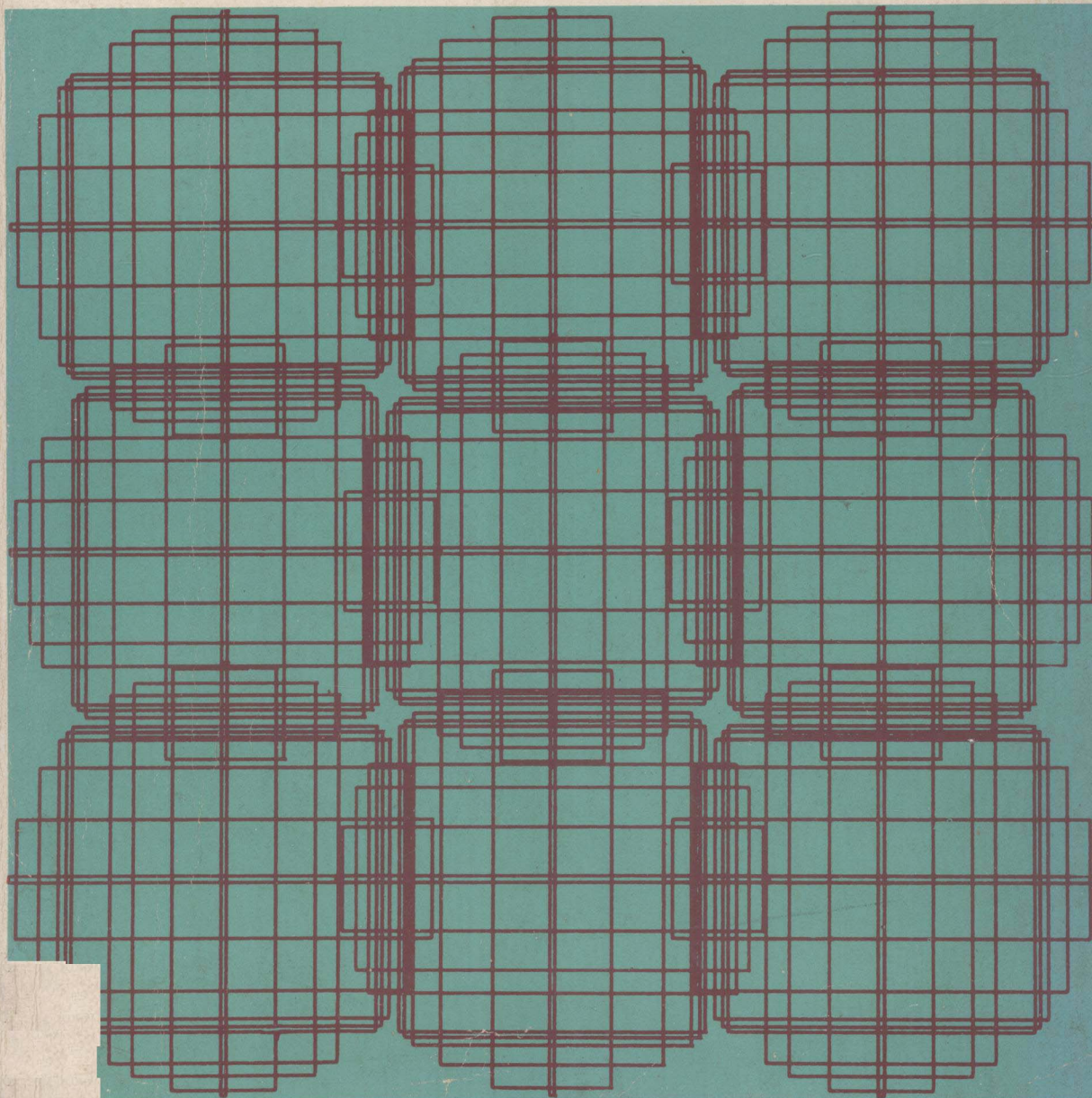


R.B. Coats
A. Parkin

Computer Models in the Social Sciences



Winthrop Computer Systems Series

Computer Models in the Social Sciences

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Foreword

Being the spouse of an anthropologist, I meet dozens of social scientists in the course of a typical year. I tag along to a few professional meetings. At cocktail parties given in honor of distinguished visitors, I get free drinks. During the dreadful annual cycle of faculty job interviews, I get dragged into the whirlpool of social events. I always try, in these situations, to palm myself off as a mere "faculty spouse," but all too often I'm introduced as someone who is "in computers" or "in systems."

Once the beans are spilled, it's only a matter of a negative exponential waiting time before I get the question: "Can you recommend a good book on simulation - one that a poor social scientist can understand?" Until recently, the answer has always been something like "Um, huh, oh, uh-hn, well . . . NO." Now, thanks to Coats and Parkin, my social life among the social scientists has taken a significant turn for the better.

In the six months since I got my hands on their manuscript, I've used it to help seven anthropologists, three sociologists, two geographers, one and one-half historians, and one-half of a political scientist.

I've become, in fact, the life of parties - even when there are biologists, chemists, and physicists present. I've given five graduate students terrific boosts on their theses. I have a waiting list of computer scientists eager to improve their own social lives - and to teach that super simulation course they've always wanted to give to the great unwashed masses.

In short, Coats and Parkin have made a wonderful difference in my life - which is why I pursued them until permission was given to put their lovely book in our *Winthrop Computer Systems Series*. Of course, now that it's published in the United States, my postage and copying bills will be reduced, but my reasons are not *entirely* selfish. I can't be the *only* person "in computers" or "in systems" who's married to a social scientist. And perhaps there are other computer people or systems people who, though not tied to the social, biological, or *whatever* science by affectional bonds have some sort of relationship with the "outsiders."

Perhaps you have a sister or a cousin or an aunt who is heavily involved in simulation - or ought to be. Perhaps you're

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tired of queuing up at your barbers' shop and would like to encourage them to make a little simulation study of their operation. Perhaps your department head would give you a well-deserved merit increase if you would only teach that service course in simulation - the course that's never quite made it over the Hill of Acceptability into the Valley of Greatness.

Or perhaps you, yourself, have always had a gnawing fear that there was more to simulation than writing programs in GPSS, DYNAMO, CSMP, GASP, SIMSCRIPT, SIMPL, SIMULA, ALGOL, or, heaven forbid, FORTRAN. Perhaps you, too, have been yearning for a text that teaches the WHY behind the HOW of simulation, and teaches in a style and language that ordinary human beings, scientists of all colors, business people, and even computer scientists can understand. If so, it's with the greatest pleasure that I introduce you to *Computer Models in the Social Sciences*. I'm sure you'll become great friends - even if it doesn't improve your social life as much as it did mine.

Gerald M. Weinberg

Preface

Our aim is to arouse your interest in the potential of computer models in explaining or predicting social phenomena; to explain to you in plain terms how such models can be built; to instruct you, if you wish, in some practical methods of designing and testing these models; and to give you leads to further information should **you** wish to extend your knowledge or skills.

In writing this book, few problems have taxed us more than this: what assumptions can we make about our readers and how can we organize the book to suit their different backgrounds? The notes that follow state our assumptions and the structure that has resulted.

May we first dispel any idea you may have that in order to understand things to do with computers you must be something of a mathematician? We are not sure about the origins of this common misconception - could it be because mathematicians played such an important part in the development of early computers? Could it be because of the spectacular feats of mathematics which have been achieved with the aid of computers? Or could it be that most people's early learning about computers came from their mathematics teacher who, naturally, drew his examples from the area he was most familiar with? Whatever the reason, let us assure you that to understand this book you need only simple algebra, a little statistics and an alert mind. Quite a large part of the book can be understood without the first two of these.

The statistical concepts used are: probability, randomness, frequency, histogram, distributions of the normal, negative exponential and uniform types, sampling from a distribution. If histograms and distributions are new to you, you will find a precise explanation in Appendix A.

We have assumed you have an appreciation of computers but little or no practical experience. The key concepts here are: program, instruction, memory, loop, branch or jump, subroutine. For readers with less knowledge than we have assumed, a very brief description of these is supplied in Appendix B. We hope the text will be sufficiently novel still to maintain the interest of readers with more computing knowledge that we have assumed.

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The particular techniques we introduce are used in a variety of places in the book and the reader may feel disappointed that there is no summary at the end which consolidates them. We find ourselves unable to summarize a collection of disparate techniques - perhaps the most useful thing we can do is list right here the techniques we describe and where they are used: processes involving sequential choices practically throughout, and sequential choices over time particularly in Chapters 3 and 4 and the hospital model of Chapter 7; stochasticity, sampling and the summation of distributions are used in Chapters 2, 3, 4 and 7 as well as the models of Smith and Vertinsky, and our suggested continuation of Reisman's experiments, in Chapter 5; the idea of discrete events is in Chapters 3, 4, and 7 and the similar notion of a threshold variable is in Smith's model in Chapter 5; the computer representation of a graphic network by a linked list, and the use of a push-down stack, are introduced in the memory model of Chapter 6; interactive models in Chapters 6 and 8; feedback through a time lag in Vertinsky's model in Chapter 5 and the macroeconomic model in Chapter 8; Greist's model in Chapter 5 is based on Bayesian statistics; solving a model based on a system of simultaneous equations is described in Chapter 8; and a number of other tricks pop up here or there.

We must apologize to readers who find the particular methods or models described do not immediately relate to their specific discipline: we can only hope that the knowledge of a technique may suggest a model - if not now, perhaps later. Certainly we find it difficult to imagine any system which could not - at least in principle - be modelled using the basic techniques we present here.

The book is organized as follows:

Chapters 1 to 4 - explanation
Chapters 5 to 8 - examples

The first four chapters are concerned with the purpose of computer models and some of the principles used in designing and constructing them. Chapter 1 may safely be skipped by readers with no interest in philosophical discussion or who consider this a digression from the important practicalities.

Chapter 5 is a survey which tries to describe the wider uses of models in the social sciences. The subsequent chapters are each concerned with a more detailed examination of an example model, drawn from the work of researchers in a variety of fields. A small part of the model analyses is in the form of technical notes aimed at those readers who have programmed a computer in the FORTRAN programming language. The general reader will, obviously, skip

over these pages, but if such a reader later feels inspired to try building a computer model without assistance from a trained programmer, he could tackle this by learning FORTRAN from one of the many books or training courses available and then returning to a consideration of the **skipped** passages. Additional notes for readers with FORTRAN experience are provided in Appendix C.

We have not made any serious attempt to give references in the subject area treated by a particular example model - if you wish to follow up one of these, the original work cites its source references and a citation index in your library will help you find any later work done on the model. What we have done is to collect together the titles of a number of interesting computer models in a bibliography at the end of Chapter 5, and we hope some of these will be useful to you.

We hope you do not expect too much from us. We cannot teach you how to easily conceive good models, for instance - if we could we would gladly make you into an instant Galileo or Newton. Moreover, although we hope to lead you gently, we have sought to add a dose of realism to the simplicity of our examples, to avoid oversimplification which may be misleading. We have come across some descriptions of computer models which are so simple that we are at a loss to understand why a computer was used at all, unless it was to impress the innocent reader. Generally, using a computer becomes worthwhile only when analyzing a system of some complexity which cannot easily be analyzed in more conventional ways. Practical techniques of analyzing complex systems is, perhaps, the unifying theme of the various parts of this book.

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1 Models in the Social Sciences

Introduction

A short while ago, one of the authors of this book set out to read a dozen or so books written on scientific method as applied to the social sciences. In his naivety, he thought he would obtain from this exercise a collection of rules or dicta which he could point to and say, 'It is the consensus of opinion of social scientists that these are the methods to apply in social inquiry.'

He had not got very far before he realized that he had plunged into a hot-bed of debate of which he was previously unaware. The issues ranged from philosophical ones (what is truth?) through self-examination (are the social sciences Sciences?, i.e. is the method of the natural sciences applicable to the social sciences?) to practical ones (e.g. how can we obtain an observed fact without the experimenter influencing his subject?).

In all this literature, he found the word *model* cropped up but rarely and the words *computer model* even less frequently. The first amazed him, for it had been his opinion that the concept of a model was fundamental to reaching a view on matters such as truth and explanation. The second was less surprising, since computers are a new invention and their potential in other applications has been more readily apparent than their possible uses in the explanation of social phenomena.

It was from this experience that the idea came to write a book, for social scientists, on the construction and use of computer models. Before that theme is developed, though, we feel it desirable to express some personal opinions on the place of models in general in the social scientist's quest for knowledge. We feel we can best do this by offering our own interpretation of some of the issues under debate.

Is social science a Science?

Let us argue from the less evident end in order to develop our point. We suppose the argument *ad hominem* is just about the most ascientific method one can imagine. The argument *ad hominem* is one where the proponent, without showing any experimental evidence, puts forward his case in the expectation that it will appeal to the prejudices or existing opinions of the listener. Arguments *ad hominem* are found everywhere, perhaps no more abundantly than in management literature distilling the wisdom of practising managers, usually propped up by examples or anecdotes. Of course, an example can be found in support of nearly any point. We are not arguing that such works are valueless for this reason - far from it, as we shall see later.

If we argue to you that there is no God, we will not have to talk for long if you are an atheist. If you are a believer, though, we will have a rough ride, probably without success even if we were to bring a scientific method into our case. If you listened at all, you would subject our method to a level of critical examination which it almost certainly would not stand. We

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might produce examples to show we did not find a God when we looked for him, while you might produce examples of events that could only have been wrought by a God. Atheism would be our prejudice, believing would be yours.

In debating the existence of a deity as an example we have, of course, chosen a case where views are usually deep-rooted - people do not have to believe in everything with the same conviction as they might believe in the existence or non-existence of God. But people hold more or less belief on many matters, particularly to account for those phenomena which confront them daily. Many such beliefs are founded on culture or tradition, and our point is that there is a culture and tradition enveloping scientists - natural or social - in the same way as any other group. A physicist will have a hard time proving something that no other physicist is willing to believe, but if he attempts to prove something which is already embodied in, or is a natural corollary to, existing beliefs, his path will be comparatively easy. Every day, experiments are made which produce results contradicting this or that law, but it is rarely that the law itself is seriously questioned in consequence. Rather, the experiment will be called into question (and this is a proper course), but experiments which support the established doctrines are perhaps not subjected to the same degree of critical examination. Another way of putting this view is that, even in the natural sciences, experiments are confounded by the observer.

Our theme is that all arguments are, at root, arguments *ad hominem*. Objectivity is impossible in a human being. Truth is founded on belief; belief is founded on confidence; confidence is founded on the explanatory and predictive capacity of models. To the extent that there is nothing else in this chain, the truth and the model are identities - there is no room in this scheme of things for an absolute or 'extra-human' truth.

Scientific method tests the models and influences our confidence.

Objectivity and subjectivity

Let us take *objective* to mean *based exclusively on observation, without being influenced in any way by prejudice or existing opinion*. (We should at this stage raise the issue of whether or not observation itself is or can be objective in the sense of being totally external to mind, but any discussion in that direction can only reinforce our case. We would be content to accept observation as purely external, if only because it does not affect our argument much one way or the other. As it happens, our personal model does not concede that observation - or anything - is external to mind, but it plays the role of an assumptive truth on which the human system works, rather as a mathematician's preliminary definitions allow him to construct an abstract algebra. The analogy with the mathematician is not complete, however, for the human system suffers from the complication that observations are unlimited and one observation can be modified in the light of others, rather like correcting the readings of an instrument for instrument error.)

It is our opinion that all facts are subjective, but we do not want to conclude that the concept of objectivity is without value. In our personal model of scientific method, subjectivity is a continuous variable bounded by two extreme values, unity and zero. At unity, subjectivity would be complete, i.e. the fact could be shown to rest on no observation whatsoever or whensoever, purely a product of the mind. At zero, subjectivity would be totally absent, the fact would rest entirely on correct observation. Our variable of subjectivity can infinitely approach either of these extremes, but cannot quite attain them. Objectivity is the limiting case at the lower bound. We cannot reach it; we can only approach it more closely.

Confidence

One writer described as 'insidious' the view that 'all knowledge is relative and the search for absolute truth without hope.' Where does truth lie in our scheme?

Our answer is that truth has little place in it, and neither does falsehood. Concepts of truth and falsity do not arise - they have no need to arise. If you asked us to *define* 'true' (in this context), we would say it is a word used to describe the attainment of a high degree of confidence in a model; but, since the degree of confidence is not explicit when the word is used, it is not a very precise descriptor.

This confidence is another important variable. Like subjectivity, we see it as a continuous spectrum with bounds of unity and zero. Unity, absolute confidence, cannot quite be attained. However many times you repeat an experiment, you can never show that the *next* repeat will produce the same result, because you cannot be absolutely sure until you have done it. You may have a very high degree of confidence but you cannot have (literally) absolute confidence. Some mind, somewhere, some day may be absolutely confident that his sun will rise tomorrow, only to be wrong. Similar arguments apply to absolute lack of confidence, except that here we would want to adjust the ordinary meaning of the phrase so that zero confidence is the antithesis of unit confidence, i.e. a model with zero confidence does not at all explain the phenomena and predicts them with only chance success, while a model with confidence of unity perfectly explains and always predicts the phenomena.

Statisticians have given us a scale with which to quantify confidence - the scale of probability - and other tools which influence it - measures of variance. A view which some writers seem to hold is that confidence exists only if you can quantify it on the scale, or that the experiment with no quantified confidence declared is somehow less scientific than one which has it. Can that be correct? Confidence is, surely, a subjective variable that existed long before there was a scale to measure it. Furthermore, quantification of some aspects of a model using the statistician's precise concept of confidence can be misleading if it draws attention away from other unquantified aspects which are fundamental to the overall judgement. For example, an opinion poll may quote 'confidence limits' which show how unlikely it is that the general population differs from the sample taken, but such limits account only for the statistical probabilities assuming that chance is the only factor that may have biased the result. Other factors such as the method of selecting the sample, the framing of the questions, the demeanour of the interviewer, etc., may have far more of a biasing influence than has chance and cannot be left out of the overall judgement of confidence just because they are unquantified.

We do not mean to suggest that quantification is not desirable, for we do believe the scientific tenet that the precise is to be preferred to the imprecise. What we are saying is that the seeker after precision must beware of illusory accuracy. Also, if you can subjectively reach an individual or collective view that this model inspires more confidence than that model (e.g. by demonstrating that that model has more, or more severe, deficiencies than this one) then we argue that progress towards truth has been made. There is an important caveat, though, for subjective judgements as well; the average human being seems to be prone to misjudgement, particularly of statistical likelihoods, when he relies solely on his intuition. Indeed, it is quite possible for a human being to make a series of judgements which, when taken together, prove logically inconsistent or irrational. This suggests that the intuitive judge should at least take steps to understand his own imperfections.

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It may be that you cannot, or cannot easily, or cannot find the way to, or cannot with the resources you have available, quantify your confidence in a model. Such a position is reached more often, perhaps, in the social sciences than in the natural sciences. It should not follow that the model must be laughed out of court - if you have any confidence in it at all, it is properly kept until you find another model which inspires more confidence.

Subjectivity and confidence

It is tempting, at this stage, to try to link our two variables by suggesting that confidence varies inversely with subjectivity. Certainly, we think this is so in relation to one given model - if you reduce the subjectivity in the model and it retains the same explanatory and predictive powers, confidence in the model increases.

Between models of different things though, or different models of the same thing, unquantified confidence is hard to compare with quantified confidence. This leads us to make a tentative distinction between natural science and social science, which may be relevant to the discussion. A sociologist, say, builds models of human behaviour and he is a human being; an atomic physicist, say, builds models of atomic behaviour, but he is not an atom. Every human being is a walking encyclopaedia of conscious and unconscious models of human behaviour and could claim a privileged insight into the workings of the human because of his membership of the class; but every human being is not a walking encyclopaedia of models of atomic behaviour and no equivalent privilege could be claimed. If a sociologist finds a given model of human behaviour subjectively appealing, there may be good reason why his confidence in the model increases. The new model appeals because it fits in with extant models, or even because it is an extant model which had not hitherto been consciously expressed. His extant models are those which have survived a test of time: personal models which remain after some process of natural selection. They may not have been tested according to a scientific method, but they have, nonetheless, been subjected to *some* tests and to that extent are worth *some* confidence. If a new model cannot be subjected to scientific tests, that does not exclude confidence. The sociologist, in other words, may use his knowledge of himself to build models of others.

It may be that this is what makes poetry so alluring to us. The poet, it seems to us, is a master modeller whose appeal lies in good part in our recognition of his model as part of our own conscious or unconscious set. If we do not recognise the model, the poetry falls flat. Perhaps poetry is the archetype of the subjective scientific method we have been describing.

It is more difficult to see the models of the atomic physicist in quite the same light. There is little reason to suppose that he might have any extant or innate model which would give him insight into the workings of the atom before he first started studying the subject. Of course, the physicist develops personal models of atomic behaviour as his reading and experimentation progress, but are these models ever subjected to the same *quantity* of tests as his personal models of human behaviour, which are more or less continuously tested from the time he is born?

To summarize, we are saying that there is some argument that unquantified confidence that may exist in models in the social sciences is generally better founded than unquantified confidence that may exist in models in the natural sciences. Quantified confidence may be preferable to unquantified confidence, but unquantified confidence is an acceptable substitute until quantification can be done and the only alternative when quantification is impossible.

Explanation and causality

In our view, there is no end to the chain of causality. As Bacon put it in his *Maxims of Law*, discussing, we think, the legal doctrine of proximate cause, 'It were infinite for the law to consider the cause of causes and their impulsions one of another, therefore, it contenteth itself with the immediate cause and judgeth of acts by that, without looking to any further degree.' The immediate cause is not a happy concept for the researcher looking for precise antecedents to an event. If we were to ask you, 'What caused the Treaty of Rome to be signed?', we would hardly be satisfied if you replied, 'It was the action of a pen moving over the document' and left the room with an air of finality. (The law, of course, does not leave matters there, and modern definitions of proximate cause seek to distinguish the 'efficient' cause from that which is merely proximate in time. However, these definitions need interpretation in the light of the circumstances and to us imply the selection of a feature cause from a general background of causality.)

In the pursuit of knowledge, what we want is explanation, which we define as the provision of a model or part of a model which was missing from the questioner's existing set. We cannot be given the cause (i.e. all of the cause; we might be given a cause or some of the cause), but we can be given the explanation. It follows that explanations can be unique to the questioner. What is an explanation to one person may not be an explanation to another, who has different extant models. It is perhaps as well that people of a culture share largely the same models, otherwise science would be chaos.

Perhaps we should not have implied that explanations are finite. Maybe explanation, in the final analysis, is as infinite as cause, but at least explanation can answer the question to the satisfaction of the questioner - the mystery remains in the infinity of questions that can be asked.

The purpose of a model

It is our case that for many phenomena that affect an individual in his everyday affairs - and for many others that affect him only by arousing his curiosity - that individual needs a mental model of the phenomena. Many such models are formed quite unconsciously for a daily practical purpose. Such models need not be correct; in fact, one does not so much ask whether or not the model is correct as whether it adequately or inadequately fulfils its purpose. When we travel to work, we unconsciously use a flat-Earth model of the terrain, as we think most people do on their ordinary journeys. In estimating when we are to arrive, we do not take into account the increased distance we shall have to travel on account of the curvature of the Earth; in our imaginations we could see our office from home if it were not for the intervening hills, trees and haze. That such a flat-Earth model is adequate is easily demonstrated by the fact that people made local journeys for quite a while before Magellan circumnavigated the world. Of course, the prediction of this model that we will fall off the edge when we reach the horizon is a limitation (not, as it happens, a particularly important one in this case since we would find we could never reach the horizon). Only when the model's limitations became important, as with a long sea voyage, would we then (consciously) choose a different model of the Earth for our journey.

We recall a phone-in radio broadcast in which a listener, anxious to prove the fallibility of scientists, made the point that 'scientists believed Newton for hundreds of years before Einstein proved him wrong.' It is hard to imagine a statement more inconsistent with the view we are putting.