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Simulation : The birth of a technology to create « evidence » in economics / La simulation : Naissance d'une technologie de la création des « indices » en économie

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Abstract

SUMMARY. — This paper situates the birth of simulation in economics within the military-industrial-social science complex of 1960 cold war America. At that date, the term referred to role-playing and computer-based experiments or to their combinations. Those economists participating in this work perceived simulation as a new epistemic technology comprised of four elements : experiments, models, games, and the computer. As a new technology, the use of simulation had to be justified to the community, where it was understood to provide economists with a microscope to investigate hitherto hidden parts of the economic world and as a means of experiment to create "evidence".

Résumé

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Simulation : The birth of a technology to create « evidence » in economics

Mary S. MORGAN (*)

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MOTS-CLÉS. — Économie ; simulation ; modèles ; ordinateurs ; Amérique de la guerre froide.

SUMMARY. — This paper situates the birth of simulation in economics within the military-industrial-social science complex of 1960 cold war America. At that date, the term referred to role-playing and computer-based experiments or to their combinations. Those economists participating in this work perceived simulation as a

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new epistemic technology comprised of four elements : experiments, models, games, and the computer. As a new technology, the use of simulation had to be justified to the community, where it was understood to provide economists with a microscope to investigate hitherto hidden parts of the economic world and as a means of experiment to create « evidence ».

KEYWORDS. — *Economics ; simulation ; models ; computer ; cold war America.*

I. — INTRODUCTION

The years around 1960 saw a sudden explosion in the use of the term « simulation » in the social sciences and statistics literatures. One of the most revealing documents of this explosion is a bibliography prepared by the economist Martin Shubik, then a consultant to the General Electric Company, on « Simulation, gaming, artificial intelligence and allied topics », published in 1960 in the house journal of the American Statistical Association (1). In that same year, there was a symposium on simulation in the *American economic review* (AER, the house journal of the American Economic Association). This contained three elements : an essay by Shubik (2) on simulation in industrial economics, Guy Orcutt's (3) first substantive report of his microsimulation studies, and a paper by Geoffrey P. E. Clarkson and Herbert A. Simon (4) reporting their attempt to programme a computer to mimic bankers' investment decisions. The practices of simulation described in these sources involve : a variety of types of « experiments » ; people playing roles (sometimes their own usual ones) ; computation machines ; probability set ups ; statistical data ; mathematical models ; and games. The term simulation obviously covered a very broad range of activities so that, apart from the notion of mimicking, or imitation, inherent in the meaning of the term, the possibilities of giving a neat definition were (and perhaps remain) small (5). Simulation, at the time, was an umbrella term.

(1) Shubik, 1960 *a*.
(2) Shubik, 1960 *b*.
(3) Orcutt, 1960.
(4) Clarkson and Simon, 1960.
(5) After 1960, the pattern of usage of the term « simulation » waxed and waned, and in settling down, its range of meaning was reduced to that of computer simulation.

I set these papers and bibliography alongside others documents of that year and the years just before and after, to explore the connotations of the term simulation in economics.

But we can, with profit, also use these documents to look back into the history of simulations in economics and trace a record stretching back into the interwar period. It seems that the roots of simulation practices in economics lie in statistics, in role playing and in modelling. And these papers show evidence not only of disciplinary beginnings and usages, but also of the political and scientific context in which this explosion of the usage of the term occurred. This is, not by intent but by content, a largely American story. The two immediate and general contexts are the cold war and the development of the computer as a research tool. But each of the particular elements covered by the simulation umbrella have their own longer roots too. For example, war games and random sampling methods each have their own separate histories ; and like a family tree, where the same first names keep re-appearing through the generations, the elements of simulation in economics often tangle together and reappear.

The literature of 1960 can also serve the cause of historical epistemology. 1960 is an apparently undistinguished moment in the history of social sciences in America. It is not a moment when the world of social science changed because « simulation » dropped into the tool box of methods, rather it is a moment when the multiple possibilities of real and quasi-experiments, and experiments with models already made evident in several different forms, seemed to coalesce together under a new and single term : simulation. The 1962 publication of a book of readings entitled *Simulation in social sciences* (6) is evidence of a topic well on the way to establishment, though the terminology is one which is still new enough that the roots of the topic remain visible, and the methods involved had to be explained carefully and justified to the reader. The literature of those years around 1960 is self-conscious in a way that earlier and later literature is not ; it sets out to explain and to justify the methods of simulation.

My investigation of this literature fits naturally into the broader epistemological enquiry about the nature of simulation undertaken by historians and philosophers of science in recent years. The notion

(6) Guetzkow, 1962.

of simulation is difficult to pin down partly because of the wide variations in its elements and practice in different fields even at around this same time. This makes problems for those seeking definitions which might travel between fields and yet still retain their meanings. For example, we have several accounts of simulations in natural sciences at about this same period. Peter Galison (7) suggests we see in simulation something which is not quite theory and not quite experiment. Evelyn Fox-Keller (8) concentrates on the thing which is being simulated in computer experiments, and points to the change in meaning of the notion from something which is false to something which imitates. In Sergio Sismondo's very nice edited volume (9) on simulation and modelling in more modern work, we find Eric Winsberg asking us to look at the many model layers required in a simulation and Deborah Dowling portraying simulation as a method of experimenting on theories.

My own particular interests in these philosophical questions about simulation are two fold : one is to examine the ways these methods produce or create « evidence », and the other is to explicate the role of models in this creation. « Evidence » will remain in quote marks, for the point about all this is that simulation methods in science are used to mimic various different parts of the subject domain. Sometimes the outputs of simulations are such that they imitate the kind of raw data or « observations » we collect as evidence in economics. Othertimes simulation produces outcomes which relate only indirectly to that level, for the elements mimicked in simulation might be the micro elements of a theory or the relations between the parts in the model. Thus, the « evidence » produced out of simulations might be evidence of the same form as the empirical evidence in the field, or it might be evidence about the characteristics of a theory or model used. With the acceptance of simulation as a standard scientific method in economics (as in many other fields), some of these imitation outcomes have themselves come to be thought of and referred to as evidence – without the quote mark warnings, *i.e.* as carrying the same status as real observations (10), though those involved know full well that such

(7) Galison, 1997.
(8) Fox-Keller, 2003.
(9) Sismondo, 1999.
(10) In some fields, these differences are more clearly labeled as « data outcomes » (from the simulations) and « data input » (inputs into the simulation).

« evidence » is produced by « artificial » means. These artificial means involve a variety of devices, particularly (but not only) models, so a sub-text throughout my history here is to look to the ways in which models get used to create « evidence ».

I begin this account by asking : What did economists mean by simulation around 1960, and how did these simulations work ? My enquiry first takes a wide-angle lens to these questions by looking at Shubik's 1960 bibliography. The second part of my enquiry takes a zoom lens to the papers in the 1960 AER symposium to understand the variety of examples of simulation and how they worked in economics at that time. This narrow focus sometimes probes further into the roots of these approaches, again to make sense of their elements. Finally (in part IV of the paper), I return to the broader questions of historical epistemology to portray simulation as a technology made up of four elements : the computer, games, models and experiments. This enables me to characterise both the role of models in simulation and the ways in which they produce « evidence ».

II. — The wide-angle lens : Shubik's 1960 bibliography on simulation and gaming

1 / *Classification and categories*

Shubik's 1960 (11) « Bibliography on simulation, gaming, artificial intelligence and allied topics » contained 5 category sections with the following labels :

1 / simulation (9 books, 3 bibliographies, 147 papers and articles) ;
2 / gaming and allied topics (4 books, 2 bibliographies, 83 papers and articles) ;
3 / Monte Carlo (2 books, 37 papers and articles) ;
4 / systems (9 books, 25 papers and articles) ;
5 / artificial intelligence and other, allied topics (1 book, 1 bibliography, 21 articles).

(11) Shubik, 1960 *a*.

As we can see, about two thirds of the items fall in the « simulation » and « gaming » categories. But, like many undertaking such exercises, Shubik clearly has difficulty in categorizing the material and placing it neatly into fields. He suggests the following definition as a way of drawing a line between the two :

> « Gaming usually (though not always) makes use of a simulated environment to study the behavior of, or to teach individuals, while simulation is directed towards studying the behavior of a system given the behavior of the individual units or *vice versa*. Gaming always involves the presence of decision-makers. Simulation does not necessarily entail the involvement of individuals. In most instances a simulation involves only the machine manipulation of a model (12). »

Shubik's attempts to make this taxonomy work are defeated both by the recalcitrance of his material and by the contemporary users of the terminology, for at least at that moment of time, simulation functioned as an umbrella term to cover both simulated environments and simulated processes. And even in his own bibliography, there are people-based experiments, *i.e.* « games » in amongst the « simulations » and machine-based research (*i.e.* simulations) in amongst the games !

The titles of works in categories 3 : Monte Carlo and 5 : artificial intelligence (AI), suggest a more defined focus. But even here, there are recognised problems. For example, Shubik began by telling us that there is no good way of distinguishing between simulation and Monte Carlo ; and looking at the entries, it appears that the bibliography includes applied examples using Monte Carlo in section 1 and technical papers on developments in Monte Carlo in section 3. The AI section seems the only self-contained, well-defined category and set of papers.

This all suggests that a considerable arbitrariness of both classification and categorization was inevitable for Shubik. And it was not just Shubik's difficulty of distinguishing one from the other, for the authors themselves did not work within these boxes. Rather, the literature surveyed suggests that those involved understood their research to be defined by a combination of research elements : models, games, experiments and computers. Nevertheless, while Shubik's five named categories : simulation, gaming, Monte Carlo, systems and artificial intelligence refer nei-

(12) Shubik, 1960 *a*, 736.

ther to separate subject topics (though AI perhaps might qualify as such), nor even to separately definable techniques, the names and categories are still important for our understanding of the historical terrain at that moment. The label, « simulation and gaming » remains a collective noun with considerable practical import and content, although these categories themselves indicate little about the subject coverage.

2 / *Subject matters*

Shubik's bibliography was published in the *Journal of the American Statistical Association,* and while statistics was certainly one of the main roots of simulation methods, it is also an essentially multidisciplinary field which spans the wide subject matter revealed by the bibliography. An analysis of categories 1 and 2, simulation and gaming, taken together produces the map of fields shown in figure 1, where I depict relations between research fields and accompanying inter-field topics. As we can see, the social sciences and engineering were linked into a network *via* a number of in-between topics and fields. The broad span of issues, from manufacturing and logistical ones to individual rationality and the behaviour of organisations, cover the disciplinary space from management to political science, from decision making to defence and from engineering to psychology.

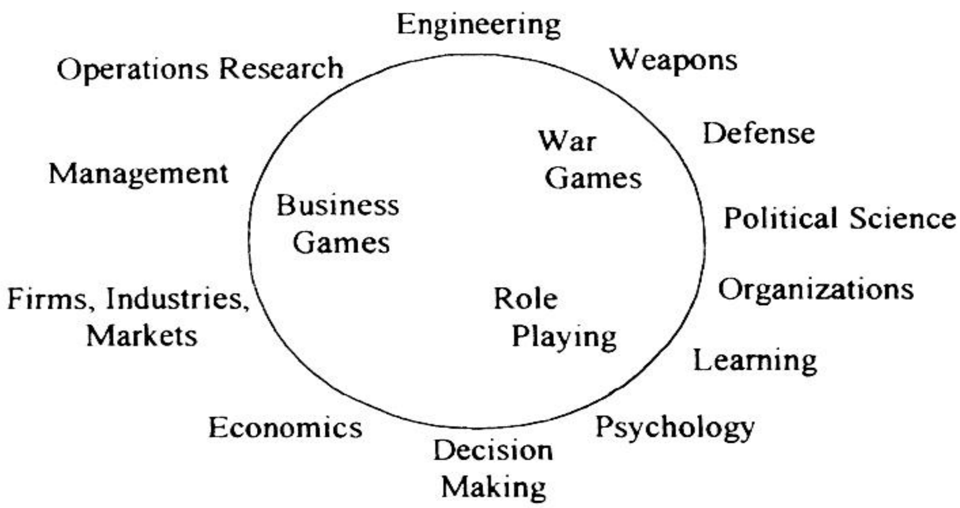

Fig. 1. — *Subject map for simulation and gaming (categories 1 and 2) in Shubik's 1960 bibliography.*

Economics finds its place in this circle. Shared between the fields, we see various kinds of games – war games with sand tables or in logistics labs, business games based on company histories, and role playing games. Different connotations attach to the term « games » in relation to the subject fields (13).

It is important to stress that Shubik's perceptions of these shared spaces of simulation were not idiosyncratic. Harold Guetzkow's 1962 book of readings on simulation in the social sciences contained accounts of dynamic flight simulators, RAND's Systems Research Laboratory air-defence experiments, computer simulations of thinking, a management game report, examples from engineering, transport queuing, and role playing from political science along with computer simulations of elections (14).

In contrast to the generous nature of the subject fields found within the 1960's categories of simulation and gaming, the majority of the Monte Carlo section 3 comes from maths, stats and computation with fewer papers from natural sciences and engineering (see figure 2). This forms a further set of axes for techniques and ideas which feed into the fields outlined in figure 1. So simulation and gaming gain not only from a wider disciplinary mix of ideas, but also from the research tools of computers and yet another kind of game, games of chance.

The majority of entries in the systems section 4 comes from economics and computation (see also figure 2). Most of the economics material according to current definitions (econometrics, mathematical economics and philosophical reflections on economics) turned up in section 4, but that section included many economics articles which now seem to have little to do with simulation or even « systems » (e.g. Tjalling Koopmans' famous 1957 series of essays on economics). Category 4 was not an economics ghetto, rather the opposite, it was a space for more specialised system-based thinking in economics (but not systems analysis : Jay W. Forrester's well-known 1958 work comes in category 1). Nevertheless, as stressed earlier, although there is no complete predictability in these catego-

(13) There are a number of possibly interesting links here which as far as I know have not been researched. One is the relation between war gaming and management games.

(14) By comparison, for example, a 1996 collection of essays on simulation in the social sciences contained no gaming or experiments with people, and its subject range was more severely restricted ; it did not include, for example, military simulations. See Hegselmann *et al.*, 1996.

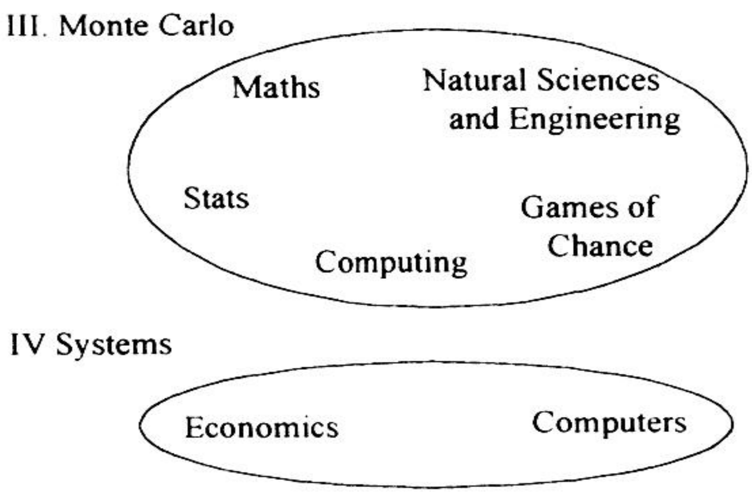

Fig. 2. — *Subject map for Monte Carlo and systems (categories 3 and 4) in Shubik's 1960 bibliography.*

risations (15), the majority of the material which is interesting for my analysis of simulation in economics around 1960 was contained in categories 1 and 2, in experiments, in gaming, and in econometric and computer simulation of models.

3 / *Secrets and the military-industrial-science complex*

The range of the subject fields and types revealed in the figures is considerable, and we are warned by Shubik that there were many other papers which could not be included in his bibliography because they were not public. There are two reasons for this : either they were classified as part of the US defence establishment's cold war effort, or they were company reports considered to constitute commercial secrets. Given the variety of published fields already depicted in the figures, it seems likely that these secret materials may not have extended the range of subjects. Nevertheless not all cold war Defense Department contract material was classified. The bibliography contained many RAND reports and

(15) For example, Strotz, Calvert and Morehouse, 1951, on « Analogue computing techniques applied to economics » appeared under section 4 whereas it could just have easily been in section 1 alongside Morehouse, Strotz and Horowitz, 1950 : « An electro-analog method for investigating problems in economic dynamics ».

many other research papers which originated from Defense Department funding of various kinds. The three most populous outputs in categories 1 and 2 indeed came from RAND, from the *Journal of the Operations Research Society of America* (16) and from a collection of management journals. But there were occasionally more clearly defence-labelled items such as from the *Naval research logistics quarterly* and from more exotic locations such as *Journal of abnormal and social psychology*.

It is evident from the sources used in the bibliography, from the known secret areas and from figures 1 and 2, that what we have here is the intersection between three elements : the military, science and management. As is becoming clear in recent literature, we must extend the military-big (natural) science complex that grew up in the war and cold war period (17) to incorporate the social sciences (18) and management studies, and we must add to that the military-industrial complex familiar to economic historians and going back to the 1914-1918 world war and intermittently to the mid 19th century. This bibliography shows how entrenched that military-industrial-science complex had become not just into natural science, but into social sciences as well ; Defense Department research contracts in this field of simulations and gaming employed a mix of social scientists, mathematicians and defence experts. This mix included some names, such as John von Neumann and Herbert Simon, already famous for their many multidisciplinary interests spreading between computers, natural and social sciences. But, with the work of Philip Mirowski (19), we can see that much of the basic and technical research in economics in the USA during the cold war period was funded directly or indirectly by various arms of the military establishment. And, as we might expect from the comparatively lavish funding going into this research at this time, many of those

(16) Operations research (OR) was surveyed in that same year 1960 in the *AER* by Robert Dorfman who defined it as an approach or method that is prescriptive rather than descriptive, and aims to solve practical problems by determining the optimal action to take based on mathematical and statistical analysis. Though a considerable (minor) chunk of the literature in the Shubik bibliography comes from operations research journals, and many of the techniques of simulation are shared with operations research, not all simulation and gaming is operations research, and much of OR is neither simulation nor gaming. On OR at that time, see Mirowski, 2001, and further remarks in section 4 of this paper.

(17) Galison, 1997.

(18) Hounshell, 1997 ; Jardini, 1996 ; Mirowski, 2001.

(19) Mirowski, 2001.

active as young talent in these groups become leaders of economic research in later 20th century American academe.

From the point of view of the historian of economics, there is a most striking subject omission from this bibliography, namely that of game theory. The omission is striking both because game theory was fostered within some of the same institutions leading the field of simulation within the military-science context (e.g. RAND) and because Shubik himself was known as a game theorist.

The absence of game theory in the bibliography makes an important point for my interests. While some may be tempted to think that the connection between game theory (the theory of games of strategy) and « gaming » is a very intimate one, the former being the theory for the latter, this quite mistakes the meaning of the term « gaming » as it was used during this period. At this time « gaming » was understood to be a broad-based experimental research and training method involving people playing roles (possibly their own) in an actual or simulated environment. Game theory was a mathematical body of work about decision making in strategic interaction. While game theorists, such as Shubik, did « play games » as part of their own research, he regarded these as exploratory, creative activities, a practice he separated from the controlled role-playing experiments falling under the label « gaming (20) ». Thus, for example, Shubik (and others) discuss « inventing » playable games of strategy to « illustrate » pathological or paradoxical properties during his early work on game theory between 1949 and 1952. However, these remained disconnected from the experimental tradition of gaming in his 1960 survey work (21), and they remain disconnected in his later autobiographical accounts (22).

This may explain why game theory was not included in his bibliography, but not how Shubik, a game theorist, came to know the breadth of technologies of simulation and gaming so intimately. I will eschew the pleasure of investigating his history at length for reasons of space, rather than because it is dull -- it is not. Suffice it to say that his was not a straightforward academic career. Following war service, Shubik joined the economics department at

(20) For this part of Shubik's history, see Weintraub, 1992, 159 and 248-252.

(21) Shubik, 1960 *a* and 1960 *b*.

(22) For autobiographical accounts, see Shubik, 1992, 1994 and 1997.

Princeton, one of the two main research centres at which the theory of games was just being developed in the late 1940s and early 1950s with the help of military funding. This led to *Strategy and market structure* (23), the first full-length serious integration of the ideas of game theory into the field of industrial economics, an initiative whose importance may easily be overlooked, but was to establish game theory's place in the heart of modern neoclassical micro-economics. Following Princeton, Shubik's path led him *via* General Electric (which « changed his views about how firms actually worked (24) » and suggested how firm planning could be done on computers) to IBM (where he developed business games), and, after a chance meeting with Sidney Siegel, to beginning his own gaming experiments (25).

Shubik was a man who did everything – and with success : game theory from the early 1950s, and operations research, simulations, gaming experiments, business games and so forth from the later 1950s, amassing immense knowledge and understanding of the whole field. His bibliographic work on the field and his *AER* survey paper (26), which I discuss next, were both based on an insider's, practitioner's, knowledge of simulation and gaming in economics, not that of an outsider.

III. — THE ZOOM LENS : THE *AMERICAN ECONOMIC REVIEW* 1960 SYMPOSIUM ON SIMULATION

In this section I will use the three papers on simulation in the 1960 *AER* symposium to explore what economists understood by the term at that date and to see how their perceptions about the roles and activities of simulation in their own field fitted into and linked across the maps of figures 1 and 2. The three papers were by Martin Shubik himself, by Guy Orcutt, and by Geoffrey Clarkson with Herbert Simon. I start with Shubik's own piece.

(23) Shubik, 1959.
(24) Shubik, 1997, 103.
(25) Shubik, 1994, 252 and 257 ; the experiments were reported *in* Fouraker, Shubik and Siegel, 1963.
(26) Shubik, 1960 *a*, 1960 *b*.

1 / Shubik's « simulation of the industry and the firm »

Simulation was defined by Shubik in terms of representational models and their operation in the following terms :

« A simulation of a system or an organism is the operation of a model or simulator which is a representation of the system or organism. The model is amenable to manipulations which would be impossible, too expensive or impracticable to perform on the entity it portrays. The operation of the model can be studied and, from it, properties concerning the behavior of the actual system or its subsystems can be inferred (27). »

In Shubik's paper, as in his bibliography, simulation and models were considered to be intimately related : « In a gaming experiment a model of an environment is provided. In other words a simulated universe is constructed (28). » Rather than discuss any of his own work under these headings, Shubik surveyed a few papers which he thought indicative of the growing shoots of the field. I follow his lead in order to give some deeper sense of the various natures of simulation at that time.

« Gaming » in the economics of industry and firms at that date was represented by two different kinds of activities : the environment might be « rich » as in business games, or more strictly controlled as in economic experiments. Economic experiments were still rather new, and this sub-field of economics might be best represented by the work of Sidney Siegel and Lawrence E. Fouraker (29). Following the practices of experimental psychology (Siegel's field), they conducted careful laboratory experiments into the outcome of price bargaining in the situation of isolated exchange between two persons, the problem of « bilateral monopoly » in industrial economics. By tradition dating from Francis Edgeworth (30), this outcome was thought to be analytically indeterminate, solved in practice by the relative bargaining power of the two firms. The experimenters used students as their subjects, placed them in a controlled environment and had their behaviour constrained by a simple economic model of the relevant information (profit levels for different prices) for the

(27) Shubik, 1960 *b*, 909.
(28) *Ibid.*, 910.
(29) Siegel and Fouraker, 1960.
(30) Edgeworth, 1881.

bargaining situation. The experiments had the students « bargaining » in such a way that the experimenters could track the exchange price as it was determined as well as the final price level.

Business games by contrast, embodied very complicated models with lots of different elements of information. These games emerged in the late 1950s and were just beginning to become popular (31). The best known one was « The Carnegie Tech management game (32) » which used the detergent industry of the day to provide the raw material of economic details for its industry model. This was a « man-machine simulation », the environment (the industrial details) was programmed on the computer, and the people playing the game took the role of managers making the decisions required by the firms participating in the industry and responding to the environment. While primarily a teaching tool, the game provided a research platform for study of oligopoly industries, for material for a study of team actions, and for a number of other aspects of decision-making behaviour.

Business games usually fell into Shubik's gaming category, because individuals role-played the decision makers and the computers acted as the calculating device which solves the individual plans according to some model of industry level activity. But not all man-machine simulations fulfilled the aims of gaming as Shubik defined them. Sometimes the people participating were « taken as given », that is, they and their behaviour were not the subject of investigation, rather, they were the « cheapest effective simulator » of the real life individuals that they were representing. For example, Murray Geisler's « ... simulation of a large-scale military activity (33) » nominally falls under the gaming category, for it used real people playing their own habitual roles in a simulated environment ; however the interest was not primarily in the behaviour of the people involved, but rather with the details of a complicated logistics problem (34). The experiment involved a simulated environment in the RAND logistics systems lab (35), a simulated

(31) According to Cohen *et al.*, 1960, there were 21 business games around at that point.

(32) Cohen *et al.*, 1960. Carnegie Tech (The Carnegie Institute of technology) was also where Simon and Clarkson were located, see part III, section 2.

(33) Geisler, 1959.

(34) Also Enke, 1958.

(35) On which, see Chapman *et al.*, 1962.

organisation of real people in the lab, a simulated on paper aircraft program to provide the overall task for phasing inventory in and out, and a simulated paper aircraft to provide the « parts » list for the task. The simulation was a man-machine system, for the complexity of the task was provided by the machine compilation from the paper-based tasks. The experimental findings were compared with the outcomes from real logistics functions, *i.e.* with « analogous data » from « real experience » from the equivalent military establishments, but where the subject at issue was the functioning of the logistic arrangements not the people.

« Simulations » in economics were represented in Shubik's paper by examples which span from those using very detailed descriptions and data from a real industry or firms to those where the firm modelled was entirely fictitious and no empirical data from firms were used.

A simulation of an entirely fictitious industry was provided by Austin Hoggatt (36) who built a mathematical model of an industry with an initial 100 firms and programmed the computer to simulate what would happen in that industry under certain conditions. It was an exercise in model exploration, with the interest lying in how various different assumptions alter the outcomes. A similar type of simulation was given by Forrester's « Industrial dynamics... » of 1958. This paper, now considered a classic, set up a very complicated model of an individual firm's flow of information, orders, inventory, production and distribution with lags and feedback control mechanisms. The model was simulated to understand how the information flows and their timing creates certain patterns in other parts of the industrial system. This « systems analysis » depended on computer simulation and the aim was to explore the patterns of output data created by the simulation and to be able to explain them to managers.

At the other extreme lay simulations with real industry data. Kalman Cohen (37) simulated two models of the « shoe, leather, hide sequence » the sequence from raw material to final output of the shoe industry, using real data as the starting point. In order to model the behaviour of a typical firm in the industry, he required data at a very detailed level and a detailed institutional description

(36) Hoggatt, 1959.
(37) Cohen, 1960.

of the firms in the sector, both of which were available in the work of Ruth Mack (38). One model he built and used was an econometric model, in which the observations were used for each time period to predict the values one period ahead. The other model was a process model of the industry sector which used the real data to estimate a closed dynamic model of the system of relations within the industry and he then simulated the aggregate industry level to produce simulated data for a long set of periods ahead. Cohen's aim was to relate behaviour at the industry level to the aggregate level of consumer behaviour. Since the mathematical form of the process model (a non-linear system of lagged simultaneous difference equations) defied analytic solutions, and the level of complexity of the model was considerable, the simulated streams of output data were computer created, though the model used as the simulating device was constructed with real data.

In the middle of this range, between fiction and fact was, for example, Richard M. Cyert, Edward A. Feigenbaum and James G. March's (39) paper-machine simulation exercise. In this exercise, they aimed to build a more behaviourally accurate model of the firm than that of standard economics. They proceeded by specifying assumptions about how firms behave and make decisions in a series of nine steps. This « process model » was shown as a « flow diagram », and with the help of a computer, the model was used to simulate the process of two firms in a duopoly industry taking various decision steps in response to certain initial conditions and events. This decision process model, and the simulation *via* computer, produced output showing certain profit outcomes of the two firms over hypothetical time. The simulation results were compared to the actual profit outcomes from the tin can industry (Continental Can Co. and American Can Co.) over the period from 1913-1956. The authors claimed a « good fit » (page 93), that is between their simulated « evidence » and the real stream of evidence, though they denied – in a footnote – that this demonstration validated their model !

We can see in this brief survey how simulations and gaming in the economics of firms and industries used people, computers and models in a considerable variety of arrangements. Some involved

(38) Mack, 1956.
(39) Cyert, Feigenbaum and March, 1959.

lots of real firm information, some relied on fictions created by the economist ; the computer played perhaps a more important part in the simulations than in the games. « Evidence » of various types was produced, some aiming to mimic real observations, and some only the outcomes of fictional firms.

GAMING		SIMULATION	
Object of interest is people's behaviour * Game (role) playing by people * Simulated environment is taken as given		Object of interest is system's behaviour * Simulation of model system * People/firm behaviour is taken as given	
Environment thin : controlled *via* rules of economic exchange	Economic experiment with people exchanging : Siegel and Fouraker	Model of fictional firm	Hoggatt, Forrester
Environment rich : laid out in description of industry	Business game with people acting as managers : Carnegie Tech management game	Model part fiction, part real inputs	Cyert, Feigenbaum and March
		Models from real inputs	Cohen

The matrix presented here, using Shubik's original bibliographic definitions (discussed in part II, section 1), suggests how we might draw his examples together, but the dividing lines are somewhat artificial. For example, recall that the behaviour of people in the economic experiment by Siegel and Fouraker was strongly constrained by the model, while in Cohen, the model was strongly constrained by the data stream of observations. The point once again is that the demarcation criteria between gaming and simulations are both thin and porous ; their similarities will be more relevant than their differences as we see in part IV. But at the same time, the extreme difficulty of generalising about these materials is why Shubik's survey seems rather inconclusive, even while he was convinced that this was the research direction for the future.

2 / *Clarkson and Simon's « simulation of individual and group behavior »*

A second paper in the AER symposium on simulation was by Clarkson and Simon. They too began their paper with a definition :

> « Simulation is a technique for building theories that reproduces part or all of the output of a behaving system [...] The process of simulation involves constructing a theory, or model, of a system that prescribes the system's processes [...] By carrying out the processes postulated in the theory, a hypothetical stream of behavior is generated that can be compared with the stream of behavior of the original system (40). »

Clarkson was a Ford Foundation fellow (previously graduate student) at the Graduate School of industrial administration at the Carnegie Institute of technology working with Simon, soon after the latter's interests had come to focus strongly on questions of artificial intelligence and cognitive psychology (41). They positioned their interests as being at the level of the individual decision maker, compared to Shubik's in the firm or industry and Orcutt's interest in the whole population and economy. The computer played a more fundamental role in their kind of simulation than in those simulations discussed in Shubik's survey.

Simon needs little introduction, for his incredible career has been well charted (42), but there are so many elements that we need here some sense of chronology for our purposes. By the mid 1950s, Simon had already made a name for himself first for his work in management sciences with his book on administrative behaviour, then by his work in econometrics and causal ordering in the early 1950s, followed by work on individual decision theory in the mid 1950s. By the late 1950s, Simon had come to understand computers as manipulators of symbols rather than as calculators. Since symbol manipulation was the basis of thinking, the idea of the computer as a thinking machine rather than a calculating machine lay right at the centre of the simulations described here (43).

(40) Clarkson and Simon, 1960, 920.

(41) Simon, 1991.

(42) Not least by himself, see Simon, 1991.

(43) Simon at this time, along with collaborators Allen Newell and Cliff Shaw, had just succeeded in building a programme to enable a computer to « think logically » (Simon, 1991).

As Clarkson and Simon confirmed, the computer was important because its talents are such that « we can simulate the behavior of a symbol-manipulation and information-processing system, regardless of whether the symbols these systems handle are numbers, or are English words, phrases and sentences (44) ». For Clarkson and Simon, it was this ability of computers to handle « non-numerical » simulations that allowed a more immediate match to the individual decision making process :

> « To write a heuristic program of a decision-making process, we do not first have to construct a mathematical model, and then write a program to simulate the behavior of the model. We can directly write a program that manipulates symbols in the same ways that (we hypothesize) the human decision-maker manipulates them (45). »

Clarkson and Simon describe this quality of the computer as the quality of non-numerical computation : the computer can read and compare symbols and can be programmed to process symbols and act on that information just as an expert decision maker reads symbols, compares information and acts on these.

Clarkson and Simon's substantive work involved a computer simulation of individual decision making in choosing stocks for investment. This was apparently Clarkson's thesis work, so I shall refer also to a chapter of that work reprinted in a 1963 collection. Clarkson (46) explained that his research started by observing exactly how investment trust officers in banks chose stocks for a portfolio. According to his account, he interviewed trust officers, and observed their meetings called to review decisions ; he went on with the historical analysis of past decisions ; and proceeded to the making of « protocols » – written down records of the investment decision and its processes. By this extended process, he built up an « expert system » record that provided the basis for programming the computer to make the same such decisions, namely to select stocks for a portfolio on the basis of the same information that the trust officer held. The raw material of information about stocks and their performance, about clients and their requirements and about the legal requirements for trusts, had all to be gathered and entered into the computer's programme in a way compatible with

(44) Clarkson and Simon, 1960, 923-924.
(45) Clarkson and Simon, 1960, 925.
(46) Clarkson, 1963.

the decision process described in detail in the protocols so that the computer could make stock selections as far as possible in the same order on the same information with the same memory elements as a trust officer.

The validation of the simulation model and exercise came first in comparing the results of computer process of selection of stocks with the portfolios selected for a set of new clients by a particular bank. According to his reported results, these showed a considerable degree of correspondence. Comparisons were also made with random choices or naive choices, and sub-processes of the programme were compared with subsections of the trust officer's selection decisions, again with encouraging results. Clarkson and Simon (47) also report further comparisons with stocks chosen at a group of banks in Pennsylvania.

Clarkson's aim was to make as accurate a « reproduction » of the trust officer's information, past experience (memory) and decision process as he could, and render that set of knowledge into the computer programme and information system. This is the meaning of Clarkson and Simon's claim that their simulation *reproduces* the behaving system rather than *representing* it, the claim that we find in Shubik's definition. Clarkson approached the simulation problem in a way that was compatible from the start with the way computers work, so that less translation was required and the computer itself (its design and operation) was an active apparatus in the simulation. Here too we see the space in which AI, computers and modelling come together in simulating a traditional economics' decision problem of making choices in the face of lots of information and, at the same time, considerable uncertainty.

3 / *Orcutt's « simulation of economic systems »*

Orcutt's 1960 AER symposium paper also, naturally, opens with an account of simulation :

« Simulation is a general approach to the study and use of models [...] An individual simulation run may be thought of as an experiment performed upon a model [...] The problem of inferring general relationships from specific results obtained in individual experiments performed on a

(47) Clarkson and Simon, 1960.

model is the same as that of inferring general relationships from specific experimental results in any of the inductive sciences. The research worker, studying or using a model, could conceivably use a purely deductive approach, but this alternative may not be attractive or it may not prove feasible with known mathematical methods (48). »

The paper is also the first substantive report on his microanalytic simulation method. In this report he discusses how to create a « sample » of individuals in households at the microeconomic level, how to assign relevant characteristics to them so that they constitute a representative sample of the population, how to prepare a description of their demographic behaviour covering birth, deaths, marriages and divorces, and how to simulate the dynamics of the demographic changes undergone by his sample over simulated time, and finally how to blow the sample up to aggregate level in order to assess both the resulting cross-section and the time-series characteristics against the actual population figures. In his 1961 book (49), he reports the full details of this work, and adds a considerable degree of extra economic content by combining the demographics with information about the labour force, spending and saving behaviour of the individual decision units.

Orcutt's 1960 article and the immediately following book are recognised and referred to (50) by the economics profession as the opening of a new research tradition in economics and in demography in which simulation is essential to the research method : that is, the research method is founded on simulation whereas other forms of simulation in economics often form a complementary technology, complementary to real experiments, to econometrics, to mathematical modelling and so forth. Orcutt's « microsimulation » methods and models became used routinely to assess the distributional, economic and demographic consequences of changes in welfare regimes, tax regimes, etc. during the 1970s in the USA and later spread – particularly into Germany and Sweden and the UK. This new approach relies on methods with earlier roots, and which shaped Orcutt's own career.

(48) Orcutt, 1960, 893-894.

(49) Orcutt *et al.*, 1961.

(50) See for example Greenberger *et al.*, 1976 ; Watts, 1991, and the special issue of *Journal of economic behavior and organization*, 14 (1990), reporting papers at a conference in honour of Orcutt for assessments of his role.

Keen to work in a socially useful science, a science which might prevent a second great depression, Orcutt's work (51) as an economist began by analysing the data used in the first US macroeconometric model built by Jan Tinbergen (1939). Tinbergen won the first Nobel prize in economics for his pioneering work building the first ever macroeconometric model : a model of the Dutch economy (1937), built with the express intent of exploring different policy options designed to get The Netherlands out of the great depression. Though such simulation projects became a standard part of econometric model usage in the post-war period, and though Orcutt greatly admired Tinbergen's work, he came to the conclusion that however good a representation of the macroeconomy the models offered, the problems of aggregate-level data meant the possibility of validating such models was extremely doubtful and their policy simulations useless. Since the microeconomic, or individual, level was where economic and demographic decisions were made. Orcutt believed that if these could be satisfactorily modelled with empirical support, they could be summed to the aggregate level to provide a more reliable simulation tool for policy purposes.

Steeped as Orcutt was in the statistical business cycle work of this field, he was already familiar with the tradition of statistical experiments using sampling devices and random numbers which culminated in the work of Eugen Slutsky (52) in the 1920s, who used random numbers to generate artificial data-series which mimicked business-cycle data. Orcutt's own investigations (53) into Tinbergen's aggregate economic time-series data relied on similar investigative techniques, namely, statistical experiments and simulations involving « experimental models » and random numbers to generate artificial series, whose properties could then be examined and compared with the real economic data used by Tinbergen.

Though Orcutt had come to distrust the results found by Tinbergen, his own micro-simulation method aligns much more closely to Tinbergen's work than to Slutsky's in the sense that Orcutt, as Tinbergen, insisted on real data inputs. Orcutt's (54) simulation

(51) Bibliographical information for Orcutt is found in the previous assessment series, and in Orcutt, 1968 and 1990.

(52) Slutsky, 1927.

(53) Orcutt and Irwin, 1948 ; Orcutt and James, 1948.

(54) Orcutt, 1957, 1960, Orcutt *et al.*, 1961.

device was based on empirical evidence at two points : the « status » variables, which determine the characteristics of all individuals in the sample, were taken from census and sample survey data of the day so that the « sample » was constructed to be a representative sample of the population. The « operating characteristics », which determine the demographic behaviour of the individuals, were estimated from the demographic evidence of the day : they provided the probabilities of outcomes for the set of individuals sharing similar characteristics. Only at the last point did the random element come in, for whether a demographic change occurs for any particular individual depends on the probabilities which govern (or accurately describe) these events. Here Orcutt made use of Monte Carlo techniques (55), with random numbers generated by computer (where Slutsky had used lottery ticket numbers) to determine which individual events occur and a computer programme to pick out and assemble the individual and family units into the aggregate population level. Orcutt's work in developing his microsimulation models was an outcome of a personal history steeped in the history of the fields of statistics, computation and econometrics with the additional element of making the computer the instrument of population aggregation.

Though Orcutt felt the main direction of Tinbergen's econometric model simulation work was not worth pursuing, others were following that path in 1960 : notably Irma Adelman and Frank Adelman (56), and James Duesenberry *et al.* (57). In the former, an adapted version of the 1955 Klein-Goldberger econometric model of the US economy was simulated into the future using the estimated parameters of the model to assess the dynamic characteristics of the model against generic empirical characteristics of the US economy. Various changes in initial conditions, and various kinds of random shocks were added to see whether the estimated model could mimic the characteristic cycles found in real economy data. In the Duesenberry *et al.* paper, a much smaller econometric model

(55) For a contemporary commentary on Monte Carlo methods from someone familiar with the econometric work in the tradition from which Orcutt emerged, see Marshall, 1958, who also discusses simulation. Whether Orcutt's use of the methods were gained from the way the technique was used in physics (see Galison, 1997) or whether they came from the longer tradition in statistical or econometric work is not clear.

(56) Adelman and Adelman, 1959.

(57) Duesenberry *et al.*, 1960.

was built for more ambitious simulation purposes. The « experiments » on the model involved investigating changes in economic events, changes in governmental policies, and different random shocks. These simulations showed the way the economic elements in the model behave over time during the run of each experiment.

There is, in fact, a continuum running from these econometric models using real data to produce their parameter values which are then simulated to produce a further « evidence » stream in a validation exercise (or in policy analysis) to small mathematical models with guessed values which are simulated to produce artificial « evidence » (58). This parallels the continuum found in the simulations of firm and industrial behaviour discussed by Shubik : for example Cohen's simulations of « evidence » depended heavily on real data inputs while Hoggatt's or Forrester's fictional firms produced artificial « evidence » from mathematical models.

IV. — « Simulation » as an epistemic technology : the digital workbench

Reading the literature discussed in this paper, I suggest that the umbrella term of « simulation », as used in the economics and management literature of this period around 1960 was formed from a set of four interlocking elements, namely : « experiment », « model », « game » and « the computer ». The first three elements seem to be rather abstract notions, and the latter a rather concrete object. In fact, I think we had best attribute both an abstract quality and a concrete sense to all four elements. All four show up both at the most practical level in descriptions and explanations of the various different natures of simulation in the papers that I have been discussing and in attempts to provide general definitions of simulation. The observation that these four elements are both sufficiently general to be discussed in abstract terms and to appear as

(58) For historical examples of such small mathematical models with no data inputs, see Frisch, 1933, or Samuelson, 1939 ; for an analysis of very recent examples, see Morgan, 2002.

particulars in practical work suggests to me that simulation, as found in this literature, is best understood as a technology.

Labelling simulation a technology gives us two advantages. First, as a technology, it presents itself in various forms, with each of the combined elements having more or less importance for any particular version, so we can be less concerned about the specifics of the techniques. Second, it draws attention to the combination of human know-how and material things associated with the meaning of « technology ». Technology is a difficult notion to discuss with any conviction in philosophical terms ; but since the power of a technology only becomes manifest in its usage, we shall continue to refer back to the many examples discussed above to see what role these four elements : model, game, experiment and computer play individually, and how they fit together in simulation.

1 / *The computer as laboratory equipment, models, games and experiments*

The powers of the digital computer provided the opening for Shubik's account of simulation in industrial economics, and he praised it for fulfilling double duty as laboratory equipment for economics (59). It provided « the means for constructing both the instruments for observation (60) » and the « equipment for experimentation », allowing the economist to study masses of data at various levels of aggregation ; facilitating the use of more « realistic », *i.e.* complicated models which do not have to be analytically solved but can be analysed by numerical methods ; and most importantly enabling « simulation ». Shubik's metaphor of the computer as an instrument of observation, more particularly as a microscope, provides us with a neat way into understanding the technology of simulation. A microscope allows us to see the details of things at a scale far smaller than we can normally see. And the things that we « see » with the instrument (ever mindful of Ian

(59) Shubik, 1960 *b*.

(60) Shubik (1960 *b*, 908) may have inherited this metaphor from Oskar Morgenstern, who likened the computer to both a telescope and microscope instruments for economists to look with in his 1954 account of the role of computers in economic experiments. Morgenstern's work was part of a Princeton economics project, again funded by defense spending.

Hacking's 1983 discussion of this issue) are often not « natural objects » but specially prepared representations of those objects.

The scale argument, if not immediately self-evident, to us now, was well-understood by the authors at that time. Although the computer makes it feasible to process data and carry out calculations at hugely increased levels than hitherto (and thus enables some authors to use the computer to simulate the economy as a whole (61)) we can often also find the argument about smallness in this literature. Contrary to what one might at first think, this power of immense calculation takes us away from large aggregates to much smaller units, enabling work at a much finer grained level of analysis than before. The economic world can be cut up into smaller units of both time and activity, to the firm level rather than the sector, to the level of individual year-by-year family demography, rather than the once-every-ten-years census level. And having allowed us to see at this level of detail, the computer also enables us to aggregate back up to a complex level, recomposing the detail into the big picture again.

But what constitutes the specimen in the microscope slide in these economics and management simulations ? This is much more difficult problem, for, if they were natural objects, such specimens ought to be small-scale « cut up » bits of the economy, the typical shoe firm or the individual family unit. But they are not. They are, rather, models of those things. Thus, where the biologist prepares slides so that they can « see » certain things with the microscope, economists prepare models so that the relevant parts of the world they specify can be « read » by the computer. And if the models prepared for the economists'computer-as-microscope are not natural, they are, of course, artificial constructions, man-made in Simon's (62) use of the term « artificial ».

Nevertheless, an important point about the kind of models involved in simulation must not be lost in this designation. The definition of « model » found in this literature is that it is a representational device, so rather than let the metaphor of computer as microscope force our understanding of economic models as especially prepared artificial versions of natural objects, it seems better to understand them as especially prepared representations of the

(61) Duesenberry *et al.*, 1960.
(62) Simon, 1969.

natural objects. Let me quote again the definitions from the three authors in the 1960 AER symposium on simulation.

1 / « Simulation is a general approach to the study and use of models [...] An individual simulation run may be thought of as an experiment performed upon a model [...] A model of something is a representation of it designed to incorporate those features deemed to be significant for one or more specific purposes (63). »

2 / « A simulation of a system or an organism is the operation of a model or simulator which is a representation of the system or organism. The model is amenable to manipulations which would be impossible, too expensive or impracticable to perform on the entity it portrays. The operation of the model can be studied and, from it, properties concerning the behavior of the actual system or its subsystems can be inferred (64). »

3 / « Simulation is a technique for building theories that reproduces part or all of the output of a behaving system [...] The process of simulation involves constructing a theory, or model, of a system that prescribes the system's processes [...] By carrying out the processes postulated in the theory, a hypothetical stream of behavior is generated that can be compared with the stream of behavior of the original system (65). »

Models are « representations » in Orcutt and Shubik. In Clarkson and Simon they claim the even stronger term, « reproductions ». One way to stress the importance of this claim about the nature of economic models used in simulations may be shown by contrasting it with those models used in operations research (OR) at that same point in time, where the aim was prescriptive, rather than descriptive.

Contrast, for example, our authors' notion of model with that found in Robert Dorfman's description of OR, where the activity is described as one of « formulating [the] problem by means of formal mathematical models » and a model is defined as « a symbolic description of a phenomenon in which its observable characteristics are deduced from simple explanatory first principles (*i.e.*, assumptions) by manipulating symbols in accordance with the laws of

(63) Orcutt, 1960, 893, and 897.
(64) Shubik, 1960 *b*, 909.
(65) Clarkson and Simon, 1960, 920.

some formal logic » (66). Shubik's emphasis on the model's representational role is the usual accompaniment to definitions and descriptions of simulations. In contrast, the OR usage of the term model refers to its derivation from first principles and its formal properties (relating to the kind of solution method used e.g. linear programming, queuing theory, inventory theory, etc.). The representing capacity Shubik and Orcutt require relates to their need to validate the model : if the model does not represent the economic system, the « evidence » produced by simulation has little value in telling us anything about that economy. The validation problem remains central precisely because good representation is required for any hope of useful inference from the simulation output back to the system being modelled (67). In operations research the validation issue is not important because the aim is to determine what *the economic system ought to be like* to ensure best performance, whereas in simulation the aim is to mimic or understand the working features of the *economic system as it is.*

Understanding models as representing devices means that we can make sense of another aspect which emerges from this survey of 1960 work practices and reasoning. The role played by models in simulation is the same as that played by gaming situations, gaming set-ups and rules of game behaviour within gaming. The representing object, the simulator, can be a mathematical or statistical model or it can be something like one of the set ups in the logistical systems lab, or it can be a particular bargaining game, or even in the AI case, the computer itself is the model. Each model represents, in its own way, something in the subject domain, but there is nothing distinctive about what material the model is made of. It may consist of people playing roles, it may consist of equations, it may consist of the computer itself. This is why gaming-and-simulation can span one bibliographic space, because the notion of the representing device is so fluid. Games and models are interchangeable in the sense of fulfilling the same role as epistemic mediators (68).

But the very nicest part about portraying games as models is that we also understand models as games. Everyone knows that

(66) Dorfman, 1960, 577.

(67) See Morgan, 2002.

(68) The precise phrase is Lorenzo Magnani's (at the Model-Based Reasoning Conference, Pavia, May 2001) ; see Morgan, 2002, and chapter 2 of Morgan and Morrison, 1999.

games have to be played, just as, I have argued (69), economic models have to be used in order to understand and come to appreciate their qualities and in order to learn with them. Neither models nor games are passive devices : specially prepared representational devices like models might make good « slides », but to understand them, we need more than observation : we need to manipulate them, to ask them questions, to solve them, to use them to create « evidence » and so forth. Here we come up against the useful analogical limit of the first element of Shubik's metaphor, the computer as a microscope. But the more general element of his metaphor : the computer as laboratory apparatus, plus the three definitional quotes (above), show us how to fit the other three elements of our technology together. Each simulation run is an experiment on the economic model, and the computer is often (though not always) the instrument for making these experiments. As long as we can understand the game-as-model in the same terms, each play of the game is also an experiment. Whether we run a model on a computer or play out a version of the game, we are effectively running an experiment.

2 / *Evidence* versus *« evidence »*

Unfortunately, this clean account of simulation as experiment immediately runs into muddy waters as we try to clarify what kind of experiment it is. In the laboratory experiment we create an artificially controlled environment in the real world and a real (*i.e.* laboratory type) experiment takes place. In constructing a mathematical or statistical model to represent the world, we create an artificial world within which only an artificial experiment – such as a simulation – can be run (70). The gaming experiment sits between these : it looks like a real world in which a real experiment is played out, but, by my argument, it is also a model world in which an artificial experiment is played out. Possibly it is both at once, or possibly it depends on the particularities of the case at hand whe-

(69) Morgan, 1999 and 2001 ; Morgan and Boumans, 2004.

(70) I have discussed elsewhere the various differences between model experiments and laboratory experiments see Morgan, 2002 and 2003 ; Boumans and Morgan, 2001.

ther we should consider it a laboratory experiment or a model experiment.

Something very important hinges on exactly how we place these game experiments, and that is the matter of the status of the evidence created. As we have seen, the elements of the technology combine in different ways to produce or create « evidence » : each simulation is an experiment with a model or with a game (with or without the computer), and each experiment produces or creates some « evidence ». I have tried to use this term rather carefully in discussions above. Of course, evidence is a rather generous term. We could use it to apply to mathematical model experiments designed to investigate theory, whose outcomes provide us with reasons either to keep going, or to tinker with the theory. We can use the term evidence to apply to such outcomes, arguing that they provide evidence for the theory or model being investigated. But evidence also gets used in a more restricted sense, perhaps something more like the set of facts a layman will associate with the term evidence when used in the legal process.

What is striking about this whole simulation movement is the desire to use artificial means to create a stream of outputs which look like the real observations we get from the world, *i.e.* to create « evidence » which will match or mimic the empirical or real evidence – using the layman's legal sense – namely the facts of the world. Boumans (71) suggests that whether we can interpret or treat such « evidence » from model experiments as real evidence is a kind of Turing test in economics.

What status should we apply to gaming then ? If gaming is simulation with a model, then we create « evidence » about the artificial world. If gaming is experiment within a controlled real world, we create evidence about the real world. If we regard gaming as a real experiment with a « model world » or « simulated universe » (as Shubik defines that world) are we creating evidence or « evidence » ? And, when we look carefully at the details of some of the gaming experiments it becomes pertinent to ask : are these model environments or simulated universes of the social scientist really so very different from the controlled environments created by natural scientists in their laboratories ?

(71) Boumans, 1998.

3 / *Conclusion : A new technology*

Hacking (1992) has argued that when a new form of reasoning comes into science, there is a period in which it is argued over and has to be justified as an epistemological instrument before it becomes acceptable and taken for granted. In many ways, the material I have covered here seems to show a style of reasoning at that very point where it is making its case for justification. In 1960, a new technology : the combination of model, game, experiment and computer, announced its arrival on the economics scene, but it had to be fully explained and demonstrated before it could be judged acceptable and become accepted. The authors writing at that time were careful to explain the technology they were using and how it worked. This was necessary not only because the combination of elements was new, but because the individual elements in themselves were also rather new. In 1960, models had been only comparatively recently introduced into economics, the computer was not yet widely used in economics or social sciences, and experiments were also just beginning. Further, by tradition, economics was not regarded as an experimental science, a view which had to be overcome. Later proponents of simulation no longer had to define the components of their approach, discuss how these fitted together, or explain what simulation did : their style of reasoning had become understood. However, the epistemological power of simulations – that is the status of « evidence » created by simulation experiments with models – remains debatable.

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