbey, D. R. Stull, H. C. Vernon, R. W. Hamming, L. M. Arnett, T. Tognola, W. P. Chapman, S. C. Hibbard, R. Hopkins, A. S. Householder, P. V. Vermont, F. A. Ficken.

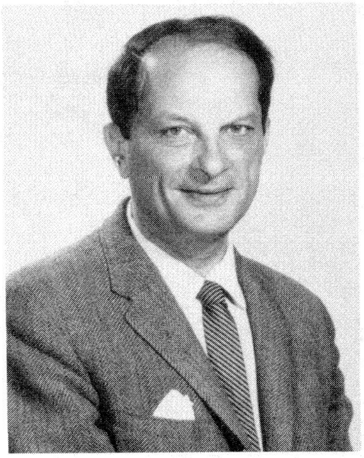
Herbert A. Meyer was the organizer, and the proceedings are published (Meyer 1956). In the acknowledgments, Meyer mentions Paul Rider, A. S. Householder, H. H. Germond, and John von Neumann as having given him encouragement. Seventy people attended; 20 papers were presented and published, along with a bibliography. I chaired one of the sessions.

Perhaps because Meyer is a statistician, or perhaps because of changing emphasis, only two of the papers were specifically on applications in physics. The emphasis was on sampling techniques, random-number generation, and statistical testing. A novel paper by Ulam (Meyer 1956, p. 63) concerned the application of the method to tactical games.

Whereas papers in earlier symposia on Monte Carlo reported most frequently the use of punched-card devices, the following were reported at the Florida symposium. Taussky and Todd: Ferranti, ERA, ENIAC, punched-card equipment, ORDVAC, UNIVAC, EDVAC, SEAC. Dismuke: ORACLE with a flow diagram. Metropolis: MANIAC. Bucher et al.: IBM 605. Berger: SEAC with a flow diagram. Beach and Theus: NARECO. Walther: "two girl students are called up as goddesses of fortune. Each of them gets a box containing 99 counters."

VI. Present-Day Monte Carlo

A notion of the present extent of usage of the Monte Carlo method is given by Rubinstein (1981), who states: "In the last 15 years more than 3000 articles on simulation and the Monte Carlo method have been published." The distinction between Monte Carlo and simulation, if one indeed exists, is not important here; although some believe the two are identical, others believe that Monte Carlo is a subset of simulation, etc. In any event, some 3000 identified articles together with the profusion of software packages with



Stan Ulam.

random-number generators, statistical analyses, and modeling techniques for micro-, mini-, medi- and macrocomputers give an indication of the size of the field.

The most dramatic statement I know concerning the status of Monte Carlo, as nurtured and interacting with the development of higher-speed and more cost-effective computers, is contained in Diaconis and Efron (1983). The paper was brought to my attention by Herbert Solomon, who also remarked that Monte Carlo is growing exponentially. These authors speak of new methods that are:

> fantastic computational spendthrifts; they can easily expend a million arithmetic operations on the analysis of 15 data points. The payoff of such methods is freedom from two limiting factors that have dominated statistical theory since its beginnings: the assumption that the data conform to a bell-shaped curve and the need to focus on statistical measures whose theoretical properties can be analyzed mathematically. These developments have profound implications throughout science, because statistical theory addresses a grand question: How is one to learn what is true?

It is amusing and symbolic that these authors call their method *bootstrap*, a term which I suppose every operator uses explicitly or implicitly every time he or she turns on a personal computer.

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John von Neumann.

COMMENTS

In the meetings I attended with von Neumann, Eckert, Mauchly, and Goldstine (1944, 1945), no one argued about ownership or priority. At the meetings we concentrated on the problems at hand. The disputes came later. Maybe that was the spirit of the 1949 meeting on Monte Carlo methods. Moreover, even if people assumed that Johnny von Neumann played the major role, I don't think Stan Ulam would have stood up and claimed credit, because that would have embarrassed von Neumann. That, at least, is the impression I got from my interactions with Stan, and from his book.

In Section VI, you might note that there are nonprobabilistic simulations; the use of probabilistic proofs that particular numbers are primes might be mentioned.

Arthur W. Burks
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I found Cuthbert's note quite interesting, and by all means it should be published.

Cuthbert wonders why I made a certain trip by train, but in those days I used only surface travel whatever. As I recall, I did not start flying until toward 1960.

He mentions having failed to locate H. H. Germond, but he has been dead for some years. His widow lives in Santa Monica, and I see her every now and then.

Alston S. Householder
6235 Tapia Drive
Malibu, CA 90265

I enjoyed reading Cuthbert's material on Monte Carlo computations. The letter of von Neumann is very much worthwhile; the accounts of the various unclassified meetings are interesting; and Stan Ulam's comments are, as always, amusing.

I feel that not enough discussion has been given to a historical, mathematical setting. That is to say that the general notions (as Hurd points out in Section V) of model sampling and simulation have been known to statisticians for a long time. But interest didn't develop because the concomitant calculations were too arduous if done by hand. Only when modern electronic computing emerged (starting, of course, with the ENIAC) did the method flourish. As Hurd suggests, the center for concentrated discussions was in Los Alamos shortly after World War II, in the wake of the extensive (first-time-ever calculations done on the ENIAC by Frankel, Metropolis, and Turkevich) classified calculations. At the time of these discussions, the ENIAC was being moved from the Moore School, University of Pennsylvania, to Aberdeen Proving Ground, Maryland (von Neumann mentions this), so it was not then available. In fact, a year was to pass.

The first ambitious Monte Carlo problems were planned for the ENIAC by Johnny and Klari von Neumann and me. This was to be preceded by a plan to convert the ENIAC from "jack-plug programming" to a relatively simple stored-program concept utilizing the so-called function tables. The idea was due to Richard Clippinger and was planned and implemented by Klari and me, with cooperation of the ENIAC staff. This is an interesting episode, but another time for it.

An extensive set of Monte Carlo problems was then run on the ENIAC, and completed in the fall of 1948. These results were reported by me at a (classified) colloquium in Los Alamos. Not long after, Stan Ulam suggested that a joint article be submitted to the *Journal of the American Statistical Association*; it appeared in September 1949 as "The Monte Carlo Method."

Subsequent sessions on the ENIAC followed, led by J. Calkin, C. Evans, and G. F. Evans and then a group from Argonne.

During the war, Emilio Segrè, who had discovered two elements, asked me for some suggestions of suitable names for them; they were named technetium and astatine. I enjoyed that kind of game. Later the names Monte Carlo and MANIAC were to follow; the first was very natural and appropriate and it caught on rapidly; the second was an attempt to put an end to the naming of new computers, but it had the opposite effect.

It should be mentioned that during the war, it had occurred to Enrico Fermi that simulation could be an appropriate direction to understand neutron diffusion, and he made some preliminary (presumably, slide rule) calculations. Later he and Percy King constructed an analog device to expedite such calculations.

With respect to recommendations concerning Cuthbert's article: since the initial activity in the revival process was concentrated at Los Alamos in an essentially classified manner, it might be well to expand on that portion and go beyond Johnny's letter. Then the subsequent unclassified meeting that Cuthbert writes about would couple nicely. Perhaps this would be at variance with his original intention and desire. But as it stands, there is a definite lacuna, which was quite exciting.

Incidentally, there was an amusing anecdotal piece about the original set of random numbers prepared on punched cards by Rand. The number of sixes was outside the expected range; this led to a certain embarrassment on Rand's part that was exploited by others. Rand rapidly corrected the deck.

N. Metropolis
Los Alamos National Laboratory
Los Alamos, NM 87545

Needless to say, there were other meetings around that time. I remember having given lectures outlining this method, the first one being a seminar I gave in the Los Alamos theoretical division I believe in the late spring of 1946.

As you undoubtedly know, there is an enormous literature on this method now. Some of my early and subsequent articles are supposed to appear in a collection of my papers under the title: *Science, Computers and People, from the Tree of Mathematics*, published by Birkhauser-Boston, sometime in 1984.

Hurd's account of the very early days, even though of necessity not complete, is admirable and I heartily recommend its publication in your journal.

S. M. Ulam
Sante Fe, NM 87501
(now deceased)

RESPONSES

I am grateful to Burks, Householder, Metropolis, and Ulam for their comments. My responses, in alphabetical order.

Burks's comments on "disputes" have, as he implies, absolutely no counterpart in any Ulam–von Neumann relation. If there ever was a proper and well-justified mutual-admiration society, those two gentlemen were the actors.

Householder comments about his preference for surface travel until about 1960. My remark was intended to cause readers to remember the well-known early discussion about the use of floating-point calculations and the Householder remark that he would never fly in an airplane in whose design floating-point calculations had been used.

Metropolis, an active and articulate participant in the whole affair, supplies interesting embellishments, and I hope that he will write more extensively.

Ulam's letter brought the good news that a collection of his papers will soon appear in print. His many friends and admirers eagerly await.

UNCLASSIFIED DECLASSIFIED UNCLASSIFIED

Classification changed to UNCLASSIFIED by authority of the U. S. Atomic Energy Commission, 7/6/59.

STATISTICAL METHODS IN NEUTRON DIFFUSION

Work Done By:
S. Ulam
J. von Neumann

Report Written By;
R. D. Richtmyer
J. von Neumann

ABSTRACT

There is reproduced here some correspondence on a method of solving neutron diffusion problems in which data are chosen at random to represent a number of neutrons in a chain-reacting system. The history of these neutrons and their progeny is determined by detailed calculations of the motions and collisions of these neutrons, randomly chosen variables being introduced at certain points in such a way as to represent the occurrence of various processes with the correct probabilities. If the history is followed far enough, the chain reaction thus represented may be regarded as a representative sample of a chain reaction in the system in question. The results may be analyzed statistically to obtain various average quantities of interest for comparison with experiments or for design problems.

This method is designed to deal with problems of a more complicated nature than conventional methods based, for example, on the Boltzmann equation. For example, it is not necessary to restrict neutron energies to a single value or even to a finite number of values, and one can study the distribution of neutrons or of collisions of any specified type not only with respect to space variables but with respect to other variables, such as neutron velocity, direction of motion, time. Furthermore, the data can be used for the study of fluctuations and other statistical phenomena.

THE INSTITUTE FOR ADVANCED STUDY
Founded by Mr. Louis Bamberger and Mrs. Felix Fuld
Princeton, New Jersey
School of Mathematics

March 11, 1947

VIA AIRMAIL: REGISTERED

Mr. R. Richtmyer
Post Office Box 1663
Santa Fe, New Mexico

Dear Bob:

This is the letter I promised you in the course of our telephone conversation on Friday, March 7th.

I have been thinking a good deal about the possibility of using statistical methods to solve neutron diffusion and multiplication problems, in accordance with the principle suggested by Stan Ulam. The more I think about this, the more I become convinced that the idea has great merit. My present conclusions and expectations can be summarized as follows:

(1) The statistical approach is very well suited to a digital treatment. I worked out the details of a criticality discussion under the following conditions:
   (a) Spherically symmetric geometry
   (b) Variable (if desired, continuously variable) composition along the radius, of active material (25 or 49), tamper material (28 or Be or WC), and slower-down material (H in some form).
   (c) Isotropic generation of neutrons by all processes of (b).
   (d) Appropriate velocity spectrum of neutrons emerging from the collision processes of (b), and appropriate description of the cross-sections of all processes of (b) as functions of the neutron velocity; i.e., an infinitely many (continuously distributed) neutron velocity group treatment.
   (e) Appropriate account of the statistical character of fissions, as being able to produce (with specified probabilities), say 2 or 3 or 4 neutrons.

   This is still a treatment of "inert" criticality: It does not allow for the hydrodynamics caused by the energy and momentum exchanges and production of the processes of (b), and for the displacements, and hence changes of material distribution, caused by hydrodynamics; i.e., it is not a theory of efficiency. I do know, however, how to expand it into such a theory (cf. (5) below).

   The details enumerated in (a)–(e) were chosen by me somewhat at will. It seems to me that they represent a reasonable model, but it would be easy to make them either more or less elaborate, as desired. If you have any definite desiderata in this respect, please let me know, so that we may analyze their effects on the set-up.

(2) I am fairly certain that the problem of (1), in its digital form, is well suited for the ENIAC. I will have a more specific estimate on this subject shortly. My present (preliminary) estimate is this: Assume that one criticality problem requires following 100 primary neutrons through 100 collisions (of the primary neutron or its descendants) per primary neutron. Then solving one criticality problem should take about 5 hours. It may be, however, that these figures (100×100) are unnecessarily high. A statistical study of the first solutions obtained will clear this up. If they can be lowered, the time will be shortened proportionately.

   A common set-up of the ENIAC will do for all criticality problems. In changing over from one problem of this category to another one, only a few numerical constants will have to be set anew on one of the "function table" organs of the ENIAC.

(3) Certain preliminary explorations of the statistical-digital method could be and should be carried out manually. I will say somewhat more subsequently.

(4) It is not quite impossible that a manual-graphical approach (with a small amount of low-precision digital work interspersed) is feasible. It would require a not inconsiderable number of computers for several days per criticality problem, but it may be possible, and it may perhaps deserve consideration until and unless the ENIAC becomes available. This manual-graphical procedure has actually some similarity with a statistical-graphical procedure with which solutions of a bombing problem were obtained during the war, by a group working under S. Wilks (Princeton University and Applied Mathematics Panel, NDRC). I will look into this matter further, and possibly get Wilks' opinion on the mathematical aspects.

(5) If and when the problem of (1) will have been satisfactorily handled in a reasonable number of special cases, it will be time to investigate the more general case, where hydrodynamics also come into play; i.e., efficiency calculations, as suggested at the end of (1). I think that I know how to set up this problem, too: One has to follow, say 100 neutrons through a short time interval Δt; get their momentum and energy transfer and generation in the ambient matter; calculate from this the displacement of matter; recalculate the history of the 100 neutrons by assuming that matter is in the middle position between its original (unperturbed) state and the above displaced (perturbed) state; recalculate the displacement of matter due to this (corrected) neutron history; recalculate the neutron history due to this (corrected) displacement of matter, etc., etc., iterating in this manner until a "self-consistent" system of neutron history and displacement of matter is reached. This is the treatment of the first time interval Δt. When it is completed, it will serve as a basis for a similar treatment of the second time interval Δt; this, in turn, similarly for the third time interval Δt; etc., etc. In this set-up there will be no serious difficulty in allowing for the role of light, too. If a discrimination according to wavelength is not necessary; i.e., if the radiation can be treated at every point as isotropic and black, and its mean free path is relatively short, then light can be treated by the usual "diffusion" methods, and this is clearly only a very minor complication. If it turns out that the above idealizations are improper, then the photons, too, may have to be treated "individually" and statistically, on the same footing as the neutrons. This is, of course, a non-

trivial complication, but it can hardly consume much more time and instructions than the corresponding neutronic part. It seems to me, therefore, that this approach will gradually lead to a completely satisfactory theory of efficiency, and ultimately permit prediction of the behavior of all possible arrangements, the simple ones as well as the sophisticated ones.

(6) The program of (5) will, of course, require the ENIAC at least, if not more. I have no doubt whatever that it will be perfectly tractable with the post-ENIAC device which we are building. After a certain amount of exploring (1), say with the ENIAC, will have taken place, it will be possible to judge how serious the complexities of (5) are likely to be.

Regarding the actual, physical state of the ENIAC my information is this: It is in Aberdeen, and it is being put together there. The official date for its completion is still April 1st. Various people give various subjective estimates as to the actual date of completion, ranging from mid-April to late May. It seems as if the late May estimate were rather safe.

I will inquire more into this matter, and also into the possibility of getting some of its time subsequently. The indications that I have had so far on the latter score are encouraging.

In what follows, I will give a more precise description of the approach outlined in (1); i.e., of the simplest way I can now see to handle this group of problems.

Consider a spherically symmetric geometry. Let r be the distance from the origin. Describe the inhomogeneity of this system by assuming N concentric, homogeneous (spherical shell) zones, enumerated by an index $i = 1, \cdots, N$. Zone No. i is defined by $r_{i-1} \leqq r \leqq r_i$, the $r_0, r_1, r_2, \cdots, r_{N-1}, r_N$ being given:

$$0 = r_0 < r_1 < r_2 < \cdots < r_{N-1} < r_N = R,$$

where R is the outer radius of the entire system.

Let the system consist of the three components discussed in (1), (b), to be denoted A, T, S, respectively. Describe the composition of each zone in terms of its content of each of A, T, S. Specify these for each zone in relative volume fractions. Let these be in zone No. i x_i, y_i, z_i, respectively.

Introduce the cross sections per cm^3 of pure material, multiplied by $^{10}\log e = .43 \cdots$, and as functions of the neutron velocity v, as follows:

Absorption in A, T, S: $\Sigma_{aA}(v)$, $\Sigma_{aT}(v)$, $\Sigma_{as}(v)$.

Scattering in A, T, S: $\Sigma_{sA}(v)$, $\Sigma_{sT}(v)$, $\Sigma_{sS}(v)$.

Fission in A, with production of 2, 3, 4 neutrons: $\Sigma_{fA}^{(2)}(v)$, $\Sigma_{fA}^{(3)}(v)$, $\Sigma_{fA}^{(4)}(v)$.

Scattering as well as fission are assumed to produce isotropically distributed neutrons, with the following velocity distributions:

If the incident neutron has the velocity v, then the scattered neutrons velocity statistics are described for A, T, S, by the relations

$$v' = v\varphi_A(\nu),\ v' = v\varphi_T(\nu),\ v' = v\varphi_S(\nu).$$

Here v' is the velocity of the scattered neutron, $\varphi_A(\nu)$, $\varphi_T(\nu)$, $\varphi_S(\nu)$ are known functions, characteristic of the three substances A, T, S (they vary all from 1 to 0), and ν is a random variable, statistically equidistributed in the interval 0, 1.

Every fission neutron has the velocity v_0.

I suppose that this picture either gives a model or at least provides a prototype for essentially all those phenomena about which we have relevant observational information at present, and actually for somewhat more. It may be expected to provide a reasonable vehicle for the additional relevant observational material that is likely to arise in the near future.

Do you agree with this?

In this model the state of a neutron is characterized by its position r, its velocity v, and the angle θ between its direction of motion and the radius. It is more convenient to replace θ by $s = r \cos \theta$, so that $\sqrt{r^2 - s^2}$ is the "perihelion distance" of its (linearly extrapolated) path.

Note that if a neutron is produced isotropically; i.e., if its direction "at birth" is equidistributed, then (because space is three-dimensional) $\cos \theta$ will be equidistributed in the interval $-1, 1$; i.e., s in the interval $-r, r$.

It is convenient to add to the characterization of a neutron explicitly the No. i of the zone in which it is found; i.e., with $r_{i-1} \leqq r \leqq r_i$. It is furthermore advisable to keep track of the time t to which the specifications refer.

Consequently, a neutron is characterized by these data:

$$i,\ r,\ s, v, t.$$

Now consider the subsequent history of such a neutron. Unless it suffers a collision in zone No. i, it will leave this zone along its straight path, and pass into zones Nos. $i + 1$ or $i - 1$. It is desirable to start a "new" neutron whenever the neutron under consideration has suffered a collision (absorbing, scattering, or fissioning – in the last-mentioned case several "new" neutrons will, of course, have to be started), or whenever it passes into another zone (without having collided).

Consider first, whether the neutron's linearly extrapolated path goes forward from zone No. i into zone No. $i + 1$ or $i - 1$. Denote these two possibilities by I and II.

If the neutron moves outward; i.e., if $s \geqq 0$, then we have certainly I. If the neutron moves inward; i.e., if $s < 0$, then we have either I or II, the latter if, and only if, the path penetrates at all into the sphere r_{i-1}. It is easily seen that the latter is equivalent to $s^2 >> r^2 - r_{i-1}^2$. So we have:

$$s \geqq 0 \quad \therefore A$$

$$s < 0 \quad \therefore B \begin{cases} r_{i-1}^2 + s^2 - r^2 \leqq 0 & \therefore B' \} \quad \therefore \text{I.} \\ r_{i-1}^2 + s^2 - r^2 > 0 & \therefore B'' \} \quad \therefore \text{II.} \end{cases}$$

The exit from zone No. i will therefore occur at

$$r^* \begin{cases} = r_i & \text{for I.} \\ = r_{i-1} & \text{for II.} \end{cases}$$

It is easy to calculate that the distance from the neutron's original position to the exit position is $d = s^* - s$, where

$$s^* = \pm \sqrt{r^{*2} + s^2 - r^2}, \qquad \left.\begin{matrix} + & \text{for I} \\ - & \text{for II} \end{matrix}\right| .$$

The probability that the neutron will travel a distance d^1 without suffering a collision is 10^{-fd^1}, where

$$f = (\Sigma_{aA}(v) + \Sigma_{sA}(v) + \Sigma_{fA}^{(2)}(v) + \Sigma_{fA}^{(3)}(v) + \Sigma_{fA}^{(4)}(v))x_i + (\Sigma_{aT}(v) + \Sigma_{sT}(v))\, y_i + (\Sigma_{aS}(v) + \Sigma_{sS}(v))\, z_i.$$

It is at this point that the statistical character of the method comes into evidence. In order to determine the actual fate of the neutron, one has to provide now the calculation with a value λ, belonging to a random variable, statistically equidistributed in the interval 0, 1; i.e., λ is to be picked at random from a population that is statistically equidistributed in the interval 0, 1. Then it is decreed that 10^{-fd^1} has turned out to be λ; i.e.,

$$d^1 = \frac{-\,{}^{10}\log \lambda}{f}.$$

From here on, the further procedure is clear.

If $d^1 \geqq d$, then the neutron is ruled to have reached the neighboring zone No. $i \pm 1$ $\left(\begin{smallmatrix}+\text{ for I}\\ -\text{ for II}\end{smallmatrix}\right)$ without having suffered a collision. The "new" neutron (i.e., the original one, but viewed at the interzone boundary, and heading into the new zone), has characteristics which are easily determined: i is replaced by $i \pm 1$, r by r^*, s is easily seen to go over into s^*, v is unchanged, t goes over into $t^* = t + \dfrac{d}{v}$. Hence, the "new" characteristics are

$$i \pm 1,\ r^*,\ s^*,\ v,\ t^*.$$

If, on the other hand, $d^1 < d$, then the neutron is ruled to have suffered a collision while still within zone No. i, after a travel d^1. The position at this stage is now

$$r^* = \sqrt{r^2 + 2sd^1 + (d^1)^2},$$

and the time

$$t^* = t + \frac{d^1}{v}.$$

The characteristic contains, accordingly, at any rate i, r^* and t^* in place of i, r and t. It remains to determine what becomes of s and v.

As pointed out before, the "new" s will be equidistributed in the interval $-r^*$, r^*. It is therefore only necessary to provide the calculation with a further value ρ', belonging to a random variable, statistically equidistributed in the interval 0, 1. Then one can rule that s has the value

$$s' = r^*\,(2\rho' - 1).$$

As to the "new" v, it is necessary to determine first the character of the collection: Absorption (by any one of A, T, S); scattering by A, or by T, or by S; fission (by A) producing 2, or 3, or 4 neutrons. These seven alternatives have the relative probabilities

$$f_1 = \Sigma_{aA}(v)x_i + \Sigma_{aT}(v)y_i + \Sigma_{aS}(v)z_i,$$

$$f_2 - f_1 = \Sigma_{sA}(v)x_i,$$

$$f_3 - f_2 = \Sigma_{sT}(v)y_i,$$

$$f_4 - f_3 = \Sigma_{sS}(v)z_i,$$

$$f_5 - f_4 = \Sigma_{fA}^{(2)}(v)x_i,$$

$$f_6 - f_5 = \Sigma_{fA}^{(3)}(v)x_i,$$

$$f - f_6 = \Sigma_{fA}^{(4)}(v)x_i.$$

We can therefore now determine the character of the collision by a statistical procedure like the preceding ones: Provide the calculation with a value μ belonging to a random variable, statistically equidistributed in the interval 0, 1. Form $\bar{\mu} = \mu f$, this is then equidistributed in the interval 0, f. Let the seven above cases correspond to the seven intervals $0, f_1; f_1, f_2; f_2, f_3; f_3, f_4; f_4, f_5; f_5, f_6; f_6, f$, respectively. Rule, that that one of those seven cases holds in whose interval $\bar{\mu}$ actually turns out to lie.

Now the value of v can be specified. Let us consider the seven available cases in succession.

Absorption: The neutron has disappeared. It is simplest to characterize this situation by replacing v by 0.

Scattering by A: Provide the calculation with a value ν belonging to a random variable, statistically equidistributed in the interval 0, 1. Replace v by

$$v' = v\varphi_A(\nu).$$

Scattering by T: Same as above, but

$$v' = v\varphi_T(\nu).$$

Scattering by S: Same as above, but

$$v' = v\varphi_S(\nu).$$

Fission: In this case replace v by v_0. According to whether the case in question is that one corresponding to the production of 2, 3, or 4 neutrons, repeat this 2, 3, or 4 times, respectively. This means that, in addition to the ρ', s' discussed above, the further ρ'', s''; ρ''', s'''; ρ'''', s'''' may be needed.

This completes the mathematical description of the procedure. The computational execution would be something like this:

Each neutron is represented by a card C which carries its characteristics

$$i,\ r,\ s,\ v,\ t,$$

and also the necessary random values

$$\lambda,\ \mu,\ \nu,\ \rho',\ \rho'',\ \rho''',\ \rho''''.$$

I can see no point in giving more than, say, 7 places for each one of the 5 characteristics, or more than, say, 5 places for each of the 7 random variables. In fact, I would judge that these numbers of places are already higher than necessary. At any rate, even in this way only 70 entries are consumed, and so the ordinary 80-entry punchcard will have 10 entries left over for any additional indexings, etc., that one may desire.

The computational process should then be so arranged as to produce the card C' of the "new" neutron, or rather 1 to 4 such cards C', C'', C''', C'''' (depending on the neutrons actual history, cf. above). Each card, however, need only be provided with the 5 characteristics of its neutron. The 7 random variables can be inserted in a subsequent operation, and the cards with $v = 0$ (i.e., corresponding to neutrons that were absorbed within the assembly) as well as those with $i = N + 1$ (i.e., corresponding to neutrons that escaped from the assembly), may be sorted out.

The manner in which this material can then be used for all kinds of neutron statistic investigations is obvious.

I append a tentative "computing sheet" for the calculation above. It is, of course, neither an actual "computing sheet" for a (human) computer group, nor a set-up for the ENIAC, but I think that it is well suited to serve as a basis for either. It should give a reasonably immediate idea of the amount of work that is involved in the procedure in question.

I cannot assert this with certainty yet, but it seems to me very likely that the instructions given on this "computing sheet" do not exceed the "logical" capacity of the ENIAC. I doubt that the processing of 100 "neutrons" will take much longer than the reading, punching and (once) sorting time of 100 cards; i.e., about 3 minutes. Hence, taking 100 "neutrons" through 100 of these stages should take about 300 minutes; i.e., 5 hours.

Please let me know what you and Stan think of these things. Does the approach and the formulation and generality of the criticality problem seem reasonable to you, or would you prefer some other variant?

Would you consider coming East some time to discuss matters further? When could this be?

With best regards;

Very truly yours,

John von Neumann

TENTATIVE COMPUTING SHEET

Data:

(1) r_i, x_i, y_i, z_i as functions of $i = 1, \cdots, N$.[1] $(r_0 = 0.)$

(2) $\Sigma_{aA}(v), \Sigma_{aT}(v), \Sigma_{aS}(v), \Sigma_{sA}(v), \Sigma_{sT}(v), \Sigma_{sS}(v), \Sigma_{fA}^{(2)}(v), \Sigma_{fA}^{(3)}(v), \Sigma_{fA}^{(4)}(v)$ as functions of $v \geqq 0, \leqq v_0$.[2]

(3) v_0.

(4) $\varphi_A(\nu), \varphi_T(\nu), \varphi_S(\nu)$ as functions of $\nu \geqq 0, \leqq 1$.[2]

(5) ${}^{-10}\log \lambda$ as function of $\lambda \geqq 0, \leqq 1$.[2]

Card C:

C_1	i
C_2	r
C_3	s
C_4	v
C_5	t

Random Variables:

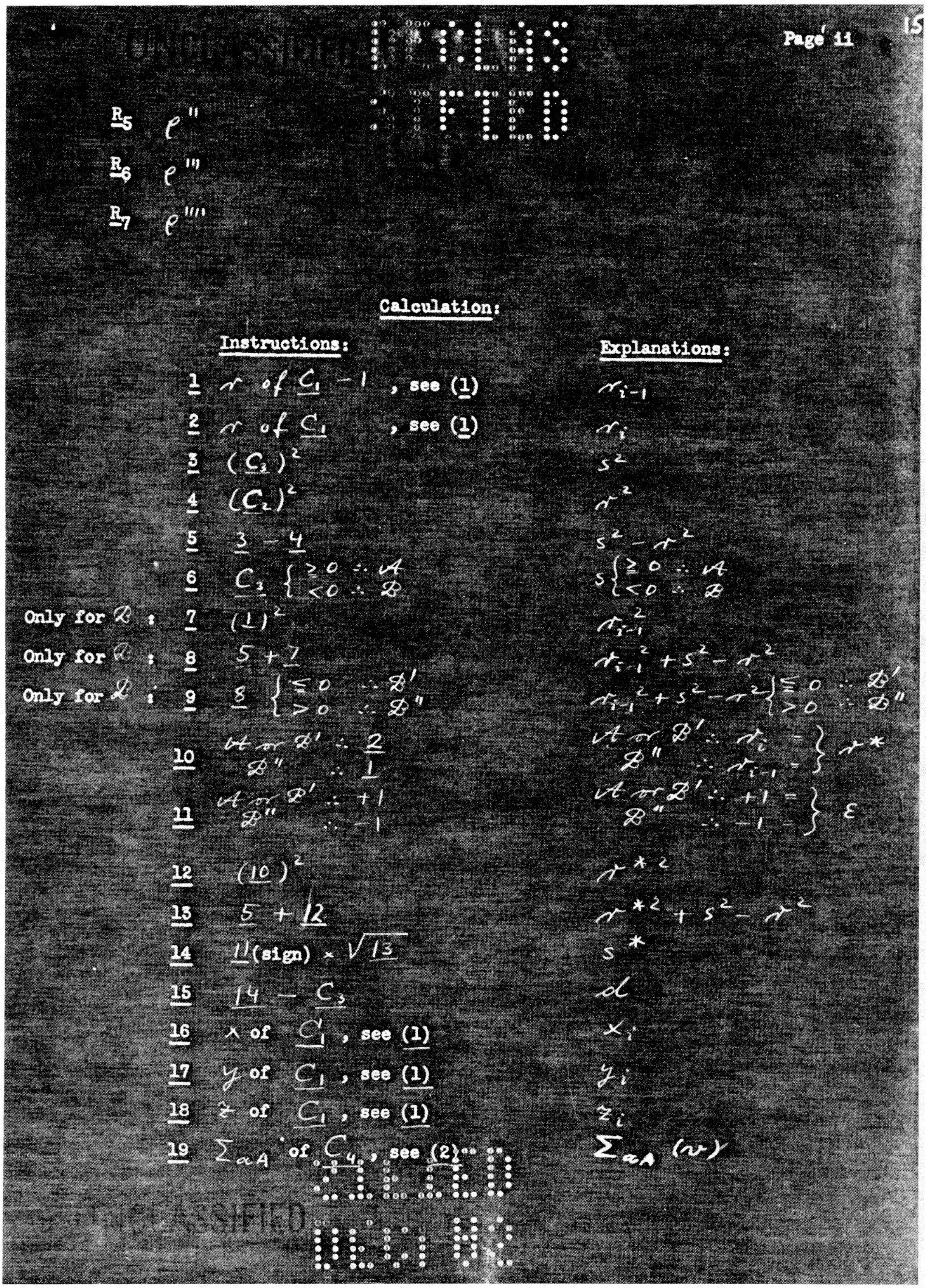
R_1	λ
R_2	μ
R_3	ν
R_4	ρ'
R_5	ρ''
R_6	ρ'''
R_7	ρ''''

Calculation:

	Instructions:	*Explanations*:
	1 r of $C_1 - 1$, see *(1)*	r_{i-1}
	2 r of C_1, see *(1)*	r_i
	3 $(C_3)^2$	s^2
	4 $(C_2)^2$	r^2
	5 *3* − *4*	$s^2 - r^2$
	6 $C_3 \begin{cases} \geqq 0 \therefore A \\ < 0 \therefore B \end{cases}$	$s \begin{cases} \geqq 0 \therefore A \\ < 0 \therefore B \end{cases}$
Only for B:	*7* $(1)^2$	r_{i-1}^2
Only for B:	*8* *5* + *7*	$r_{i-1}^2 + s^2 - r^2$
Only for B:	*9* *8* $\begin{cases} \leqq 0 \therefore B' \\ > 0 \therefore B'' \end{cases}$	$r_{i-1}^2 + s^2 - r^2 \begin{cases} \leqq 0 \therefore B' \\ > 0 \therefore B'' \end{cases}$
	10 A or $B' \therefore$ *2*; $B'' \therefore$ *1*	$\left.\begin{matrix} A \text{ or } B' \therefore r_i = \\ B'' \therefore r_{i-1} = \end{matrix}\right\} r^*$
	11 A or $B' \therefore +1$; $B'' \therefore -1$	$\left.\begin{matrix} A \text{ or } B' \therefore +1 = \\ B'' \therefore -1 = \end{matrix}\right\} \epsilon$
	12 $(10)^2$	r^{*2}
	13 *5* + *12*	$r^{*2} + s^2 - r^2$
	14 *11*(sign) × $\sqrt{13}$	s^*
	15 *14* − C_3	d
	16 x *of* C_1, see *(1)*	x_i
	17 y of C_1, see *(1)*	y_i
	18 z of C_1, see *(1)*	z_i
	19 Σ_{aA} of C_4, see *(2)*	$\Sigma_{aA}(v)$
	20 *16* × *19*	$\Sigma_{aA}(v)\, x_i$
	21 Σ_{aT} of C_4, see *(2)*	$\Sigma_{aT}(v)$
	22 *17* × *21*	$\Sigma_{aT}(v) y_i$
	23 *20* + *22*	$\Sigma_{aA}(v) x_i + \Sigma_{aT}(v) y_i$
	24 Σ_{aS} of C_4 see *(2)*	$\Sigma_{aS}(v)$
	25 *18* × *24*	$\Sigma_{aS}(v) z_i$
	26 *23* + *25*	$f_1 = \Sigma_{aA}(v) x_i + \Sigma_{aT}(v) y_i + \Sigma_{aS}(v) z_i$
	27 Σ_{sA} of C_4, see *(2)*	$\Sigma_{sA}(v)$
	28 *16* × *27*	$f_2 - f_1 = \Sigma_{sA}(v) x_i$
	29 *26* + *28*	f_2
	30 Σ_{sT} of C_4, see *(2)*	$\Sigma_{sT}(v)$
	31 *17* × *30*	$f_3 - f_2 = \Sigma_{sT}(v) y_i$
	32 *29* + *31*	f_3
	33 Σ_{sS} of C_4, see *(2)*	$\Sigma_{sS}(v)$
	34 *18* × *33*	$f_4 - f_3 = \Sigma_{sS}(v) z_i$
	35 *32* + *34*	f_4
	36 $\Sigma_{fA}^{(2)}$ of C_4, see *(2)*	$\Sigma_{fA}^{(2)}(v)$

[1] Tabulated. (Discrete domain.)

[2] Tabulated, to be interpolated, or approximated by polynomials. (Continuous domain.)



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$\underline{R_5}$ ρ''

$\underline{R_6}$ ρ'''

$\underline{R_7}$ ρ''''

Calculation:

		Instructions:	Explanations:
	1	r of $\underline{C_1} - 1$, see ($\underline{1}$)	r_{i-1}
	2	r of $\underline{C_1}$, see ($\underline{1}$)	r_i
	3	$(\underline{C_3})^2$	s^2
	4	$(\underline{C_2})^2$	r^2
	5	$\underline{3} - \underline{4}$	$s^2 - r^2$
	6	$\underline{C_3} \begin{cases} \geq 0 \therefore \mathcal{A} \\ < 0 \therefore \mathcal{D} \end{cases}$	$s \begin{cases} \geq 0 \therefore \mathcal{A} \\ < 0 \therefore \mathcal{D} \end{cases}$
Only for $\mathcal{D}$:	7	$(\underline{1})^2$	r_{i-1}^2
Only for $\mathcal{D}$:	8	$\underline{5} + \underline{7}$	$r_{i-1}^2 + s^2 - r^2$
Only for $\mathcal{D}$:	9	$\underline{8} \begin{cases} \leq 0 \therefore \mathcal{D}' \\ > 0 \therefore \mathcal{D}'' \end{cases}$	$r_{i-1}^2 + s^2 - r^2 \begin{cases} \leq 0 \therefore \mathcal{D}' \\ > 0 \therefore \mathcal{D}'' \end{cases}$
	10	$\mathcal{A}$ or $\mathcal{D}' \therefore \underline{2}$; $\mathcal{D}'' \therefore \underline{1}$	$\left.\begin{matrix} \mathcal{A} \text{ or } \mathcal{D}' \therefore r_i = \\ \mathcal{D}'' \therefore r_{i-1} = \end{matrix}\right\} r^*$
	11	$\mathcal{A}$ or $\mathcal{D}' \therefore +1$; $\mathcal{D}'' \therefore -1$	$\left.\begin{matrix} \mathcal{A} \text{ or } \mathcal{D}' \therefore +1 = \\ \mathcal{D}'' \therefore -1 = \end{matrix}\right\} \varepsilon$
	12	$(\underline{10})^2$	r^{*2}
	13	$\underline{5} + \underline{12}$	$r^{*2} + s^2 - r^2$
	14	$\underline{11}$(sign) $\times \sqrt{\underline{13}}$	s^*
	15	$\underline{14} - \underline{C_3}$	d
	16	x of $\underline{C_1}$, see ($\underline{1}$)	x_i
	17	y of $\underline{C_1}$, see ($\underline{1}$)	y_i
	18	z of $\underline{C_1}$, see ($\underline{1}$)	z_i
	19	Σ_{aA} of $\underline{C_4}$, see ($\underline{2}$)	$\Sigma_{aA}(v)$

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Page from 1947 Los Alamos report. The entire report is reprinted here.

	37	16×36	$f_5 - f_4 = \Sigma^{(2)}_{\mathrm{fA}}(v)x_i$
	38	$35 + 37$	f_5
	39	$\Sigma^{(3)}_{\mathrm{fA}}$ of C_4, see (2)	$\Sigma^{(3)}_{\mathrm{fA}}(v)$
	40	16×39	$f_6 - f_5 = \Sigma^{(3)}_{\mathrm{fA}}(v)x_i$
	41	$38 + 40$	f_6
	42	$\Sigma^{(4)}_{\mathrm{fA}}$ of C_4, see (2)	$\Sigma^{(4)}_{\mathrm{fA}}(v)$
	43	16×42	$f - f_6 = \Sigma^{(4)}_{\mathrm{fA}}(v)x_i$
	44	$41 + 43$	f
	45	$^{-10}\log$ of R_1, see (5)	$^{-10}\log \lambda$
	46	$45:44$	d^1
	47	$46 \begin{cases} \geqq 15 \therefore P \\ < 15 \therefore Q \end{cases}$	$d^1 \begin{cases} \geqq d \therefore P \\ < d \therefore Q \end{cases}$
	48	$\left.\begin{array}{l} P \therefore 15: \\ Q \therefore 46: \end{array}\right\} C_4$	$\left.\begin{array}{l} P \therefore d: \\ Q \therefore d^1: \end{array}\right\} v = \tau$
	49	$C_5 + 48$	$t^* = t + \tau$
Only for P:	50	$C_1 + 11$	$i^* = i + \epsilon$
Only for P:	51	C_1': 50	C_1': i^*
		C_2': 10	C_2': r^*
		C_3': 14	C_3': s^*
		C_4': C_4	C_4': v
		C_5': 49	C_5': t^*

From here on only Q:

	52	$R_2 \times 44$	$\bar{\mu} = \mu f$
	53	$52 < 26 \therefore Q_1$	$\bar{\mu} < f_1 \therefore Q_1$
		$\left.\begin{array}{l} \geqq 26 \\ < 29 \end{array}\right\} \therefore Q_2$	$\left.\begin{array}{l} \geqq f_1 \\ < f_2 \end{array}\right\} \therefore Q_2$
		$\left.\begin{array}{l} \geqq 29 \\ < 32 \end{array}\right\} \therefore Q_3$	$\left.\begin{array}{l} \geqq f_2 \\ < f_3 \end{array}\right\} \therefore Q_3$
		$\left.\begin{array}{l} \geqq 32 \\ < 35 \end{array}\right\} \therefore Q_4$	$\left.\begin{array}{l} \geqq f_3 \\ < f_4 \end{array}\right\} \therefore Q_4$
		$\left.\begin{array}{l} \geqq 35 \\ < 38 \end{array}\right\} \therefore Q_5$	$\left.\begin{array}{l} \geqq f_4 \\ < f_5 \end{array}\right\} \therefore Q_5$
		$\left.\begin{array}{l} \geqq 38 \\ < 41 \end{array}\right\} \therefore Q_6$	$\left.\begin{array}{l} \geqq f_5 \\ < f_6 \end{array}\right\} \therefore Q_6$
		$\geqq 41 \therefore Q_7$	$\geqq f_6 \therefore Q_7$
	54	$C_3 \times 46$	sd^1
	55	2×54	$2sd^1$
	56	$(46)^2$	$(d^1)^2$
	57	$55 + 56$	$2sd^1 + (d^1)^2$
	58	$4 + 57$	$r^2 + 2sd^1 + (d^1)^2$
	59	$\sqrt{58}$	r^*
Only for Q_1:	60	C_1': C_1	C_1': i
		C_2': 59	C_2': r^*
		C_3': $\cdots$	C_3': $\cdots$
		C_4': 0	C_4': 0
		C_5': 49	C_5': t^*

From here on only $Q_2, \cdots, Q_7$:

Only for Q_2:	61	φ_{A} of R_3, see (4)	$\varphi_{\mathrm{A}}(v) = \varphi$
Only for Q_3:	62	φ_{T} of R_3, see (4)	$\varphi_{\mathrm{T}}(v) = \varphi$
Only for Q_4:	63	φ_{S} of R_3, see (4)	$\varphi_{\mathrm{S}}(v) = \varphi$
Only for Q_2, Q_3, Q_4:	64	$C_4 \times$ (61 or 62 or 63)	$v\varphi$
	65	$Q_2, Q_3, Q_4 \therefore 64$ $Q_5, Q_6, Q_7 \therefore (3)$	$\left.\begin{array}{l} Q_2, Q_3, Q_4 \therefore v\varphi = \\ Q_5, Q_6, Q_7 \therefore v_0 = \end{array}\right\} v'$
	66	$2 \times R_4$	$2\rho'$
	67	$66 - 1$	$2\rho' - 1$
	68	59×67	s'
	69	C_1': C_1	C_1': i
		C_2': 59	C_2': r^*
		C_3': 68	C_3': s'
		C_4': 65	C_4': v'
		C_5': 49	C_5': t^*

From here on only Q_5, Q_6, Q_7:

70	$2 \times R_5$	$2\rho''$
71	$70 - 1$	$2\rho'' - 1$
72	59×71	s''
73	C_1'': C_1	C_1'': i
	C_2'': 59	C_2'': r^*
	C_3'': 72	C_3'': s''
	C_4'': 65	C_4'': v'
	C_5'': 49	C_5'': t^*

From here on only Q_6, Q_7:

74	$2 \times R_6$	$2\rho'''$
75	$74 - 1$	$2\rho''' - 1$
76	59×75	s'''
77	C_1''': C_1	C_1''': i
	C_2''': 59	C_2''': r^*
	C_3''': 76	C_3''': s'''
	C_4''': 65	C_4''': v'
	C_5''': 49	C_5''': t^*

From here on only Q_7:

78	$2 \times R_7$	$2\rho''''$
79	$78 - 1$	$2\rho'''' - 1$
80	59×79	s''''
81	C_1'''': C_1	C_1'''': i
	C_2'''': 59	C_2'''': r^*
	C_3'''': 80	C_3'''': s''''
	C_4'''': 65	C_4'''': v'
	C_5'''': 49	C_5'''': t^*

April 2, 1947

Professor John vonNeumann,
The Institute for Advanced Study,
School of Mathematics
Princeton, New Jersey

Dear Johnny:

As Stan told you, your letter has aroused a great deal of interest here. We have had a number of discussions of your method and Bengt Carlson has even set to work to test it out by hand calculation in a simple case.

It has occurred to us that there are a number of modifications which one might wish to introduce, at least for calculations of a certain type. This would be true, for example, if one wished to set up the problem for a metal system containing a 49 core in a tuballoy tamper. It seems to us that it might at present be easier to define problems of this sort than, for example, problems for hydride gadgets. It is not so much our intention to suggest that the method you are working on now should be modified as to suggest that perhaps alternative procedures should be worked out also. Perhaps one of us could do this with a little assistance from you; for example, during a visit to Princeton.

The specific points at which it seems to us modifications might be desired are as follows:

1. Of the three components A, T, S that you consider, only one is fissionable, whereas in systems of interest to us, there will be an appreciable number of fissions in the tub-alloy of the tamper, as well as in the core material.

2. On the other hand, we are not likely for some time to have data enabling one to distinguish between the velocity dependence of the three functions

$$\Sigma_{\mathrm{fa}}^{(2)}(v), \qquad \Sigma_{\mathrm{fa}}^{(3)}(v), \qquad \Sigma_{\mathrm{fa}}^{(4)}(v)$$

that you introduce so that for any particular isotope these might as well be combined into a single function of velocity with a random procedure used merely for determining the number of neutrons emerging. If there is a single such function of velocity for each of the three isotopes 25, 28, 49, the total number of function tables required would be the same as in your letter.

3. It is suggested that in the case of 25 or 28, one might wish to allow also for the possibility of one neutron emerging from fission. The dispersion of the number of neutrons per fission is not too well known but we think we could provide some guesses.

4. Because of the sensitive dependence of tamper fissions on the neutron energy spectrum, it might be advisable to feed in the measured fission spectrum at the appropriate point. This would, of course, require introduction of one or two additional random variables and would raise the nasty question of possible velocity correlation between neutrons emerging from a given fission.

5. Material S could, of course, be omitted for systems of this sort. On the other hand, when moderation really occurs, it seems to us there would have to be a correlation between velocity and direction of the scattered neutron.

6. For metal systems of the type considered, it would probably be adequate to assume just one elastically scattering component and just one inelastically scattering component. These could be mixed with the fissionable components in suitable proportions to mock up most materials of interest.

In addition, we have one general comment as follows: Suppose that the initial deck of cards represents a group of neutrons all having $t = 0$ as their time of origin. Then after a certain number of cycles of operations, say 100, one will have a deck of cards representing a group of neutrons having times of origin distributed from some earliest t_1, to a latest, t_2. Thus all of the multiplicative chains will have been followed until time t_1 and some of them will have been followed to various later times. Then if one wishes, for example, to find the spatial distribution of fissions, it would be natural to examine all fissions occurring in some interval Δt and find their spatial distribution. But unless the interval Δt is chosen within the interval $(0, t_1)$ one cannot be sure that he knows about *all* the fissions taking place on Δt, and the fissions that are left out of account may well have a systematically different spatial distribution than those that are taken into account. Therefore, if, as seems likely $t_1 \ll t_2$, it would seem to be necessary to discard most of the data obtained by the calculation. The obvious remedy for this difficulty would seem to be as follow the chains for a definite time rather than for a definite number of cycles of operation. After each cycle, all cards having t greater than some preassigned value would be discarded, and the next cycle of calculation performed with those remaining. This would be repeated until the number of cards in the deck diminishes to zero.

These suggestions are all very tentative. Please let us know that you think of them.

Sincerely,

R. D. Richtmyer.

cc: S. Ulam
C. Mark
B. Carlson