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SESSION I
PRESIDENTIAL ADDRESS

Johannes Kepler's computer simulation of the universe: Some remarks about theory in psychology

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Computer simulation may be an overly seductive way of formulating theory in the social sciences. The capability of constructing complex computer simulation models may remove some of the incentive for finding simple laws. This point is illustrated via a fantasy in which the sixteenth century astronomical theorist, Johannes Kepler, uses his computer instead of his mind to account for extant data on stellar and planetary motion. Similarities between sixteenth century cosmology and twentieth century psychology are discussed.

Two years ago, in Minneapolis, Russ Church gave the Presidential address to this society (Church, 1983). In his talk, Church presented an excellent summary of the usefulness and pervasiveness of computers in all phases of psychological research. He started with the literature search, proceeded through the phases of experimental control, recording of results, storage of data, analysis of results, development of theory, comparison of theory with data, preparation of figures, and ended with the processing of the manuscript. For each of these phases, Church compared the tedium of the precomputer technique with the ease and efficiency of the corresponding postcomputer technique. Presented in this way, the enormous facilitative impact of the computer on our research endeavors was breathtaking: Indeed, as we listened to the talk, it became difficult to imagine how scientific research ever got done in those bygone days before there were computers.

The optimistic and upbeat mood of Church's talk was marred only slightly during the question period by Misha Pavel, who tactlessly inquired if everything could really be this good—if there weren't potentially anything wrong with the way in which computers were used in the research process. My interpretation of Misha's question was this. Isn't there a danger in the extreme ease with which the computer allows us to do things? Isn't it possible, in other words, that the computer is seducing us away from some of the hard thinking that remains a sine qua non of good science?

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I resonated to this possibility. Consider, as an example, the use of the computer in statistical analysis of data. Over years of reviewing manuscripts, I had been developing the nervous conviction that there was too much emphasis on pouring raw data into SPSS or BMD programs and simply accepting whatever numbers emerged—chi-squares, F ratios, whatever—as the conclusion, without further ado. This didn't seem like a very imaginative or fruitful way to go about analyzing data. It seemed to me that off-the-shelf statistical analysis programs were producing off-the-shelf conclusions.

Today (at the risk of being drummed out of this society for displaying an unacceptably negative attitude) I want to pick up on this theme. My focus will be on the use of the computer as a theoretical tool—in particular, on the use of computers in the construction of simulation models of mind and behavior.

Computer simulation models, along with their close cousins, artificial intelligence programs, are becoming an increasingly popular way of instantiating theory in the social sciences, especially in psychology and in economics. I believe that sometimes in the social sciences, as in the natural sciences, computer simulation is an endeavor that's entirely appropriate and scientifically productive. Later, I'll return to describe one such simulation. But often, I believe that computer simulation in the social sciences isn't so useful. It may be lots of fun—but I will argue it has its drawbacks. First, I believe that computer simulation promotes the building of complex theories, which are really no more than restatements of the complex behavior that the simulation is designed to simulate. Second, I believe that computer simulation removes the incentive to do the hard conceptual work necessary to produce simple, elegant theories. Third, I believe that computer simulation promotes accounting for data rather than searching for truth. Indeed, it may promote the belief, that I find scientifically distasteful and unesthetic, that there's really no truth to be found at all.

THE HISTORICAL APPROACH

I want to illustrate these points using an example from one of the oldest of the natural sciences, astronomy. In so doing, I'll provide a little nutshell history of theory in astronomy.

I've chosen a historical approach because I believe it provides perspective. It allows us to draw back—to pull ourselves out of the day-to-day activities involved in the research enterprise and to view a scientific problem through a wide-angle lens. Psychology is, as the cliche goes, a young science. This youth provides us with a convenient excuse when we confront the occasional confusion and seeming directionlessness of the field. But since psychology is young, we, as psychologists, can perhaps learn some lessons by using this historical approach. We can look at what happened to other sciences when they were in the same developmental stage as psychology is in now, and we can see how they dealt with similar problems. We can see not only what the solutions to these problems turned out to be—but we can imagine what the solutions might have been, had circumstances been a little different. Perhaps with this perspective, we can anticipate some of the potential traps and cul-de-sacs that our discipline is headed for before it's too late to avoid them.

Observational Origins

So let's first turn the clock back to prebiblical times, when people first started trying to figure out the heavenly system. In hindsight, it all seems so simple. Most of us have a pretty good image in our heads at least of the nine planets revolving around the sun, set against some backdrop of fixed stars. But in the beginning, it must have seemed unbelievably complex. David Freedman (1983), a statistician at Berkeley, has made this point very nicely. He says:

> Some problems in the natural sciences now look very clean and simple, but only because of the analytic work that has been done. To appreciate this point, imagine trying to figure out the orbit of Mars for yourself. You go out on a clear night, look up in the sky, and see thousands of points of light. Which one is Mars? To start closer at the beginning, which ones are the planets and which the stars? Continuing to watch for several hours might only confuse matters further: for the pattern of stars will gradually change as the night wears on. Even recognizing this change depends on prior knowledge: for it is hard to see the shifting pattern of the stars without using the constellations. (p. 11)

So that was the beginning. Things must have seemed pretty complicated. Over the centuries, however, theories began to emerge that, in one way or another, began to impose some order on this celestial confusion. Some of these theories were simple and fanciful, and they accounted for the data in only the crudest, qualitative ways. Other theories—notably the enduring one of the Egyptian astronomer Ptolemy—were much more complex, but represented serious attempts to account for what was observed. Indeed, Ptolemy's system, although belittled now for being earth-centered rather than sun-centered, accounted for the extant astronomical data quite well. It encapsulated the general view of the universe at the time, and it also served tolerably for practical applications such as compilation of the planetary position tables that were used by generations of seafarers as they navigated the ancient Mediterranean trade routes.

Epicycle Madness

We now move forward several millennia to the mid-1500s, by which time enormous strides had been made in cosmological theory. The major conceptual achievement of the era was the heliocentric model, conceived by the Polish cleric, Nicholas Copernicus, and published in 1543. In the Copernican theory, the sun was placed more or less in the center of the solar system. This innovation, of course, represented quite a radical departure from the Ptolemaic system, according to which, everything revolved around the earth.

That was the good news. The bad news was that, despite the conceptual advance represented by Copernicus's model, the state of knowledge and theory in cosmology was still pretty much of a mess. Much of the problem derived from Copernicus's (and everybody else's) refusal to abandon the ancient Platonic dictum that any celestial orbit must assume the form of a perfect circle. In hindsight, we know that this constraint was bound to cause problems, since planetary orbits are, in fact, ellipses, not circles. But if you were a sixteenth century astronomer, you didn't know that, and you were committed to circles. This meant that you had to come up with some rather exotic system in order to fit the data. The system of choice, for both the Ptolemaic and the Copernican systems, was based on epicycles, an example of which is illustrated in Figure 1. Figure 1 shows a simplified version of the earth's orbit, according to the Copernican system. As is evident, the center of the earth's orbit is actually not the sun. Rather, it is an imaginary point in space (labeled C) somewhere in the general vicinity of the sun. But this point was not stationary. Instead, it revolved, in a perfect circle, around yet another imaginary point in space (point A), which itself revolved in a perfect circle centered, at last, on the sun. By appropriately arranging these circles, along with their periods of revolution and their phases, it was possible to work things out so that the earth, considered in isolation, wobbled around the sun in more or less of an elliptical path.

This wasn't, of course, the whole story. Copernicus actually needed a few more epicycles for the earth, plus other epicyclical systems for all the other known planets, including the moon. Indeed, Copernicus actually wound

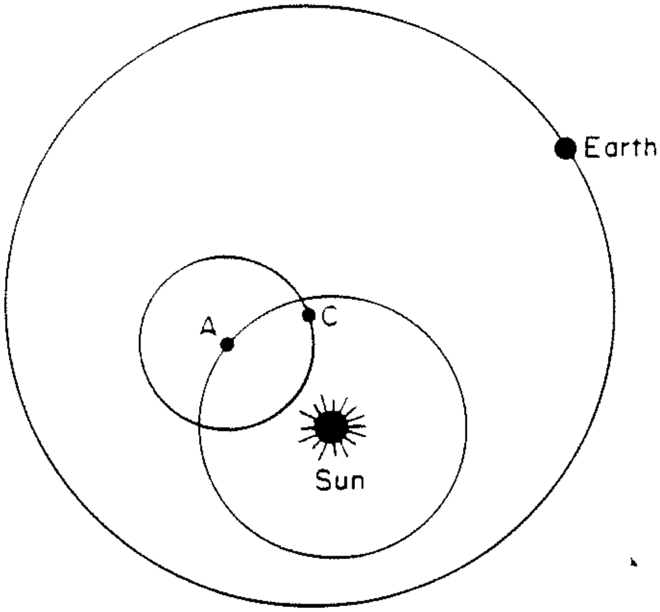

Figure 1. A schematic view of the earth's orbit, according to Copernican system.

up with a system that incorporated 48 epicycles. In contrast, Ptolemy's earth-centered system incorporated, when all was said and done, only 40 epicycles. Copernicus had achieved his conceptual advance at the cost of decreased parsimony. Figure 2 shows a partial depiction of the entire system.

As a psychologist, it's fascinating to read accounts of this period. There's an eerie similarity between the state of cosmology in the 1500s and the state of psychology today. There were lots of isolated bits and pieces of theory to describe various phenomena, all relatively unconnected to one another. The data were haphazard, and were fit by theory in only the loosest kind of way. Discrepant data presented no problem. Either they were ignored, or the theory could be tuned and expanded until the offending observations were forced into submission. Epicycles could be moved around, added here, and deleted there. Distances and angular speeds could be modified. In short, there were lots of parameters in the system. Substitute "memory" for "universe" and "strength of association" for "epicycle," and the resemblance of cosmology to at least one area of psychology becomes disconcertingly striking.

There was a kind of angst among sixteenth century astronomers about the prospect of ever coming up with a simple elegant theory of the universe. The general notion seemed to prevail that the structure of the universe was just awfully complicated, and that it was hopeless and arrogant to think that it could be described in any simple way. An episode involving Georg Rheticus, who was a contemporary and disciple of Copernicus, dramatically illustrates the frustration engendered by this state of mind.

> When on one occasion he [Rheticus] became perplexed and got stuck in the theory of Mars and could no longer see his way out he appealed as a last resort to his guardian angle as an oracle. The ungracious spirit thereupon seized Rheticus by the hair and alternately banged his head against the ceiling then let his body down and crashed it against the floor, to which treatment he added the following oracular pronouncement "These are the motions of Mars." (quoted in Koestler, 1959, p. 160).

As this vignette illustrates, it did not seem that scientists were at all optimistic.

And finally, theory was not taken seriously as a reflection of truth. Rather, it was viewed as merely a means of accounting for data. One Andreas Osiander, another of Copernicus's colleagues, forcefully expressed this view on a number of occasions. He wrote, for example, to Copernicus,

> For my part I've always felt about hypotheses [by which Osiander meant astronomical theory] that they are not articles of faith but bases of computation, so even if they are false it does not matter, provided that they exactly represent the phenomena (Koestler, 1959, p. 167).

Osiander wrote in a similar vein to the bedeviled Rheticus, offering advice about how to deal with opponents of the Copernican view:

> The Aristotelians and theologians will be easily placated if they are told that several hypotheses can be used to explain the same apparent motions; and that the present hypotheses are not proposed because they are in reality true, but because they are the most convenient to calculate the apparent composite motions. (Koestler, 1959, p. 167).

And finally, Osiander took it upon himself to write an anonymous preface to Copernicus's book in which he made his position quite clear:

> So far as hypotheses are concerned, let no one expect anything certain from astronomy, which cannot furnish it, lest he accept as the truth ideas conceived for another purpose (i.e., as mere calculating aids) and depart from this study a greater fool than when he entered it. (Koestler, 1959, p. 167).

Arthur Koestler (1959), in his superb history of cosmology, *The Sleepwalkers*, notes that Copernicus himself seemed quite satisfied with this general philosophy of cosmological theory. Koestler writes,

> He [Copernicus] did believe that the Earth really moved but it was impossible for him to believe that either the Earth or the planets moved in the manner described in his system of epicycles and deferents, which were ge-

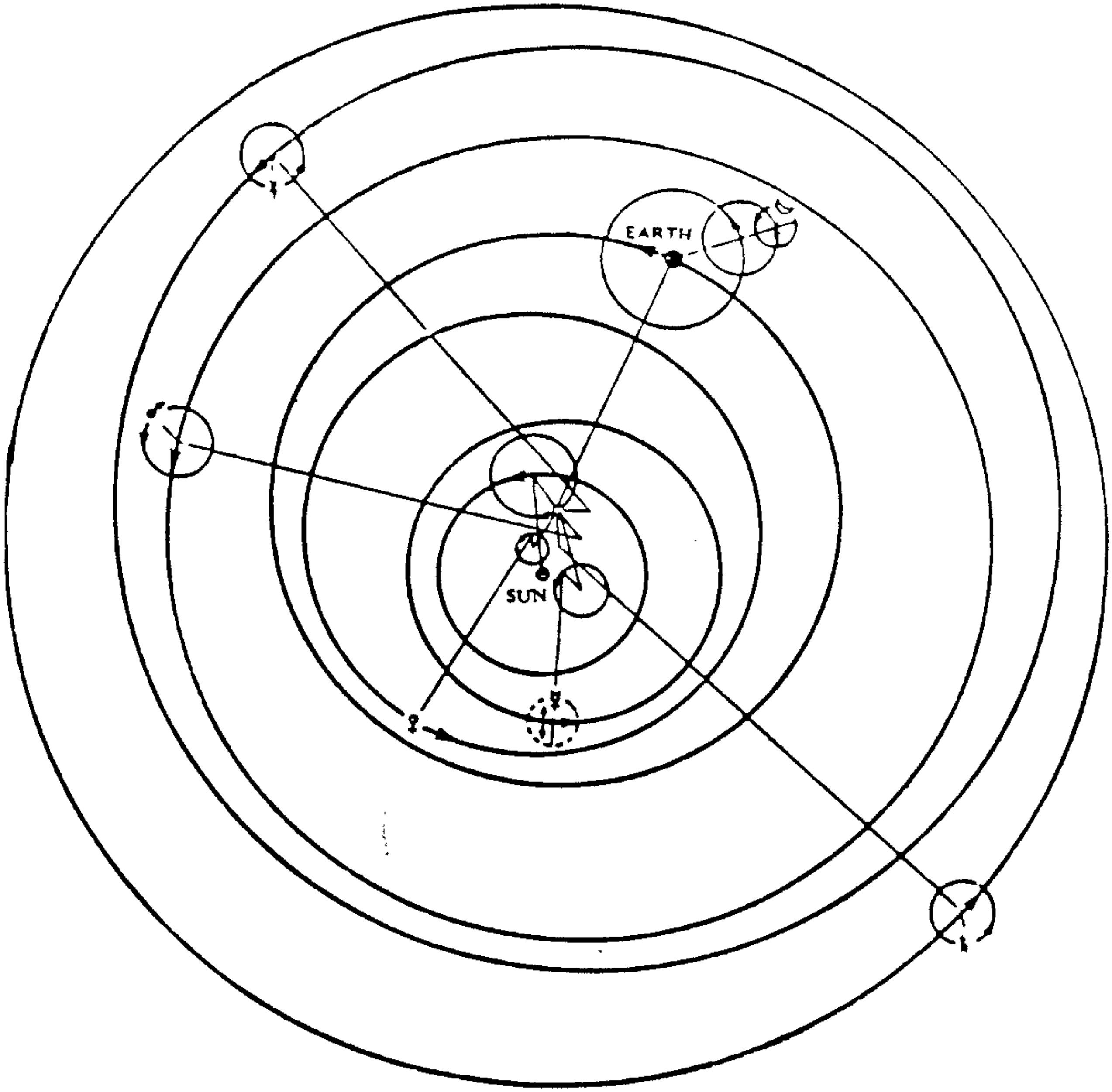

Figure 2. A schematic view of most of the Copernican solar system.

> ometrical fictions. And so long as the why and the how of the heavenly motions rested on a purely fictional basis with wheels on wheels which the astronomer manipulated with happy unconcern for physical reality, he could not object to Osiander's correct statement about the purely formal nature of his hypotheses. (Koestler, 1959, p. 171)

In other words, Copernicus did believe in a rough, qualitative version of his theory. But he didn't believe the details—rather, the details were just inserted arbitrarily for the purpose of fitting the data.

Later, Koestler continues,

> The physical causes of the motions, the forces of nature behind them, were not the astronomer's concern. Whenever necessary, a few epicycles were added to the existing machinery of wheels—which did not matter much since they were fictional anyway and nobody believed in their physical reality. (Koestler, 1959, p. 274).

Kepler's Laws

So this was the state of affairs when Johannes Kepler entered the picture. Kepler, however, was unique in that he couldn't accept the idea that the purpose of a cosmological theory was just to account for data. On the contrary, he believed that the universe, complex though it appeared to be, really and truly operated according to some simple scheme. And Kepler took it upon himself to figure that scheme out. He spent 40 years of his life immersed in this endeavor, in the process producing the three laws of planetary motion for which he is famous. These laws are:

(1) The planets have elliptical orbits with the sun at one focus.

(2) A line, connecting a planet to the sun, sweeps out equal areas in equal amounts of time.

(3) The square of a planet's period of revolution is proportional to the cube of its mean distance from the sun.

They're really true, these laws—they describe what really goes on.

Kepler's working out of these simple laws was, relative to the theoretical cacophony that preceded them, a monumental achievement. But it wasn't arrived at lightly. In the process of formulating his laws, Kepler spent uncountable numbers of hours, both in thinking and in tedious computation. He went through enormous amounts of frustration and lost years exploring several blind alleys. But in the end, it all paid off. As Freedman (1983) succinctly points out, "Astronomers still use Kepler's model. He got it right."

What motivated Kepler in his fanatical search for simple laws? I think two major factors were responsible. First, Kepler had an unyielding belief that the laws were there to be found. I've mentioned this earlier and I will return to it later. But there was a second factor, not quite as romantic as the first, perhaps, but nonetheless important. This is that the sheer amount of boring computation involved in making predictions from any kind of complex astronomical theory was enough to put almost anyone off. Consider the Copernican system with its 48 epicycles. As one might expect, it was awfully cumbersome to actually use. From a scientific point of view, simply churning out the computations involved in making predictions from theory was impossibly tedious. From a practical point of view, the navigational tables and other practical devices that issued from the theory were filled with errors. Moreover, the theory wasn't stable. It kept getting modified in response to new data and new ideas. And every time it was modified, all the computations had to be cranked out all over again.

So it's not surprising that there would be plenty of motivation to seek simple, accurate, and endurable laws. Johannes Kepler seemed especially absorbed by this motivation. He did the work and he won the big prize.

Kepler's Computer Simulation

So that's what really happened. Kepler started with a mess and, after 40 years of work, managed to turn it into something simple and elegant.

But as I noted earlier, an advantage of the historical approach is that, in addition to illuminating what really happened, it allows you to speculate about what might have been. So let's now depart into a fantasy world. Suppose that Kepler had had a computer at his disposal. In this case, the situation might have been a lot different. Kepler could have instantiated any model of the universe that he wanted as a computer simulation model, and, as a result, his life could have been a lot easier—he could have had much more time for fun and relaxation. Cranking out the computations necessary to derive predictions and test models would no longer have been a problem, no matter how complex the model. So Mars is not working quite right according to some new data just in from the observatory? It's necessary to add a new epicycle to Mars's orbit and recompute the predictions? No problem. All Kepler needs to do is to add another loop in the Mars subroutine, let the revised simulation chug away overnight, and there would be the fresh new predictions waiting for him the next morning along with the least squares fit to the data.

The danger that lurks in this situation is, I hope, evident. With a computer, Kepler might not have had the incentive to search for simple laws, and they might never have been found. If Kepler had had a computer, we might now have a universe in which all the extant data are accounted for almost perfectly—but a universe that's awfully complex. Moreover, it wouldn't occur to us that there might be anything better. Our very conception of the universe, now so simple, would have been much different. We wouldn't even think of it as something real. We would think of it instead as a set of complicated motions of mysterious points of light in the sky that scientists account for and deal with using some complicated model. The model, in fact, would probably seem more real to anybody who worried about such things than the universe itself.

It turns out that I'm not the only person who has these kinds of fantasies. In a paper called "Statistics and the scientific method," David Freedman (1983), deals specifically with the problems and misuse of regression models and structural equation models in the social sciences. He demonstrates the disadvantages of these models relative to the classic theories of the natural sciences, including Kepler's. Freedman concludes his paper with the following:

> I sometimes have a nightmare about Kepler. Suppose a few of us were transported back in time to the year 1600 and were invited by the emperor Rudolph II to set up and Empirical Department of Statistics in the court at Prague. Despairing of those circular orbits, Kepler enrolls in our department. We teach him the general linear model, least squares, dummy variables, everything. He goes back to work, fits the best circular orbit for Mars by least squares, puts in a dummy variable for the exceptional observaton—and publishes. And that's the end, right there in Prague at the beginning of the 17th century (p. 23).

It's a chilling thought.

PSYCHOLOGY AND COSMOLOGY

Why have I been telling you about Copernicus and Kepler and cosmological theory? The reason is that, as I've suggested, I believe that much of today's psychology is, in many respects, similar to the cosmology of the 1500s. Let me review what I consider to be some of the relevant shortcomings of theory in psychology.

Chaos

First, nobody seems to agree on theory. There are different theories for different phenomena, and, in many instances, there are different theories to account for the same phenomena. As Michael Watkins (1981, 1984) points out, every psychologist worth his salt has to have a theory of something, and theories have become so personal that they're sort of like toothbrushes. A given theory is used and explored by its owner, and, although occasionally acknowledged, it is not actively used by anyone else. When the owner disappears or loses interest in the theory, the theory dies.

What current theory, instantiated as a computer simulation, will still be in psychology textbooks, centuries down the road?

Complexity

Second, theories are often designed not to explain simple behavior but rather to describe complex behavior. There seems to be an increasing tendency toward the belief that human beings and their behavior are fundamentally complex and there is no way around it. This is seen, for example, in the current movement toward ecological validity, wherein it is argued that a psychological phenomenon is not a suitable topic of scientific investigation unless that phenomenon is demonstrated to be an obvious and pervasive part of everyday behavior (e.g., Haber, 1983; Neisser, 1976; see Loftus, 1983, and Uttal, 1983, for opposing views). If theories are designed initially to account for complex behavior, it's no wonder that the theories themselves are complex.

Instability

Third, theories in psychology are constantly in flux. It's exceedingly rare that a theory will last long without encountering discrepant data. But it's rarer still that a theory will be abandoned in the face of such adversity. Rather, discrepant data generally lead to a retuning or an extension of the theory—to the modern-day equivalent of adding another epicycle or two.

It's disquieting to compare this state of affairs in the social sciences with what happens to theories in the natural sciences when they are faced with discrepant data. Theories in the natural sciences are generally quite stable, and the consequences of discrepant data are pretty dramatic. Kepler himself rejected a theory based on several years of work when new observations showed a discrepancy of 8′ of arc between the observed and the predicted orbit of Mars. Eight minutes of arc is not very much—it's about the apparent size of a penny at a distance of 10 yards—but it was clearly greater than the error in the state-of-the-art measuring instruments and, as such, was sufficient to force rejection of the entire theory. Many years later, in the mid-1800s, the French astronomer Leverrier discovered some small anomalies in the predicted orbital path of the planet Uranus. These discrepancies couldn't be explained unless there was another, as yet undiscovered, planet out there in the void beyond Uranus. There was. It was Neptune. The postulation of another planet might, of course, be viewed as a "retuning of the theory." But it was a retuning that turned out to be firmly based in reality.

Accounting for Data

And finally, there's typically not the assumption of an underlying truth, an underlying reality in the construction of psychological theory. Instead, like the Copernican model of the solar system, psychological theories seem to be endowed with the implicit understanding that a theory or model is just a device to account for the data. The goal is 99% of the variance.

To illustrate this point a fortiori, I can't resist relating an anecdote about a computer simulation of short-term memory that I carried out many years ago with a computer science undergraduate whom I'll call Elaine. Elaine seemed relatively indifferent to most of her academic work, but for some reason became fanatically interested and involved in this simulation. She'd spend night after night at the computer center, poring over her Pascal code, and would appear in my office the next morning to discuss her findings. One day, we were discussing some data from the literature on intrusion and omission errors in a Brown-Peterson task. We had discovered that our simulation logically could not predict the data correctly, even qualitatively. Elaine's reaction was to announce that the data couldn't be correct. When I asked why not, she said, "Because the way short-term memory works is . . . ." and went on to describe the workings of our model. I was amazed. It wasn't just that Elaine had ceased to believe in an underlying reality that our model was simulating. For her, the model itself had become the reality.

COMPUTER SIMULATION IN THE SOCIAL SCIENCES

I've talked about the similarities between twentieth century psychology and sixteenth century cosmology. However, there's a major difference between the two disciplines. We twentieth century psychologists are now equipped with some extremely sophisticated twentieth century research tools for collecting data and constructing theory. Foremost among the theory construction tools is computer simulation. As should be evident at this point, I believe that computer simulation leads to a potentially serious problem: By making it so easy to construct and test extremely complex models, computer simulation removes much of the motivation to find simple and elegant theories—or even to search for simple kinds of behaviors to make theories of. Adding the degree of theoretical power allowed by computer simulation to a model that's only vaguely conceptualized to begin with is like equipping a covered wagon with a turbojet engine. The fit is just entirely inappropriate.

The other side of the coin is, as I mentioned earlier, that it's great fun to make computer simulation models. You can endow the model with anything you feel like, and you can get the data and the theoretical fit appearing in front of you on the computer screen almost instantaneously. As with video games, you get immediate feedback, and it's incredibly reinforcing. With a little work, the model can always be made to fit the extant data, thereby producing the illusion of success, at least for the moment (cf. Freedman, 1983; Keil, 1984). The whole enterprise becomes a game that's almost irresistible.

As pointed out to me by my colleague, Walter Schneider, good examples of computer simulation run amuck are found in the field of economics. Economic models combine economic theory with a host of assumptions, along with various types of economic data, in order to forecast some important economic phenomenon, say, energy usage. These models have been criticized for a variety of reasons, ranging from the validity of the assumptions on which they are based, to the quality of the data that go into them, to the ability of their users—often governmental agencies—to mold them in such a way as to obtain predictions that are favorable to their policies, instead of the other way around (e.g., Commoner, 1979; Freedman, Rothenberg, & Sutch, 1983).

Most of the criticism that is leveled against economic models can be boiled down to the problem that the models are simply too large, unwieldy, and ad hoc to allow unambiguous, trustworthy predictions. They are sufficiently malleable to be able to fit extant data rather well, but they fall apart when asked to forecast the future. I think the reason for this is clear: The models have been constructed more with the idea of accounting for data than of reflecting economic reality.

SHOULD COMPUTERS BE BANNED?

It may seem thus far as if I've painted an unnecessarily gloomy picture of the role of computer simulation in science. Am I being like the ancient naysayers who decried the invention of paper as evil because it would eliminate any motivation to memorize, and who then later bemoaned the invention of the printing press because it would spell doom to the art of calligraphy? Do I believe that computer simulation has no legitimate place in the scientific enterprise? Of course not. Despite the dangers that I've tried to illustrate, I think that there are several very valuable roles that computer simulation can and should play in the social sciences or in any other branch of science. This conference is, in part, an exposition and a celebration of these roles. Let me just mention a couple.

Extension of Theory to the Real World

Computer simulation is a superb technique for extending a well-established, durable theory from the domain of the laboratory to the domain of the uncontrolled, disorganized, external world. Wolfram (1984) provides some interesting illustrations. Consider, for example, the path of an electron in a uniform magnetic field. The behavior of an electron in such a situation is well understood and can be well predicted from fundamental principles. However, the path of the electron in some complex, nonuniform field cannot be solved analytically and cannot be predicted at all, except by computer simulation.

I want to reemphasize at this point that the laws governing behavior of electrons in general and of electrons in magnetic fields in particular are already well understood. A computer simulation whose purpose is to extend a well-established theory from simple to complex situations is very different from a computer simulation whose purpose is to model the complex situation to begin with without having the theory as a starting point.

Are there examples of these kinds of situations in psychology? I believe that, indeed, there are some very nice examples, particularly in the area of visual science. Consider, to briefly illustrate, the invited address given to this society last year by Brian Wandell (1984) on the topic of color constancy. The problem that Wandell addressed was: How is it that objects continue to appear to be the same color independently of the spectral composition of the ambient illumination? That is, how is it that an object in two different lighting situations (say, daylight and fluorescent light) reflects light to our eyes consisting of two entirely different spectral compositions and yet is perceived and reported to be the same color?

Wandell began with a very well worked out, very well tested, and very precise theory of visual information processing. This theory dealt with the relationships among entities that are firmly rooted in optics and physiology, such as spectral distributions, number of quanta arriving at the retina, and size of retinal ganglion cell fields. It was *only* after the theory itself was well worked out, well tested, and widely accepted that it was applied, via computer simulation, to actual scenes whose complexity exceeded the laboratory situations of uniform ambient lighting and sine-wave gratings.

Practical Applications

Sometimes, there's a need for information that ideally requires a theory, but for which no good theory exists. In pre-Keplerian times, for example, maritime navigators needed tables of planetary positions. Such tables were generated by the Ptolemaic or Copoernican theories. Tedious to generate, and error-filled though they were, these tables were certainly better than nothing.

Similar situations exist in the domain of psychology. For example, there is a need (or, at any rate, a market) these days for expert systems. An expert system is, of course, a computer program that simulates an expert in some technical field, say, aeronautical engineering. In principal, interested parties can seek advice from the expert system in the same way that they could seek advice from the expert him- or herself. The initial cost of the expert system is more than that of a human expert, but the subsequent salary is presumably lower.

Ideally, one would produce an expert system by combining a simple, durable theory of human cognition with a suitable knowledge base.[1] That's not possible, of course, since a simple, durable theory of human cognition is not available. But from the standpoint of those who would like to use an expert system, a system based on a mishmash of a theory is better than no system at all. Expert systems users are, like the ancient mariners, relatively indifferent to the sources of their practical devices.

CONCLUSIONS

I want to end this talk on an optimistic note by returning to the mind and motivation of Johannes Kepler. Afficianados of Kepler and his work might raise an objection to Kepler's behavior as I portrayed it in my fantasy. They might challenge the supposition that Kepler would capitulate to the computer's seductive powers. Such an objection might well be justified. As I said earlier, Kepler was a fanatical believer in the existence of underlying truths. He believed that the simple laws were there to be found if you were just willing to do the work required to find them. Much of what we know about Kepler, his motivations, and his personality leads us to believe that he would have resisted the temptation to build an enormously complex theory designed solely to account for data, computer or no computer. I would like to hope that social scientists will similarly come to resist the temptation to build complex computer simulation models designed with the purpose of accounting for complex data derived from complex situations. We can call that enterprise fun, we can call it a game, we can even call it a rough shortcut to the solution of practical problems. But let's not think of it as the ultimate goal.

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NOTE

1. At least this is my opinion. Workers in artificial intelligence who construct expert system models may well argue that a theory of cognition is irrelevant to their purposes.