Global Modeling: Origins, Assessment, and Alternative Futures

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Simulation Gaming 2000; 31; 50
DOI: 10.1177/104687810003100105

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This essay reviews the origins of key concepts used in most global models. Philosophical preconditions for validity are examined. A framework for critically evaluating existing global models is suggested. A philosophy for global modeling is outlined. Global modeling is a much deeper enterprise than its critics seem to be aware. However, because global modeling lacks an academic or institutional home, key issues that have arisen with early global models are left essentially untouched. To critique global models or global modeling meaningfully, at least three perspectives are essential: those of science, philosophy, and practical application (praxis). A firm foundation for global modeling requires that its practitioners adopt an academic paradigm (i.e., university research institutes, schools and departments, a professional association, and a professional peer-reviewed journal). Global modeling is the only methodology capable of helping humanity to self-consciously envision itself and its environment in a time frame long enough and a scale large enough to provide an effective guide in its transition from a global to an interstellar species.

KEYWORDS: *decision making; global models; instability; international relations; simulation*.

Origins of Global Modeling

Despite the proliferation of global modeling and models, most people doing simulation and gaming are in fact unaware of this development. The history of global modeling paradoxically suggests that the same may be true among global modelers in terms of their own political and cultural roots. What are the nature and purposes of global modeling? To my knowledge, efforts to present answers to these questions in the past have been focused on particular threads of development and particular purposes associated with specific projects rather than focused on the field as a whole. So the first task is to acquaint this audience with that history, albeit as briefly and as cursorily as space dictates. After this review, we will be in a position to evaluate the field critically and explore the merits of alternative directions for its further development.

A Definition of Global Modeling

What is global modeling? My old professor Donald Campbell used to remark that the best way to traumatize a discussion is to ask people to define their terms! For the

SIMULATION & GAMING, Vol. 31 No. 1, March 2000 50-73

sake of offering some focus, let us assume that for a model to be "global" in scope, it must be meant to characterize some features of human thought, behavior, and the human environment that are held to be typical if not universal across cultures and history. Specifically, it must represent these features in logical and mathematical forms amenable to describing dynamics. It must offer a causal explanation for the interactions modeled. It must be "testable" in the sense that it can be edited through empirical investigation. And it must be intended to be useful in practical application, that is, have variables or parameters that a user of the model can identify as factors amenable to being manipulated to produce desired effects in some aspects of the world being modeled. By contrast, let us consider a very good alternative definition. Brecke (1995) suggests the features of global models to be the following: geographically global in scope, of long duration (25-50 years), and integrative of diverse sectors (population dynamics, economic dynamics, politics, and the environment). I prefer a fuzzier conceptualization. Hickman & Klein's (1998) Link model, Leontief's WIOM (Leontief & Duchin, 1983), and Onishi's (1977) FUGI model are primarily economic and shorter term, for instance, and would seem not to be included in Brecke's definition. The Meadows team's WORLD3 model (D. H. Meadows, Meadows, Randers, & Behrens, 1972) excludes political dynamics, and its treatment of economics as a single world system obviates the need to represent trade. Bremer's (1987) GLOBUS model excludes environmental considerations. I have purposely left the features of time frame, geographic coverage, and scope of coverage out of the definition to accommodate the wide variety of global modeling taking place. Instead, I have focused on scientific universality, logical and mathematical construction, explanatory power, testability, and practical, applied usefulness as characteristics of this type of simulation and left the geographic and sectoral scope and time horizon as issues to be addressed rather than as criteria for exclusion.

Let us see how well this rather complicated definition holds up not only as a descriptive but also as an assessment tool by reviewing some works commonly labeled "global models." First, however, let us examine some intellectual roots and precursors to see whether "global modeling" has been in existence longer than commonly supposed in a recognizable, albeit "pioneering," form.

Lewis Fry Richardson (1880-1953) and Arms Race Modeling

The origins of global modeling are commonly dated with the Club of Rome's support of Jay Forrester's (1971) creation of WORLD2. But there is reason to extend this date back decades, at least to the turn of the century. The system dynamics approach to modeling is much earlier and—as applied to several global systems, war, and weather—begins with the work of Lewis Fry Richardson. Richardson was a Quaker who earned a living as a respected meteorologist in the United Kingdom and whose equations for atmospheric churning were used up until very recently. By 1913, however, his interests were broadened and applied to include making an effort to forestall the "Great War," World War I. He had completed a comprehensive statistical and mathematical study of war. The essence of his understanding is formulated in a simple family of equations:

$$dy = ax - by + c,$$

where y = change in Y's resource allocations for coercion, defense, or war; $x = X$'s current allocations (X is one or more opponents or target groups opposing Y, from Y's perspective); a = "fear," produced by the difference in two factors: conflict – cooperation; b = "fatigue," the result of competition for resources given other values; and c = "ambition," "revenge," or other motivations endogenous to a leadership group, Y.

Now you might think as Rapoport (1957, p. 297) did that sets of such equations, used to indicate the direction and properties of arms races, implied a deterministic philosophy ("gross determinism of the classical physical type assumed by Richardson" [Rapoport, 1957]). The type of equations Richardson used are certainly that in form and reflect the prevailing philosophical view in Richardson's youth that human society might lend itself well to being modeled by such equations. Richardson himself, however, introduces his philosophy and equations in quite a different way:

> Critic: Can you predict the date at which the next war will break out? Author: No, of course not. The equations are merely a description of what people would do if they did not stop to think. . . . They follow their traditions . . . and their instincts . . . because they have not yet made a sufficiently strenuous intellectual and moral effort to control the situation. The process described by the ensuing equations is not to be thought of as inevitable. It is what would occur if instinct and tradition were allowed to act uncontrolled. (Richardson, 1960, p. 12)

We certainly cannot conclude from this that Richardson was philosophically a determinist. His equations are deterministic in form because that was what he had available to him to represent his ideas formally, not because he believed in the inevitability of a particular outcome given sufficient information. He did not attempt to quantify "sufficiently strenuous intellectual and moral effort," but rather to transform the thinking of the leaders of his day to avoid what would otherwise be an inevitable catastrophe. In 1913, he published 300 copies of his book at his own expense and sent it to world leaders with a warning that they were headed toward war and should do something about avoiding it.

It took more than 50 years for global modelers to begin adopting equations similar to his, but they still have not adopted his philosophy. For instance, in an early evaluation of the precursor to GLOBUS, Bremer (1977) examined some possibilities as to why his SIPER simulation exhibited a tendency to produce arms races:

> Three possibilities seem worth considering. First, it may be that we have successfully captured the essence of the decisional calculus decision-makers use in making their assessments of national security needs. . . . A second possibility is that some fine-tuning is required. That is, the decision processes are correct but some of the parameter values are in error. A third possibility is that the processes in this part of the model are wrong in a fundamental respect. For example, the acquisition of armaments . . . is largely attributable either to the needs of the military-industrial complex, or to the defense bureaucracy specifically. (p. 205)

His examination of the SIPER model's behavior is limited to achieving a good fit to real-world behavior, to what Richardson referred to as "habit and instinct." The usefulness of the model is as a way to "gain insights into the over-all impact of environmental factors upon the behavior of nations." It is "a means of integrating new knowledge into a coherent and consistent framework, making it possible to give a preliminary assessment of the larger and long-term implications of a new discovery" (Bremer, 1977, p. 208). The "strenuous intellectual and moral effort to control the situation"—Richardson's raison d'être for modeling was to guide such an effort—lies entirely outside the paradigm. Bremer's paradigm at the time—and I do most certainly appreciate this; this is not a criticism—was that of science. Science ever seeks, through data generation, empirical analysis, theoretical inference, and logical reconstruction, to reduce gaps between models of the possible, generated by theory, and models of the real, as made operational through data generation. The limitation of this approach—widely accepted—is that it does not and cannot address the philosophical and practical problems that motivate its supporters to fund such efforts.

Wassily Leontief (1906-) and the World Input-Output Model (WIOM)

Leontief's 1973 Nobel Prize in economics for modeling economies is in a manner analogous to S-R psychologists' (popularly characterized) "rat experiments." S-R models and I-O (input-output) models are formally identical. $Oi = f\{bijOj\}j = 1, n$, where Oi is a unit of output, i; Oj is a set of inputs in the production process; and bij is the set of weights in a series of linear, homogeneous, simultaneous equations—for instance, a typical multiple regression equation. The matrix of bij-coefficients is expected to vary from time to time as new technologies and other factors change the types of inputs, outputs, and relations between them. They are not expected to change so much so rapidly as to make any given set of coefficients useless for forecasting purposes. Also to be noted is that the output can be a service as well as a good; therefore, it is possible to say that the behavior of people as well as the quantity of goods is predictable, contingent on a given application of inputs.

This way of thinking, also as formally deterministic as Richardson's, lies at the core of all global models representing national and global economies. For example, Onishi's FUGI (Future of Global Interdependence, financed by the Japanese government), the British SARUM (Systems Analysis Research Unit Model), and of course Leontief's own WIOM (Leontief & Duchin, 1983), announced in 1977 as the United Nations (UN) World Model, all include such I-O models at their core.

In an application to arms production estimates aimed not at warning of war potential or likelihood but at economic performance, Leontief and Duchin (1983) suggest a number of scenarios under which consumer goods per capita could increase or decrease depending on rates of military spending on armaments. Note that such a model could have included Richardson-like equations but did not. Also interestingly, although this study makes projections to the year 2000, no scenario considers the economically destructive uses to which arms might be put. Nor is the survivability of any of the actors considered (e.g., the possible disintegration of the USSR). Furthermore, all applications assumed the same I-O coefficients for all arms produced in all countries. Leontief and Duchin justify this particular assumption as follows:

> Considering the significant increase in the overseas transfer of U.S. military technology since the early 1970s and given the paucity of information on military technology in other countries, it was assumed that the same input structures held for all regions. The same technologies were also assumed for future years. (p. 15)

The limitations of this type of analysis seem clear enough. Data are acknowledged to be incomplete and to some extent incorrect, with consequentially poor parameter estimates for the model. Choices of actions are therefore based on reasonable but likely incorrect speculation. The accuracy of projections depends in part on policies remaining constant and other general cateris paribus caveats that accompany all such applied research (e.g., the requirement that applied technology remain constant).

Unlike purely scientific research, illustrated by Bremer's early work, this type of modeling gives a clear and central place to policy choices and consequences. Nevertheless, questions can be but are not raised as to which equations and which parameters represent historically specific and ungeneralizable situations and dynamics and which are more broadly applicable.

There seems to be a paradigm limitation as regards what is fair to ask of this type of research, that is, to what standards it should be held accountable. Research reports themselves are usually mute on this subject. Results and how they were achieved are simply presented. This style of presentation is characteristic of applied research or praxis. It is meant to achieve a particular, applied purpose. Leontief and Duchin's (1983) work was supported by the U.S. Arms Control and Disarmament Agency and later published under UN auspices. Such work is part of a larger enterprise, namely, policy advising. William Webster, former head of the CIA, stated a succinct paradigm for such applied research in the context of describing work in the National Security Council:

> Questions are asked: Can we do it? Can we do it lawfully? Are the benefits worth the cost? And subjectively: Is it a reasonable project? Is it consistent with American values? (Quoted in Maas, 1991, p. 5f)

In such a context, refinements of science and philosophy have no priority per se. What is a priority is performance: making a contribution to achieving goals consistent with legal and moral criteria and a high benefit/cost ratio in terms of desired ends such as survival and security, as well as means to these ends such as power and wealth. Issues such as how rapidly are the variables and coefficients changing in the real world represented by an I-O model are of technical interest only. Issues such as whether habit and instinct will dominate over strenuous moral effort have no priority per se; in the context of strategy and tactics, it is only the expected value of an outcome, a situation resulting from a policy or action, that matters. This explains why Leontief and Duchin (1983) seemed less concerned about technical refinements of their applied research than they were with making definite estimates of arms spending and the effects of reduction in spending levels. In a policy development context, one must be disciplined to contract deadlines and the limitations of available resources to reach an acceptable conclusion.

Harold Guetzkow (1915-), the Inter-Nation Simulation (INS), and the Simulated International Processes (SIP) Project

It is my understanding from many personal dialogs with Harold Guetzkow (he was the chair of my PhD committee in the 1960s) that he was as concerned as Richardson was with preventing war, although he was and remains far more circumspect than Richardson was. In Guetzkow's case, I surmise it was a concern with the spread of nuclear weapons and the likelihood of nuclear war or nuclear blackmail, or what Herman Kahn (1960) came later to call "spasm" nuclear war. Decision sciences had not developed adequately to study these or similar problems, much less to help teach leaders how to avoid global nuclear war; for this and other reasons, Guetzkow embarked on a three-decade effort to promote modeling research that could be of such use. The major product of this effort was the first large-scale foreign policy simulation tool, the Inter-Nation Simulation (INS), reported on in his book, *Simulation in International Relations* (Guetzkow, Alger, Brody, Noel, & Snyder, 1963) and by his then doctoral student, Richard Brody (1963). Guetzkow's promotion of global modeling—in this case, political systems modeling—was motivated neither by a desire to advance science per se (though as a scientist, he certainly had that interest) nor by a specific application of already proven science, but he was motivated by a passion for removing a fear. Specifically, this was the fear of world nuclear annihilation.

Although the methodologies employed (mathematics, statistics, data generation, empirical analysis) shared much in common with the other paradigms (science and praxis), their application raised philosophical issues the others did not. In what sense, for instance, is the INS a simulation, a simulation of what? And how would one test this assertion? Guetzkow characterized simulation as a "reduced and simplified form" of some phenomenon. But how would one know whether the reduction and simplification resulted in a product that was not analogous to the real phenomenon in ways too important to ignore? Lacking adequate theory to apply to the problem being addressed and lacking adequate data to edit or "test" a developing theory of decision making, how could one answer these questions?

It would at first appear that Guetzkow's simulation enterprise was, in a fundamental sense, a philosophical rather than a scientific enterprise. Consider that the goal of science broadly conceived is to reduce dissonance between models of the possible (theory application) and models of the real (data gathering and organization). Consider that the goal of praxis (practical application) is to reduce dissonance between models of what is desirable (derivative of prevalent culture) and of the real as given by data. In a parallel construction, then, one can envision the aim of philosophy, again broadly conceived as to reduce dissonance between culture-generated models of the desirable and theory-generated models of the possible. Guetzkow was trying to find a way to make possible and even likely what was desirable, minimizing the likelihood of nuclear holocaust. But he recognized that there was no theory sufficiently developed to guide him. And there was no way to do the basic research on decision making that seemed so necessary. Snyder, Bruck, and Sapin's (1962) then contemporary text, *Foreign Policy Decision Making*, for instance, laid out a research agenda that included hundreds of variables in as yet unknown, unquantified, untested relations with one another.

My understanding is that Guetzkow reasoned that if you could create a decision-making environment for people who in some sense shared the culture of real decision makers, you could "black box" the decision process, much as Leontief did with economies, by using an S-R-like I-O model. You would have inputs to the decision process resembling inputs to referent system ("real-world") decisions and outputs resembling referent system outputs. In a sense, Guetzkow's simulation was a collective "mental experiment" played out by real people in their heads rather than in Guetzkow's head.

Yet, the very construction of the INS simulation went beyond the "mental experiment." Acculturated human beings—naval petty officers in the Western Behavioral Sciences Institute in La Jolla, diplomats at Arlie House, high school students from the Chicago area, college students at Northwestern University—were doing things, using the language and implements, that were similar to the real thing. Thus, questions could be raised about "verisimilitude," and standards such as "passing the laugh test" ("face validity," as Hermann [1967] said) could be put forth, and no one laughed too loudly or long. The object was to develop a theory that would tell us nuclear war did not have to occur and then a practical strategy for preventing it. Terms grew up such as "islands of theory," for which Guetzkow (1950) is today widely quoted. "Islands of theory" embedded the hope that links between decision making, small group dynamics, and systems theory in international relations would eventually be invented, and those bridges could ultimately result in desired explanations as to how and why nuclear holocaust could and perhaps would be prevented.

That this was an enterprise that hinged on the success of a new philosophy for empirical investigation was perceived by some. The relations between science, philosophy, and praxis or applied policy research, as paradigms, were not carefully examined in those terms, however. Many articles were written trying to come to grips with some of the questions raised above (e.g., Campbell, Raser, & Chadwick, 1970; Chadwick, 1972; Hermann, 1967). These focused on facets of the enterprise that were too limited in scope, such as the comparison of "simulation data" with real-world data. Simulation data were treated as if they were generated from an experiment (Brody, 1963). But whether such data were from a simulated experiment or from an actual experiment was ultimately unclear because the question of whether the phenomena constituting the simulation were representative of the phenomena theorized about remained unanswered. We were told simply to wait and see if what was predicted would actually occur (Brody, 1963).

To complicate this philosophical and empirical issue, Guetzkow's INS began to be absorbed by representatives of the very subculture it purported to simulate. It was the inspiration for William Coplin's (1968) World Politics Simulation (WPS) used at the U.S. Department of State's Foreign Service Institute when Coplin was at Wayne State University. It was the foundation of Charles Elder's WPS II (Elder, 1969), used at the Industrial College of the Armed Forces (ICAF). It was modified and extended by Paul Smoker (1981) in his design of the International Processes Simulation (IPS, which embodied a major revision of the INS and served for simulation "experiments" similar to the INS use). And it inspired a "factbook" effort for a simulation project I participated in at System Development Corporation in 1967-1968 under a DARPA contract to Gerald Shure.

These developments are precisely what one would expect if a new philosophy was catching hold. INS-type simulations were entering the education programs of the military, intelligence, and diplomatic communities. In a small and fleeting way, perhaps, the culture of the real world was changing in response to the new philosophy of simulation. Although there were serious efforts at grounding the INS equations in empirical analysis and tests (e.g., Bremer, 1977; Chadwick, 1967, 1969, 1972; Elder & Pendley, 1981; Smoker, 1981), this work had virtually no impact. Indeed, the major early conclusions were that the INS equations needed to be thoroughly overhauled, but they were nevertheless used for another decade until Bremer abandoned his SIPER model in the early 1980s as a core model for GLOBUS (see below).

Guetzkow himself ceased work on human computer simulation in favor of the traditional all-computer modeling methodology that had grown up around him during the decades of the INS. The INS began when only mainframes existed and complex calculation was still very, very tedious. It ended about the time of the advent of the PC. Today, it is possible to redesign INS-style games that are global and interactive via the Internet. The beginnings of such may be found at the University of Hawaii in my international relations simulation, currently on the Web, used in an educational rather than research or policy development context. In research and policy development contexts, models of decision making, some Richardson-like, have replaced the human players. In others, the decision processes are represented primarily by parameters analogous to keys on a modern analog to the old player piano, a clavinova. The user decides the variations on the tune (policy), but in the absence of an exogenous decision, a default tune (policy) is there to be played. We will now discuss these newer models.

Lawrence R. Klein (1920-) and the Link Project

A 1980 Nobel Prize winner, Klein conceived of the Link Project in 1968, under a Social Science Research Insititute grant at Stanford University; the Link Project started shortly thereafter at the Wharton School of Business at the University of Pennsylvania. By the 1980s, the project had become a massive econometric enterprise situated at the University of Toronto and in the UN headquarters. The project involves literally hundreds of economists worldwide and is used commercially through WEFA, Inc. (formed in 1987 as a merger between Wharton Econometric Forecasting Associates, founded by Klein, and Chase Econometrics). Klein's unique contribution to global modeling was to invent methods by which more than 75 highly disparate national economic models could be linked in their trade, exchange rates, and financial exchanges to make projections for national and global trade, inflation, and gross national product (GNP) growth. The Link Project regularly discusses these forecasts at meetings twice a year, once in New York and once in a participating country. Results function in decision-making contexts essentially the same way weather forecasts do (i.e., for contingency planning).

As successful as this type of modeling is in terms of user acceptance in government and industry, one is tempted to ignore the obvious shortcomings. The errors of omission are glaring. National econometric models generally do not model social and political conflict and change explicitly. And, vice versa, models such as the INS and its derivatives modeled economic factors in only the most cursory of ways. Neither gave attention to environmental factors.

Akira Onishi (1929-) and the Future of Global Interdependence (FUGI) Model

Akira Onishi's FUGI model was developed by 1977 and has since been very substantially expanded and researched at the Institute of Applied Economics at Soka Gakkai University in the Tokyo area, principally under MITI funding. As mentioned earlier, it included a Leontief-like I-O model for national economies. But unlike most of the others, it also has a linear programming component enabling the projection of alternative futures under different criteria for optimization—say, an automotive or a telecommunications industry. Today, the model represents the economies of more than 200 countries as regards gross domestic product (GDP), population, labor force, investment, and trade and continues to be used in government and industry planning processes.

Jay Forrester (1918-) and the 1971 World Dynamics Model; The Dennis Meadows Team: The *Limits to Growth* 1972 WORLD3 Model; and the 1992 *Beyond the Limits* Update

Jay Forrester, the inventor of the magnetic core for mainframes in the 1950s, was invited by the newly formed Club of Rome in 1969 to try to represent basic features of the world economy in the context of population dynamics and resource availability in agricultural land, energy, and minerals. His book, *World Dynamics* (1971), went almost unnoticed until his protégés' work, *The Limits to Growth* (D. H. Meadows et al., 1972), made its public and alarming debut. With these works, the application of system dynamics modeling finally took hold in the field of global modeling.

It is often said that Forrester invented the system dynamics approach (Edwards, 1996, p. 152; Forrester, 1989, p. 6). Yet even the most cursory examination of Richardson's work, discussed earlier, reveals that this type of thinking was already in its early stages at the turn of the century. Richardson's "reaction curves" and differential equations clearly showed as early as 1913 that the interaction of policy decisions and their effects over time could result in stable or unstable equilibria unintended by the participants.

And in business and industrial applications, Walter Shewhart's (1931) control charts, used by his friend W. Edwards Deming (1986, 1991), were already applying the system dynamics insight in a workable management philosophy from the 1920s on. The efforts continued exceedingly successfully in war industries during World War II, as well as in Japanese reconstruction beginning roughly from 1950. (Deming, himself a physicist and contemporary of Forrester, died in 1993, widely recognized for his contribution to management philosophy in business in Japan and the United States, based on system dynamics principles.) Similarly, Leontief's WIOM (Leontief & Duchin, 1983) explicitly uses feedback loops linking outputs from his I-O matrix to inputs to assess medium-term trends. It is certainly true that up until Forrester's work, none of these modeling efforts applied system dynamics methodology to the world's physical resources. This, however, may well be due primarily to the coincident advent of the digital "mainframe" with Forrester's invention of the magnetic "core," transforming his WORLD1 (a paper-and-pencil model) to WORLD2 (programmed in DYNAMO, a system dynamics modeling language of Forrester's invention).

The notoriety the Meadows team received was the result of excessive attention given to what were tentative conclusions from a model that was, in their own words, "imperfect, oversimplified, and unfinished" (D. H. Meadows et al., 1972). But there were several reasons for the unwanted attention in my judgment, despite their careful caveats, some not so clear at the time. It was true that the Club of Rome, seriously concerned about the tendency of the world system as modeled to drift into collapse by the mid-21st century, freely circulated thousands of copies of *The Limits to Growth* to national leaders worldwide. I suspect it was the Club of Rome's publicity efforts that attracted much of the undesirable, critical reviews (e.g., in the *New York Times Book Review* and in *Science*, *Nature*, and *Science News*). Much of this criticism was hurried and misplaced, creating public controversy. Yet this may also have served as a smokescreen, intentionally or not, as a deflection of public attention away from a very fearful and more than potential problem—namely, global economic collapse and massive and large-scale death. This inference is at least consistent with two developments: first, the sudden and quietly expanding interest among governments' military and intelligence communities in global modeling, discussed below, and second, the end of this type of criticism, that is, the sudden lack of such criticism of the Meadows team's successors.

Peter C. Roberts and the Systems Analysis Research Unit (SARU) Model; Gerald O. Barney and the *Global 2000 Report to the President*; Mihajlo Mesarovic and the World Integration Model (WIM)

The large number of global modeling projects that immediately followed the *Limits to Growth* work suggests that they were taken far more seriously than their critics suggest. In the United Kingdom, the Systems Analysis Research Unit (SARU) was put together from staff at the Departments of Environment and Transport at the request of the U.K. parliament's newly formed World Trends Committee. SARU created a global model, SARUM, in 1975, outputs from which were used by the Interfutures Project at the International Institute of Applied Systems Analysis (IIASA) in the late 1970s. I am told that much of SARUM's applications still remain classified. Given the United Kingdom's vulnerability to disruptions in international trade, this is understandable. To treat the U.K. situation usefully in such a model, it and countries of interest to it would have to be made distinct entities in a global model. Hence, SARUM represented about a dozen world regions, one of which was the United Kingdom.

Half a decade later, SARUM was substantially revised through my direction of the Global Models and the Policy Process (G-MAPP) Project at the East-West Center in Honolulu. I was appointed its coordinator from 1981 to its end in 1983, by Sumi Makey, then director of their Office of Student Affairs and Open Grants, thanks to the recommendation of Dr. Don MacRae, the G-MAPP Project's first coordinator (this is probably the first time in history that an interviewee in a survey came to be the project leader!). MacRae, then head of Australia's Environmental Studies Branch, supplied the G-MAPP Project with a version of SARUM, and thanks to the work of Dr. Paul R. Williamson (PhD, University of Hawaii, 1978), more than 60 errors in the SARUM source code were found and corrected. It was then further modified and documented as the Australian Resources and Environmental Assessment (AREA) model in the Environmental Studies Branch, Department of Home Affairs and Environment in Canberra (Poldy, 1986). Among these modifications of SARUM/AREAM under my supervision was the inclusion of Richardson's arms race equations; this was designed in Canberra and completed during my trip to the World Model Project at the University of Groningen in 1984; some preliminary applications were reported by Gigengack (1987).

Earlier during this same period (1970s), Gerald Barney was commissioned by President Carter to investigate world systems dynamics and produced a multivolume report, *The Global 2000 Report to the President of the U.S.* (1980). Barney spent about $3 million of U.S. government money putting together a global trends analysis instead of a formal global model for the Carter administration. By going to various U.S. government departments, he was able to take output data estimates from one model and use that as input for others, eventually to arrive at trends that again looked ominously like those of the Meadows team's original work. After the publication of his multivolume work, little more was heard about it or its impact, probably due to the Reagan administration's distaste for long-term planning. Also during this period, the Russian government (Central Committee) initiated a global modeling enterprise (System of Integrated Models/Global Development Processes [SIM/GDP]); this is reported on by Brecke (1995), who has noted the lack of interest in translating SIM/GDP technical documents into English.

Both the SARU model and Barney's (1980) report suggested the same ominous conclusions presented by the Meadows team. Perhaps this and the addition of defense budget and arms race equations were what brought the project to the attention of the Dutch military intelligence community; I was asked to do a debriefing with Gigengack and the World Model Project director, Catrinus Jepma. We also tried to get the attention of the Brundtland Commission, visiting them several times, once with Kim

Parker, the chief designer of SARUM. The latter was to no avail because some there thought that global modeling was a methodology biased to convince sub-Saharan Black African countries that their efforts at development were hopeless and doomed to catastrophe unless they cut their population growth.

At the same time the SARU team was forming, the Club of Rome commissioned a follow-up team to Meadows. Mihajlo Mesarovic put the new team together in 1972 at Case-Western Reserve University, to give the WORLD3 model greater applicability by dividing up the world into 10 regions and including a simple international trade routine, tracking each region's trade with the rest of the world as a whole. The Meadows team had successfully resisted such a division of the world into regions on the grounds that they were modeling the world as a whole, and those long-term global trends ultimately were not affected by regional variations in the trends. However, national interests in the use of this type of model dictated that such users be explicitly represented. For example, no nation could control global population growth, but their policies might have some impact on themselves. Mesarovic and Pestel's (1974) *Mankind at the Turning Point* again brought to the world's attention the instability of the mid-21st century but this time showed how to represent national policies in a global context.

There were other efforts that also started late in the 1970s. The World Bank developed several varieties of global models under Supramasad Gupta (on loan from the Indian Planning Commission). Various agricultural and other models such as the UN's MOIRA (Linnemann, De Hoogh, Keyser, & Van Heemst, 1979) sprung up. Two of these have lasted: Hughes's (1999) International Futures (IFs) and Bremer's (1987) GLOBUS, to which we now turn to complete this review.

The Emergence of International Futures (IFs) by Barry Hughes

Barry Hughes's (1999) IFs simulation was at first little more than a rewrite of Mesarovic's WIM. Indeed, Hughes had worked closely with Mesarovic and rewrote it first in FORTRAN, then in Visual Basic with his permission. Hughes, however, used IFs as a platform for much further development. Up to Hughes's IFs, the general public did not have a documented global model that could be run on a PC by any interested user. It was Hughes's hope to make the general public aware of global modeling with a practical example and get it introduced into academic curricula. Up to this point, all the global models in existence were directed toward government and major corporate policy applications bought and paid for by them. Also up to that time, only one model existed that effectively integrated policy dynamics with economic dynamics—namely, Bremer's SIPER (1977) model (discussed below). Hughes undertook to incorporate armament dynamics and war potential, even war loss estimates, into IFs. Furthermore, he went on to adapt it to a Microsoft Windows environment and append to the first and only general textbook on global modeling, *International Futures*, and expanded it to include a wider variety of environmental, human rights, and democratic process variables. In my view, Hughes's vision and perseverance and high academic standards in continuing year after year for the past two decades to improve, apply, and interpret his IFs model are gradually revolutionizing the field. What Guetzkow did for bringing "man-machine" simulation into the educational curricula of international politics courses, Hughes is doing for formal system dynamics modeling of international politics.

In several places, Hughes makes clear that his global model, and perhaps all global models, is not to be used as a forecasting tool but rather for policy planning. Hughes senses that unlike weather forecasting, international futures modeling requires the model user to participate in bringing into being a future that the user is in part shaping. When the user makes parameter changes in the model to assess potential policy impacts, the user is changing the forecast in an attempt to envision how to create a future more desirable to the user than it (the originally forecast future) otherwise would have been. Whether the most desired future (of those forecast by simulated alternative policy implementations) is likely to occur depends not only on the overall quality of the model but also on the choices other actors may make in similar exercises. This indeterminacy of outcomes gives Hughes's philosophy of use of such models a particularly realistic tone.

Stuart Bremer's SIPER and GLOBUS

In developments parallel to the above, SIPER (Simulated International Processor), the forerunner of GLOBUS (Bremer, 1987), developed by Stuart Bremer (1977), was based substantially on the INS and Richardson-like equations. It used the equations connecting output (such as trade and budgetary allocations) to input (such as basic and force capabilities) to decision making but for the first time included equations similar to those of Richardson's, replacing the human decision makers with a decision calculus. Leader's policy aspiration levels for political stability, international security, and economic growth were represented as variables in equations and changed dynamically as a function of "reference" countries' (peers' or rivals') attainment of these goals.

This transition from a "man-machine simulation" to a "all-machine simulation" put Guetzkow's program of research back fully into the science paradigm. The set of equations, parameters, and initial conditions that constitutes SIPER is not a "simulation" in Guetzkow's sense because the insights gained with such a model are derivative of the dynamic interaction of equations that in the design are well known (if not well understood in their implications for dynamics). The validity question is simply—often not so simple in practice—one of matching the equations' behavior to real-world observations. The basis for Guetzkow's hope to leapfrog the step-by-step efforts of normal science (i.e., through the presence of human beings, their cultures, and personalities, with changes in their aspirations, logic, and culture still neither understood nor modeled) is no longer present. The human participants in the simulation are no longer there to make (or not make) "sufficiently strenuous intellectual and moral effort" to supplement the knowledge embedded in the equations, to in turn tell us something we did not program into the simulation.

Note that this is neither a criticism of Guetzkow's enterprise nor Bremer's. It is simply to note that paradigm shifts have taken place apparently without the awareness of the very modelers themselves. But let us continue the review before we dig deeper into this philosophical quagmire.

GLOBUS was the outgrowth of a 10-year project begun under Karl Deutsch's tenure at the Wissenschaft Centrum in Berlin in 1978. Deutsch, then on leave from Harvard, hired Bremer to head the GLOBUS Project, in part on Guetzkow's recommendation. The first few years were spent on trying to develop a global model based on SIPER and using an unfamiliar system dynamics language. Both were scratched and the project restarted using the familiar FORTRAN language family and a much more elaborate political-economic model grounded in considerable empirical research by team members. Unlike the Club of Rome's projects, GLOBUS has an elaborate political dimension, even including estimates of strength of political protests, in part probably because this was of great interest to the German government at the time. No other model is so elaborate in this dimension or in its extensive empirical parameter estimation.

Today, both Bremer and Hughes are regularly supported by the Pentagon to have their databases updated and models refined, as part of the post–cold war concern with regional political instability worldwide and the political-economic instability implications for political-military activity (missions). But although GLOBUS is not generally available, Hughes's IFs is. Mesarovic's work seems to have moved into two directions. First, there has been work for the military intelligence community with WIM, rechristened GLOBESIGHT (I acted as a scientific adviser to the Pentagon in 1990, reviewing this model for the Forecast II Project at the Defense Resources Management Education Center at the Naval Postgraduate School in 1990). Second, there is Mesarovic's GENIE, an international education program sponsored by UNESCO with a core model. If this project succeeds, it will enable students worldwide to share in an educational simulation of the world political economy.

Philosophical Conundrums

A number of observations of a philosophical nature came forth regarding global modeling as we reviewed its history.

Deterministic in Form but Not in Substance?

First, global modelers seem to prefer deterministic mathematical forms that they themselves do not take literally. From Richardson to Hughes, this disclaimer is made. Richardson calls for rising above habit and instinct with sufficiently strenuous moral effort. Hughes confides that we cannot predict the future with global models but must act as if we can. Why is there no explanation or resolution of this conundrum? I believe that the answer lies in paradigmatic confusion. The paradigm for model editing is that of science, constantly trying to reduce to acceptable noise levels the dissonance or gap between models of the possible, generated by theory, and models of the real, generated by data collection and analysis. Yet there is a constraint not to exclude the needs of policy analysts and policy makers who have visions, models of the desirable, which they want to achieve, to "realize" in the sense of make real. The paradigm of praxis is analogous to that of science. Praxis aims at reducing the gap between your model of the desirable and your model of the real—in short, to either change the world or your goals or a bit of both, as distinct from science's goal to improve your understanding of the world. Leontief (Leontief & Duchin, 1983) and Onishi (1977), for instance, use I-O tables that are no more than statistical "black boxes"; you do not need to understand what is going on as long as outputs are a nonrandom function of inputs.

To develop this idea a bit further, there has been a long-standing divide among scientists, between those who aim to develop theory that is parsimonious, predictive, cumulative, and hierarchically structured (from axioms to theorems), as distinct from those who seek simply to accumulate interrelated generalizations based on empirical observation and statistical inference. This is the classic divide between theorists seeking fundamental laws of the universe and empiricists seeking practical knowledge through searching for regularities. It would seem that in global modeling, we also have both and a number in between. The closer to and more dependent on government or industrial support you are, it seems, the more likely you are to adopt something resembling the empiricist's viewpoint. You settle for what works because one is getting closer to the policy architect's role and more distant from the pure scientist pursuing knowledge for its own sake. Unfortunately, this is not commonly understood as a paradigm shift but rather as a paradigm distortion. The result is an opportunity for what is essentially unfair criticism. For instance, almost as soon as *The Limits to Growth* made its debut (D. H. Meadows et al., 1972), *Models of Doom* appeared (Cole, Freeman, Jahoda, & Pavitt, 1973) with what from my perspective were clearly unnecessary and inappropriate criticisms. These ranged from the antideterminism argument, faulty data, poor theory, and poor theory testing, despite the Meadows team's clearly stated caveats and purposes. This was topped off near the end with rather strident conclusions such as,

> In common with other chiliasts, the new scientific chiliasts are utopians at heart. Like the great prophet of world salvation through world breakdown, Karl Marx, their apocalyptic visions of the immediate future are tempered by the glittering image of utopia barely discernible through the fire and brimstone that rages in the historical background. This is not to denigrate the beliefs of the Forrester/Meadows school in any sense; rather it is to suggest that they too, despite the surface appearance of scientific neutrality and objectivity, bring us a message which can only be fully understood in the context of their own beliefs, values, assumptions and goals. (Harvey Simmons, quoted in Cole et al., 1973, p. 207)

I surmise that these reactions were themselves politically motivated given the immediate formation of the U.K. parliament's World Trends Committee and the SARU team to do in a more practical and elaborate way what the Meadows team did.

Forecasting in Form but Planning in Substance?

Second, is global modeling about forecasting or about planning? If the former, then one would expect a search for predictive power, for example, like weather forecasting. If the latter, then we would expect a search for strategy and tactics, benefit/cost analyses, preparation of alternative plans of action, and their comparative evaluation in terms of some hierarchy of values and basic needs. So far as I know, it would seem fair to say that the global modelers discussed here prepare far more for planning than for hypothesis testing. I have not read a single claim to the discovery of a new fundamental law or even that the modeler was in hot pursuit of such. If you read carefully the empirical testing that takes place, reported on, or referred to in most of the works cited or in their companion reports (e.g., D. L. Meadows et al., 1973; *Systems Analysis Research Unit Models*, 1976), with few exceptions you find more parameter estimation than hypothesis testing or empirically based theory development. It has pretty uniformly been the aim of policy relevance that has been pursued by devising the most appropriate model to the envisioned needs. In this sense, global modelers are more like architects designing buildings than like cosmologists scrutinizing the heavens for some invariant truths (of course most astronomers in the old days earned their livings as court astrologers!).

There are some exceptions to this general conclusion. Reported in Chadwick (1967, 1972), Bremer (1977), and Elder and Pendley (1981), for instance, are among the explicit hypothesis testing exercises. In all these cases, however, the emphasis is not on the testing of a new theory-generated hypothesis but rather on the goodness of fit of referent system data to expectations based on equations embedded in a simulation and the behavior of a simulation. The overall intent was to increase confidence in using the simulation as a policy exploration tool by increasing the degree of correspondence of simulation behavior to referent system behavior.

If I am correct, then global modeling is at present dominated by the praxis paradigm, not the science paradigm. To be blunt, science is only necessary if needed to pass the "laugh test" of "face validity" or "verisimilitude." This is a dangerous situation from the science paradigm perspective because it implies that a model's validity is judged acceptable if and only if policy-focused communities accept it and its implications. There is no community acting as a dispassionate observer, independent evaluator, or constructive critic. The obvious conclusion is that users of global models are running risks of unknown proportions and likelihood, not to mention the modelers themselves.

Let me illustrate these "dangerous consequences" with two very different examples. As mentioned earlier, we found more than 60 errors in the SARU model code. For instance, we found two equations that multiplied by exchange rates, effectively doing a double conversion, effectively multiplying by the square of an exchange rate. Elsewhere we found division of trade flow estimates—division by a factor of 100. At first we thought both were simple algebraic errors, but when we fixed either one, trade flow figures became unbelievably low or unbelievably high. We concluded that someone had “fixed” a “scaling” problem by division by 100 to offset the error in the exchange rate equations without knowledge of the source of that error. This sort of rushed work is typical of the attitude, “don’t fix it if it ain’t broke” and “do what works” in praxis but not in science. SARUM would do general “ballpark” estimates that had “face validity” as a consequence, within the range of empirical data used to make its parameter estimates but would yield increasing biased results outside that range—biased in unknown ways because of the multiple feedback loops in which trade was embedded.

Another and very different case in point of that was the fate of the Latin American World Model (LAWM) team at the Bariloche Institute in Argentina (Herrera et al., 1976). In their search for a way to feed not only Latin America but also the world’s growing population, they found that if they set their price of land parameter to zero, they could produce all the food in Latin America needed not only by Latin America but also the world. When it became known that this implied that Argentina should adopt a Communist policy toward land ownership, I was told personally that the team was disbanded and threatened with death by the then ruling junta. Needless to say, perhaps, but true: I know of no global modeling team in the world since then that has publicly examined this policy parameter’s impact again. And although I have tried repeatedly on hearing of its existence here and there, I have never been able to get my hands on a complete, readable copy of the computer source code, much less the LAWM computer program itself. I was, however, able to acquire the last remaining copies of their report from the IDRC.

Realism or Idealism?

Among international relations scholars, there is a similar dichotomy between “realists” and “idealists.” An aspect of the realist paradigm in assessing situations and appropriate policies is the assumption that you cannot change the course of history, but you can change whether you are on the winning or losing side by an appropriate maneuver. The idealist, on the other hand, keeps a firm fix on his or her vision of a desirable future and struggles by all means available to attain it. “Let’s find some land,” says the realist captain in a dangerous storm. “No, let’s stay the course!” replies the idealist.

Who is right? Facts, knowledge, experience, skill, risk-taking tolerance, and authority position all come into play. Global modelers write about the fate or destiny of humanity. The Meadows team twice (D. H. Meadows et al., 1972; D. H. Meadows, Meadows, & Randers, 1992) and Mesarovic and Pestel (1974) have written about the tremendous life-and-death responsibility of the world’s leaders from now through the 21st century because of the potential for global cataclysm. Speaking to business and finance communities, not to global political leaders, the Link Project and FUGI modelers are much more circumspect and narrowly focused on profitability and financial stability questions. Perhaps those who focus on environmental dynamics such as the Club of Rome teams tend toward the idealistic because they recognize and focus on measuring the potential for long-term threats to human existence. They try to model it and alternatives, whereas those who focus on economics and investment strategy in a realist mode simply do not have either the relevant data modeled or the time frame needed to get a grip on the survival problem.

The Town/Gown Gap

Another problem emerged in addition to the above, not epitomized by a particular global modeling team but one uncovered in interviews conducted by the G-MAPP Project at the East-West Center. Its first coordinator, Dr. Don MacRae, from 1980 to the beginning of 1981 (I coordinated the G-MAPP Project from mid-1981 to the end of the project in December 1983), worked with Sripada Raju to assess relations between policy makers and global modelers. They found a wide gap between what policy makers perceived the value of global modeling to be and what global modelers thought that policy makers wanted (Raju & MacRae, 1981).

Over the two decades preceding this study and the nearly two decades that have followed, I do not see that much has changed. In my own experience with Guetzkow's INS and his Simulated International Processes (SIP) Project in the early 1960s, I recall Col. Thane Minor, then with the JCS, once giving SIP several boxes of Hollerith (IBM) cards containing Clark Abt's TEMPER (Technological, Economic, Military, Political, Evaluation Routine) model (Abt & Gordon, 1969). He asked Guetzkow if someone on the project could evaluate TEMPER and extract from it a model that might be useful. The Pentagon had wanted a checklist that a battlefield commander in Vietnam could use to decide on the most effective strategy in an area, Minor said (as I recall), and instead Abt had given them more than 2,000 variables! Mike White took on the task as a summer project; at the end, he quipped, as I recall, that all he really could figure out was how Abt's mind worked.

A second situation: My understanding is that sometime after Margaret Thatcher took office as prime minister, the SARU team was asked to come in and tell her what global modeling was about and why it was useful. As I have been given to understand it, the team members present began to explain to her the systems dynamics approach. After patiently listening for perhaps 15 minutes, I understand that she told them they were all fired, but they had a year to find jobs, and she got up and left the room.

Another anecdote from G-MAPP days: I was told that in the early years of the Indian Planning Commission, when Nehru was head of the government and Mahalanobis head of the commission, the modeler and the statesman got along just fine. This was because Mahalanobis would reply to a policy question by asking for a few days or a week or two to work on it and then present to Nehru alternatives, each of which had consequences. This simple "if you do this, that will happen; if you do this other, this other will happen" approach worked just fine. By contrast, Truman used to joke, "I'd like to chop off the right hand of every economist; they are always telling me, on the one hand this and on the other hand, that!"

How true these anecdotes are is difficult for me to document today; I use them to make a simple point that was echoed in the G-MAPP survey conclusions: Global modelers need a far better, systematic education as to how to deal with policy formulators and policy makers. And policy analysts and leaders need a seminar or "debriefing" on the value of global modeling and the modelers. Both can be taught. But modelers especially need training in their own art. Global modelers are in roughly the same position that urban planners were before there were academic centers and departments of urban planning. Some global modelers are probably doing things that are analogous to the early planning of cities that by most accounts unintentionally created high-rise slums and high crime rates.

The need for such education and training is obvious when one reviews the policy orientation of the Meadows (D. H. Meadows et al., 1972, 1992) and Mesarovic (Mesarovic & Pestel, 1974) teams. After pointing to the grave potential for disasters such as population disease and death due to a Mathusian future of scarcity and population growth checked only by starvation, they proceeded to call for global planning for birth control and capital control. Politicians generally wrote them off because the option of creating a world vision with policy teeth in it just was not realistic. The military intelligence and policy formulation communities, on the other hand, paid a great deal of attention (for a small personal sample, see Chadwick, 1986) and continue to do so today because if crises are likely to occur, the security communities must be prepared to cope with them. For instance, the current concern in the "MIC" is with regime instability. "Instability analysis" and "instability workshops" in the post–cold war era remind me of the 1970s after the Kent State University killing. Then, *civil strife* became the research buzzword, and millions of dollars could be found for research promises of quantified estimates of how much, when, where, by whom, and under what conditions civil strife was likely to break out. Until global modeling is sufficiently comprehensive and produces sufficiently "stable" models, modelers are likely to continue similarly to be pushed and pulled in various theoretical and applied directions but contribute little by way of "value added."

Some Proposals

I would like to put some of my conclusions into the form of proposals. Global modeling has grown very fast since the advent of the mainframe and the PC and now the Internet. But there is no academic home for this new discipline. Many of the problems with global modeling and among global modelers and between them and policy formulating communities stem from lack of formal study of global modeling in an academic environment. Insensitivity to paradigmatic framework, inappropriate application, and, above all, unnecessarily cantankerous dialogue between policy formulators and makers, on one hand, and global modelers, on the other, could be obviated by better education.

So I am proposing that modelers out there make a serious and sustained effort to create yet another field of specialization recognized in academe and pursue the creation of centers and institutes and departments of global modeling, as well as a journal, a professional society, and a publicly available database. Here are some details. The basic courses should include the science of global modeling, the policy-making context (praxis) of global modeling, the philosophy of global modeling, and the methodology of global modeling. There then should follow specialized courses in each, and these should be complemented with lists of courses in cognate fields in the social and physical sciences. Corresponding coursework should be developed at the graduate level (MA and PhD). Furthermore, at least one journal of global modeling should be founded, although eventually there might perhaps be four such journals dealing with the science, practice, philosophy, and methodology of global modeling. Some foundation should be approached for seed money to initiate this effort at one university or a few more, to serve as a model for others. A professional society should be created with an online global modeling database for models, documentation, discussion, and empirical data archives. I suggested the latter in 1984 at an International Economics Association (IEA) meeting in Stockholm when I was affiliated with the World Model Project at the University of Groningen. Alas, to no avail; the IEA effort was agreed to, then deflected, and the leadership of the World Model Project limited their vision to their resources. The fact is that all sorts of national and transnational organizations need national and global data in spreadsheet format to conduct longitudinal research on global system and subsystem dynamics. It needs to be permanently and publicly available and "cleaned up" because there are metric and collecting idiosyncrasies within and between national account and other statistical series. Many organizations such as the UN and OECD do much of this work and do not make it public for reasons ultimately of political and economic advantage and agreements made with data donors.

This is not the first time some such recommendations have been made. Harold Lasswell (1963), for instance, conceived of a "social planetarium," an idea that has found a new and sympathetic audience (Warfield, 1996). And Forrester himself has become an advocate of a revolution in education (Forrester, 1998).

Another proposal is of a more theoretical nature. Global modeling, as mentioned earlier, straddles three paradigms: those of science, praxis, and philosophy. We need models that not only project "drift" states (system tendencies excepting exogenous change) but also guide policy analysis by suggesting alternative goals and means. As regards goals, for instance, Maslow (1954) suggested a basic needs approach that could be modeled (recall his hierarchy of needs: survival, security, belongingness, self-esteem, and self-actualization). Bremer (1977) came close to embodying such an approach with his definition of security, growth, and political stability goals. Such demands varied in their competitiveness for system resources depending on how much they were not attained. But there was no theoretical basis for assessing completeness of the basic needs list used in the simulation. In Maslow's terms, in simulations such as SIPER, GLOBUS, and many others, the practice of international relations is missing motivations related to self-esteem and self-actualization of leaders and followers alike. Similarly, as regards generic means for goal attainment, there is no effort at comparing the means provided by simulation variables and parameters with a basic checklist of means, such as Lasswell and Kaplan's (1950) checklist of means values (wealth, respect, affection, power, skill, well-being, enlightenment, and rectitude). A Lasswell-inspired global model would have indicators for each of these means values. A Parsonian structural framework, such as used by Smoker (1981), would alert global modelers to possible errors of omission. Without such theories as Maslow's, Lasswell's, and Parson's as regards ends, means, and system structure, any global modeling evaluation is adrift in a sea of uncertainty as to what evaluation criteria to employ, not to mention as to what is essential to model and what is superfluous. Thus, the philosophy of global modeling in the long term is as important as its practical or its scientific merit.

Conclusion

At the beginning of the 20th century, there were no such things as microchips, yet people such as L. F. Richardson were envisioning a world system that needed to be understood if war was to be obviated. At the end of the 20th century, we have an existing world system in trade, finance, and communication and the capacity for self-destruction not only in nuclear but nanotechnology forms, be they biological, genetic, radiant energy, or mechanical. But we are only at the very beginning of global self-understanding and self-control. Like Richardson's arms race opponents, we do not understand that it is in our collective interaction that dynamic equilibria, stable or unstable, or chaotic, indeterminate states are created that in turn affect us all. A few, such as Shewhart (1931) and Deming (1986), recognized this and initiated a true revolution in business management practices and philosophy, but the "profound knowledge" (human system dynamics) base from which they worked is still too rarely understood. Global modeling is still in its infancy: not enough data, few tested empirical hypotheses, too much ungrounded theory, inadequate philosophical understanding, and a lack of practical wisdom. Much as astrologers preceded astronomers and alchemists preceded chemists, we are still beset with trend analysts with rulers who seem to do just as well (which is to say poorly in terms of value added) as global modelers struggling with their programming languages and skimpy data and yesterday's deadlines.

Despite all this, global modeling holds out the promise of human self-understanding at a system dynamics level. It seems not yet to have been the focus of sufficiently strenuous moral effort. To paraphrase Richardson (1960), global modeling needs a boost to academic prominence and social significance, a boost that it deserves and needs if it is to mature as a contribution to science, to praxis, and to philosophy for the sake of world civilization. Let us see what we can do.

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