

## **Computer Simulation in the Controversy over Limits to Growth<sup>1</sup>**

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## **Abstract**

In the early 70's, scientists debated hotly the provoking claims that Meadows and his co-authors put forward in their book 'Limits to Growth' and which they based on computer simulation runs. This lively debate is reconsidered to flesh out two more general aspects of scientific computer simulation. The first point deals with the question where simulation scientists locate the agency in the activity of computer simulation: Is it the assumptions made by humans or is it the calculations done by the machine that are to be held responsible for the simulation results? The second aspect hints to the problem of how much data are necessary to make a computer simulation a true and meaningful representation of reality. In the case of the debate over Limits to Growth these two questions were answered differently by the proponents and the critics of the simulation study. Whereas many scientific controversies involve scientists who have done research which has lead them to diverging conclusions about the same matter, the debate over Limits to Growth differed in this respect. It can be construed more adequately as the repulsion of researchers who tried to intrude social scientific expertise with the help of computer simulation. This is why computer simulation became one of the key issues in this scientific debate.

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# 1 Introduction

In 1972 Dennis Meadows and his collaborators published 'Limits to Growth' in which they argue that several pressing global issues endanger the survival of humankind in the 21st century. The conclusions drawn in Limits to Growth were derived from simulations run with a model called WORLD3. It was constructed during a two-year long project, based at the Massachusetts Institute of Technology (MIT) and financed by the Volkswagen foundation. The book and its strong statements about the future were hotly debated in both, public and scientific media in the 70's. It was also a topic for sociologists who proffered analyses of the simulation study showing what actors came together to bring it about and what social factors influenced the results. Golub and Townsend (1977) argue that multinational companies, Sandbach (1978) that the environmentalist movement, and Bloomfield (1985) that the organizational context of the study can be held responsible for the results found in the simulation runs.

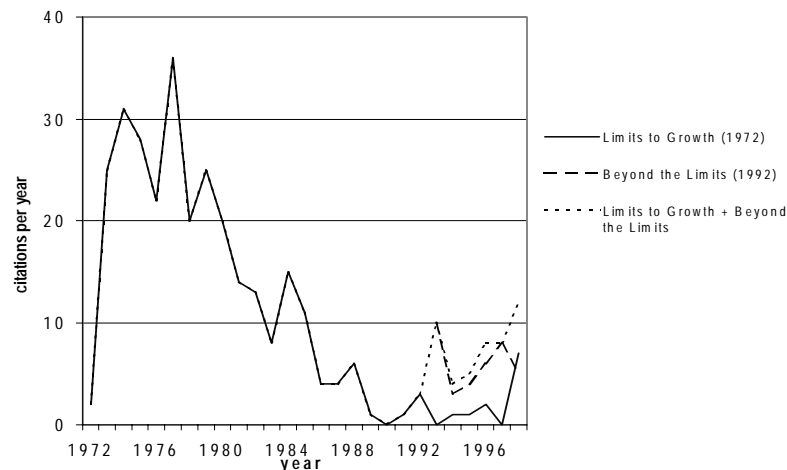


Figure 1: Number of scientific publications per year that cite the Limits to Growth study<sup>2</sup>

Today, discussions about Limits to Growth have mostly petered out (see Figure 1), even though the sequel, 'Beyond the Limits' (Meadows et al. 1992), has led to a slight increase in received attention. Sociological observers have had their say about how Limits to Growth was possible, and recently Elichirigoity (1999) published a comprehensive and detailed history of the project. Why is it useful to continue to dwell on the past and examine the scientific controversy over Limits to Growth? I think there are two reasons why one should reconsider this case:

First, only fairly recently have science and technology studies become aware of the wide use of computer simulation in the production of scientific knowledge. For many years the specific problems and potentials that accompany computer simulation have been largely overlooked. From an analytical point of view it is not sufficient to say that simulation is similar to experimenting or similar to theorizing. To

<sup>2</sup> The data for this chart stems from an electronic version of the Social Science Citation Index from 1999.

do so would be dangerous since these distinctions form a discursive resource for those who engage in computer simulation (Dowling 1999). Instead, it is necessary to take comparisons of simulation to either experiment or theory as the explanandum. The specific characteristics of computer simulation in scientific practice and discourse are being fleshed out in something one could aptly call ‘social studies of scientific computer simulation’<sup>3</sup>. *Limits to Growth* is an ideal case to contribute to such an endeavor because computer simulation featured prominently in it, and the controversy around it attracted a lot of attention from laypersons as well as experts.

Second, if one takes a closer look at the sociological studies that already exist regarding the scientific controversy over *Limits to Growth* one finds that they are biased against the simulation-study. Some employ the same strategy as those who attacked the results of the simulation: They present its results as caused not by scientific reasoning, but by some overwhelming *Zeitgeist*, like a general feeling of anxiety, the trend of globalization in the business world, or the environmental movement<sup>4</sup>. Even when it is taken into account that social factors are present not only in the case of the production of *Limits to Growth* but also in the production of the counter-arguments, one gets the impression that sociological reflections aim to denigrate the *Limits to Growth*-study<sup>5</sup>. If the *Zeitgeist* or other social factors are the accepted causes for ‘unvalidated’, ‘confusing’, and ‘demoralizing’ simulation results there is no space left for the computer simulation to play an active role in the production of scientific knowledge<sup>6</sup>. Therefore the assumption that social factors can be made responsible for the flawed results of the *Limits to Growth*-study need to be left behind, if one is interested in the phenomenon of scientific simulation.

The examination to follow tries to attend closely to what is known in the sociology of scientific knowledge as ‘symmetry’<sup>7</sup>: It will not picture the simulation study as erroneous or arrogant and it will not try to explain the misguided research as caused by something social. Rather, the positions of the *Limits to Growth*-researchers and their critics will be discussed side by side. The goal here is to make visible the form of the debate and how it is arranged around two lines of conflict concerning computer modeling. These lines separate discussants as they give diverging answers to the two questions: (I) Who is the agent: modeler or model? (II) How much data does a valid model need? Thereby giving computer simulation a central role to play in the scientific controversy. Answers to both questions can

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<sup>3</sup> The volumes edited by Morgan and Morrison (1999) and Sismondo (1999) include studies that make a step toward this direction.

<sup>4</sup> Hagen (1972: 15), as well as Golub and Townsend (1977) and Sandbach (1978) follow this pattern of explanation.

<sup>5</sup> This includes the technically very detailed study by Bloomfield (1985), that contrasts the simulation study of the rigidly regulated, male dominated, and élitist MIT-group with the relaxed and egalitarian research group at Sussex.

<sup>6</sup> Kusch (1991, part I) gives a short overview of various attempts to rid historical accounts of such totalizing explanations in favor of more flexible and sensitive vocabularies.

<sup>7</sup> This concept was, of course, first introduced by Bloor (1976).

only be given by going back to the original sources, i.e. the scientific articles where the controversy took shape. Arguments will be presented in the terms of those who were involved in the debate. In contrast to how controversies are pictured usually in the sociology of science, the interpretation of these positions will suggest an understanding of the debate as a repulsion, as one among other possible reactions to an intrusion into a field of scientific expertise.

## **2 Who is the agent: modeler or model?**

Sociological studies of technology struggle with the phenomenon that not only sense-making human subjects but also human-made mechanistic objects participate in chains of action<sup>8</sup>. Whereas sociology usually relies on coordinated interaction or communication of human actors as its matter of interest, studies of technology have to acknowledge that certain procedures of action can be put into manufactured things that perform as subject-free entities. In the case of the production of software - a specific kind of technology - such transcribing rules for performing effective action into an artifact is signified by the word 'coding'. By writing computer code does the programmer transcribe a certain program of action into the computer.

In a study situated in an engineering context, Downey (1998: 150f) describes the immediate factuality of such a transcription of performative competence that is called coding by engineers. He reports about Dr. Jayaram who had written a lengthy computer program to provide an intended functionality. After several years, when Jayaram had changed his work place, other programmers found it impossible to understand the code he had written. Turning to Jayaram for help they got the answer that even he, as the author of the program, did not know anymore how exactly it works. All involved humans, well-trained programmers and the original author of the computer program in question, did not have the understanding necessary to change and elaborate the running computer code. Jayaram, the programmer, had inscribed the ability to perform certain tasks into a computer program. The program continued to perform and to provide the desired functionality. However, it was not fully clear to the author and other professional programmers how the program achieved that. This transcription of performative competence begs the question of who it is that produces effects: the programmer - a human subject - or the program - a manufactured object. That issue arises not only for students of technology but also for the technologically interested scientists.

The situation is similar in the case of scientific computer simulation<sup>9</sup>. Researchers – not only sociologists of science interpreting their work – discuss who effects

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<sup>8</sup> The most influential explications of this point have been made by the protagonists of Actor-Network-Theory, e.g. Callon (1986) or Latour (1988b).

<sup>9</sup> The similarity between coding a functional piece of software and programming a computer simulation is also accompanied by dissimilarities. One lies in the fact that the engineer Jayaram could joke about his ignorance of how the code that he had written actually func-

the results that come out of running a computer model. Is it the programming scientist or the code-processing computer? In what follows, I show how conflicting positions were developed in the context of the Limits to Growth-study:

*What Limits to Growth-researchers write:*

”At the moment, our only alternatives to a model like this, based on partial knowledge, are mental models, based on the mixture of incomplete information and intuition that currently lies behind most political decisions. A dynamic model deals with the same incomplete information available to an intuitive model, but it allows the organization of information from many different sources into a feedback loop structure that can be exactly analyzed. Once all the assumptions are together and written down, they can be exposed to criticism” (129f)<sup>10</sup>

Although they share the problem of incomplete information, there is a crucial difference between mental and computer models: A mental model relies on the researcher’s intuition. A computer processes only the explicit input given by the programmer – in this case in the form of a ‘system’ of ‘feedback loops’. The computer forces the modeler to construct an explicit model that is visible to and therefore criticizable by other researchers.

There is another reason why computer models are valuable:

”The human brain, remarkable as it is, can only keep track of a limited number of the complicated, simultaneous interactions that determine the nature of the real world. ... [We have used] a formal, written model of the world. It constitutes a preliminary attempt to improve our

*What critics write:*

”The Sussex team points out that the apparent detached neutrality of a computer model is as illusory as it is persuasive; ... a computer model is only a mental model in more sophisticated guise” (Streatfield 1973: 210)

”This pessimistic [Malthusian] attitude has nothing to do with any modelling technique, but rather underlines a basic philosophy which the Sussex authors see as the major limitation of the Meadows approach.” (Streatfield 1973: 211)

The differences between mental and computer models are not as fundamental as it appears. A computer model is just an extended mental model, and is therefore subject to the same fallacies. This is obvious in the pessimistic attitude that underlies the model that was developed by Meadows and his colleagues.

This begs the question as to how biased judgements can be implemented in a machine that performs mathematical – and thereby presumably unbiased – calculations:

”It is sometime [sic] implied in discussions of the analysis presented in *The Limits to Growth* that the results are obtained by making the same assumptions about the world that other analysts make, but combining them in a computer-aided system. Systems analysis, it is suggested, is the superior method that yields the result. I do not find this assertion explicit in the book itself. It is incorrect. The assumptions of their model are not the assumptions

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tioned. In contrast, it is of utmost importance that the simulation scientist who wants to draw justifiable conclusions from simulation runs understands the details of the computer programs in use. This is exemplified by Evans (1997: 403f) who reports about a crisis that was caused by the unexpected and unexplained behavior of a computer model. It made researchers work frantically to find out why a change of allegedly no relevance in the model led to a substantive change in the simulation results. The crisis was overcome as soon as the researchers understood what in the simulation had caused the change.

<sup>10</sup> Quotes that do not give any further information about authors and year of publication always refer to the original publication of *The Limits to Growth* by Meadows et al. (1972).



mental models of long-term, global problems by combining the large amount of information that is already in human minds and in written records with the new information-processing tools that mankind's increasing knowledge has produced - the scientific method, systems analysis, and the modern computer" (26)

The computer model draws together the available information that people already possess and allows the modeler to explore the resulting implications. Integrating and processing the information in one large model is not a feasible task for the human mind. The computer however, can perform such information processing in an exact way using a large amount of information.

*Summary:*

Computer simulation results gain their credibility from combining (a) the explicit construction of the model and (b) the scope and precision of the calculations. In contrast to human reasoning the computer yields results that are easier to verify and more precise.

that other analysts make. They are peculiarly unrealistic ones. ...

Indeed, given these assumptions, any intelligent person would conclude in a minute that the system would end in catastrophe. ... No computer-aided analysis or any other elaborate analysis is necessary." (Hagen 1972: 12)

The key to uncovering the biased character of the results of the computer simulation is realizing that the assumptions in constructing the model are not the standard assumptions of economics. On the contrary, the assumptions made by Meadows and his colleagues are chosen so that the results necessarily follow. The simulation runs do not add any insight that could not have been derived from the assumptions by mental powers alone.

*Summary:*

The results presented in *Limits to Growth* are due to the assumptions made by the researchers. From these assumptions the conclusions can be derived in a straightforward manner. Therefore, the use of the computer model is superfluous and misleading.

Most contributions to the Limits to Growth-debate take the simulation as consisting of two parts: Human modelers make assumptions that define the model and specify initial and boundary conditions. The computer merely derives the implications of the model under the chosen circumstances. This separation of modeler and model is the basis of the experimental character of computer simulation. If the model were fully dependent on the modeler it would not be possible to replicate the simulation runs with that computer model (Evans 1997: 408). The autonomy of the computer model is what enables scientists to gain insights from exploring the space they construct in the modeling procedure. In this sense, computer simulation is experimental: it provides controlled surprise (Dowling 1999: 264f). Simulation scientists work on implementing the model in computer code. They fiddle around with the machine so that it becomes possible to scan systematically through the parameter space. After the scientists set up the computational model and all necessary variables, the computer takes over and determines the model's response to the specified parameter settings. While the computer does its work, the modeler can (only) watch to see what happens. The excerpts above show that even if this distinction between the model and the modeler, and the unbiased character of the computational operations, is taken for granted, there can be disagreement about what part of the simulation is to be held responsible for the simulation results.

The modelers take their assumptions to be essentially self-evident and consensual. According to them, the results of the simulation are due to the precise and complex computations executed by a machine that outperforms the human mind when it comes to calculating the evolution of a large number of interdependent variables over a long period of time. The modelers stress that without the possibility of performing a large number of computations in an exact way, the results could not have been found. Therefore, the computer model is regarded as the crucial agent leading to the conclusions of the book.

Those who oppose the simulation results claim that the assumptions made by the modelers were such that the expected results followed readily without much computational effort. In their view, the computer is only a humble servant to the modelers, turning the biased assumptions into computationally, i.e. apparently objectively derived, results. The computer model adds neither reliability nor quality to the results. Therefore, it is the modelers that are to be held responsible for the derived conclusions.

It should be mentioned that this conflict over who is responsible for simulation results, the modelers who make the underlying assumptions or the model that does the calculations, is not an old-fashioned, superseded one that was only relevant in the very early days of computer modeling. It was equally visible in the controversy over the explanation of scientific knowledge between sociologists and cognitive scientists in the late eighties (Slezak et al. 1989). Such a difference in locating the agency in the computer modeling can show up in all cases where computers are used to furnish scientific insights. This is true for conflicts between scientists who support computer simulation and scientists who oppose to computer simulation as a helpful method for scientific

work. It can also be relevant for different strategies that are used within the computer modeling community itself (Evans 1997).

There was a second line of conflict in the debate over Limits to Growth, which made critics doubt the believability of the results of the simulation study. It developed around the question whether the amount of empirical data invested in the model was sufficient or deficient. In the next section I turn to discussions about this issue.

### **3 How much data does a valid model need?**

WORLD2, an earlier version of WORLD3, was the last of a series of systemic, dynamic models that Jay Forrester developed (1971). It is the application of a general analytic approach<sup>11</sup>, called ‘system dynamics’, to yet another domain: first a business, then a city, and now the world<sup>12</sup>. The experience he gained from developing these earlier models allowed Forrester to make a rough sketch of a model of the world on a plane returning to the US from a meeting of the Club of Rome (Pestel 1971: 12; Elichirigoity 1999: 81-84). After all, it was he who had invented and applied the relevant systemic concepts. Writing down the WORLD-model presented the task of filling in values for the many variables of the model. This temporal order of the modeling procedure – first specify a set of interrelated variables and then fill in the empirical values – is characteristic of a theory-driven modeling approach. This has been pointed out by Evans in the case of economic modeling. Discussing the implications of the theory-driven modeling-approach he writes: ”This is not to say that developing a macro-economic model and using it to produce forecasts is a straightforward or easy task, but it is to say something about the sorts of problems that will be encountered. Principally, these will be technical problems caused by the limited amount of data available, the quality of these data, and so on.” (1997: 409) Such technical problems of how to integrate data into the model also showed up in the debate over Limits to Growth. They crystallized in the statement that only 0.1 per cent of the data required to construct a satisfactory world model were available (Freeman 1973: 8). Both, the modelers and the critics agreed on that. They differed, however, on the relevance of this fact for the argumentative power of the model. In addition to the line of conflict presented above about who is to be held responsible for the simulation results, one finds diverging views on the consequences of this uncontested statement for modeling global issues:

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<sup>11</sup> The researchers from MIT present ‘System Dynamics’ as a ‘method’ as well as a ‘theory’ (38).

<sup>12</sup> The very use of the name *System Dynamics* indicates that such an analysis cannot be characterized by a specific subject area. Systemic approaches claim to be general so that they can be adopted fruitfully in many subject areas.

*What the Limits to Growth-researchers write:*

”Questions of detail cannot be answered because the model simply does not yet contain much detail<sup>13</sup>. ... In terms of exact predictions, the output is not meaningful. ... The data we have to work with are certainly not sufficient for such forecasts, even if it were our purpose to make them. On the other hand it is vitally important to gain some understanding of the causes of ... our socio-economic systems when the limits are reached.” (102)

Detailed predictions cannot be made from a model that does not possess the respective level of descriptive detail. However, it is necessary to get a better understanding of current global developments.

How then can the not so detailed model WORLD3 help to gain such insights?

”[E]ven in the absence of improved data, information now available is sufficient to generate valid basic behavior modes for the world system. This is true because the model’s feedback loop structure is a much more important determinant of overall behavior than the exact numbers used to quantify the feedback loops. Even rather large changes in input data do not generally alter the mode of behavior, as we shall see in the following pages. Numerical changes may well affect the period of an oscillation or the rate of growth or the time of a collapse, but they will not affect the fact that the basic mode is oscillation or growth or col-

*What critics write:*

”The notorious lack of data stood in the way of an empirically founded quantitative specification of the relationships between variables.” (Harbordt 1972: 416)<sup>14</sup>

”Deficiencies are understandable, considering the many difficulties that were in the way of the modeling process; to a certain degree, they were unavoidable. It is completely inexcusable though that the authors disregard one fundamental distinction: the distinction between statements about the model and its behavior and statements about the real system to be represented. Of course, you can experiment with a preliminary, deficient, not fully tested model, ‘just to see what happens’. However, you may not present these conclusions as statements about reality – and this is what the authors constantly do.” (Harbordt 1972: 418)<sup>15</sup>

There is not enough empirical data available to construct and thoroughly verify a model of the world. Therefore, the claim that the model is an adequate representation of the world cannot be justified. Results from simulation runs need to be treated and presented with caution and not as hard and fast facts about the real world.

Is it possible to make informed guesses with a model that has not been validated empirically?

”Whether or not economic growth will continue, and at what rate, depends on the rela-

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<sup>13</sup> Note the ‘yet’ and other more explicit remarks on the same page, that promise: The future will see more detailed and precise models.

<sup>14</sup> In the original: ”[Der notorische Datenmangel stand] einer empirisch begründeten quantitativen Spezifizierung der Variablenbeziehungen im Wege”

<sup>15</sup> In the original: ”Angesichts der vielen Schwierigkeiten, die der Modellbildung im Wege standen, sind die genannten Mängel ... verständlich; in einem gewissen Ausmaß waren sie unvermeidlich. Völlig unentschuldig ist aber, daß die Verfasser eine grundlegende Unterscheidung mißachten: die Unterscheidung zwischen Aussagen über das Modell und dessen Verhalten und Aussagen über das abzubildende wirkliche System. Natürlich kann man auch mit einem vorläufigen, mangelhaften, ungenügend getesteten Modell experimentieren, ”um mal zu sehen, was herauskommt”. Nur darf man dann die daraus abgeleiteten Schlüsse nicht als Aussagen über die Wirklichkeit ausgeben – was die Autoren ständig tun.”

lapse.” (127)

The exact values for the variables in the model do not affect the model’s basic behavior. Its behavior mode is dominated by the structure of the system’s feedback loops. Analysis of this structure lays open the options for the future development of the system, be it dynamic stability or collapse.

*Summary:*

The available data and the model WORLD3 are not detailed enough to make accurate predictions. However, it is still valuable to examine a model of the world, since its structure – and not detailed numerical values – determines the possible behavior modes of the system.

relationship between the future rate of technical advance and that of the emergence of the problems to be met. The outcome is a question of a little more, a little less. No computer can aid us in making this forecast.” (Hagen 1972: 14)

Small errors in estimating the actual values of variables can cause relevant differences in the behavior of the system. Reliable predictions about economic systems are therefore impossible to make as long as detailed data are not available.

*Summary:*

Data of the necessary detail is not available to construct a valid model of global developments. This fact disqualifies all attempts to use a model to make forecasts since detailed empirical data decide future developments.

Modelers as well as critics were aware of the fact that most of the data necessary to give the world model an empirically substantive grounding was inaccessible. Indeed, other modeling attempts show that collecting data as input for computer models can be a challenging task that involves major organizational efforts<sup>16</sup>. Laws need to be made to allow the collection of such data, institutions need to be installed to collect the data in an adequate form, salaries of the people working at these institutions need to be paid, etc. Having performed simulation runs with WORLD3, a model that could not be validated sufficiently with empirical data, the question needs to be addressed what consequences can be drawn from these computational results. Answers to this question diverged among debating scientists:

The authors of Limits to Growth expect problems for the world system as it is approaching its limits. They aim to learn more about these limits with a model that relies on experts’ guesses rather than on measured values. According to them, this is feasible because there can be no doubt that the variables and relationships of WORLD3 are basic and essential ingredients of any world model. System Dynamics is a method that allows one to figure out the behavior modes that such a basic structure imposes on the evolution of a system. Working out the details can be left for future work. Replacing guessed values with measured quantities will not change those behavior modes, since they result from the structure of the system and not the inserted numerical values.

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<sup>16</sup> Van den Bogaard (1999: 311ff) reports some of the institutional difficulties that were involved in collecting data to be fed into a model of the Dutch economy.

Other scientists do not take the System Dynamics approach and a clear-cut set of ‘basic’ parameters for granted. They assume that there is not only one unique way of modeling the world. A modeling procedure does not necessarily start with the variables that the Limits to Growth-researchers call basic and it will not just refine the stable behavior mode dominated by the basic structure. Instead of accepting that the modeled variables and relationships dominate the overall behavior of the world system they call for an empirical verification of this claim. Furthermore they argue that small errors that enter the model through the informed guesses have the potential to fundamentally change future developments. Comprehensive and detailed data is therefore necessary for scientific modeling.

#### **4 The alignment of the conflict**

So far, two lines of conflict have been presented that separated debating scientists. They indicate yet another repetition of a constellation of controversy that Collins introduced to science studies (see e.g. Collins 1983). In such a Collinsian controversy, a limited number of researchers, the so-called core-set, negotiates the received view in a scientific field by confronting incompatible experimental evidence. These researchers face the experimenter’s regress, i.e. the fact that an experiment in itself can never prove a theory right (or wrong) since the interpretation of experiments always relies on theoretically based expectations. This regress allows for individually consistent but mutually diverging views on the same scientific question. Since the opposing positions are equally possible explanations of the same question, eventually some social mechanism has to come in to end debate and lead to the closure of the controversy.

The controversy over Limits to Growth was different in various respects from such a Collinsian controversy. The least consequential difference seems to be the following: Whereas Collins is concerned ‘with controversies which involve experiment and observation’ (1981: 8) the Limits to Growth-study relies mainly on results from computer simulation. Even though one can argue – as has been done above – that computer simulation is also an experimental activity, it is obvious that Collins had empirical evidence, traces of ‘nature’, in mind when he wrote about the necessity of experiment and observation. However, recent studies seem to indicate that strictly Collinsian controversies over computer simulation (Evans 1997) and mathematics-based theorizing (Kennefick 2000) are also possible. It should be possible to extend Collins’ notion of controversy beyond the realm of the strictly empirical to activities that involve the interpretation of observations, regardless of whether the observed is ‘nature’.

A far more serious deviation of the debate over Limits to Growth from a Collinsian style controversy becomes visible if one takes a closer look at the notion of core-set: According to Collins, the core-set includes all scientists ‘who are actively involved in experimentation or observation, or making contributions to the theory of the phenomenon, or of the experiment, such that they have an effect on the outcome of the controversy.’ (1981: 8) He further elaborates that both active involvement and having an effect are necessary preconditions to make a scientist an element of the core-set (1981:

8, footnote 8). In the controversy over Limits to Growth, the core-set, as Collins defines it, would consist of the research team at MIT (Meadows et al. 1972) and the research team from Sussex, England, that set out to analyze the simulation study in depth some time after MIT's study had been published (Cole et al. (ed.) 1973). These were the only parties that had conducted research specifically to answer the question of whether it is feasible to make statements on the future dynamics of the world using a computer model and whether the predictions made in Limits to Growth are sound<sup>17</sup>. Where critical comments on Limits to Growth were commonplace, original research to check the promoted results was rare. Since the research-group from Sussex took action only after Limits to Growth had been out and since they published their results only in 1973, early in the debate, Gabor could make the following point:

“The rational way of rejecting [Forrester's work] would be to show that it is so critically dependent on the model, and on the policies which are fed into it, that its predictive value is nil or less than that of intuitive forecasts. To my knowledge this has never been attempted, by any of the critics. Instead they reject it out of hand, intuitively, with angry aspersions at the author and the Club of Rome.” (1972: 109)

In fact, the majority of scientists who wrote about the results presented in Limits to Growth, had *not* done any research to give different and conflicting answers to the questions raised in Limits to Growth. For most of them, it was neither viable nor desirable to attempt something comparable to the simulation study. The research project had sought to answer questions that many other scientists did not consider answerable. Following the above definition of the core-set, most discussants would have to be ignored in a sociological reconstruction of the controversy. Since those researchers, who are *not* elements of the core-set, did have a strong impact on the scientific controversy over Limits to Growth, it will not be pictured as a Collinsian controversy. Rather, the debate suggests a different descriptive vocabulary. In this case, it is more helpful to say that the results presented in Limits to Growth were perceived by social scientists as an *intrusion* into the domain of expertise that was occupied by social science. This intrusion forced social scientists to react in one way or the other. In the following I will describe social scientists' reaction to the intrusion of the System Dynamics researchers. The terminology of 'intrusion' is adequate to describe the debate around Limits to Growth because it emphasizes two important aspects:

First, the intruders were perceived as being different and using different methods from what was conventional in the social sciences. Researchers and methods came from a technical background that had no reputation of having worked successfully on topics of social sciences before. From the point of view of social scientists these scientists applied 'electrical engineering and servomechanism principles to social systems' (Shubik 1971: 1014). Additionally, social scientists claimed that the simulation study neglected the current state of the art in the social sciences and drew its inspiration ex-

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<sup>17</sup> Bloomfield (1985), who studies controversy in the context of the Limits to Growth-debate, turns to a conflict between exactly these parties.

clusively from the methods that Forrester had brought from MIT's servomechanisms laboratory. As Shubik continues to complain:

"The application of careful dimensional analysis and the specification of good measures in the right dimensions are all difficult and critical. For instance, what is a measure of 'welfare'? Even given a measure, what is its operational significance? Answers to questions such as these call for an intimate knowledge of subjects such as economics, sociology, psychology, and political science. Why are so few social scientists referred to by Forrester? Are there none whose knowledge is worth considering when building models of social, political, economic processes?" (1972: 110)

Second, the intruders came equipped with means that made them confident that they had a superior grasp of the matters in question. Here, computer simulation plays a leading role in the debate. These superior means consisted of computer simulation as the crucial element to enable the intruders to study global issues. As has been discussed above, the reasons for this claim were twofold: (I) Computer models were supposed to be clearer, more comprehensive, and more exact than mental models. (II) System Dynamics could do what other methods could not do: make valuable forecasts on behavior modes even in the absence of detailed quantitative data.

Technical researchers intruded the field of social science with the help of computer models. They presented themselves as using methods that were different from and superior to those of social scientists. How can social scientists react to such an intrusion? Various options are possible: indifference, incorporation, or repulsion of the intrusion. I will discuss these options in turn:

First they might deem it unnecessary to react visibly at all. Total indifference to the claims made in *Limits to Growth* would be a way of maintaining that there is simply no point in taking those statements seriously. The methods as well as the conclusions would be portrayed as utterly beyond reason, the mistaken opinion of a marginal minority. However, there was one strong reason why indifference was hardly a feasible option for social scientists in the case of *Limits to Growth*: Its immense success in the public media. Discussions about the looming collapse of modern society had become commonplace in the wake of *Limits to Growth*<sup>18</sup>. If the public takes statements seriously that suggest that there is a way to find out about global issues that is different and superior to conventional approaches in the social sciences, then social scientists have every reason to get involved in the debate. Otherwise they run the risk of losing scientific authority<sup>19</sup>.

The other possible reactions to the results of *Limits to Growth* urge social scientists to engage in the discussion of the simulation study. Attending to the immense publicity

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<sup>18</sup> Harbordt (1972: 421, Fn. 21) gives some examples how the statements from *Limits to Growth* were used in political, economic, and even theological arguments. A quick look at the headlines of leading newspapers at the time reaffirms the impression that a major public debate was taking place.

<sup>19</sup> Similarly, Collins and Pinch (1982: 41-43) were surprised by the vigor with which paranormal work was attacked by academic researchers. They argue that the positive motivation of getting public attention was the main reason for scientists to spend resources on arguing against the results from parascientific.



of Limits to Growth and using the publicity for the purposes of the social sciences could lead either to an incorporation or rejection of its conclusions and methods.

Incorporation would imply that social scientists and the System Dynamics researchers understand themselves as having common scientific interests and as applying mutually benefiting knowledge. Social scientists could have made the System Dynamics researchers a part of their own scientific endeavor and supplemented their conventional approaches with the technique of computer modeling. Latour (1988a: 59ff) attributes the continuing success of Louis Pasteur to the successful application of exactly this kind of interaction of actors. When Pasteur ‘rushed into previous bodies of knowledge’, i.e. when he intruded into the field of e.g. public hygiene, he kept his ‘laboratory practices that were different enough to render irrelevant the colleagues who were already engaged in those disciplines.’ (Latour 1988a: 69) According to Latour, the crucial element in Pasteur’s amazing success story is to having found allies that would support him. He ‘imposed on them a way of formulating [a] demand to which only he possessed the answer’ (Latour 1988a: 71), and thereby turned hygienists into Pasteurians. Pasteur was perceived as a substantial help for hygienists, as their ‘advocate’. In the case of the debate over Limits to Growth social scientists reacted mainly critically and did not perceive the System Dynamics researchers as their advocates<sup>20</sup>.

In the case of Limits to Growth, intrusion into the domain of social science expertise was mainly counteracted by repulsion. The institutionalized experts on questions of global economic, political, and social evolution did not let computer modelers get away with claiming competence on these topics without paying attention to the social sciences. Social scientists defended their field by arguing against the claims of the computer modelers, that System Dynamics is (I) *no more convincing* than traditional approaches in the social sciences. The use of computer simulation does not free research of value-based judgements since it is the underlying assumptions of the model that are crucial, just as in the more traditional mental models. Furthermore, social scientists explained why System Dynamics is actually (II) *less convincing* than traditional approaches in the social sciences, because the choice of the model characteristics cannot be justified by empirical verifications. The kind of detailed data that is crucial for a reliable understanding of long-term processes, is simply not available.

## 5 Conclusion

This study is an invitation to reconsider the debate over Limits to Growth as a case where one can learn about how computer simulation is situated in scientific discourse. The controversy exemplifies one strategy regarding how to deal with an intrusion into a field of disciplinary expertise: repulsion rather than indifference or incorporation. Computer simulation plays a prominent role in the debate by providing the intruders with visibly different and presumably powerful means to make the intrusion potentially

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<sup>20</sup> Alternative positions were formulated in letters to editors of scientific journals, e.g. Gabor (1972); Hardin (1972); Hemond et al. (1972). Limits to Growth became the advocate not of a scientific field but of a political movement – environmentalism.

successful. Therefore, it is not surprising that computer simulation became one of the main topics of the debate. Undermining the believability of computer simulation would have been equal to a successful repulsion of the intruders. The two main points of discussion for technical and social scientists concerned the questions where responsibility for the simulation results were to be found and what importance detailed data had for the validity of the model.

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