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Computer Simulations and the Trading Zone



At the root of most accounts of the development of science is the covert premise that science is about ontology: What objects are there? How do they interact? And how do we discover them? It is a premise that underlies three distinct epochs of inquiry into the nature of science. Among the positivists, the later Carnap explicitly advocated a view that the objects of physics would be irretrievably bound up with what he called a "framework," a full set of linguistic relations such as is found in Newtonian or Einsteinian mechanics. That these frameworks held little in common did not trouble Carnap; prediction mattered more than progress. Kuhn molded this notion and gave it a more historical focus. Unlike the positivists, Kuhn and other commentators of the 1960's wanted to track the process by which a community abandoned one framework and adopted another. To more recent scholars, the Kuhnian categorization of group affiliation and disaffiliation was by and large correct, but its underlying dynamics were deemed deficient because they paid insufficient attention to the sociological forces that bound groups to their paradigms. All three generations embody the root assumption that the analysis of science studies (or ought to study) a science classified and divided according to the objects of its inquiry. All three assume that as these objects change, particular branches of science split into myriad disconnected parts. It is a view of scientific disunity that I will refer to as "framework relativism."

In this essay, as before, I will oppose this view, but not by

invoking the old positivist pipe dreams: no universal protocol languages, no physicalism, no Comtian hierarchy of knowledge, and no radical reductionism. Instead, I will focus on what appears at first to be a chaotic assemblage of disciplines and activities: thermonuclear weapons, enhanced A-bombs, poison gas, weather prediction, pion-nucleon interactions, number theory, probability theory, industrial chemistry, and quantum mechanics. No entities bind them together; they fall into no clear framework or paradigm; they have no single history that can be narrated smoothly across time. Yet the practice of these activities was sufficiently congruent in the years just after World War II for Enrico Fermi, John von Neumann, Stanislaw Ulam, and others to move back and forth across widely divergent domains. What they shared was not common laws, and most certainly not a common ontology. They held a new cluster of skills in common, a new mode of producing scientific knowledge that was rich enough to coordinate highly diverse subject matter.

Their common activity centered around the computer. More precisely, nuclear-weapons theorists transformed the nascent "calculating machine," and in the process created alternative realities to which both theory and experiment bore uneasy ties. Grounded in statistics, game theory, sampling, and computer coding, these simulations constituted what I have been calling a "trading zone," an arena in which radically different activities could be *locally*, but not *globally*, coordinated.

Simulations

During and shortly after World War II, nuclear-weapons builders created a mode of inquiry to address problems too complex for theory and too remote from laboratory materials for experiment. Christened "Monte Carlo" after the gambling mecca, the method amounted to the use of random numbers (*à la roulette*) to simulate the stochastic processes too complex to calculate in full analytic glory. But physicists and engineers soon elevated the Monte Carlo above the lowly status of a mere numerical calculation scheme; it came to constitute an alternative reality—in some cases a preferred one—on which "experimentation" could be conducted. Proven on what at the time was the most complex problem ever undertaken in

the history of science—the design of the first hydrogen bomb—the Monte Carlo ushered physics into a place paradoxically dislocated from the traditional reality that borrowed from both experimental and theoretical domains, bound these borrowings together, and used the resulting bricolage to create a marginalized netherland that was at once nowhere and everywhere on the usual methodological map. In the process of negotiating the relationship of the Monte Carlo to traditional categories of experiment and theory, the simulators both altered and helped define what it meant to be an experimenter or theorist in the decades following World War II.

At Los Alamos during the war, physicists soon recognized that the central problem was to understand the process by which neutrons fission, scatter, and join uranium nuclei deep in the fissile core of a nuclear weapon. Experiment could not probe the critical mass with sufficient detail; theory led rapidly to unsolvable integro-differential equations. With such problems, the artificial reality of the Monte Carlo was the only solution—the sampling method could “recreate” such processes by modeling a sequence of random scatterings on a computer. Simulations advanced the design (more particularly, the refinement) of fission weapons, but remained somewhat auxiliary to the A-bomb theorists. When, at war’s end, nuclear-weapons work turned to the thermonuclear bomb, the Monte Carlo became essential. For there was no analog of Fermi’s Metallurgical Laboratory nuclear reactor—no controlled instance of fusion that physicists could study in peacetime. Instead, Stanislaw Ulam, Enrico Fermi, John von Neumann, Nicholas Metropolis, and a host of their colleagues built an artificial world in which “experiments” (their term) could take place. This artificial reality existed not on the bench, but in the vacuum-tube computers—the JONIAC, the ENIAC, and, as Metropolis so aptly named it, the MANIAC.

This story proceeds on several intersecting planes. It is a tale in the history of epistemology: a new method for extracting information from physical measurements and equations. It is a narrative of the history of metaphysics: a scheme of representation that presupposed a nature composed of discrete entities interacting through irreducibly stochastic processes. It is a workplace history: traditional professional categories of experimenter and theorist challenged by an increasingly large and vocal cadre of electrical engi-

neers, and later by computer programmers. Overall, it is an account of fundamental physics inextricably tied to the development of the Superbomb, the weapon with no limit to its potential destructive power, and a description of the calculating machine from computer-as-tool to computer-as-nature.

Bombs and Models

The computer began as an instrument, a tool like other tools around the laboratory. Von Neumann could refer to it in precisely the way that he would discuss a battery or a neutron generator. To Julian Huxley in 1946, for example, von Neumann wrote: "We want to use this machine in the same sense . . . in which a cyclotron is used in a physics laboratory."¹ As has been rehearsed endlessly in the histories of the computer, there were two fairly distinct traditions of computer building. One sought to create machines that would function in an analog fashion, recreating physical relations in media or at scales different from their natural occurrence. Among these could be counted harmonic analyzers, gunnery computers, and network analyzers, as well as analog models including model ship basins or wind tunnels.² Such models have long histories; one thinks, for example, of nineteenth-century British electricians, as they put together pulleys, springs, and rotors to recreate the relations embodied in electromagnetism. But it is the second tradition, devoted to digital information processing, that concerns us here. Indeed, my concern is considerably narrower still: the attempt to use the emerging digital devices of the late war and postwar periods to simulate nature in its complexity.

With the Japanese surrender, the laboratory at Los Alamos began to disperse. But before they left, scientists convened to collect and record their knowledge, should it later prove necessary to return to a major effort in nuclear weapons. One such meeting was called for mid-April 1946, to discuss the still-hypothetical fusion weapon, a device that the staid, classified report regarded with some awe—even after the devastation of Hiroshima and Nagasaki: "Thermonuclear explosions can be foreseen which are not to be compared with the effects of the fission bomb, so much as to natural events like the eruption of Krakatoa. . . . Values like 10^{25} ergs for the San Francisco earthquake may be easily obtained."³ No longer

content to mimic Krakatoa *in vitro* (as had the Victorian natural philosophers discussed by Arnold Davidson in this volume), the physicists of Los Alamos now contemplated its imitation *in vivo*.

If imagining the destructive power of the H-bomb was a matter of no great difficulty, designing it was. For though the early bomb builders had thought that heavy hydrogen could be set into self-propagating fusion with relative ease, work during the war had indicated that the problem was much more complicated. Elsewhere, the Los Alamos physicists commented that⁴

the nuclear properties of the key materials are still fundamental in that the energy is supplied by nuclear reactions; but control and understanding of the phenomenon involves purely atomic consideration to a much greater extent than was the case in the fission bomb. What the reaction depends on is the complex behavior of matter at extremely high temperatures. For prediction, then, the primary requisite is a deep insight into the general properties of matter and radiation derived from the whole theoretical structure of modern physics.

When the authors said that they would need the “whole theoretical structure of modern physics,” they were not exaggerating. Not only were the nuclear physics of hydrides, the diffusion of hard and soft radiation, and the hydrodynamics of explosion difficult in themselves; they had to be analyzed simultaneously and in shock at temperatures approaching that of a stellar core. How was energy lost, what was the spatial distribution of temperature, how did deuterium-deuterium and deuterium-tritium reactions proceed? How did the resultant helium nuclei deposit their energy? These and other problems could not be solved by analytical means, nor did they lend themselves to “similarity” treatments by analog devices. Experiments appeared impossible—a hundred million degrees Kelvin put the laboratory out of the picture; there was no thermonuclear equivalent to Fermi’s reactor, no slow approach to criticality obtained by assembling bricks of active material. Where theory and experiment failed, some kind of numerical modeling was necessary, and here nothing could replace the prototype computer just coming into operation in late 1945: the ENIAC (Electronic Numerical Integrator And Calculator).⁵

Here is an example from von Neumann’s 1944 work on numerical methods, designed for the digital processing of hydrodynam-

cal shocks. In question is a compressible gas or liquid in which heat conduction and viscosity are negligible. In the Lagrangian form, the equations of motion are constructed by labeling the elementary volume of substance by a quantity a , which is located by its position x at the time t . We characterize the gas by its caloric equation of state, which gives its internal energy, $U = U(V, S)$, where U is a function of specific volume, $V = \partial x / \partial a$, and specific entropy, S . Normalized mass units made the density equal unity, so the amount of substance located between x and $x + dx$ is just da . It follows that the gas density is the inverse of its specific volume, and pressure, p , and temperature, T , are given by the usual thermodynamical equations,

$$p = -\frac{\partial U}{\partial V}, \quad T = \frac{\partial U}{\partial S}. \quad (1)$$

The equations of motion are designed to provide x as a function of the piece of substance labeled by a , and time, t . Conservation of momentum ($dp/dt = 0$) has two parts: a time derivative of $m \partial x / \partial t$ and a term corresponding to the change of pressure as one moves along in space:

$$\frac{\partial^2 x}{\partial t^2} = -V \cdot \left(\frac{\partial p}{\partial x} \right)_{t = \text{constant}} = 0. \quad (2)$$

Substituting $V = \partial x / \partial a$ leads to

$$\frac{\partial^2 x}{\partial t^2} = -\frac{\partial}{\partial a} p \left(\frac{\partial x}{\partial a}, S \right). \quad (3)$$

The conservation of total energy (internal plus external) is just

$$dE = \frac{\partial U}{\partial V} dV + \frac{\partial U}{\partial S} dS + \left(\frac{\partial^2 x}{\partial t^2} \right) dx = 0, \quad (4)$$

recalling that mass units are normalized to unity. When combined with the thermodynamic relations of Equation 1, Equation 4 yields

$$dE = -pdV + TdS + \left(\frac{\partial^2 x}{\partial t^2} \right) dx = 0. \quad (5)$$

From Equation 3, we can substitute $-\partial p / \partial a$ for $\partial^2 x / \partial t^2$. With the help of an integration by parts it follows that the first and third terms on the right-hand side of Equation 5 cancel, yielding $TdS = 0$, or

$$\frac{\partial S}{\partial t} = 0. \quad (6)$$

This is a crucial part of von Neumann's argument: because of the result that entropy is conserved not as a condition but as a consequence of energy conservation, the solution to Equation 3 can be made assuming p is a function of V alone. Analytic and numerical techniques work on this well-understood hyperbolic differential equation perfectly well. If, however, a shock is introduced to the system, the entropy changes as our bit of matter crosses the shock. Since changes of entropy (and therefore the coefficients in the equation of motion [3]) depend on the trajectory of the shock, the problem is rendered vastly more complicated. Instead of a hyperbolic differential equation, one has a hyperbolic differential equation with variable coefficients, which in general cannot be solved either analytically or numerically.

Von Neumann's central idea was to recapture the simplicity of Equation 3, even for the shock case (where S is not constant), by assuming that the internal energy could be divided into two noninteracting parts: $U(V,S) = U^*(V) + U^{**}(S)$. Then only $U^*(V)$ contributes to p in the thermodynamical equation (1), and this quantity, which we can label p_0 , is as free of entropy dependence as it was in the nonshock case. In plain English, von Neumann's assumption that the energy $U(V,S)$ divides up this way amounts to the postulation of an "interactionless" substance that can be precisely modeled by beads of definite mass on a chain of springs. Shocks can still propagate, but entropy does not contribute to the pressure.

Letting a run over integers from negative to positive infinity, and

$$x = x(a,t) \equiv x_a(t), \quad (7)$$

can reduce the differential equation (3) to an approximate system of difference equations, well suited for machine coding:⁶

$$\frac{d^2 x_a}{dt^2} = p_0(x_a - x_{a-1}) - p_0(x_{a+1} - x_a). \quad (8)$$

Once on the machine, the simulation could produce a graphic representation of the shock wave as it propagated.

Here is what has happened. For purely computational reasons (that is, in order to process the shock problem on an electro-mechanical calculator) von Neumann redefined the goals of his work to include the elimination of entropy dependence. Until then, the only way to do this was to cancel the shocks; his new approach was to alter the model, introducing the beads-and-springs. Thus the beads-and-springs represent a model of the real gas that has been constructed largely to conform to the new calculational apparatus. It was this model, calculable only by machine, that would in effect simulate a physical system even when analytic techniques led nowhere.

Now von Neumann had to defend the validity of this form of machine representation, and he did so with a long string of plausibility claims. First, he turned the tables on the visual "representational" argument for differential equations. In particular, he pointed out that it is the hydrodynamical equation (3) that distorts the world by using a continuum to describe what in "reality" (von Neumann's term) is a discrete set of molecules. By contrast the "beads-and-springs" "corresponds" to the "quasi-molecular description of this substance." Of course, as von Neumann acknowledged, no device is about to process 6×10^{23} of these beads, but then again even the hydrodynamical equation never explicitly demands such vast numbers.

Somewhat wistfully, von Neumann concluded: "The actual $N \sim 6 \cdot 10^{23}$ is certainly great, but much smaller numbers N may already be sufficiently great. Thus there is a chance that $N \sim 10^2$ will suffice." Other arguments for why this representation could stand in for the "true" state of affairs were more involved, and contained a defense of the simplified intermolecular forces, the process of energy degradation between forms of energy, and so on.

On these grounds, von Neumann prophesied success for more complicated equations of state and more spatial dimensions—especially for the case of spherical symmetry. Not surprisingly, inward-bound shock waves in spherical space were precisely what one would begin with in studying the blasting of plutonium toward its supercritical assembly at the heart of an atomic bomb. Metropolis and Stan Frankel presented their results to the April 1946 Superbomb meeting. Present were all the members of Teller's wartime group, along with many others, including the theoretic-

cal physicist Robert Serber, who had greatly contributed to the A-bomb; the wartime head of the Los Alamos theory group, Hans Bethe; and, to the Americans' later chagrin, the Soviet spy Klaus Fuchs. Ulam was at this meeting and, five days before, had completed a speculative Los Alamos paper with J. Tuck on how thermonuclear reactions might be initiated in deuterium by the intrusion of jets from a fission device. Now, at the meeting itself, Ulam heard Frankel and Metropolis's results, fresh from the ENIAC. Having processed over a million IBM punchcards, they reported that the program had run successfully, leaving them provisionally optimistic (if one should use that term) that the Super would detonate as designed.⁷ But even with this massive cybernetic assistance, it was apparent that the Super problem far exceeded the ENIAC's capacity to model, and Ulam began to search for a more efficient way to exploit the capabilities of the new machine.

The name "Monte Carlo" was coined by Metropolis and was first printed in the formal contribution by Metropolis and Ulam in the September 1949 volume of the *American Statistical Association Journal*. Clearly following von Neumann's lead in his hydrodynamic computations, Ulam and Metropolis began by pointing to the uncharted region of mechanics between the *terra firma* of the classical mechanician, in which only very few bodies could be treated, and the newer world of the statistical mechanician, in which N was 10^{23} . Something of this mesoscopic *terra incognita* existed as well in combinatoric analysis, where the numbers were too large to calculate and not yet large enough to call in the law of large numbers. Ulam and Metropolis staked their claim on the new turf, announcing the key to their method:⁸

To calculate the probability of a successful outcome of a game of solitaire (we understand here only such games where skill plays no role) is a completely intractable task. On the other hand, the laws of large numbers and the asymptotic theorems of the theory of probabilities will not throw much light even on qualitative questions concerning such probabilities. Obviously the practical procedure is to produce a large number of examples of any given game and then to examine the relative proportion of successes.

Sampling could apply even when no stochastic element was present, as in the evaluation of a finite volume within a many- (say

twenty-)dimensional space. The volume could be defined, for example, by specifying a series of inequalities along each of the axes. Dividing the unit cube into ten parts along each of the twenty axes would produce 10^{20} cubes. Systematically checking each box to see if there was a point in it would be, to put it mildly, impractical. Instead, the authors suggest, one could simply sample the cubes, say 10^4 times, and find the ratio of points falling inside the required volume to the total number of points (10^4). More physically, the same sort of difficulty arises in understanding cosmic-ray showers, in which a high-energy proton collides with a nucleus and produces a shower of other particles; these in turn give rise to more, and the chain continues in an extraordinarily complex fashion. Although each step of the process is played out according to previously given probabilities, and therefore is calculable through a specifiable algorithm, obtaining the full solution is out of reach of analytic methods.

The key process, however, as one might suspect from Metropolis and Ulam's return address, was nuclear fission. There the problem was to calculate the diffusion of neutrons through an active substance such as plutonium. In such cases the neutron can scatter from the nucleus (inelastically or elastically), be absorbed by the nucleus, or cause the nucleus to fission (with the emission of n neutrons). Each process has predetermined probabilities, and a sampling of some finite number of neutrons at given energies. Unmentioned, but undoubtedly coequal with the fission, was fusion—on which Ulam was then hard at work.

For the Monte Carlo method to work, Ulam and von Neumann would need a large collection of random numbers, preferably generated by the computer itself, to pick samples of ("perform experiments on") the infinite world of scatterings, fissions, and fusions. Borrowing thousands of "true" random digits (random numbers generated from "truly random" physical processes such as radioactive decay) from books was thoroughly impractical, so the two physicists set about fabricating "pseudorandom" numbers. In one of their earliest postwar formulations, von Neumann's scheme was to take some eight- or ten-digit number to start the process, and a sequence of further numbers would emerge by the iterative application of a function (typically a polynomial) $f(x)$. Not surprisingly, this procedure left people rather puzzled at how the

Monte Carlo players could expect a definite function, applied iteratively, to produce a truly random number. The short answer is that he and Ulam didn't expect $f(x)$ to do anything of the kind; unable to generate random numbers in the classical sense, they expanded the meaning of the term.

Von Neumann's attitude at the time emerges strikingly from a January 1948 exchange with Alston Householder, a physicist at the Oak Ridge Laboratory, who in puzzlement asked, "Is it the case that one starts with a single number, randomly selected, and generates therefrom an infinite sequence of random numbers?"⁹ Work at Oak Ridge on the Monte Carlo would stop, Householder added, pending the master's reply. Far from being infinite random generators, von Neumann responded, a finite machine sequentially applying $f(x)$ would, in the fullness of time, repeat a finite cycle of numbers over and over again *ad infinitum*. Suppose, for example, that the machine had a ten-digit storage capacity. Then, even if every ten-digit number were produced before repeating, after 10^{10} numbers $f(x)$ would necessarily produce a number already generated, causing the entire cycle to repeat precisely. "Consequently," von Neumann acknowledged, "no function $f(x)$ can under these conditions produce a sequence which has the essential attributes of a random sequence, if it is continued long enough." His and Ulam's hope was more modest. They wanted their computer to fabricate somewhere between a thousand and ten thousand numbers that "up to this point look reasonably 'random.'" Such sequences need be only "random for practical purposes."¹⁰

By "random for practical purposes," von Neumann meant that the numbers would satisfy two conditions. First, the individual digits would be as equidistributed as one would expect for a random sample of the same size. Second, the correlation of the k th neighbors would be independent for k between 1 and 8. Procedurally, the generative process worked this way: the first x would be a randomly chosen eight-digit number. This would then be inserted into, say $f(x) = x^2$, and the middle eight digits extracted for reinsertion into $f(x)$.¹¹ Perhaps acknowledging the legacy of his training in "pure" mathematics, von Neumann confessed: "Anyone who considers arithmetical methods of producing random digits is, of course, in a state of sin. For, as has been pointed out several times, there is no such thing as a random number—there

are only methods to produce random numbers, and a strict arithmetic procedure of course is not such a method."¹²

Von Neumann went on to specify the way the simulation would run. First, a hundred neutrons would proceed through a short time interval, and the energy and momentum they transferred to ambient matter would be calculated. With this "kick" from the neutrons, the matter would be displaced. Assuming that the matter was in the middle position between the displaced position and the original position, one would then recalculate the history of the hundred original neutrons. This iteration would then repeat until a "self-consistent system" of neutron histories and matter displacement was obtained. The computer would then use this endstate as the basis for the next interval of time, Δt . Photons could be treated in the same way, or if the simplification were not plausible because of photon-matter interactions, light could be handled through standard diffusion methods designed for isotropic, black-body radiation.¹³

Specifically, von Neumann introduced a series of velocity-dependent functions that gave the probability of neutron absorption, neutron scattering, or neutron-induced fission in each of the three materials:

Absorption in A , T , S :

$$\sum_{aA} (\nu), \sum_{aT} (\nu), \sum_{aS} (\nu); \quad (9)$$

Scattering in A , T , S :

$$\sum_{sA} (\nu), \sum_{sT} (\nu), \sum_{sS} (\nu); \quad (10)$$

Fission, which only occurs in the active material A , producing 2, 3, or 4 neutrons:

$$\sum_{fA}^{(2)} (\nu), \sum_{fA}^{(3)} (\nu), \sum_{fA}^{(4)} (\nu). \quad (11)$$

With these assumptions, one can specify the precise location of the stochastic element in these calculations. Define $\lambda = 10^{-fd'}$ as the probability that a neutron will travel a distance d' through zone i (in which a fraction x_i is active, y_i is tamper, and z_i is slower-down material) without colliding, and where f is given by

$$\begin{aligned}
 f = & \left[\sum_{aA} (\nu) + \sum_{sA} (\nu) + \sum_{fA}^{(2)} (\nu) + \sum_{fA}^{(3)} (\nu) + \sum_{fA}^{(4)} (\nu) \right] x_i \\
 & + \left[\sum_{aT} (\nu) + \sum_{sT} (\nu) \right] y_i + \left[\sum_{aS} (\nu) + \sum_{sS} (\nu) \right] z_i.
 \end{aligned} \tag{12}$$

Somehow, by throwing dice, by consulting a table, or by creating a computer-generated algorithm, λ must be chosen from an equi-distributed set of points on the unit interval. Each choice specifies d' for one neutron making one excursion through the reactor.

Robert D. Richtmyer (who succeeded Hans Bethe as head of the theory division at Los Alamos) responded in ways that might be expected from his position at the weapon laboratory. For nuclear weapons, there was no need for slower-down material of a sort characteristically employed in a reactor. So it is no surprise to find him saying, "Material S could, of course, be omitted for systems [of interest to us]." Similarly, in the bomb, the tamper itself typically would contain fission, especially in the new, enhanced weapons then under consideration. None of this is made explicit, but would have been perfectly clear to cleared readers at the time. The simple formal substitutions $\sum_{fT} (\nu) \neq 0$ and $\sum_{a,sS} (\nu) = 0$ set a reactor

problem into a bomb problem; the basic Monte Carlo strategy remained the same.¹⁴

Ulam too began to use the method, though his efforts were even more consistently directed toward the Super than von Neumann's. In response to a lost letter from Ulam that probably indicated his tentative efforts to execute a Monte Carlo by hand for the Super (on other grounds it is clear this is what he was doing), von Neumann sent his enthusiastic endorsement in December 1947: "I am waiting with great expectations to see the details of the manual Monte-Carlo procedure. What you tell me about it is intriguing, and in any case, this is the first large scale application of the statistical method to a deep-lying problem of continuum-physics, so it must be very instructive."¹⁵

As Los Alamos geared up for a major new simulation of the Super, this time with the Monte Carlo, von Neumann wrote Ulam in March 1949: "I did, however, work on the 'S[uper],' and about a week ago I finished the discussion of the non-M[onte] C[arlo]

steps that are involved.”¹⁶ This “discussion” meant setting out the logical structure of the calculation: the equations, storage requirements, and logical steps that would be needed. At the end of the day, he wanted to be able to calculate what he called the “time economy” of the calculation—how it would run on different machines, what ought to be calculated precisely, and what through Monte Carlo. These decisions were not given unambiguously, and he reported a certain amount of “demoralization” because there remained “several, mutually interdependent and yet not mutually determining, choices as to procedure [that were] both possible and important.” Hardest of all would be the treatment of photons, because they could traverse the greatest number of zones in an interval Δt . Neutral massive particles (neutrons) and charged particles cut across fewer (usually only one or two) zones per Δt , and so demanded less computer time.¹⁷

To make the calculation, von Neumann set up a space-time division in which Δt is 1/10 sh (shakes are 10^{-8} seconds in bomb builder jargon), and there were 100 such intervals. Space (in radius r) was to be divided according to

$$0 = r_0 < r_1 < \dots < r_{19} < r_{20} \sim 100; r_{i+1} - r_i = \alpha q^i \quad (i = 1, \dots, 19), \quad (13)$$

where the simulated Super was described as a series of these 20 concentric zones. If one assumes that the zones grow geometrically in radius, then q must be 1.1, because $r_{20} = 100$, and the radius of the initial zone is given by $\alpha = 1.75$.

Many of the simulation steps did not involve Monte Carlo methods; they built on just the sorts of hydrodynamic calculations in which von Neumann had become expert. These deterministic operations used less than 100 multiplying steps [designated 100 (M), where (M) is a multiplying step with all accompanying operations, and M indicates a multiplying step without this complement of operations] per unit time Δt and per zone Δr . There are 20 zones and 100 time intervals. Estimating a need for about 4 iterations per step, this left $20 \times 100 \times 4 = 8,000$ repetitions. Multiplying the number of steps by the multiplications per step gives $8,000 \times 100$ (M); in other words, the non-Monte Carlo part of a Super calculation would take 0.8 mega(M). Additional multiplications are needed to check and run the Monte Carlo, but these 0.8 mega(M) give a rough guide to a comparison of “hand” computing with various computers.

If the time for a hand multiplication M is 10 seconds, then a hand $(M) = 8M = 80$ seconds; $\text{mega}(M) = 8 \times 10^7$ "man" seconds $= 2.2 \times 10^4$ "man" hours. I should pause here to point out that von Neumann put "man" into scare quotes, presumably because the people doing these millions of calculations were all women. So von Neumann's "man" = woman. Moreover, von Neumann's wife, Klara von Neumann, Herman Goldstine's wife, Adele Goldstine, Foster Evans's wife, Cedra Evans, Teller's wife, Augusta Teller, and John William Mauchly's wife, Kathleen McNulty Mauchly, along with many others, were all programming these early computers. (In fact, the word "computer" itself designated the usually female calculators, and only later did the term shift from the woman to the device.) All this is background to von Neumann's peculiar quotational terminology in which a "man" year $= 50$ "man" weeks $= 50 \times 40$ "man" hours $= 2 \times 10^3$ "man" hours. A single $\text{mega}(M)$, therefore, would take 11 "man" years to do by hand, and so the deterministic part of the Super simulation would demand 8.8 years for a single computing person to reckon.

The estimate of 0.8 $\text{mega}(M)$ enabled von Neumann to contrast the way a Super simulation would be calculated by the ENIAC, by the SSEC (Selective Sequence Electronic Computer) IBM computer in New York, and by "our future machine" to be built at the Princeton Institute for Advanced Study.

For the ENIAC, the time of multiplication M was a mere 5 msec, so $(M) = 40$ msec; $\text{mega}(M) = 4 \times 10^4$ sec $= 11$ hours. Therefore 0.8 $\text{mega}(M) = 8.8$ hours, which, when compounded with typical running efficiency, could be accomplished in one 16-hour day. However, this estimate was academic, as von Neumann knew. Because the ENIAC lacked sufficient memory (a mere 20 words), the problem simply could not be executed. For the SSEC IBM machine in New York, however, there was enough memory, and, to boot, it was faster: multiplication ran about 25 percent more slowly than the ENIAC, but the speed was made up in other ways. And the future machine could run the same operation fast enough to simulate the deterministic part of an identical exploding Super in about 15 minutes.

Von Neumann next described the effect of adding in the Monte Carlo steps, which have so far been ignored. If one defines a as the ratio

$$a = (\text{time for MC steps})/(\text{time for non-MC steps}), \quad (14)$$

then the total time would be the non-MC time multiplied by $(1 + a)$. The Monte Carlo steps are such that a single photon would take less than 60 (M), while any other particle needs less than 40 (M). There are 300 photons and 700 particles of other species. This yields $(300 \times 60) + (700 \times 40)$ (M) = 46,000 (M). While this appears to be much greater than the 100 (M) required for the non-Monte Carlo step, there are 20 zones, each of which must be examined each time for the non-Monte Carlo steps (the hydrodynamics), whereas the Monte Carlo step (a photon interaction, for example) would take place in only one zone. So $a = 23$, and the total amount of time needed for the Super would be 24 times the non-Monte Carlo totals. This meant that were the calculation assigned to a single computer (woman), a given thermonuclear-bomb simulation would take 211.2 years. In a sense this number is a crucial factor (among others, to be sure) in the decision to devote vast resources to replace the human computer with an automatic one.

Working with these highly schematized Monte Carlo simulations, over the course of 1949 Ulam and C. J. Everett (Ulam's collaborator) did manage to put a crude version of the Super problem on the computer, but they had to invoke assumptions and simplifications at every stage to accommodate the limited capacity of the ENIAC. With the program finally installed, Ulam wrote von Neumann in March 1950: "Everett managed to formalize everything so completely that it can be worked on by a computer. . . . It still has to be based on guesses and I begin to feel like the man I know in Poland who posed as a chess champion to earn money—gave nine 'simultaneous' exhibitions in a small town playing 20 opponents—was losing all 20 games and had to escape through the window!"¹⁸

In the midst of one such run, Ulam penned a message to von Neumann in late January 1950. Initial results apparently were not promising for detonation, and Ulam judged that earlier fears that hydrodynamic instabilities would quench the reaction were misplaced: "Hydrodynamics, so far at least, far from being a danger, is the only hope that the thing will go!" Interwoven with his cautious technical optimism came a similar political forecast: "I think that in

matters of 'politics' a victory of unbelievable proportions is preparing." He was right: within four days, President Truman delivered his decision to proceed with an intensified effort on the H-bomb. Whether Ulam had inside information to this effect, I do not know. It certainly seems so.¹⁹ In any case, von Neumann was delighted with the outcome: "I need not tell you how I feel about the 'victory.' There are, however, plenty of problems left, and not trivial ones either."²⁰ Mixing the political and technical inextricably, "hope," "victory," and a "go" all stood on the side of a simulated detonation; "danger" and "problems" lurked in a computational dud.

In public, both Ulam and von Neumann kept low profiles in what had become a raging debate over whether or not the United States should build the hydrogen bomb. Albert Einstein appeared on television to warn that the weapon was bringing the world inexorably closer to total annihilation. Hans Bethe and Victor Weisskopf made public appearances arguing that the level of destruction contemplated with thermonuclear weapons was practically genocidal; in addition, Bethe made no secret of his hope that the bomb would prove scientifically impossible to build.

Ulam dutifully read these moral-political tracts, but from the midst of the immensely complex calculations that had been proceeding more or less unaffected by the outside world, this contretemps appeared laughably ill informed, even irrelevant. To von Neumann, he joked:²¹

Read with constant amusement a whole series of new articles in the press about hydrogen bomb—the statements by Zacharias, Millikan, Urey, Einstein and Edward in the Bulletin of A[tomic] S[cientists] in Chicago each contribute a share of merriment.

I propose to you hereby to write jointly a "definitive" article on this subject. It will be signed by fictitious names of two foreign born scientists, "key men" in various projects, *not* atomic scientists *but* experts on the hydrogen bomb, former scientists on radar and submarine detection work etc. The first paragraph will say how secrets and lack of free exchange thwarts progress in basic (science) and prevents development of new ideas. The next [paragraph] will document it by pointing out how there are surely no secrets in nature & how *any* scientist can figure out these secrets by himself in 5 minutes.

The next [paragraph] how hydrogen bomb is too big & very immoral,

the next one that it is too big to be useful, but not really big enough to be decisive.

The next that it is not clear to anyone whether it can be built at all; after that Russia probably has it already but let us all pray that it is mathematically impossible.

After that, like Edward's, a pitiful plea for all scien[tists] to work on [it], spending 7/8 of the time on the projects and 3/4 on [other topics].

It was not that Ulam thought the bomb was necessarily even possible. In one study during 1950, Ulam and Everett showed that the initiation of fusion using deuterium and tritium would demand prohibitive quantities of the fantastically expensive hydrogen isotope tritium. Then, during the summer of 1950, the two authors used the Monte Carlo method to study the behavior of thermonuclear reactions in the mass of deuterium. In the still-classified 1950 report LA-1158 ("Considerations of the Thermonuclear Reactions"), Ulam and Fermi invoked the hydrodynamics of the motion of the material, the interaction of radiation energy with matter, and various reactions between nuclei that were dependent on temperature, density, and geometry. Though the size of the calculations was small (they were still able to use desk computers and slide rules), their conclusion was that the reaction would not propagate in a volume of deuterium. Together, the two pieces of work appeared to sound the death knell for the Super: it would neither light nor burn. Later, massive simulations on electronic computers by von Neumann and by Foster Evans and Cedra Evans confirmed their assessment.²²

By all accounts, Teller took Ulam's fizzling-bomb news hard. Ulam's pocket diary for 10 May 1950 reads, "Fights with Edward"; by 13 June 1950, with more data in hand Ulam relished his vanquishing of Teller's greatest project: "Victorious end of fights with Edward."²³

Here the dense, terse entries carry multiple meanings. At the larger scale, Ulam was preoccupied with "victory," where the victory was one of national politics (Truman's endorsement of the H-bomb); in Ulam's diary "victory" designates triumph in Ulam's personal struggle with Teller (Ulam showed that Teller's classical Super would not explode). Given the extraordinary publicity and resources already devoted to the project, it is not too surprising that Ulam's dim reports ended neither the technical nor the moral

battles, which continued throughout the latter part of 1950. By January 1951, almost exactly a year after the presidential order, there was little prospect of technical success with the weapon, and tempers were wearing thin. On Thursday, 18 January 1951, Ulam recorded one such encounter between Bethe (who hoped and argued that the bomb was impossible) and Teller, "Amusing fights: Hans-Edward"; or a few days later, "Big fight—fairly amusing." Then, sometime between 18 and 25 January 1951, Ulam realized that a radically different configuration of the hydrogen bomb might be possible. Instead of trying to create enormous temperatures with a fission bomb, the A-bomb could be used to compress the fusible material; high pressure would enable the reaction to proceed with much less heat.²⁴

On 25 January 1951, the tone of his entries abruptly changed: "Discussion with Edward on '2 bombs.'" Apparently, Ulam had brought the idea of shock compression to Teller, and Teller had added his own idea—that the same effect could be obtained more easily by "radiation implosion," the compression of the fusion fuel by an expanding plasma created by X rays from the fission bomb. (There were therefore two bombs in question, both involving compression.)

The fights ended. Ulam recorded on 26 January, "Discussion with Bethe[,] Evanses[,] Carson [Mark] on the set-up for the cylinder propagation. Write up dis[cussion] with Johnny [von Neumann] & write to Hans [Bethe]." Over the next few weeks Ulam finished his part of the paper and sketched the introduction. Its somewhat unwieldy title appeared the next month on 15 February—"Wrote Lenses. (jointly with Teller) 'Heterocatalytic deton[ation]' Radiation lenses and hydro[genous] lenses"—and was formally issued on 9 March 1951.²⁵

With this classified paper, debate over the hydrogen bomb "inside the fence" virtually ended among physicists privy to the classified breakthrough. In June 1951, faced with new simulations done on the SEAC (Standards Eastern Automatic Computer) and elsewhere, the Atomic Energy Commission's General Advisory Committee, whose startling earlier moral position against the weapon had stunned the secret community, retracted their opposition and endorsed a full-tilt effort to test the bomb. Even Bethe, who had long been the strongest public scientific voice against it,

ceased to object. On 30 October 1952, the United States detonated a "Teller-Ulam" bomb on the South Pacific island of Eniwetok. The force of the 10.2 megaton explosion wiped the island from the face of the earth.

Thermonuclear and enhanced-fission weaponry had crystallized a new mode of doing scientific work. During 1948 and 1949 statisticians, mathematicians, electrical engineers, and a restricted few others were called in to join with the physicists in the creation of this new trading zone in which talk of games, pseudorandom numbers, difference equations, and logical coding were common parlance. Now the style of work grew outward, and through a series of conferences passed beyond the domain of the secret and into the wider community of researchers. As it crossed that fence, debate grew about who might count as an experimenter or as a theoretician, and what would count as an experiment or theory.

The *Tertium Quid*

"Computational physics," the physicist Keith V. Roberts wrote, "combines some of the features of both theory and experiment. Like theoretical physics it is *position-free* and *scale-free*, and it can survey phenomena in phase-space just as easily as real space. It is symbolic in the sense that a program, like an algebraic formula, can handle any number of actual calculations, but each individual calculation is more nearly analogous to a single experiment or observation and provides only numerical or graphical results."²⁶ But if the frankly symbolic character of labor resembled techniques of the blackboard, it was still true that the techniques of error analysis and problem shooting held more in common with techniques of the bench. Roberts continued:²⁷

Diagnostic measurements are relatively easy compared to their counterparts in experiments. This enables one to obtain many-particle correlations, for example, which can be checked against theory. On the other hand, there must be a constant search for "computational errors" introduced by finite mesh sizes, finite time steps, etc., and it is preferable to think of a large scale calculation as a numerical experiment, with the program as the apparatus, and to employ all the methodology which has previously been established for real experiments (notebooks, control experiments, error estimates and so on).

In short, the daily practice of error tracking bound the Monte Carlo practitioner to the experimenter.

Other activities joined the simulator with the experimenter as well. In experimental practice it is routine to use the stability of an experimental result as a sign of its robustness: Does it vary as one repeats it, or shift around parameters that ought (on prior grounds) to be irrelevant to the outcome? From the inception of the Monte Carlo, its practitioners were equally aware that their nightmare would be the production of results without constancy. In Ulam's 1949 paper with Metropolis, the authors made this clear: a "procedure is repeated as many times as required for the duration of the real process or else, in problems where we believe a stationary distribution exists, until our 'experimental' distributions do not show significant changes from one step to the next."²⁸ Problems of locality, replicability, stability, and error tracking are thus some of the reasons (I will come back to other, deeper ones) that led the simulators to identify their work as experimental.

At the same time, however, other practice clusters tied the simulator to the mathematician or theoretical physicist. J. M. Hammersley and D. C. Hanscomb took this link of mathematics to the computer to demand a fundamentally new classification of mathematics, one that cut across the old typology of "pure" and "applied" mathematics.²⁹

A relatively recent dichotomy contrasts the theoretical mathematician with the experimental mathematician. These designations are like those commonly used for theoretical and experimental physicists, say; they are independent of whether the objectives are pure or applied, and they do not presuppose that the theoretician sits in a bare room before a blank sheet of paper while the experimentalist fiddles with expensive apparatus in a laboratory. Although certain complicated mathematical experiments demand electronic computers, others call for no more than paper and pencil. The essential difference is that theoreticians deduce conclusions from postulates, whereas experimentalists infer conclusions from observations. It is the difference between deduction and induction.

While this remark may be naive in its dichotomy of induction and deduction, it is important as an indicator of the powerful identification of the simulator with the experimenter. This language of "theoretical experiments" or "mathematical experiments" saturates the literature. In the pages of *Physical Review* from the early to

mid-1950's one finds tens of such examples; of the albedo of 1-MeV photons, two authors write: "It occurred to us that a more reliable estimate of this quantity, on which there really existed no information, could be obtained by a 'theoretical experiment' using the Monte Carlo technique."³⁰

Caught between a machine life and a symbol life, the computer programmer in physics became both a pariah and an irreplaceable intermediary, establishing a precarious transactional function known to border peoples on every continent. Over the course of the 1960's, the computer utterly transformed particle physics; among the changes it brought was the creation of a category of action (data analysis) that was, in its own way, as all-embracing a career as accelerator building or field theory. The novelty of this situation was not lost on the physics community, as the French nuclear physicist Lew Kowarski made abundantly clear in the summer of 1971: "As scientists get used not only to writing their own programs, but also to sitting on-line to an operating computer, as the new kind of nuclear scientist develops—neither a theoretician, nor a data-taker, but a data-processor specialized in using computers—they become too impatient to sit and wait while their job is being attended to by computer managers and operators."³¹ Quoting approvingly from a colleague at New York University, Kowarski presented mathematics as the analog of mining diamonds—finding extraordinary theorems among the dross of uninteresting observations. Computers, on the other hand, sought truth the way one mines coal; with the massive, everyday labor that methodically moves earth from pits to furnaces. "This analogy," Kowarski observed, "illustrates the difference between the spirit of mathematics and that of computer science and helps us to realize that being a computational physicist, or a computational nuclear chemist, or what not, is not at all the same thing as being a mathematical physicist and so on, so that, in fact, a new way of life in nuclear science has been opened."³²

For Kowarski, the *Lebensform* of the computer console was one destined to be more akin to that of coal miners, oceanographers, selenologists, and archaeologists than it was to physics as it was previously understood: "There will be a lot of attempts to judge such new situations by old value criteria. What is a physicist? What is an experimenter? Is simulation an experiment? Is the man who

accumulates print-outs of solved equations a mathematical physicist? And the ultimate worry: are we not going to use computers as a substitute for thinking?"³³ This anxiety over the identity of physics and the physicist was both social and cognitive. The fear that "thinking" might be destroyed must be glossed as a fear that the particular pleasure (and status) of controlling a wide range of cognitive activities previously associated with experimenting would be reduced. Underlying this worry was a very real change of structure in the physics workplace and, more generally, an altered concept of demonstration.

Take replicability. For theoretical work, the recreation of argumentation is generally considered to be unproblematic. Derivations are relatively easy to repeat, if not to believe. Experimental efforts, by contrast, present notorious difficulties as many historians and sociologists of science have so effectively illustrated: air pumps, prisms, lasers all turn out to be more profoundly local in both structure and function.³⁴ From the start, simulations presented a hybrid problem. On one side, the work was unattached to physical objects and so appeared to be as transportable as Einstein's derivation of the *A* and *B* coefficients for quantum emission and absorption. But in practice, this was hardly the case. Roberts, among many others, bemoaned this fact, and considerable effort began to delocalize simulations and computer-analysis programs more generally. Opening the "bottleneck" (as it was commonly called) required three concurrent efforts. First, programs had to be openly published.³⁵ While desirable, this proved in the 1950's (and ever since, I should add) a rather wistful desire, since most interesting programs were far too large to allow distribution on the printed page. Second, Roberts pressed for what he called "portability": the use of universal languages and the physical distribution of data tapes. While the former encountered difficulties associated with variations between machine types and local programming customs, the latter ran into a host of property-rights difficulties. For example, in the 1980's questions arose as to whether the distribution of data tapes could be considered analogous to the distribution of cell samples in biology. Finally, Roberts insisted that "modularity" ought to be considered a goal for programmers, analogous to the routine elements of practice in theoretical physics such as Laplace's equation, group theory, vector algebra, or the tensor calculus.³⁶

Each of these programmatic responses—advocacy of publication, portability, and modularity—was partial; none could truly universalize a set of practices that bore the deep stamp of its localized creation. Lamenting this state of affairs Roberts commented: “There are many good programs that can only be used in one or two major laboratories (notably the Los Alamos hydrodynamics codes), and others which have gone out of use because their originators moved on to other work.”³⁷ In more recent times this phenomenon has become known as “program rot”—people and machines move on, leaving older programs dysfunctional, often irretrievably so.

At the same time that physicists struggled to uproot the world of simulated realities from a particular place, others applauded the deracination already achieved. Kowarski, for example, began to speak about a simultaneous “liberation in space” and a “liberation in time” afforded by the new modality of research: “Perhaps, when links as comprehensive as those used in television become available at long distance, there will be even less reason for the user to spend a lot of his time on the site where his physical events are being produced. This may even abolish the kind of snobbery which decrees today that only those may be considered as physicists who are bodily present at the kill, that is at the place and time when the particle is actually coming out of the accelerator and hitting the detector.”³⁸ Kowarski had in mind events originally encoded from particle collisions at a centralized laboratory. But just as events produced at Brookhaven or Berkeley could be issued as a stack of cards or spool of tape, so too could events created by simulations. In both cases, analysis, still the central stage of an experiment, was removed from any single place.

Liberation in time similarly mixed experiment and the life of the experimenter. Kowarski extolled the fact that the computer expanded the timescale of events—often 10–31 seconds—to the scale of minutes, hours, and days in which we live. Second, the time became repeatable as physicists reprocessed the same set of events in ever-different ways to reveal different patterns of order. Finally, the computer allowed the physicists to break the ties between the accelerator “beamtime” and their own time, time to live the lives of university-based scholars with teaching and departmental and familial duties. It therefore links, in yet one more way, the different worlds of beamtimes and lifetimes (to borrow the evocative title of

Sharon Traweek's anthropological study of the Stanford Linear Accelerator Center).³⁹

Artificial Reality

As Monte Carlo simulations developed, it became clear that their practitioners shared a great deal with experimenters—I gave the examples of a shared concern with error tracking, locality, replicability, and stability. But the self-representation of Monte Carlo users as experimenters is so pervasive that I now want to zero in on this notion in two different ways in an effort to uncover the practices underlying this talk of “experiment” done on keyboards rather than lab benches.

The first point about the relation of the Monte Carlo to experimentation is that the simulator spends time processing as well as generating “data”—the scare quotes denoting that the term has now been expanded to include those generated from pseudorandom numbers in Monte Carlo simulations. These practices, as the Monte Carlo folks immediately recognized, held more in common with experimental than with theoretical activity. As much as anyone, Herman Kahn of the Rand Corporation (later famous for his *On Thermonuclear War*) continuously emphasized that the simulator had no business simply reporting a probability, say the probability that a neutron will penetrate the concrete shielding wall of a nuclear reactor. Instead, the only meaningful statement would be a probability p along with a certainty m : “This situation is clearly not unknown to the experimental physicist, as the results of measurement are in this form.” If the problem is surprising, Kahn insisted, it is because the need to reduce variance is not a situation theorists typically encounter. Variance reduction arises in the context of the Monte Carlo because “one is not carrying out a mathematical computation in the usual (analytic) sense, but is carrying out a mathematical experiment with the aid of tables of random digits.”⁴⁰

With a limited sample of “particles” that the computer can track, it is frequently the case that the interesting phenomena occur so rarely that uncertainty runs riot. If, for example, a thousand particles are sent out from a simulated reactor and only ten penetrate the barrier, then the accuracy of statements about these ten particles will be extremely slight. Kahn and others (following on

early work by Ulam and von Neumann themselves) particularly pressed three strategies for reducing this uncertainty known as splitting, statistical estimation, and importance sampling. For example, the idea of the importance sample is to augment examination of a particular region of phenomena of interest, get a reduced variance, and then compensate for this bias in the final estimate of the probability. More realistically, neutrons might be given an extra large probability to scatter toward that part of the outside of a reactor shield where the people are in an effort to reduce the error associated with the estimate of neutrons penetrating the concrete barrier.

I conclude from these variance-reduction techniques, and from the earlier discussion of error tracking, that there are two sides of the simulators' concern with error. The first (error tracking) bears on the ability of the Monte Carlo to get the "correct" expectation values; it is experienced by the simulators as akin to the experimentalists' search for accuracy by quashing systematic errors in the apparatus itself. The second (variance-reduction techniques) is the direct analogue of the experimentalists' search to increase the precision of their examinations. Taken together, these day-to-day commonalities between the practices of the simulator and those of the bench experimentalist tended to press the two groups together in their self-identification. Some simulators went farther, arguing that, because they could control the precise conditions of their runs, they in fact had an edge on experimentalists; in other words, it was the simulator, not the experimentalist, who should be seen as the central figure in balancing theory. "The Monte Carlo methods," Kahn concluded, "are more useful . . . than experiments, since there exists the certainty that the comparison of Monte Carlo and analytic results are based on the same physical data and assumptions."⁴¹ Simulators, not bench workers, could make the green-eyed, six-toed, curly-haired pigs that science now demanded.

All the forms of assimilation of Monte Carlos to experimentation that I have presented so far (stability, error tracking, variance reduction, replicability, and so on) have been fundamentally epistemic. That is, they are all means and practices by which the researchers can argue toward the validity and robustness of their conclusions. Now I want to turn in a different direction, toward

what amounted to a *metaphysical case* for the validity of Monte Carlo as a form of natural philosophical inquiry. The argument, as it was presented by a variety of people (including occasionally Ulam himself), was based on a purportedly fundamental affinity between the Monte Carlo and the statistical underpinnings of the world itself. In other words, because both Monte Carlo and nature were stochastic, the method could offer a glimpse of reality previously hidden to the analytically minded mathematician. As Ulam himself once put it, his and von Neumann's hunt for the Monte Carlo had been a quest for a *homomorphic image* of a physical problem—where the particles would be represented by fictitious “particles” in computation.⁴²

Gilbert King, a chemist at Arthur D. Little who had been in operations analysis at the Office of Scientific Research and Development during the war, is a good spokesman for this *simulacrum* interpretation of the Monte Carlo. Already in late 1949 at the IBM Seminar on Computation, he insisted that “from the viewpoint of a physicist or a chemist, there really is no differential equation connected with the problem. That is just an abstraction.”⁴³ Two years later he amplified on these comments, arguing that the computer should “not be considered as a glorified slide rule” but as an “organism” that could treat a problem in an entirely new way. To King, it was clear that the directness of the Monte Carlo gave it a role vastly more important than just another approximation method.⁴⁴

Classical mathematics is only a tool for engineers and physicists and is not inherent in the realities with which they attempt to deal. It has been customary to idealize and simplify the mechanisms of the physical world in the form of differential and other types of equations of classical mathematics, because solutions or methods of attack have been discovered during the last few hundred years with means generally available—namely, pencil, paper, and logarithm tables.

Engineering was far too complex for such traditional paper-and-pencil solutions; engineers substituted difference equations for the differential equations, and sought approximate solutions by numerical methods. The classical mathematical physicist looked down on such “crude” methods as a “poor man's solution” adopted in the absence of an aesthetically satisfying abstract and analytic

one. King's worldview entirely inverted the mathematical physicists' epistemic hierarchy.

Refusing the mathematical physicists' invitation to the sacred realm of partial differential equations, King argued that such expressions refracted the world through a distorting prism, and insisted that the engineer's tools mapped directly onto something deeper.⁴⁵

There is no fundamental reason to pass through the abstraction of the differential equation. Any model of an engineering or physical process involves certain assumptions and idealizations which are more or less openly implied in setting up the mathematical equation. By making other simplifications, sometimes less stringent, the situation to be studied can be put directly to the computing machines, and a more realistic model is obtained than is permissible in the medium of differential or integral equations.

King's claim is radical. For, contrary to a long tradition supervaluating the differential and integral equations as reflecting a Platonic metaphysics hidden behind appearances, it is King's view that it is the engineer, not the mathematical physicist, who has something to say about reality. This "more realistic model" is so because nature is at root statistical, and the representative schemes such as integrodifferential equations that eschewed the statistical were bound to fail.

Consider the diffusion equation, the bread and butter of industrial chemists like King:

$$\frac{\partial \mu}{\partial t} = D \frac{\partial^2 \mu}{\partial x^2}. \quad (15)$$

For the specific, simple case of a capillary tube and dye released at its center point, the solution is known in closed form. According to the solution, the dye molecules will move a specifiable mean distance Δx in a time Δt . This can be simulated by a simple Monte Carlo conducted with a coin: we increase x by Δx if the coin comes up heads, and decrease x by Δx if the coin reads tails. A random generator in a computer can proceed in a similar manner by using even numbers as the basis for a positive increment and an odd number for a decrement. In this way, repeated over many runs, one obtains a distribution. King celebrated this sequential process of random events.⁴⁶

The mathematical solution [that is, the analytic solution] of the diffusion equation is an *approximation* of the distribution. The mathematical solution of the diffusion equation applies to the ideal situation of infinitely small steps. In setting up the diffusion equation, more assumptions were used than were put directly, in an elementary fashion, into the computing machines, and a solution has been reached by an entirely different computing scheme from any that would be used by hand.

The most concise formulation of King's view emerged in an animated exchange between King and the New York University mathematician Eugene Isaacson at one of the earliest meetings on the Monte Carlo method. King had just spoken on the problems of applying Monte Carlo methods to quantum mechanics:⁴⁷

Mr. Isaacson: Isn't it true that when you start out on your analysis of the physical problem and you have a complicated finite difference process, you then look at a continuous differential equation and approximate that by a simpler finite difference process and so cut down some of your work?

Dr. King: I think one can dodge a good deal of differential equations by getting back of the physics of the problem by the stochastic method.

King's view—that the Monte Carlo method corresponded to nature (got “back of the physics of the problem”) as no deterministic differential equation ever could—I will call *stochasticism*. It appears in myriad early uses of the Monte Carlo, and clearly contributed to its creation. In 1949, the physicist Robert Wilson took cosmic-ray physics as a perfect instantiation of the method: “The present application has exhibited how easy it is to apply the Monte Carlo method to a stochastic problem and to achieve without excessive labor an accuracy of about ten percent.”⁴⁸ And elsewhere: “The shower problem is inherently a stochastic one and lends itself naturally to a straightforward treatment by the Monte Carlo method.”⁴⁹ These two radically different metaphysical pictures of how simulations relate to nature are illustrated in Figure 1. On the Platonic view, “physical reality” was or ought to be captured by partial differential equations. In the particular case of diffusion, the physical reality was the spread of red dye in a thin capillary tube. It was “represented,” as it rightfully should have been, by a differential equation. This equation would, in any particular case, have a

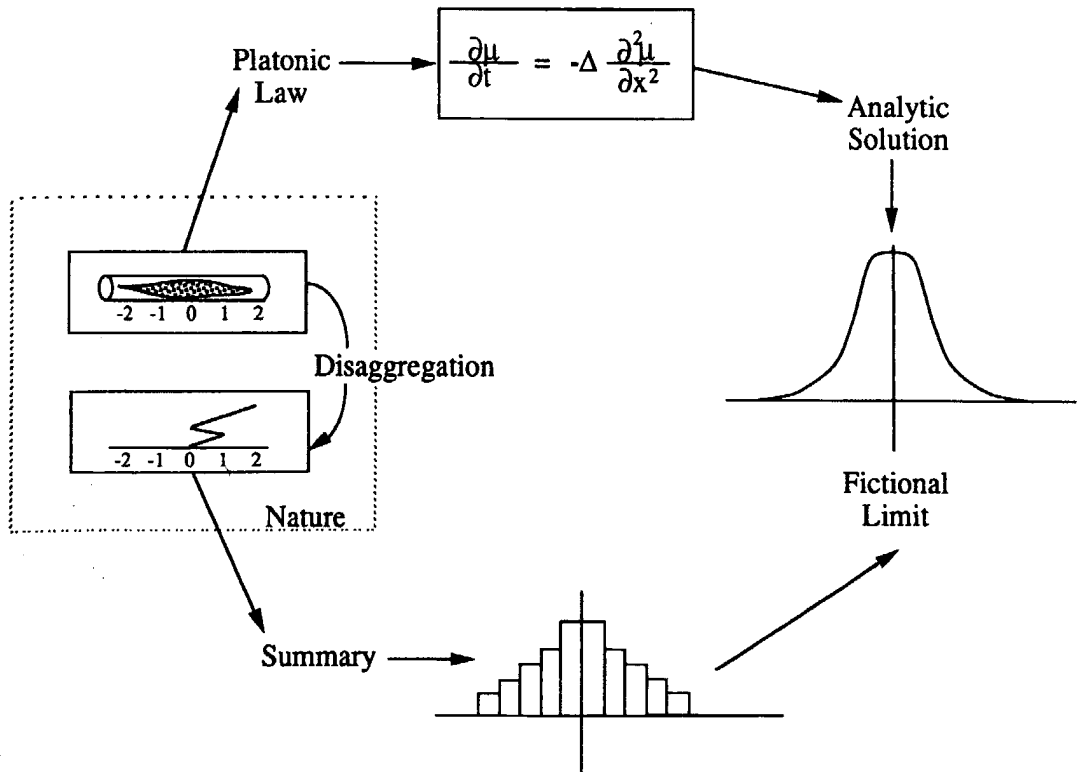


Fig. 1. Ontology of the Monte Carlo. In the Einstein-Dirac view, the capillary-tube diffusion process in nature (left) is modeled by a partial differential equation (top), and the solution for the diffusion equation is obtained analytically (right). By contrast, the King-Kahn view (stochasticism) has it that the capillary-tube diffusion process is *imitated* by the stochastic process of, for example, flipping a coin and moving to the right when a head falls and to the left when a tail falls. These “runs” are then tabulated in the bar graph (bottom), which summarizes the diffusion process without ever passing through the stage of an equation. In the case of diffusion, Einstein clearly saw the differential equation as an approximation; in the long run he believed differential equations would capture the underlying continuity of nature. King and Kahn saw the diffusion process as typical: nature is discontinuous and random—the computer Monte Carlo could imitate this sort of process without the approximation of differential equations.

solution, as illustrated at the right of the figure. On the stochasticist view, the walk of a counter right and left in the simulation, like the diffusion itself, was predicated on what at root was a fundamentally random process. Thus for the stochasticist, the simulation was, in a sense, of a piece with the natural phenomenon. The simulation was not just an alternative representation; it was a phe-

nomenon of the same type as diffusion, but disaggregated into the motion of individual entities. Each individual particle ends at a particular point on the x axis; when we collect these ending points into a histogram, we get the summary indicated at the bottom of the figure. Smoothing the curve, at the right of the figure is a fictional limit, obtained when one (imaginatively) extrapolates to an infinite number of "runs" of the experiment. To the Platonist, the stochasticist has merely developed another approximative method. To the stochasticist, the Platonist has interposed an unnecessary conceptual entity (the equation) between our understanding and nature—stochasticism, he or she claims, offers a direct gaze into the face of nature.

As I have already stressed, the problem of neutron diffusion was at the heart of the creation of the method, and it continued to provide difficult arenas of application, mostly in the sphere of weapons design and effects, and in related civilian applications of reactor design and safety problems. Householder, writing on neutron age calculations in water, graphite, and tissue, contended that "the study of the diffusion of heavy particles through matter provides an ideal setting for the Monte Carlo approach. Here we are not forced to construct an artificial model to fit a given functional equation, but can go *directly* to the physical model and, in fact, need never think about the functional equations unless we choose."⁵⁰ This notion of directness is central; it underlines the deep philosophical commitment to the mimetic power of the Monte Carlo method. This same faith in the metaphysical replication of process and representation appears in "health physics," where one finds a prominent Oak Ridge contributor, Nancy Dismuke, writing of neutron propagation through tissue: "The type of problem I am concerned with is a *natural* Monte Carlo problem in the sense that the physical model suffices as the model for proceeding with the calculation. An experiment is carried out (on a computing machine) which at every stage resembles closely the true physical situation. Whenever a random selection must be made in our experiment, the corresponding physical situation seems to be a matter of random choice."⁵¹ A. W. Marshall spoke of the Monte Carlo as most productive when it was applied to "nature's model" of a process such as diffusion, avoiding the differential equations.⁵²

Unlike the authors just cited, some advocates for this "natural-

ness," "directness," or mimesis of "nature's model" were people unwilling to endorse the program on abstract grounds of correspondence; instead, they argued on pure pragmatic grounds that it was the set of stochastic processes that were most effectively modeled by Monte Carlo. Listen to the intervention of a programmer (a Dr. Howlett) at the British atomic weapons research center at Harwell during a conference in the 1950's:⁵³

I am responsible for the computing service at Harwell and represent the user of the Monte Carlo technique who views it, professionally at any rate, as just another numerical weapon with which to attack the problems he is asked to solve. . . . I have to answer the questions, what are the problems to which Monte Carlo is best suited, how good a method is it—how much better than conventional numerical analysis—and how can it be improved?

In a sense this is the hardest-line pragmatic view possible: simulated reality would be accorded just that degree of credence that it earned in competition with other numerical processing packages. A slightly softer version of the pragmatic approach emerged from those who wanted to marginalize the Monte Carlo to a heuristic function, a kind of suggestive scaffolding to be kicked aside at the earliest possible moment. This emerged clearly at the December 1949 IBM Seminar in the following two interventions, the first by the Princeton mathematician John Tukey:⁵⁴

One point of view for the use of Monte Carlo in the problem is to quit using Monte Carlo after a while. That, I think, was the conclusion that people came to. . . . After you play Monte Carlo a while, you find out what really goes on in the problem, and then you don't play Monte Carlo on that problem any more.

A bit later, the Cornell mathematician Mark Kac commented on Robert Wilson's use of the Monte Carlo to examine cosmic-ray showers in a similar way:

They [Wilson and his collaborators] found the Monte Carlo Method most valuable because it showed them what goes on. I mean the accuracy was relatively unimportant. The five per cent or the seven per cent accuracy they obtained could be considered low; but all of a sudden they got a certain analytic picture from which various guesses could be formulated, some of them of a purely analytical nature, which later on turned out to

verify very well. . . . One of the purposes of Monte Carlo is to get some idea of what is going on, and then use bigger and better things.

In recent years this faith in the directness of the method has continued; even as late as 1987, a textbook on experimental particle physics could stress the special relation between stochastic objects and their Monte Carlo representations:⁵⁵

Historically, the first large-scale calculations to make use of the Monte Carlo method were studies of neutron scattering and absorption, random processes for which it is quite natural to employ random numbers. Such calculations, a subset of Monte Carlo calculations, are known as direct simulation, since the "hypothetical population" . . . corresponds directly to the real population being studied.

This more contemporary view wanted it both ways—metaphysical justification for simulations of stochastic processes, and a pragmatic validation of deterministic ones. But not everyone was so sanguine. Some scientists responded with barely concealed fury toward the pervasive introduction of the Monte Carlo method into physics. Monte Carlos, as far as the applied mathematicians John Hammersley and Keith Morton were concerned, ought to be expurgated line by line from the discipline. In January 1954, they put it this way:⁵⁶

We feel that the Monte Carlo method is a last-ditch resource, to be used only when all else has failed; and even then to be used as sparingly as possible by restricting the intrinsic random processes to bare essentials and diluting them wherever possible with analytic devices. . . . We cannot emphasize too strongly that the random element of Monte Carlo work is a nuisance and a necessary evil, to be controlled, restricted, excised, and annihilated whenever circumstances allow; and this applied mathematician's attitude, of seeing how much can be done without appealing to random processes, we contrast with the pure mathematician's attitude, of seeing how much can be done by appeal to random processes.

To these two authors, the battle line was drawn between the applied mathematicians and the pure mathematicians. Why? To these applied mathematicians, the extraction of physical laws from the world demanded analytic techniques because they led to reliable results. On this view of the world, pure mathematicians were merely playing with curiosities when they spent their efforts eval-

uating simple problems like linear differential and integral equations with only one or two variables, for these were easily tackled with everyday numerical methods. Such frivolous mathematical activities would be better replaced by serious inquiry into the true challenges, such as nonlinear integral equations with several unknowns.

Two distinct attacks on Monte Carlo enthusiasm had thus emerged by the early 1950's. There were those who, on principle, remained deeply suspicious of the artificiality of pseudorandom numbers and the simulations themselves. And there were those, like Hammersley and Morton, who for pragmatic reasons doubted the new method's reliability. Either way, it was the status of the enterprise itself that was in question. Not only was the broader community split over whether these new simulations were legitimate; it was split even further within the camps of the defenders and the attackers. Viewed "globally" the enterprise of simulating nature was on the shakiest of grounds. In the absolute absence of any agreed-upon interpretation of simulations, it might be thought the whole enterprise would collapse. It did not. Practice proceeded while interpretation collapsed.

The Pidgin of Monte Carlo

Pure mathematician, applied mathematician, physicist, bomb builder, statistician, numerical analyst, industrial chemist, numerical meteorologist, and fluid dynamicist: each has a view about what the Monte Carlo was. To the pure mathematician, the Monte Carlo was a measure defined on the space of infinite graphs, for a coupled set of Markovian and deterministic equations. For statisticians, the method was another sampling technique, with special application to physical processes; they considered such techniques well known, and as a result were at first hesitant to join the plethora of postwar conferences, discussions, and research efforts. To the numerical analyst like Howlett at Harwell, the method was just one more numerical tool for the solution of integrodifferential equations. To the industrial chemist such as King, the stochasticist view made his subject amenable to a form of direct representation never before possible. Bomb builders saw a modeling technique for a physical process so complicated by radiation transport and hydrodynamics

in hot media that all their usual means failed, both experimental and theoretical. The symbols and procedures of the method, therefore, sit variously in each domain, and connect differently to the terms, theorems, and style of each discipline.

Yet for all this diversity, it is clear from our discussion thus far that at meeting after meeting, beginning with the Institute for Numerical Analysis in Los Angeles of June 1949, and continuing through the IBM Scientific Seminars of 1949 and the 1954 Gainesville meeting, representatives of these various professions could and did find common cause. Moreover, individuals, including Ulam, King, Householder, John Curtiss, and others could alternate between problem domains without difficulty. Von Neumann passed as quickly to meteorology as he could to the Superbomb, to reactor problems, or to fluid-dynamical shock calculations. A chemical engineer could speak easily to a nuclear physicist in this shared arena of simulation; the diffusion equation for chemical mixing and the Schroedinger equation were, for purposes of this discussion, practically indistinguishable. Yet this is not to say that the nuclear physicist and the chemical engineer themselves became identical or interchangeable—far from it. If anything, with the growth of nuclear engineering and cyclotron design, and an expansion of meson theory, nuclear physicists were growing ever farther from their chemist colleagues.

In the heat of the moment, a kind of pidgin language emerged, in which procedures were abstracted from their broader significance. Everyone came to learn how to create and assess pseudorandom numbers—without the full mathematical apparatus for evaluating the “essence” of being random. It became a matter of common ability to transform these pseudorandom sequences into the particular weighted distributions needed for a particular problem. Everyone learned the techniques of variance reduction, such as importance sampling and splitting. Terms like “sampling” and “finite game” captured pieces of each of the broader and fuller fields that interacted with one another. What had been arcane knowledge of the mathematician—Courant’s criterion—became a standard test to be applied to the difference equations as they were readied for loading onto computers.

Slowly, this pidgin language began to build into something more than a matter of provisional utility. Research in the Monte

Carlo, and into its inner structure, began to gather a momentum of its own: theorems were postulated and proved; rules of thumb were generated. The crude mediating language began to acquire its own journals, its own experts. By the 1960's, what had been a pidgin had become a full-fledged creole: the language of a self-supporting subculture with enough structure and interest to support a research life without being an annex of another discipline, without needing translation into a "mother tongue." The principal site of creation for this hybrid language was Los Alamos. For it was there, in the search for improved nuclear weapons, that a new mode of coordinating activities was built, where scientists from different disciplines (different practice and language groups) could form a trading zone.

Of course not everyone shared all the skills of this new "trading zone." Some focused on the game-theoretical aspect; others, more on variance reduction or convergence problems. There were formalists and practically minded researchers, workers interested in particular method problems such as inverting matrices and others content to exploit basic results. For a few years, between 1944 and 1948, the exploration of sampling and stochastic processes proceeded in the hothouse environment of the weapons laboratory, nurtured with the endless resources of the Atomic Energy Commission and the devoted efforts of von Neumann and Ulam. Already during this time, a shared proficiency developed that made the translation of problems into the pidgin of the Monte Carlo inviting, almost required. Perhaps because secrecy at first had both bound together and isolated the originators of the method, when the news broke outside the AEC enclave it had an even greater fascination among the great uncleared; it was a fascination so intense that it sparked warnings about overenthusiasm.

Fiction writers have recognized (more clearly than philosophers or historians) the powerful and problematic temptations of this artificial reality. In William Gibson's remarkable trilogy,⁵⁷ cyberspace—the unlocated computer-driven reality outside physical existence—contains as much significance as our daily three-dimensional space.

The seductiveness of cyberspace as an alternative to coping with the harsh edges of the everyday is, and was, apparent to those who work with simulations. Years after working on simulations of the

first H-bomb, a physicist (then a young postdoc) told me: "I had a strange attitude toward the reality of hardware and the reality of explosions, which it's hard for me to explain now. But [it] was intense and real at the time. I didn't want to see the actual hardware of an atomic bomb in the laboratory . . . in the machine shop, in the metallurgical facility. And I didn't want to see a nuclear explosion."⁵⁸ The alternative world of simulation, even in its earliest days, held enough structure on its own to captivate its practitioners. And in their fascination they learned a new way of work and a new set of skills that marked them for a long time to come.

Elsewhere, one mathematician went so far as to identify an incident that occurred in the seventeenth century as the first recorded instance of what might be regarded as an application of the Monte Carlo method, while other statisticians, in some annoyance, presented genealogies of precedents and antecedents to show that sampling methods were nothing new to the statisticians. A few days after the IBM Gainesville conference, Herman Kahn, then writing a brief "historical" introduction that would set Ulam's place in this now-extended pantheon of Monte Carlo progenitors, queried Ulam on how to set the scene. Ulam responded this way:⁵⁹

It seems to me that while it is true that cavemen have already used divination and the Roman priests have tried to prophesy the future from the interiors of birds, there was not anything in literature about solving differential and integral equations by means of suitable stochastic processes. In fact the idea of Monte Carlo seems to me to consist mainly in inverting the procedures used before, that is to solve problems in probability by reducing them to certain special differential equations. Of course, sampling processes were used in statistics but the idea of using probabilistic schemes to solve problems in physics or pure mathematics was not used before Johnny v. N. and myself were trying it out.

This seems to me about right: the Monte Carlo in some ways was the culmination of a profound shift in theoretical culture, from the empyrean European mathematicism that treasured the differential equation above all, to a pragmatic empiricized mathematics in which sampling had the final word.

When it came time to justify the new technique, the community's responses were varied. If the borrowed error-analysis methods of experimentation provided a first, epistemic argument for

calling Monte Carlo runs "experiments," a second, metaphysical one came close on its heels: like nature itself, and unlike differential equations, the computer-based Monte Carlo proceeds by a series of (at least simulated) random occurrences. In this strong sense, the early Monte Carlo applications to the diffusion of gases, the scattering of neutrons, the production of cosmic-ray showers simulated nature. On this view (the view of many early practitioners of the Monte Carlo), the Monte Carlo offered a resemblance relation between sign and signifier in a pre-sixteenth-century semiotic sense. As the Monte Carlo became a standard tool for the resolution of problems with no stochastic elements, King's position became harder to defend, and the vision of simulations as offering a uniquely privileged vantage point began to dissolve. Nonetheless, the sense of direct access to a problem "as nature poses it," "behind" the equations never quite left the players of Monte Carlo.

Others challenged the legitimacy of knowledge based on simulations. For many theorists, an analytic solution continued to have a cachet forever inaccessible to the electrical engineers and their approximation methods. If any theoretical representations stood in Plato's heaven, they were the delicate hypersurfaces of differential equations—not batch-generated random numbers and the endless shuffle of magnetic tape. For experimenters, the Monte Carlo never came to occupy a position of "true" experimentation, as exemplified in debates that continued decades later over the legitimacy of according doctorates to students who had "only" simulated experiments. This left the engineers and their successors, the computer programmers, in a peculiar position. They spoke an intermediate language, a kind of formalized creole: the language of computer simulations understood both by theorists and by experimenters. (It was no accident that conferences flourished with names such as "Computing as a Language of Physics.") Accordingly, the simulators became indispensable as links between high theory and the gritty details of beam physics and particle collisions. But just as they occupied an essential role in this *delocalized trading zone*, they also found themselves marginalized at both the experimental and the theoretical end of particle physics.

I have argued here that the atomic bomb served as both the subject and the metaphorical generator of the Monte Carlo technique. Simulations were essential in enhanced-fission-weapon work and

in the basic design of the thermonuclear bomb. Not only did the weapons projects provide the resources; they yielded the prototype problems on which virtually all early thinking about simulations were predicated: neutron transport, stability analysis, radiation diffusion, and hydrodynamics. Everywhere one turns—from von Neumann's early work on mounting the Monte Carlo on computers, to Ulam's original reflections on the method; from Herman Kahn's variance-reduction methods, to a myriad of applications in air bursts, tissue penetration, and the diffusion of gamma rays—one sees the hand of weapons design. There were, naturally, exceptions: Robert Wilson continued his work on cosmic-ray showers with the Monte Carlo, and Enrico Fermi used the computer for calculations on pion-proton resonances.

But the early world of simulations was steeped in weapons considerations; nuclear bombs saturated every aspect of these early discussions, from language to the self-representation of the simulators. In this respect, I close with a paraphrased summary of Jerzy Neyman, a statistician at the Statistical Laboratory (Berkeley) who opened a roundtable discussion on the Monte Carlo with an analysis of origins of the new method:⁶⁰

Speaking first of the history of science, he [Neyman] observed that it seemed rather general that an idea begins to explode with sporadic, disconnected events. Each of these may trigger off further explosions in turn, as we imagine the events to occur in a chain reaction. In time these explosions of ideas occur more frequently and we eventually have what might be likened to a mass explosion in which the ideas blossom out and become common knowledge.

Perhaps. Looked at from a microscopic point of view, an atomic bomb does proceed in this way: disconnected fissions, scatterings; a Markovian universe plummeting into detonation. But from outside, we see a mission-oriented laboratory, a group of scientists allied with a military infrastructure struggling to create particular weapons and the intellectual superstructure to facilitate their design and implementation. Have we witnessed the cascade of the participants' narrative of history into the narrative of the physicists' simulations?

In baldest possible form: the computer began as a "tool"—an object for the manipulation of machines, objects, and equations.

But bit by bit (byte by byte), computer designers deconstructed the notion of a tool itself as the computer came to stand not for a tool, but for nature itself. In the process, discrete scientific fields were linked by strategies of practice that had previously been separated by object of inquiry. Scientists came together who previously would have lived lives apart, and a new subfield came to occupy the boundary area. Notions of simplicity were stood on their heads: where compact differential equations previously appeared as the essence of simplicity, and numerical approximations looked complex, now the machine-readable became simple, and differential equations complex. Where the partial differential equation had appeared as the exalted furniture decorating Plato's heaven, now Monte Carlo methods appeared to re-present truly the deeply acausal structure of the world.

Methodologically, the lesson—stated as provocatively as possible—is this. A contextualized, practice-based analysis serves nicely to underwrite a specific kind of social constructivism: categories of simplicity and conformity to the world will not survive as transcendent categories. But the picture of science in total rupture between frameworks strikes me as ever more chimerical. Three generations of framework relativists are enough.