Program FAKE: Monte Carlo Event Generators as Tools of Theory in Early High Energy Physics

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The term Monte Carlo method indicates any computer-aided procedure for numerical estimation that combines mathematical calculations with randomly generated numerical input values. Today it is an important tool in high energy physics while physicists and philosophers also often consider it a sort of virtual experiment. The Monte Carlo method was developed in the 1940s, in the context of U.S. American nuclear weapons research, an event often regarded as the origin of both computer simulation and “artificial reality” (Galisson 1997). The present paper interrogates this strong claim by focusing on the emergence of Monte Carlo event generators in particle physics in the early 1960s. This historical case study shows how, as Monte Carlo computation became part of the toolbox of particle physicists around 1960, it was neither usually referred to as a “computer simulation” nor was it regarded as a surrogate for experimentation. In revising the history of this method, this paper asks, in what context did particle physicists of the 1960s decide to create FAKE, the first high-energy-physics Monte Carlo event simulator? What was their goal? And what epistemic role did FAKE play? In answering these questions, it is argued that Monte Carlo computations were not introduced into particle physics to simulate experiments, but rather they played the role of theoretical tools. The Monte Carlo method was able to do this thanks to its random component, a property which provided a means of modeling a specific phenomenon, so-called “(particle) resonances”. Indeed, in doing so, event generators even came to mutually assimilate and reshape the notions of particle and resonance, taking up an epistemic function which had previously been confined to physical-mathematical formulae: that of a medium which could express aspects of particle theory.

Keywords: Monte Carlo method, computer simulation, high energy physics, elementary particles, resonances, medium of theory

Programm FAKE: Monte Carlo Eventgeneratoren als Werkzeug der Theorie in der frühen Hochenergiephysik

Als Monte-Carlo-Methode versteht man jedes computergestütztes Verfahren der numerischen Schätzung, das mathematische Berechnungen mit zufallsgenerierten numerischen Eingaben kombiniert. Sie ist ein wichtiges Werkzeug der Hochenergiephysik und gilt bei Physikern und Philosophen oft als eine Art virtuelles Experiment. Die Monte-Carlo-Methode wurde in den 1940er Jahren im Kontext US-amerikanischer Forschung über Nuklearwaffen entwickelt und ihre Entstehung wird heute oft mit dem Ursprung von Computersimulationen und „künstlicher Wirklichkeit“ (Galisson 1997) gleichgesetzt. Der vorliegende Beitrag hinterfragt diese Sicht der Dinge, indem er die Entstehung von Monte-Carlo-Eventgeneratoren in der Teilchenphysik der 1960er Jahre untersucht. Diese historische Fallstudie zeigt, dass Monte-Carlo-Berechnungen, als sie um 1960 dem Werkzeugkasten der Teilchenphysik hinzugefügt wurden, weder „Computersimulationen“ genannt, noch als eine Art Ersatz zum Ausführen von Experimenten betrachtet wurden. Der Aufsatz wirft neues Licht auf diese Ereignisse und untersucht, welcher Kontext Teilchenphysiker der 1960er Jahren dazu veranlasste, FAKE zu entwickeln, den ersten Monte-Carlo-Eventgenerator der Hochenergiephysik. Was war ihr Ziel? Welche epistemische Rolle spielte FAKE? Ergebnis der Untersuchung ist, dass Monte-Carlo-Berechnungen in die Teilchenphysik nicht eingeführt wurden, um Experimente zu simulieren, sondern als Werkzeuge der Theorie und zwar aufgrund der Zufallselemente der Monte-Carlo-Methode, welche die Modellierung eines spezifischen Phänomens erlaubten, die sogenannte „(Teilchen)Resonanz“. Aus dieser Anwendung der Monte-Carlo-Eventgeneratoren ergab sich mit der Zeit sogar eine wechselseitige Assimilation der Begriffe „Teilchen“ und „Resonanz“, die zur Umwandlung beider führte. So nahmen Monte-Carlo-Berechnungen eine epistemische Funktion, die bis dahin nur physikalisch-mathematische Formeln ausübten: die eines Mediums, das Aspekte der Teilchentheorie ausdrücken kann.

Introduction

Today, the term “Monte Carlo method” (or simply “Monte Carlo”) indicates any computer-aided procedure of numerical estimation which makes use of random numbers. Monte Carlo computation is often, more or less naively, presented by scientists as some kind of computational experiment, and in the last decades it has been studied by philosophers of science as paradigmatic example of the broader class of methods known as computer simulations (Borrelli & Wellmann 2019). In his monograph *Image and Logic*, Peter Galison suggested that the Monte Carlo method, introduced in the 1940s in context of nuclear weapons research, was the origin of computer simulation and the very idea of “artificial reality” which soon came to dominate computing discourse (Galison 1997: 698). As discussed in detail in the Introduction to this Special Issue, Galison’s view was accepted by most later authors, but the present contribution will show how the spread of the Monte Carlo method and the rise of artificial reality were less rapid and straightforward than such a narrative assumed. I will do so by showing that Monte Carlo computations became a central component of high energy physics only by the 1960s and at that time they were not yet linked to an idea of artificial reality. The notion that Monte Carlo computations and the machines they ran on went on to constitute an artificial reality was only one of various theses brought forward by Galison in his pathbreaking monograph, and it was the result of a highly original, yet rather coarse-grained investigation into the history of this method based on a relatively small number of sources spanning two or three decades. Since no later authors provided more detailed assessments of the material discussed by Galison, his nuanced thesis has at times come to be presented with much sharper contours than it initially had, leading to a widely held conviction that already in the 1950s and early 1960s the Monte Carlo method was generally seen as a sort of surrogate to experimentation. In high energy physics, though, this was not the case: the Monte Carlo method took center stage in that discipline in the context of a very specific historical-epistemological constellation the coming-into-being of which will be reconstructed in the pages that follow.

The first Monte Carlo code to be widely used in particle physics was the program FAKE, written in 1961 at the Lawrence Radiation Laboratory in Berkeley by physicists Gerald Lynch, Grove Nooney and Fred Safier

(Lynch 1962a: 3). The term FAKE was not an acronym, as would become standard practice later on in regard to software naming, instead it referred to the program's ability to both produce "fake" particle data and help spot "fake" events, that is real data which could be mistaken for the signature of a specific phenomenon to be studied in a given experiment (Lynch 1962a: 2–3).

FAKE provided the core for many later Monte Carlo programs used in high energy physics both at U.S. American research institutions and at CERN, the European Organization for Nuclear Research. In its user manual, program FAKE was described as "a Monte Carlo simulation of bubble chamber events" and an "event generator" (Lynch 1962a: 3), apparently much in line with how Monte Carlos are seen by contemporary physicists. However, I will endeavor to show that at the time the status of Monte Carlo event generators was quite different than it is today, and that they were not seen as capable of simulating experimentation or mimicking physical processes, but rather as means to produce outputs which looked like experimental results. The difference, I will argue, is a fundamental one.

The paper begins with a sketch of the origin and early years of the Monte Carlo method, underscoring how historical actors often made a distinction between two types of Monte Carlo computation, of which only the first could be seen as simulating a physical process, while the second type rather counted as a computational tool. As we shall see, the distinction has so far been ignored by historians and philosophers discussing the Monte Carlo method, but it is found in sources from particle physics up to the late 1960s, and is still mentioned in a review from 1980, though only as a side remark. I will argue that taking this distinction seriously will allow us to better grasp the way in which early practitioners were—or were not—conceptualizing the Monte Carlo method. To provide context, an overview of some key research questions in particle physics of the 1950s follows, which in particular discusses the emerging notion of "particle resonances". In the sections that follow, I present the first systematic uses of Monte Carlos of the second type to implement a specific theoretical approach in particle physics, the so-called "Fermi statistical model". After that, I reconstruct the experimental and theoretical constellations that led to the first use of Monte Carlo computations in the analysis of bubble-chamber data at the Lawrence National Laboratory in Berkeley, and the ensuing construction of program FAKE. At the end of the paper I spell out two broader historical-epistemological claims based on the case study: (1) Monte Carlo computations entered particle physics as theory explicating computational tools, but in the early 1960s they went on to reshape the conceptual landscape of this science and in particular the fundamental notion of 'particle'; (2) between the late 1960s and early 1970s the Monte

Carlo method's epistemic status in particle physics further shifted, with historical actors coming to regard them initially as simulations of experimental results and later on as simulations of experiments. The concluding section provides a summary of this case study.

The Origin of the Monte Carlo Method and the Beginning of Modern Computing

The origin of the Monte Carlo method can be clearly traced back to U.S. American research on nuclear weapons pursued in the years immediately after World War II by John von Neumann, Stanislaw Ulam, and other collaborators using the Los Alamos ENIAC (Electronic Numerical Integrator and Computer). Early accounts of this work are found in the recollections of historical actors (Hurd 1985; Metropolis 1987). Later on, historian and philosopher Peter Galison used the example of von Neumann's and Ulam's use of Monte Carlo computation in the development of the H-bomb in 1949–50 to argue that computers—and in particular computer simulations—came to be regarded as generating a sort of “artificial reality”, and that they could play the role of a “trading zone” in which practitioners with different backgrounds like mathematicians, physicists and engineers, could productively interact (Galison 1997). Galison's account closely follows John von Neumann's view on the development of this new numerical method, and conveys the impression that the emergence of Monte Carlo computations could be framed in terms of the production of a new kind of computer program to be implemented on the recently developed, at-the-time fast electronic computer ENIAC. However, although the speed of the new machine was indeed a determining factor in the decision to develop the Monte Carlo method, a recent in-depth study by Thomas Haigh, Mark Priestley and Crispin Rope has shown how the situation was much more complex (Haigh et al. 2016). These authors reconstructed the conception, design and implementation of the Monte Carlo method on the ENIAC on the basis of ample, so far largely unexplored archival material, showing how that process was in fact an inextricable combination of code production and hardware (re)configuration. The ‘coding’ of the Monte Carlo on the ENIAC implied a reshaping and rewiring of the ENIAC with respect to its existing configuration, while at the same time it required the progressive transformation of the computing plan that had initially been drawn up by von Neumann (Haigh et al. 2016: 173–191). Indeed, the three authors claim that the implementation of the Monte Carlo method on the ENIAC transformed it into a machine conforming to von Neumann's views of what

constituted computing, and to the “modern code paradigm”, converting the ENIAC into what a posteriori is often described as a “stored-program machine” (Haigh et al. 2016: 175).¹ In their words:

In the process [of implementing the Monte Carlo method from von Neumann’s original computation plan], the team overcame a range of practical and conceptual challenges to develop and apply techniques we would now recognize as indispensable idioms of programming practice, such as the use of a subroutine and the efficient coding of loops. (Haigh et al. 2016: 191)

Setting aside the much-discussed issue of whether or not the ENIAC was the first modern computer, it is important to note that, at the time, the emergence of the Monte Carlo method was not the creation of new software to fit new, fast hardware, but rather an innovative step which contributed to create the distinction between software and hardware as it is known today. From this background, Galison’s account appears to project much later views onto the specific historical constellation of the immediate post-war period, allowing it to appear that the ENIAC shaped by von Neumann’s ideas was the same ENIAC which earlier users employed. In her contribution to this special issue Liesbeth De Mol has instead succeeded in portraying the peculiarities of the von Neumann ENIAC by contrasting it with the way in which the machine was configured and employed by two other mathematicians who employed it: Derrick H. Lehmer and Haskell B. Curry (De Mol 2019). Thanks to a careful reconstruction of the use of the ENIAC by Lehmer and by Curry and a comparison with von Neumann’s work De Mol shows how much all three approaches were shaped by the goals and thought styles of the scientists using the machine: there was no single ENIAC, just like there was no ‘simulation’ in the early Monte Carlo, if we understand that term in today’s meaning.

Early Diffusion of the Monte Carlo Method: “Nature’s Model” Versus an “Analogue Idea”

This short account of the very early days of the Monte Carlo method should have made clear that the Monte Carlo was not simply an algorithm or a program, but both the motivation for and the product of a reconfiguration of different layers of both machine and code. Being aware of this situation is important in order to realize that one cannot frame the developments of the 1950s purely in terms of a transfer of a new coding technique to other machines or problems, but rather as the result of the slow spread of

different ways of formulating and solving problems with the help of rapidly developing, and not yet standardized electronic computers. Let us now turn to the physics problem which the first Monte Carlo was supposed to solve, as not only does it go to the heart of our concerns, but also provides a good example to describe the key features of the method.

In the late 1940s, the main problem in attempting to gain a closer understanding of the process of nuclear chain reactions, of both fission and fusion kinds, was how to obtain numerical estimates of the outcome of neutron diffusion among nuclei after a given time. The equations modeling the process were too complex to be solved analytically, due both to the large number of particles and to the many different kinds of interactions involved (Galison 1997: 698–727). Because of this, von Neumann and Ulam decided to use a simplified stochastic model of the diffusion process. Initially, the method was developed for neutron diffusion among heavier nuclei, and later among Hydrogen nuclei for the development of the H-Bomb. In the model, neutrons with different velocities moved independently from each other in a series of discrete steps. For each step the model prescribed probabilities of different outcomes, such as the absorption of the neutron by a nucleus, scattering, production of further neutrons and so on. Both the initial distribution of neutron velocities and the outcomes of the individual interactions were determined in the program by random numbers and, because of this similarity with games of chance, the procedure was soon nicknamed ‘Monte Carlo method’ in reference to the gambling destination in Monaco. As its creators fully acknowledged, the Monte Carlo method could be seen as a form of statistical sampling, a traditional procedure for obtaining numerical estimates of the average properties of a set of systems by randomly choosing among its elements and averaging over the results of the choice. The novelty was that, thanks to newly developed ENIAC, many sampling steps could be computed in a very short time. Computations of nuclear fission and fusion were of course classified information, but the general principles of the stochastic procedures were less secret and soon spread among physicists by word of mouth through personal communications and conference presentations (Curtiss 1951; Hurd 1985). Already by 1948 a project to further develop the Monte Carlo method was established at the Los Angeles unit of the National Applied Mathematical Laboratories of the National Bureau of Standards (Householder 1951a). The first published report on ‘The Monte Carlo method’ appeared in 1949 (Metropolis & Ulam 1949), and in June of the same year a conference on the subject was organized by the RAND Corporation² and the National Bureau of Standards. A specific aim of the meeting was to promote exchange between physicists, mathematicians and statisticians in the study of the practical applications and theoretical foundations of the method (House-

holder 1951a). In his “Foreword” to the proceedings of the meeting Alston Householder underscored the novelty of using stochastic procedures to estimate expressions which had in themselves no probabilistic nature:

[T]he novelty [of the Monte Carlo method] lies rather in the suggestion that where an equation arising in a nonprobabilistic context demands a numerical solution not easily obtainable by standard numerical methods, there may exist a stochastic process with distributions or parameters which satisfy the equation, and it may actually be more efficient to construct such a process and compute the statistics than to attempt to use standard methods. (Householder 1951b: v)

Thus, the availability of new, fast methods for numerically implementing stochastic processes had led to the idea of using them to solve numerical problems which in themselves had no probabilistic character. If an analytical expression could be shown to be the limit of a stochastic distribution, then a Monte Carlo computation on a fast machine could be used to numerically estimate that expression by performing many steps of the stochastic process. The distinction between probabilistic and non-probabilistic problems to be solved by the Monte Carlo method was also prominent in later discussions on the subject. In his “Preface” to the proceedings of the second conference on Monte Carlo methods held in 1954, Herbert Meyer quoted the passage by Householder and a similar distinction made by Monroe D. Donsker and Mark Kac that distinguished between two types of Monte Carlo computations: one in which particles “play a game of chance” and a “more sophisticated” version where a game is found that is related to the solutions of a mathematical problem (Meyer 1956: vi). In his “Introductory Note” to the proceedings of the same meeting, Albert W. Marshall, too, described two kinds of Monte Carlo computations: the first type was a form of machine-aided statistical sampling which made use of the known dynamics of a system, for example by discretizing its equations of motions and using the results as the basis for stochastic procedure, as in the case of neutron diffusion. The other type of Monte Carlo, which Marshall characterized as “analogue idea”, consisted in solving non-probabilistic problems “by finding a probabilistic analogue and then obtaining approximate answers to this analogue by some experimental sampling procedure” (Marshall 1956: 2–3). In the second case, the procedure implemented in the computer could be entirely unconnected to the mathematical problem to be solved, even if the latter had a physical origin. For Marshall, the “analogue idea” was the key innovation of the Monte Carlo method. He regarded Monte Carlos of the first kind as less interesting, and he referred to them as “nature’s model” because they were

based on the equations describing the dynamics of the system (Marshall 1956: 4).

A further development emerged when scientists noted how, even when no stochastic procedure approximating a given integral could be found, it was nonetheless often an advantage to numerically compute it by random sampling of its integrand, rather than performing a traditional, non-random step-by-step integration over predetermined intervals.³ This kind of Monte Carlo integration, as it was called, did not imply the implementation of a stochastic process, but only the use of random numbers, yet later on we shall see how, at least in high energy physics, it eventually came to be seen as a sort of ‘simulation’ of physical events.

In 1968, when the Monte Carlo method had become a standard component of high energy physics research, lectures on “Monte Carlo phase space” were given in the context of CERN’s Academic Training Programme by Frederick James and published as a CERN report in 1968 (James 1968). Significantly, James began his lecture by defining the Monte Carlo method in terms of two kinds of problems, “Type A: statistical, probabilistic, involving random fluctuations” and “Type B: analytic, exact, classical problems” (James 1968: 1). The two types corresponded to the distinction made by Householder, Marshall and others. James acknowledged that, in practice, the two types could not be fully distinguished from each other, but the fact that he made the distinction at all shows how important it still was for those approaching Monte Carlos, at least in high energy physics.

Interestingly, Galison took no notice of the distinctions between the two different types of Monte Carlo computations made by historical actors, and instead chose to underscore how the Monte Carlo method could be seen in a more general way by scientists of various disciplines, taking up the role of a “tertium quid” between theory and experiment (Galison 1997: 689–692). The historiographical importance of Galison’s insight on the role of the computer and of Monte Carlo computations in disrupting the ideal distinction between experimental and theoretical practices of science can hardly be overestimated. However, a detailed reconstruction of specific historical moments in the long history of the Monte Carlo method discloses a landscape of greater complexity than the one he sketched.

For the subject at hand, the distinction between the two types of Monte Carlo applications is crucial, since it is my contention that the use of the Monte Carlo method in high energy physics research around 1960 was not initially conceptualized as an attempt to mimic a physical process, but was rather regarded as a technique to obtain numerical estimates of analytical expressions. It was, in short, seen as an instance of the second type of Monte Carlo computations. But let us go back to the early 1950s, when the Monte Carlo method became known among scientists of different dis-

ciplines. Other than in nuclear or solid state physics (Battimelli & Ciccotti 2018; Mareschal 2018), until the second half of the 1950s particle physicists made no systematic use of either type of Monte Carlo computations. This situation is hardly surprising, considering that no reliable theories of particle interactions existed that could be used for stochastic modeling or for complex analytical computations. Around 1957, the Los Alamos research group led by Nick Metropolis tentatively extended the stochastic models developed for simulating nuclear processes to interactions at energies high enough to lead to the production of additional particles, mainly pions, and these models were later developed further by other scientists (Crew et al. 1957; Metropolis et al. 1958). Qualitative agreement with measurements were obtained and interpreted as confirmation of the validity of the approach, though it was not developed further in the following years. While a research group from the Soviet Union implemented various stochastic models in Monte Carlo calculations for nuclear interactions at high energy (Nikolski et al. 1957). The authors acknowledged that no reliable theory for such phenomena existed, and their computations explicitly aimed at testing the various models by comparing the results of Monte Carlo computations with experiments. In conclusion, even Monte Carlo methods of the first type, when used in early particle physics, were not considered as surrogates for experimentation, but rather took up the role of a numerical implementations of a theory, albeit of a kind that could not have been performed analytically.

What about Monte Carlo computations of the second type? The “analogue idea” of using stochastic processes to solve non-probabilistic problems began in high energy physics from the late 1950s onward, and this happened in the context of the application of a model of particle production proposed by Enrico Fermi in 1950. This line of development eventually led to the creation of program FAKE and, to understand it, it is necessary to briefly sketch some developments in the field of particle physics in the 1950s.

High Energy Physics in the 1950s and Early 1960s

High energy physics emerged as an independent research field in the years after World War II and by the late 1950s it had been fully established as a discipline (Brown & Hoddeson 1986; Brown et al. 1989). In this early period its development was largely motivated by two lines of experimental work. The first was the study of interactions between pions and nucleons (i.e. protons and neutrons), from which scientists hoped to learn about

nuclear forces. The second line of research was the investigation of the properties of a number of ‘new’ particles whose existence had been revealed by cosmic ray experiments from the late 1940s onward. From these lines of research emerged two conceptual innovations of great relevance for the development of high energy physics in general and for the present theme in particular: the study of pion-nucleon interactions led to the notion of ‘particle resonance’, while the investigation of the new particles resulted in the classification of old and new particles into groups (‘multiplets’).

Let us now look at the first development. Pions were discovered in cosmic rays during the 1930s, and in the early 1950s interactions between them and nucleons began to be studied in the laboratory by shooting beams of pions onto a static target of ‘normal’ matter, so that the pions would have a certain probability of interacting with the protons and neutrons it contained. Experimental physicists discovered that the frequency of interaction was particularly high when positive pions with an energy of approximately 180 megaelectron volts (MeV) were shot onto a static target of protons (Hydrogen nuclei) (Borrelli 2017a: 89–92), while for other energies, or for negative pions, the frequency of interaction was much lower. When plotted as a function of the energy value, the frequency of interaction between positive pions and protons took the form of a peak which qualitatively, and in part also quantitatively, matched the shape of known resonance phenomena in optics, acoustics, electronics and nuclear physics. At this time, the term ‘resonance’ began to be employed to describe the frequency peak in pion-proton scattering. Moreover, physicists noted that the shape of the resonance peak could be approximated quite well by using a simple formula developed by Gregory Breit and Eugene Wigner for a different phenomenon, nuclear resonances (Breit & Wigner 1936; Walker 1989). By 1955 the existence of the 180 MeV pion-proton resonance peak had been established, but there was no consensus among theorists on its physical interpretation: some of them suggested that it indicated the coming-into-being and almost immediate decay of a compound state of pion and proton, or of an excited state of a proton, or perhaps of a thoroughly new particle, while others claimed that pions and protons remained distinct, but interacted more strongly.⁴

The details of this discussion are not central to our subject, but it is important to note that the pion-proton resonance only became visible when the same experiment was repeated various times at different energies, measuring the frequency of the various outcomes. In other words, the resonance manifested itself only when looking at the statistics of events, and not if one considered a single outcome. Although only one pion-proton resonance had in fact been observed at the time, in the second half of the 1950s some theorists suggested that perhaps also further resonances might

exist. As we shall see, around 1959 experimenters started regarding these hypothetical resonances as potentially interesting phenomena to search for with their increasingly powerful accelerators and detectors. At this point, the experimental research program focusing on pion-proton interactions combined with the other line of investigation mentioned above, that is the production and study of ‘new’ particles.

Soon after the end of the Second World War, a growing number of detector tracks were observed which could not be interpreted in terms of the few particles so far known, and by the early 1950s a broad range of suggestions as to how to group together old and new particles existed. Around 1957 one classification scheme, which went under the names of Murray Gell-Mann and Kazuhiko Nishijima, had turned out to be particularly successful, allowing for the prediction of the existence of further particles (Borrelli 2020; Bresolí Catà 2006; Gell-Mann 1982). By the late 1950s all particles that scheme predicted had been found, and many possible extensions had been proposed. As we shall see, one of these schemes played a role in the establishment of Monte Carlo computations as a tool for particle research. Before discussing these developments however, we have to step back to the early 1950s and sketch the development of the so-called Fermi statistical model of particle production.

The Fermi Statistical Model of Multiple Particle Production

A key conceptual development at the core of high energy physics was the recognition that, mainly due to ‘strong’ (i.e. nuclear) interactions, in the collision of two particles a broad range of other particles could be created, provided enough energy was available. Unraveling the rules according to which certain types and numbers of particles were produced under given conditions was no easy task, and during the 1950s and 1960s no theory of particle creation was accepted as valid by all theorists. At the same time though, experimenters were often confronted with the necessity of estimating the particles, energies, and velocities produced in planned experiments. To do so they often turned to a rather simple model originally proposed by Enrico Fermi in 1950 (Fermi 1950).

Fermi called his proposal “an attempt [...] to develop a crude theoretical approach for calculating the outcome of nuclear collisions with very great energy” (Fermi 1950: 570). His key assumption was that, due to the great intensity and short range of strong interactions, when two particles collided their energy remained confined within a very small sphere in space. Within a negligible amount of time, one among the many possible outcomes of the

collisions would be realized, with the final state particles flying away with specific velocities and directions of flight. As usual in quantum physics, Fermi assumed that it was not possible to determine a priori with certainty the outcome of the collision, and he instead attempted to estimate the relative probabilities of different final states by assuming equal probabilities for all outcomes that were not forbidden by some conservation law (energy, momentum and so on). For example, if a proton and an antiproton collided and mutually annihilated, leaving enough energy to produce two or three pions, then all final states with two or three pions with that total amount of energy should be regarded as equally probable. Thus, in principle, one only had to count how many outcomes were possible, and then equally divide the probability among them. However, in practice the computations were in no way trivial: a final state containing three pions with a given total energy, for example, corresponded to infinitely many specific configurations, with the energy variously distributed among the three particles, each of which could also have continuously varying directions of flight. Therefore, it was necessary to integrate over all these possible individual states. This integral was referred to as the relevant “phase space integral” and it will play a central role in our story, because Monte Carlo methods of the second type were eventually used to estimate it.

During the 1950s, much effort was invested in finding good analytical or numerical approximation for the phase space integral. Moreover, the question remained whether Fermi’s key hypothesis of an equal distribution of probability among all allowed outcomes was correct, at least in a first approximation. Indeed, it soon became clear that that assumption was in general incorrect, and various authors proposed improvements to the model, for example by taking into account the possibility of resonances making some outcomes more probable than others (Kretzschmar 1961; Milburn 1955). In the U.S.A. the statistical model mostly attracted the attention of experimenters, and in a review paper on the subject written in 1955 Richard Milburn noted that more had been written about the model in internal reports of experimental laboratories than in published research papers (Milburn 1955). Milburn listed various improvements and modifications to the model and also mentioned two attempts at numerically estimating its predictions with the help of electronic computers (Block 1956; Yang & Christian 1953). As far as can be reconstructed, these computations employed traditional methods of numerical integration, and not the Monte Carlo method. The Fermi statistical model was also used in the late 1950s at the Berkeley Laboratory by the research group of Luis Alvarez, where eventually program FAKE would be developed.

While in the U.S.A. the statistical model was mainly considered as a simple approximation useful to experimenters, in the Soviet Union and in Eu-

rope some authors came to regard it as capable of providing a guideline for physically understanding multiple particle production at high energies, by including resonances and other effects for example (Belen'ki et al. 1958; Hagedorn 2016: 149–152; Kretschmer 1961). In short, the Fermi statistical model was no straightforward physical approximation, but could be seen both as a physically problematic yet useful computational tool and as an innovative theoretical approach.

The Fermi Statistical Model and the Monte Carlo “Analogue Idea” I: Gercen I. Kopylov

Let us now turn to the numerical methods employed to estimate the phase-space integrals needed for applying the statistical model, because in that context some researchers in the Soviet Union and in Europe started using the second type of Monte Carlo computations. In 1958 Gercen Isaevich Kopylov from the Joint Institute for Nuclear Research in Dubna proposed a method for estimating the rates of different outcomes of particle collisions by taking a modification of the Fermi statistical model in which the effect of resonances had been introduced as a starting point (Kopylov 1958).⁵ He explained that, for each collision of two specific particles with given energies and other properties, one could construct a table in which each possible outcome of the collision appeared with a frequency proportional to the probability assigned to that outcome by the (modified) Fermi model. Kopylov's table offered a statistical sampling of phase-space volume, with eventual additional resonance effects. To compute the average value of some quantity after the collision, one had to average over all relevant elements of the table. Since each outcome of the collision would appear in detectors as a star with many prongs, Kopylov described his idea as the construction of “a table of random stars” (Kopylov 1958: 996). He presented the table of random stars as a kind of substitute for experimental results:

The table of random stars should contain a series of entries each of which represents one case of particle production, i.e., contains the magnitudes and directions of the individual secondary-particle momenta. The statistical analysis of such a table will give the same kind of information on the multiple-production process (statistical weights, momentum distributions, angles, charges, etc.) as will the analysis of actual stars in photographic emulsions or chambers. (Kopylov 1958: 996)

Here it is important to note that the table did not simulate the steps of a stochastic process, but simply presented the possible outcomes of a given collision according to a pre-determined probability distribution. We see here, as anticipated, an interference of the different types of Monte Carlos: the sampling of random points in phase space could be interpreted as (at least in some sense) analogous to producing data which mimicked experimental results. However, this was a very different kind of ‘simulation’ than the one performed when employing a stochastic model of a physical process like neutron diffusion, where the steps in the code could be seen as mirroring the motion of diffusing particles. Finally, a peculiar aspect of Kopylov’s paper was the fact that, not having a computer at his disposal, he could only propose the method without being able to carry out the numerical computation involved. Despite its lack of implementation, Kopylov developed his model further, using a new Lorentz-covariant formulation of the phase space integral found by Prem Prakash Srivastava and George Sudharshan (Kopylov 1961; Srivastava and Sudharshan 1958). His work was often quoted in papers on particle physics’ Monte Carlo in the 1960s and also featured in James’s CERN lectures on Monte Carlo phase-space computation (James 1968: 21).

The Fermi Statistical Model and the Monte Carlo “Analogue Idea” II: Frans Cerulus and Rolf Hagedorn

A key contribution to the development of Monte Carlo methods of the second type for computing the phase space integral in the Fermi statistical model was provided by Frans Cerulus and Rolf Hagedorn when they were working at the Theory Division of CERN in the years 1958–1962 (Cerulus, private communication; Hagedorn 1992; Hagedorn 2016: 151). In 1957 the head of the CERN Theory Division Bruno Ferretti asked Cerulus and Hagedorn to use the Fermi statistical model for estimating the number and energies of the particles that would be produced in a planned supplementary beam of the CERN’s main accelerator.

The two researchers soon realized the technical difficulties of estimating the relevant phase space integral for their problem, and eventually Cerulus had the idea of employing the Monte Carlo method for that aim (Cerulus, private communication; Hagedorn 1992: 4). Like most particle physicists at the time, Cerulus had never used the Monte Carlo method before but he recalls having had some knowledge of it from an article in a manual of *Modern Mathematics for Engineers* (Brown 1956). Since CERN at the time had no computing machine capable of handling such complex tasks, Hage-

dorn and Cerulus turned to the Institute of Applied Mathematics at the Technische Hochschule Darmstadt, where an IBM 650 was in use (Cerulus & Hagedorn 1958a). The first results of their work were published in 1958 (Cerulus & Hagedorn 1958a; Cerulus & Hagedorn 1958b). These first publications were specifically devoted to the formal problem of phase space integration, and the authors explicitly stated: “We do not consider here the physical side of the problem, i.e. the validity of this type of theory [the Fermi statistical model]. We only try to solve the mathematical problem of calculating the phase space integral, which has not been achieved in a simple and reliable manner so far” (Cerulus & Hagedorn 1958a: 646).

Cerulus and Hagedorn first analytically manipulated the integral to put it into a recursive form fitting for numerical computation, and then explained how to sample the distribution in an effective way, stating that the Monte Carlo approach converged faster than traditional numerical integrations. Although the process of generating random points in phase space for computing the integral was introduced primarily as a mathematical procedure, the authors noted how it automatically provided a set of points whose distribution of energy values (the “energy spectrum”) corresponded to the one expected for the products of particle collision: “Another advantage of this M[onte] C[arlo] method for solving the phase space problem is that by the very nature of the computation process one gets easily without further calculations the energy spectrum for each kind of particle considered, more accurately: that part of the spectrum which is determined by phase space considerations.” (Cerulus & Hagedorn 1958a: 651). Kopylov made the same observation, which led him to describe his Monte Carlo as the construction of a “table of random stars”, and we can notice how Cerulus and Hagedorn referred to their procedure as a “calculation” and not as a simulation. In fact, neither they nor Kopylov employed the term simulation at this stage.

In their first publications Cerulus and Hagedorn did not consider effects of resonances, but they did so in later papers and reports. At the same time, the computational aspects of the research, were also significantly improved, as in the meantime CERN had acquired its first mainframe computer: a Ferranti Mercury (Hagedorn 1959). Cerulus later developed Monte Carlo methods to estimate the possible effect of a pion-pion resonance on multiple production of pions in processes of proton-antiproton annihilation (Cerulus 1959; Hagedorn 1992: 7–8). We shall come back to this work presently, as it provides a possible connection between the Monte Carlo activities at CERN and later work at Berkeley. Given how much Cerulus and Hagedorn had achieved with Monte Carlo methods at CERN around 1960, one might think that their programs would constitute the starting point for later CERN Monte Carlo packages, but this was not

the case. Instead, Berkeley's program FAKE eventually became the core of CERN Monte Carlo tools for data analysis. A reason for this development was apparently that Cerulus and Hagedorn were not using the relativistic expression of the phase space integral developed by Srivastava and Sudharshan (1958), the employment of which would have enhanced computing efficiency (James 1968: 21–22). However, further factors may have been both the fact that their programs were written for the Ferranti Mercury, which was soon replaced at CERN by IBM computers of type 704 and 709 (programmable in Fortran), and that their computations did not seem to have raised the interest of CERN experimenters, while the Monte Carlo programs in Berkeley, as we shall see, were developed in close contact with experimental activities.

The Fermi Statistical Model and Berkeley Experimentalists

As already noted, U.S. American physicists often referred to the Fermi statistical model in the internal reports of experimental groups, and for our theme it is interesting to follow its uses at the Berkeley Laboratory, where the program FAKE would eventually be developed. There, in the late 1950s various experiments were being performed under the guidance of Luis Alvarez using a recently developed kind of detector: the bubble chamber (Galison 1997: 313–431). In this context, computer-aided methods had been developed to reconstruct events from bubble chamber photographs, but so far, no Monte Carlo computations had been used to do so. One main line of research in Alvarez' group was the study of high energy collisions between protons and their antiparticles, the antiprotons, whose mutual annihilation could lead to the creation of a relatively large number of particles (Solmitz 1960). The internal reports of the Berkeley physics division show how the predictions of the Fermi statistical model were compared with experimental results. For example, Theodore Kalogeropoulos' PhD thesis on "A study of the antiproton annihilation process on complex nuclei" (1959) described experimental results on antiproton annihilation and compared them to the predictions afforded by the most recent versions of the Fermi statistical model, stating: "It has become customary to compare experimental results with the modified Fermi statistical theory" (Kalogeropoulos 1959: 49). A Berkeley colleague, Bipin Dosai, had provided him with new phase-space formulas based on the Lorentz-covariant approximation mentioned above (Desai 1960: 1390), and the same expressions were used by Tchang Fong Hoang and Jonathan Young, who in a report from January 1960 described a program for computing the phase-space integral needed

to estimate with the statistical model the production of four to six particles in proton-antiproton annihilation on an IBM 650 (Hoang & Young 1960). However, Hoang and Young did not use the Monte Carlo method to compute the integral, but rather a traditional step-by-step integration procedure.

In April of the same year Gerald Lynch, who would become the author of FAKE, wrote an internal report on “Statistical model calculations of antinucleon annihilation into pions” (1960) and his results were presented, together with those of other colleagues, at the 1960 Rochester Conference by Frank Solmitz (Solmitz 1960). The annual Rochester conferences were the main meeting point for the high energy physics community, and it was usual for laboratory representatives to present overviews of recent results at their institutions. Solmitz discussed “Antiproton experiments in a Hydrogen bubble chamber” and section 4 of his paper dealt with “Comparison of high-energy antiproton annihilation with statistical model predictions” (Solmitz 1960: 168–170). Solmitz also referred to the predictions on proton-antiproton annihilation which had been obtained by Cerulus with a Monte Carlo computation of the modified statistical model (Cerulus 1959). It is interesting to speculate how far the idea of using Monte Carlo computations for estimating phase space integrals at Berkeley might have been in part inspired by that example, since up to this point Monte Carlo computations had apparently played no role in the activities of Berkeley’s experimental groups.

“The First Monte Carlo Simulation Program in Particle Physics”

Beside proton-antiproton annihilations, other studies were being performed at Berkeley under the guidance of Alvarez with the new bubble chamber. As the brief overview of particle physics in the 1950s showed, theorists had been developing various proposals on how to classify old and new particles into multiplets, and the scheme by Gell-Mann and Nishijima had been the most successful. Among the experiments in Berkeley was one in which the last missing element of that scheme was observed: the Ξ_0 particle. In the course of that experiment, many data on the interactions of a beam of K-mesons with protons had been collected, and team members started wondering if further interesting physics might be extracted from those data. Among these physicists were two young doctoral students: Stanley Wojcicki and Bill Graziano. In his recollection of that period, Wojcicki explains how the group had reacted to the discovery of the Ξ_0 :

This was a very important particle discovery [...]. But all [the group] had to show was one example, albeit the first, of the Ξ_0 particle. The natural question was, What else could we get out of the film? Or, more pragmatically, What could we two graduate students do for our theses? The problem of extracting more useful physics out of the films was frequently discussed with resident and visiting theorists. (Wojcicki 1987: 166)

According to Wojcicki, local theorists' suggestions were not particularly helpful. It was via a talk given by Bud Good, an experimental physicist and former Berkeley colleague, that he and Graziano became aware of a new, broader classification scheme which Gell-Mann had recently proposed under the name of "global symmetry" (Wojcicki 1987: 166–167). In this model, the classificatory multiplets combined known particles with a number of still largely hypothetical resonances, somehow implying, in effect, that resonances should be regarded as particles, an idea not shared by all theorists at the time. The global-symmetry model suggested the existence of a resonance in the energy range explored when looking for the Ξ_0 particle. To test this hypothesis, Wojcicki and Graziano started a new analysis of the tracks that had already been collected (Wojcicki 1987: 166–167). The signature for the new resonance was expected to be a peak of frequency in the combined creation of two particles, which in this case were a pion (π) and a Λ particle (Wojcicki 1987: 167–168). This meant that Wojcicki and Graziano had to count, for various energies, how often a Λ particle and a pion were co-produced. Moreover, an additional pion was expected to always accompany the resonance, although it did not participate in it, so that in the end the events of interest contained two pions and one Λ particle in the final state. The first analysis showed that for energies of about 400 MeV such events happened more often than for other energies, yet Graziano and Wojcicki worried that not all the events they had counted were actually what they seemed to be, since a Σ_0 particle might at times have been misinterpreted as a Λ . It was at this point that they came to the idea of performing a Monte Carlo computation:

About a month before the [1960 Rochester] Conference our data sample was almost completely analyzed, and about 150 examples of $\Lambda\pi\pi$ events were found. That particular data set was quite pure [...] with possibly a small contamination due to the $\Sigma_0\pi\pi$ events, which were kinematically quite similar. We worried for a while about this contamination and tried to estimate it in a quantitative way. We decided that the only way to do this was to generate random $\Sigma_0\pi\pi$ events and see how often their kinematic would look like $\Lambda\pi\pi$. And thus Bill Graziano

and I embarked in what was probably the first Monte Carlo simulation program in particle physics. (Wojcicki 1987: 168)

Of course, we know that these two PhD students were not the first ones to use Monte Carlos in particle physics, but there is no reason to doubt that they actually used the method on their own initiative, as Wojcicki also explains in detail how they “had one of the scanners laboriously keypunch” the entries of a book of random numbers into an IBM 704 (Wojcicki 1987: 168). Wojcicki also recalls that one reason why he and Graziano were able to perform the computation at all was that the IBM 704 could be programmed in Fortran, while its predecessor, the IBM 650, could only be coded in machine language (Wojcicki, private communication). The result of the test confirmed that contamination was minimal, and Wojcicki and Graziano convinced Alvarez that their data on the new resonance, eventually called Y^* , should be presented at the upcoming Rochester Conference, the same one in which Solmitz would give his talk on proton-antiproton annihilation. No mention of Monte Carlos was made in the internal reports and published papers on the Y^* resonance, and little or no trace remains of Wojcicki and Graziano’s Monte Carlo. Wojcicki recalls it as having been a rather simple program tailored to their needs: “I think we used the 3 body invariant phase space (that could be obtained from existing analytical programs) and then allowed Σ_0 to randomly decay. The points used from the phase space were chosen by Monte Carlo” (Wojcicki, private communication).

Nonetheless, Wojcicki suggests in his recollections that their program provided at least in part the inspiration for FAKE: “A few months later, Gerry Lynch embarked on creating a much more sophisticated and general program along these lines, called FAKE” (Wojcicki 1987: 168). Before turning to program FAKE, and thus to the last act of our story, I would like to add a few remarks on the way in which the story I just told was represented by Galison in *Image and Logic* (Galison 1997: 412–413, 747–748). Galison presented the events described by Wojcicki as an example of how “[u]sing known or established physics, physicists would simulate the data that would emerge from a particular experimental setup” (Galison 1997: 748). Yet, as we saw, Wojcicki and Graziano were not simulating known physics, since at the time there was no theory predicting how many events of one type or another would occur. They were using the widely known, but nonetheless approximate and speculative Fermi statistical model to test how often certain events might be mistaken for the phenomenon they were interested in, and they did so because the special circumstances they were working under required it. It was no routine procedure. Moreover, Galison did not discuss the fact that, according to Wojcicki’s recollections,

though a text quoted by Galison, the two young researchers had resorted to a Monte Carlo because they were searching for a resonance predicted by Gell-Mann's global symmetry scheme. Instead, Galison presented the discovery of the resonance as a theory-independent development, closely and somehow uncritically following the representation given by Alvarez in his Nobel Prize Lecture (Alvarez 1972: 277–279). Alvarez wrote: “But when Wojcicki and Graziano finished transcribing their data from KICK printout into histograms, they found that phase-space distributions were poor approximations to what they observed” (Alvarez 1972: 279), suggesting, though not actually stating, that they had expected no peak. Galison picks up and even makes more explicit Alvarez' suggestion that the appearance of the peak was surprising: “Intriguingly, when Wojcicki and Graziano plotted the energy of the final-state positive and negative pions, each displayed a peak at about 285 MeV” (Galison 1997: 412). Yet we know both from Wojcicki's recollection and from internal reports that Graziano and Wojcicki were actively looking for a resonance peak for that final state configuration, and therefore could hardly have been surprised by it. In this way, the role of theoretical assumptions in the story remains hidden and the full complexity of the situation cannot be appreciated. At the same time, the Monte Carlo method is presented as a simulation of experiment, a view which does not fit the historical picture.

Program FAKE

At the 1960 Rochester Conference, researchers from Berkeley presented both the new Y^* resonance and data on proton-antiproton annihilation. In the following months, both results were studied in more detail and, at this point, Monte Carlo computations started to take center stage. Although, as already noted, contemporary sources make no mention of Wojcicki and Graziano's Monte Carlo, there is reason to believe that it indeed provided inspiration for FAKE, probably along with the work by Cerulus on pion-pion resonance. As we recall, Graziano and Wojcicki had built their Monte Carlo in the attempt to estimate contamination in the search for a resonance, and Lynch faced a similar problem when looking for the pion-pion resonance in proton-antiproton annihilation as already studied by Cerulus (Lynch 1961).

In a 1961 paper on “Final states of the proton-antiproton system”, Lynch described the search for the pion-pion resonance and stated that he had used a Monte Carlo computation to check how far a state of two charged pions could be “faked” by one in which they were accompanied by a third,

neutral pion which was invisible to the detectors: “In order to obtain a better understanding of how often an event can ‘fake’ the two-body annihilations, 1,000 $\pi\pi\pi_0$ events were generated by a Monte Carlo program which chose events distributed uniformly in phase space” (Lynch 1960a: 5).

This procedure was nothing but a Monte Carlo computation of the phase space integral necessary for predicting, along with the Fermi statistical model, the production frequency of three pions. However, at this point Lynch did not present the computation as a theoretical hypothesis to be tested, as had been done in previous publications, but rather as a means to estimate the background for his analysis. This is perhaps not surprising, since there was no way to test the validity of the statistical model in this context, one rather had to assume it in order to estimate the background. This situation was due to the peculiar character of resonances, which is worth underscoring here once more. To search for a particle leaving tracks in a detector, one looks at many track photographs until a clear picture of its creation and decay is found, the so-called “golden event”. Whereas resonances only manifest themselves at the statistical level, in the relative frequency of a given type of event: there is no ‘golden event’ for a resonance.⁶ Due to the probabilistic nature of elementary particle interactions, however, when performing the same experiment over and over again, all events which are not forbidden will take place, and so it is imperative to be sure that one is counting the right kind of events. If events of other kinds are mistakenly counted, the data is contaminated by “fakes”, and the overall statistics are not reliable. This was the problem faced by Graziano and Wojcicki, and by Lynch when searching for resonances. The only theoretical means physicists had at their disposal for estimating “fake” events was the Fermi statistical model whose predictions, as Cerulus, Hagedorn and Kopylov had shown, could be most efficiently obtained by a Monte Carlo integration over relevant phase space.

In early 1961 this situation eventually led to the realization of FAKE: a flexible Fortran program for computing space-phase integrals with or without resonance effects. With such a program, it was possible to compare the statistics of experimental results with the estimates made on the basis of a pure phase-space distribution (the original Fermi model) or of one which included additional resonance effects. Program FAKE was thus a more refined, flexible version of the codes by Kopylov and by Cerulus and Hagedorn. However, the way in which the program was presented was in some aspects new. The title of the relevant manual written by Lynch in 1962 was “Program FAKE: Monte Carlo simulation of bubble chamber events” (Lynch 1962a), and Lynch wrote:

Program FAKE is a Fortran-Monte Carlo program that generates events in a bubble chamber according to prescription and simulates the output of the PANG track-reconstruction program. Descriptions are given of the method used to generate events that are distributed in accordance with a phase-space distribution, and the method used to modify the measurable quantities to simulate the effects of measurement errors and multiple Coulomb-scattering. (Lynch 1962a: v)

In the FAKE manual, the Monte Carlo computation of the phase space integral was described in different terms from those employed by Cerulus and Hagedorn or Kopylov: FAKE “generates events” and “simulates” events, measurements, and errors. Moreover, the Fermi statistical model was not mentioned at all, although it continued to provide the conceptual basis for the program. Despite the change in terminology and the lack of references to the statistical model, Lynch never claimed to simulate physical processes or experiments: FAKE generated events and then used them as input for a track recognition program in order to “simulate” the latter’s output. The program also added random modifications to the parameters of the generated events to “simulate” measurement errors. The term ‘simulation’ was used here to describe something that looks like something else, and not to indicate some deeper analogy: the program was characterized as “simulating” experimental results, and not experiments or the physical processes involved in them. Lynch explained that the name of the program was chosen precisely to describe its function: “Thus the FAKE program merits its name for two reasons: (a) it fakes PANG [track recognition] output and (b) it is designed to study those cases in which one type of event is faked by another” (Lynch 1962a: 2).

Although the term “fake” was not yet so negatively loaded as it is today, it nonetheless expressed a fundamental gap between appearance and reality, and in the report the terms “fake” and “simulate” are used interchangeably. Lynch then went on to explain that, although FAKE’s main use was to generate fake events on the basis of pure space phase distribution, one might also use it to generate events according to some other “assumption” and compare these results with an experiment: “Because FAKE operates on an event-by-event basis it is able to produce data with the experimental resolution folded in a much more realistic way than can be done with other methods. Therefore it can be useful to use FAKE to see how experimental distributions corresponding to particular assumptions should look” (Lynch 1962a: 2–3). Thus, when Lynch used FAKE for hypothesis testing, he regarded this as a different function than employing it to estimate data contamination. If the theoretical “assumptions” used in the Monte Carlo were hypotheses to be tested, the code appeared as a computational tool

to generate numerical predictions for comparison with experiments. If, on the other hand, its output was used as (untested) input in data analysis, then the theoretical assumption behind it, the Fermi statistical model, was seen as a “prescription” implicitly assumed to be valid. From the point of view of the code, there was however no distinction between the two cases, which would both be regarded as computer simulations today. Towards the end of this paper I will discuss this epistemic shift more in detail.

Although during the 1960s Monte Carlo event generators became essential tools for data analysis, they were rarely mentioned in physics research papers and were usually described only in internal reports of experimental groups, or in scientific instrumentation journals (Chandler and Tilger 1967; Bordner et al. 1966). Alfred Brenner, one of the authors of a Monte Carlo program NVERTX, written at Harvard for particle physics research, recalls: “I had great difficulty in publishing a paper (Bordner et al. 1966) in 1965 on the Monte Carlo generation of elementary particle events. No physics journal considered computation to be in its province. After much discussion, the *Review of Scientific Instruments* reluctantly agreed to publish the paper” (Brenner 1996: 28).

Yet Monte Carlo event generators had come to stay in particle physics. In 1964 Lynch spent a period at CERN, where he wrote a modified version of FAKE, named OWL, which is also described only in a CERN internal report (Lynch 1964). Lynch acknowledged the help of a young German PhD student in developing a new algorithm for faster phase space computation: Eckhard Raubold, who would go on to become the director of the DESY computing center (Reimer & Rihaczek 2010). Both FAKE and the Lynch-Raubold algorithm would become central elements of James’ 1968 CERN lectures on phase space Monte Carlos for particle physicists, and the Lynch-Raubold algorithm is still part of the core of today’s Monte Carlo event generators (Alves & Sokoloff 2017).

From Resonance to Particles: Monte Carlo Computations as a New Medium of Theory

During the 1960s physicists increasingly often employed FAKE and other Monte Carlo event generators in the search for further particle resonances (Pickering 1984). Monte Carlos still served to estimate the background of “fake” events and for testing detectors, but they were also used for seeing how given experimental results would look if a resonance with certain characteristics was present. This was done by generating events according to a distribution containing a resonance peak (the Breit-Wigner formula)

at a certain energy value, and then comparing the outcome with experimental data. In a scientific context where statistical effects were the signature for phenomena, Monte Carlo provided a method for expressing and manipulating theoretical hypotheses in a way which was not possible when using only analytical expressions. Monte Carlo contained an element which could not, at least in principle, be represented by any analytical formula: random numbers. In practice, of course, the numbers were not really random, but were so-called “pseudo-random”, that is numbers computed according to some analytical procedure (Galison 1997: 701–709). However, this fact made no difference in the practice of particle physics.

The epistemic implications of Monte Carlo event generators may be seen in the way in which the notion of particle resonance was rapidly reshaped in the early 1960s. Although theorists still could not agree on the nature of resonances (and, indeed, not even of particles), experimenters could simply define both of them in terms of events and events distributions, bypassing all further theoretical issues. In this way, the Breit-Wigner formula for a peak in event frequency was no longer simply the observable signature of a resonance, but also became its operational definition (Jackson 1964: 1647). In his 1968 CERN lectures James wrote: “By a resonance I mean any Breit-Wigner function of an invariant mass”, and went on to describe how to maximize the efficiency of a Monte Carlo for “the production of a narrow resonance” (James 1968: 33). In the 1960s, thanks to Monte Carlo event generators, experimenters could search for resonances like they had searched for particles in the 1950s, and in this context, resonances eventually came to be regarded as though they were particles. Already in a 1962 review paper on newly discovered resonances, Lynch wrote: “Actually, most of the [...] pion-nucleon resonances [...] were well known eighteen months ago. The fact that they appear here along with the longer-lived particles reflects more a change in outlook by high-energy physicists than an increase in experimental knowledge” (Lynch 1962b: 46). In his 1968 Nobel Lecture, Alvarez attributed this change of outlook to the Y^* resonance: “the impact of the Y^* resonance on the thinking of particle physicists was quite different—the Y^* *really acted like a new particle*, and not simply as a resonance in a cross section” (Alvarez 1972: 279, emphasis in the original).

By 1963, the catalogue of particle properties regularly issued by the Berkeley Laboratory contained tables both of “stable particles” and of “resonances”, with the latter now regarded de facto as very short-lived particles. The change of attitude towards resonances must also be seen in the context of discussions about the status of both particles and resonances. At the time, this was still a hotly disputed issue among theorists, especially due to the rise of S-matrix theory and to Geoffrey Chew’s claim that it im-

plied that no really elementary particles existed (Pickering 1984: 73–79). Of course, other factors, too, contributed to the assimilation of resonances to particles, especially classification schemes representing both of them in the same way (Borrelli 2020). However, the fact that Monte Carlo event generators could be seen as operationalizing the notions of both particle and resonance in the context of experimental researches provided a fundamental, and so far barely acknowledged element in this constellation: Monte Carlo computations had come to mediate theoretical concepts, like symbolic mathematical formulas had been doing for a very long time. It is worth shortly pausing to reflect on this idea.

Today, physical-mathematical expressions like Newton's gravitation or Schrödinger's equation, as well as other symbolic forms such as Feynman diagrams, are often, implicitly or explicitly, regarded by physicists not just as means to produce numerical predictions, but also as in some way allowing one to grasp fundamental natural structures. I suggest, at least for the purposes of the present paper, to speak in the first case of a "tool" of theory, and in the second one of a "medium" through which physicists believe they may access more fundamental aspects of the workings of nature (Borrelli 2017b). Clearly, the distinction between what I call here "tool" and "medium" is linked to the perception of a specific, smaller or larger (scientific) community. In this paper I have suggested that, like formulas, Monte Carlo computations could function not only as a tool but also as a medium of physical theory. More specifically, in the 1960s, Monte Carlo event generators came to be perceived as a medium to better grasp the phenomena known as resonances in particle physics, and so helped assimilate them to particles. At the same time, the notion of particle was also modified, and became less closely linked with detector tracks. Epistemic shifts of this kind in the case of instruments, formulas or images have often been noted by historians and philosophers of science and by scholars from media and cultural studies, but so far computer-aided methods, among them computer simulations, have only rarely been considered from this perspective.⁷

The present case study provides a small, but significant example of how important it is to take into account how such epistemic shifts may have accompanied the growing use of computing methods, and especially those referred to today as 'simulations' in various areas of science and technology.

I believe that, in the particle physics of the 1960s, Monte Carlo event generators played a role similar to that of the new symbolic mathematical formalisms of Newton and Leibniz in the emergence of the mathematical analysis of motion, or by infinite matrices and Dirac's bra-kets in the construction of quantum mechanics (Borrelli 2005; Borrelli 2017b). In those cases, the appearance of a new means to represent and manipulate ex-

perience in (at the time) non-rigorous mathematical ways eventually led to a reconceptualization of knowledge elements and to the emergence of new physical-mathematical concepts, such as acceleration, force, or quantum probability. Together with media theorist Marshall McLuhan we may note that, in those cases, the “medium” becomes in some sense transparent, naturalized and is perceived as directly expressing features of nature (McLuhan 1965). However, the path of Monte Carlo event generators to becoming transparent media of theoretical knowledge—and eventually also a kind of artificial reality—was quite slow, as the next section will discuss.

Monte Carlo Event Generators Between Fake and Simulation

The reconstruction pursued in the previous sections has shown how the use of Monte Carlo computations to “generate events” in the particle physics of the late 1950s and early 1960s was not generally regarded by historical actors as the creation of an “artificial reality”. The few uses in particle physics of the first type of Monte Carlo could at times be described as mirroring physical processes. However, this was not the case for Monte Carlo computations of the second type, that is those based on the “analogue idea” of numerically estimating non-probabilistic expressions by means of stochastic processes. The use of the Monte Carlo method to compute the phase space integral for the Fermi statistical model of particle creation clearly fell into this second category. Not even when such Monte Carlo programs were characterized as “generating events” were they regarded as performing computational steps analogous to some physical process. The code was rather seen as a theoretical tool providing numerical estimates which could not be obtained analytically.

In describing program FAKE the term ‘simulation’ was occasionally employed, but a closer look at the context has shown how this usage was different from that which is common today, when talk of ‘computer simulations’ generally suggests that what goes on when the code runs has some kind of analogy with (real or hypothetical) physical phenomena. Today, when Monte Carlo event generators in particle physics are referred to as computer simulations, this term may indeed be seen as communicating the sense of an “artificial reality”. In 1962, when Lynch described FAKE as “simulating” bubble chamber events and detector output, he was expressing the quite different idea of “faking”: producing results which superficially looked like the outcomes of experiment. There was no suggestion of an analogy between the code and physical processes, as there might be when using Monte Carlos of the first type for estimating neutron diffusion. Lynch

spoke on the one hand of estimates of “fake” background events, on the other of tests of “assumptions”.

Nonetheless, the first hints of a change were there. Lynch presented the events generated by using a uniform probability distribution in phase-space as a straightforward, unproblematic means of estimating the frequency of fake events, and did not mention the fact that this “prescription” was also based on a theoretical assumption, namely the Fermi statistical model. Before this, the Fermi model was always explicitly mentioned as the theoretical basis for the use of the phase space integral, but this reference had disappeared precisely at the moment when that integral had been transferred from symbolic formulas to a Monte Carlo program and used to estimate and subtract background in data analysis. Nothing had changed in data analysis apart from the fact that the numerical estimate for a given analytical expression (the phase space integral) was now obtained by a rather refined Monte Carlo method. However, that estimate was not presented anymore as something linked to a theoretical model but simply as “generated” by the program. At the same time, though, this attitude only extended to the use of the statistical model for estimating data contamination. Computations based on other “assumptions” were still explicitly described as attempts to see what the corresponding experimental results “should look like” (Lynch 1962a: 3). In this second case, terms like “simulating” or “faking” were not used.

I regard this distinction as very important, it implies that at this early stage, a Monte Carlo event generator was regarded as a simulation only in specific cases and not in general. The fact that output was produced which “looked like” experimental results was not in itself a reason to speak of a “simulation”. This suggests that, at least in particle physics, no general notion of computer simulation as an “artificial reality” that mimicked natural processes existed. Without a detailed study of later sources, it is not possible to say when exactly that notion emerged, but as a general indication it is worth noting that, in his 1968 CERN lectures on phase space Monte Carlo, James explicitly dwelled upon this point in a section entitled “Integration versus direct simulation” (James 1968: 27). Like Kopylov, Cerulus and Hagedorn had done before him, James pointed out that integrating over phase space by means of a Monte Carlo event generator automatically provided an estimate for the distribution of events with respect to energy or some other physical parameter, and added: “It should therefore be clear that the end result of all our phase space calculations is the value of an integral or a set of integrals. However, we do this by generating points in phase space, or ‘events’, and so we may alternatively consider that we are directly simulating an experiment” (James 1968: 28).

The possible change of perspective that led James to regard the performance of an integration as a “direct simulation” of an experiment was not present in the early 1960s, and as late as 1968 James explicitly noted both perspectives, which may indicate that in his time, and in this case, the modern notion of simulation was not yet fully established. Indeed, in the introduction to his 1980 review article on “Monte Carlo theory and practice” James subsumed both cases under the heading “Simulation,” yet still distinguished between the two kinds of Monte Carlo formulation:

Whether or not the Monte Carlo method can be applied to a given problem does not depend on the stochastic nature of the system being studied, but only on our ability to formulate the problem in such a way that random numbers may be used to obtain the solution. [...] The problem to be solved may already be of a probabilistic or statistical nature, in which case its Monte Carlo formulation will usually be a straightforward simulation, or it may be of a deterministic or analytic nature, in which case an appropriate Monte Carlo formulation may require some imagination and may appear contrived or artificial. (James 1980: 1147)

However, by the 1970s regarding a Monte Carlo event generator as a simulation of experiment had become orthodox. The user guide and manual for CERN event generator GEANT (“Generation of Events AND Tracks”) from the late 1970s described it as a “simulation program for particle physics experiments” (Brun et al. 1978). GEANT could perform a broad range of Monte Carlo event generations and at the same time show how the generated events would look in different kinds of detectors. Different layers of ‘simulation’ were mixed with each other, this was the stated goal of the code:

In the past few years the fast development of counter experiments required the design of a more and more complex chain of programs characterized by the following tasks—event simulation, pattern recognition, momentum evaluation and vertex fitting. [...] The aim of the program GEANT is to cover a wide variety of particle detectors for the Generation of Events AND Tracks, the first link of any high energy physics data analysis. (Brun et al. 1978: 1)

Here we note analogies to the case of Building Information Modelling discussed by Nathalie Bredella in this Special Issue (Bredella 2019): from the last decades of the twentieth century onward, design software for architecture developed into a composite, hybrid tool comprising different aspects and stages of the design and construction process in which different kinds of visualization also played an important role. In particle physics,

at this stage, Monte Carlos could be regarded as simulating experiments, and not just experimental results. Suggesting an analogy between running an event generator and performing an experiment might today seem like a rather straightforward move, but it is not. The output of a Monte Carlo event generator is not obtained by performing a stochastic process with a computer more or less faithfully mirroring a time-based physical phenomenon like neutron diffusion. As we have seen in the previous pages, Monte Carlo event generators in particle physics use random numbers to directly generate the final configuration according to a predefined probability distribution. There is no time-based stochastic procedure which might be seen as corresponding to a physical process. Regarding the event generation as analogous to performing experiments would seem to suggest that the probabilistic element of the Monte Carlo computation mirrors the intrinsically probabilistic nature of quantum field theory. This view is in no way straightforward, since the randomness of Monte Carlo is based on classical probability, and not on the quantum probability which is assumed to rule microphysical phenomena.

It is not my aim to discuss philosophical issues like the tension between classical and quantum probability here, but rather to underscore how easily the change of perspective implicit in regarding Monte Carlo event generators as “simulations of experiment” managed to eschew those tensions. To fully understand the premises and implications of this development a broader study of its cultural and historical context may be necessary, but I believe that the present investigation offers a significant contribution to grasp the many facets of the historical-epistemological constellation and to pose broader, relevant questions. Of particular interest will be to compare and contrast the developments in High Energy Physics with those in other branches of science and technology. An interesting perspective may be gained by considering the case study contributed by Sibylle Anderl to this Special Issue, which concerns epistemic changes in the interpretation of a computational model in astrophysics during the last decades of the twentieth century and the beginning of the twenty-first century: the Paris-Durham model for interstellar shock waves (Anderl 2019). Although the code itself remained largely stable during a long period of time, scientists first employed it as a theoretical means of gaining qualitative understanding of certain astrophysical phenomena, and later as a computational tool to produce “synthetic data” to be qualitatively and quantitatively compared with observations. As in the case of Monte Carlos in High Energy Physics, that shift, too, was linked to the increased availability of new measurements, and one may assume that this fact moved the epistemic focus of the code from the explanation of the possibility of given phenomena to the production of “synthetic” or “simulated” data to be compared to

measurement. Both the Paris-Durham shock code and Monte Carlo event generators are today described as simulations, yet it is worth noting a key difference between them. While the astrophysical code can be seen, at least in principle, as numerically implementing various physical and chemical processes taking place in interstellar space, no such claim can be made for event generators in High Energy Physics, which simply produce events according to given frequency distributions, but their functioning does not mimic in any way particle interactions. This comparison highlights how nuanced the realities of computer simulations are in scientific practice, even when only regarding two different branches of physics.

Summary and Concluding Remarks

Today, Monte Carlo event generators in high energy physics are often regarded as an “artificial reality” (Galison 1997: 689), but the path to this view was much less straightforward and unproblematic than usually assumed. In my reconstruction of how Monte Carlo computations became a fundamental component of particle physics research I have highlighted a number of historical-epistemological constellations which had so far remained unappreciated in research.

I have shown how in the 1950s and 1960s, a tentative, yet significant distinction was made between two types of Monte Carlos. On the one hand, those using probabilistic procedures to “directly model” physical processes like neutron diffusion, and on the other the “analogue idea” of numerically approximating integrals and other analytical expressions by means of ad-hoc stochastic processes performed on fast computing machines (Marshall 1956). It was the second kind of Monte Carlos that came to be widely used in particle physics after 1960. More specifically, Monte Carlo methods of the second type were used as a means to perform numerical computations for the Fermi statistical model of particle production. They were not employed to perform the steps of a stochastic process mimicking some physical system, but rather functioned as tools to extract numerical predictions from the model by estimating the phase-space integral through random sampling among all possible outcomes of a given particle collision. Since the random points in phase space corresponded to particles events, the program could also be seen as “generating events”.

The first authors using the methods did not regard the distribution of generated events as a series of virtual experiments, but rather as a theoretical prediction to be empirically tested. In 1961–62, at the Lawrence National Radiation Laboratory in Berkeley, physicists wrote the Monte

Carlo event generator FAKE to use it primarily as a means to estimate background and data contamination, and presented its results not as theory-laden numerical computations, but rather as unproblematic means to analyze data. The context of this change of attitude was the upcoming search for ‘particle resonances,’ a high energy physics phenomenon which manifested itself only at the statistical level, through peaks in the frequency distribution of events of a certain kind, and which therefore required statistical methods to be investigated. In this situation, predictions obtained with the help of the Monte Carlo method often had to be used as input for data analysis, and not as a hypothesis to be tested. As often the case with physicists, the practical success of this method in helping locate new resonances in the data quickly swept away any doubts on the validity of the hypotheses, and Monte Carlo event generators like FAKE soon came to be seen as unproblematic computational tools. At the same time, though, they were helping to reshape the conceptual landscape of particle physics, because they provided a way not simply to search for, but actually to represent and manipulate in new ways both resonances and particles. It was only at this stage that, during the late 1960s and the 1970s, Monte Carlo event generators slowly came to be seen (and described) not simply as simulations of experimental results, but rather as simulations of experiments. There is a fundamental difference between the two views, since in the second one the suggestion is implicit that, at least in some cases, the running of the code is perceived as analogous to a physical process. Here the term “artificial reality” may apply. This change of attitude was not due to any technical or physical development, but concerned the epistemic status of established practices.

On the basis of the case study I have suggested that the shift from fake to simulation was linked to the fact that in the early 1960s Monte Carlo computation took up an epistemic function in particle theory, because the introduction of Monte Carlo event generators allowed researchers to theoretically manipulate the phenomena known as particle resonances in a way which was analogous to how particles could be manipulated. When combined with other developments in high energy physics, this fact contributed to transform the notions of both resonance and particle, leading to the current view according to which resonances are particles with a very short half-life. I have tentatively characterized this change as an epistemic shift in which Monte Carlo computations were no longer regarded only as a “tool” to extract predictions from a theory but also as a new “medium” to express theoretical notions which could so far not be fully captured by analytical means. Perhaps, one might suggest that Monte Carlo simulations came to be perceived by high energy physicists as an “artificial reality” at the same time as they helped construct a new particle reality.

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Endnotes

- 1 Haigh, Priestley and Rope explicitly avoid using the term “stored program” in their historical reconstructions, and prefer to speak of “modern code paradigm” (Haigh et al. 2016: 238–257).
- 2 The Research and Development (RAND) Corporation is a private corporation founded in 1948 by the U.S. Government to provide expert advice for its Armed Forces.
- 3 Random sampling reduces bias due to correlations between integrand and sampling procedure and in many dimensions often converges faster (Brown 1956: 283–291; James 1968: 4–5).
- 4 For various views on pion-nucleon resonances, see for example (Goldberger 1956).
- 5 Kopylov was a part of the Dubna physics community, but was also involved in underground publishing activities, writing poems satirizing the Soviet academic community.
- 6 The move from visual tracks of a ‘golden event’ to statistical signatures is what Galison (1997) characterized as a shift from “image” to “logic”. However, as also suggested by (Thomas 2012), the change was due to experimental interest, and not just to aesthetic preferences or to the influence of computing.
- 7 Baird (2004); Coopmans et al. (2014); Hanson (1965); Kaiser (2005); Klein (2003); Lenoir (1998); Lynch & Woolgar (1990); McLuhan (1965); Rheinberger (1992); Rotman (1993); Wise (1988). On computer simulations as medium of theory, see Dippel and Warnke (2017).

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