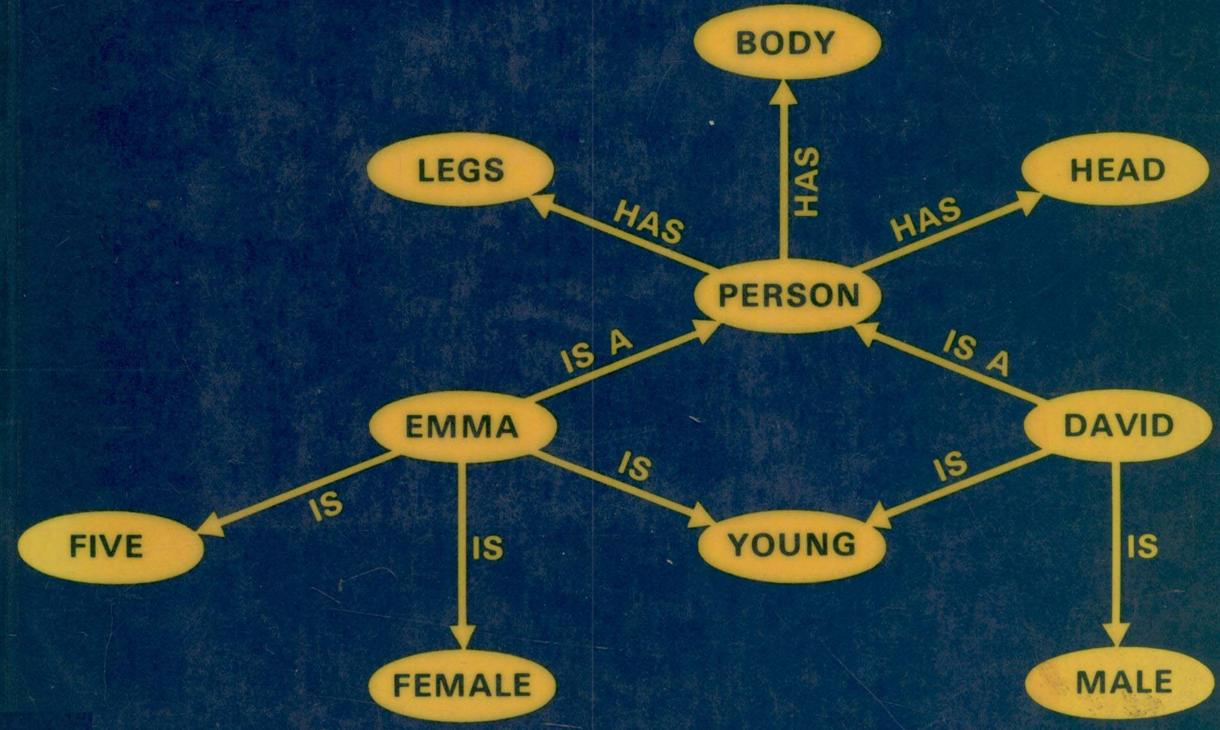
Computer Models in the Social Sciences

R. B. Coats and A. Parkin



Computer Models
in the Social Sciences

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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 précis 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 than we have assumed.

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 over-simplification 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.

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.

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Contents

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 value-less 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 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.

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 cicumnavigated 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.

Newton's model of the physical world was brilliantly 'right' when it was built, and is still 'right' to this day. The limitations it has are of consequence in only a minute proportion of its possible applications. Some sort of argument could even be raised that Newton's model is more 'right' than Einstein's, because it explains and predicts by building on the extant models of a large number of individuals, whereas Einstein's is useful only for a class of person with a minority set of extant models; but this argument would involve us in comparing the value of explanation and prediction for the ordinary educated man with the value of explanation and prediction for nuclear physicists and astronomers. We cannot make this value judgement, so we simply say that all models are 'right' when applied to a purpose for which they are useful. Only when you define the purpose of a model can you begin to argue that this model is more 'right' than that.

We do not think this suggestion is as far removed from classical scientific views as might at first appear. If we were to suggest that scientists want, as a common purpose of their models, that they should suffer from as few limitations as possible so that they tend towards universality of prediction (if not of explanation), perhaps we would be locating the place of scientific models in the class of all models. (To make this complete, we should also say that between equally powerful scientific models, the simpler is to be preferred to the more complex.) We hope that by putting matters this way we have exposed the value judgement implicit in science and shown the choice that may have to be made between universality of prediction and universality of explanation.

Scientific method

We see the rôle of scientific method as one of rigorous testing of models to increase or reduce confidence in their predictions, by exploring their limitations.

Although such methods may be rigorous, they cannot be perfect. A strictly controlled experiment is a philosophical impossibility, since two treatment conditions cannot occupy the same place at the same time. It may be that the chemist feels justified in assuming that his molecules will exhibit the same behaviour on one table top as another, or from one hour to another, but a social scientist cannot strictly allow himself similar assumptions, for it is common knowledge that human behaviour can change with time and environment.

The controlled experiment must be seen as another ideal concept, the variable being the degree of confounding that takes place. The less confounded experiment inspires more confidence than the more confounded experiment.

We suggest that in the social sciences confounding is often more difficult to avoid, or is at any rate more patent, than in the natural sciences. Even in the laboratory, a human subject (unlike a molecule, we assume) is at least aware that he is the subject of an experiment, even if he has been misled as to its purpose, and that knowledge might influence his behaviour. Outside the laboratory, the exposure to a large number of other factors is obvious and the degree of confounding may be very large.

Again, we suggest that just because an experiment is confounded there is no reason for rejecting the model under test. If the model is still the best available, that is progress. The proposition that social science is not scientific because all social experiements are confounded is defeatist; surely, the only way forward is to take one's best models and try to eliminate through ingenuity or patience such confounding factors as one can identify - if the model still holds water, it is worth more confidence than before. It seems to us that this is precisely what happens in the natural sciences, any difference being only one of the degree of initial confounding likely to be present.

Summary

We have tried to demonstrate the value of the concept of a model in truth and explanation and that objectivity, confidence and confounding of experiments are not black-and-white issues but variables that may take all shades of grey. It so happens that in many areas of the social sciences, the inquirer is more open to subjective inference, less able to quantify confidence and more easily confounds his experiments than his natural-science counterpart. This does not mean that the social scientist is unworthy of the title 'scientist'; at worst, it means only that he must be especially circumspect and that he may have to live with models that are difficult to test. While testing of the models is incomplete, the best model is still the best model.

The computer may be a tool which can help in the testing of models under more controlled conditions, or which can give insight into the working of a complex model, or which can be used to work a model of a process which may be too complex or tedious to work in any other way. These themes are developed in the subsequent chapters.

Further reading

There is a long history of philosophical debate in the social sciences, and the voluminous current literature indicates that the issues are not dead. We have tried twice to produce a short bibliography for the reader who wants to consider more of this subject, but we are not satisfied with either attempt. The first one had the classic material which could probably be uncontentiously included, but which would not reflect modern authors' viewpoints. The second tried to be more modern, but would probably be attacked from all sides for its lack of balance. We have decided that we prefer to let the reader decide his own strategy, if he wants to go down this path (after which exercise, we must forewarn, he may well feel he is coming out along the same route as in he went). The only other precautionary word we would offer is that the reader should not fight shy of some of the early classics of writers such as Popper, Russell, Ayer or Kuhn - these authors had a powerful command of language and are a good deal more understandable than many of the modern commentators.

There are plenty of introductory books to help; the reader need only inspect the library shelf. We have not found an introductory book which we felt gave a fair representation of ideas across the breadth of social science disciplines, schools of thought, and cultures, but in the British idiom an interesting book is Barker, P. (ed.), *The Social Sciences Today*, Edward Arnold, London, 1975 - a slim volume in which ten authors give a concise introduction to some of the values and literature of their disciplines.

If you do go down this path, you will be in need of refreshment when you come staggering back out of the fog. A taste of Ford, J., *Paradigms and Fairy Tales*, Routledge and Kegan Paul, London and Boston, 1975 (two volumes) may be reviving, or perhaps you will warm to the aggression of Andreski, S., *Social Sciences as Sorcery*, André Deutsch, London, 1972.

Postscript (reproduced with permission)

Science report

Physics: Fundamental law questioned

Dr Daniel Long, of Eastern Washington State College, has cast doubt in *Nature* on the inverse square law for gravity, which is close to the core of scientists' conception of the physical world. So fundamental is the inverse square law that he is likely to have an uphill struggle to persuade other physicists to take his ideas seriously.

For some years Dr Long has been drawing attention to the inadequate verification of the law of Newtonian gravitation, namely that the force between two bodies is proportional to the product of their masses and inversely proportional to the square of the distance separating their centres of mass.

Whereas astronomical observations have now largely confirmed the perturbations which Einstein's general relativity puts into the simple inverse square law at great distances, laboratory-scale tests at distances of a few tens of centimetres have not been done in any great number (the relativistic perturbations on the inverse square law would be trivial at such distances).

In an earlier study Dr Long accumulated such measurements of gravitational attraction as have been made in the laboratory (some of them from nineteenth-century experiments), and showed that they were not strikingly good evidence in favour of the inverse square law ; in fact, a law in which there was a slight decrease in attraction at short ranges relative to the inverse square laws was a better fit to the data. He has now pursued that further by designing his own apparatus to measure those gravitational attractions.

The equipment is simple and closely resembles that used in earlier experiments. A thin suspended fibre supports a crossbar, from one side of which is suspended by threads a ball of mass 50 grams. On the other side of the crossbar is a small electrostatic plate mounted vertically and in the plane of the crossbar.

Heavy rings, weighing up to 50 kg can be brought close to the suspended ball ; they will exert a small horizontal gravitational attraction roughly a hundred millionth of the vertical attraction of gravity. That horizontal force should cause a slight rotation of the crossbar, but it is counterbalanced by a force on the electrostatic plate on the other side of the crossbar. That force comes from a voltage applied between the plate and another plate near by unattached to the suspended system. The size of the voltage is a measure of the gravitational attraction.

Dr Long found that a large 50kg ring at almost 30cm distance, and a small 1kg ring at 4cm distance which, according to the inverse square law, should have exerted the same attraction on the 50g ball failed to do so. The discrepancy, measured over repeated experiments, was a fraction of 1 per cent and is strikingly in agreement with the earlier observations. Dr Long proposes a small modification to the inverse square law.

In the covering letter which Dr Long sent to *Nature*, he accepts that the paper is controversial, " and in need of sympathetic treatment ". Many physicists will reject the implications of the experiment out of hand as going against their intuition. Something, it will be asserted, must have been wrong with the experimental conditions.

That attitude has often been adopted in the past and, despite the mythology of science, has often been entirely justified. But the experiment is not very difficult to repeat, nor is it impossible to find another type of experiment to approach the question from a different angle. Who is going to try to prove Dr Long wrong ?

By Nature-Times News Service.

Source: *Nature*, 260, 417 (1976).

2 Computer Models

A case study

The Ruritanian Sports and Social Club Committee was discussing its expenditure for the coming year. The debate was particularly pointed because, in the season just ended, payments had exceeded receipts from its two annual fund-raising events (the Gala Day and the Barbecue Dance) by a substantial amount.

'Look,' said the Membership Secretary, 'we can't possibly afford another year like the last - the members won't stand for it. We started the year with 25 000 Ruritanian dollars; we authorised expenditure on new buildings and equipment to the tune of $20 000; yet the Gala and Barbecue were such a wash-out that we netted only $5000, leaving us with a credit balance of a mere $10 000. We must make quite sure we don't make a loss this coming year, so we can build up our reserves again.'

The Treasurer looked thoughtful. 'What do you mean', he said, 'by *quite* sure? If you mean there must be absolutely no risk of a loss, then there is only one course of action open to us - we should not spend anything on new facilities next year. Indeed, to be strictly accurate, we should not even authorise the preliminary expenditure on the Gala and Barbecue - after all, it's on the cards that we could make a loss on both events. Look what happened this year at the Gala.'

'That's a bit thick, isn't it?', interposed the Events Organiser. 'The only reason we did so badly this year is that it poured with rain on both days. The chance of that happening two years running is so remote that we can ignore it.'

The Treasurer thought to himself, 'There's something wrong there. I don't see how this year's weather can affect next year's. The chance of bad weather next year must be the same, irrespective of what weather we had this year.' He opened his mouth to make this point but hesitated, wondering if the Events Organiser would understand him. Then he had a vision of an ensuing argument, a considerable detour from the matters in hand and an even more protracted meeting. He shut his mouth again.

The Membership Secretary ended the hiatus with a bland inconsistency. 'We can't have *no* expenditure next year - there would be uproar from the members if we failed to provide any new equipment at all. Then there's the repairs to the pavilion roof, a new filtration system for the swimming pool,...'; he went on with a long list which made it evident that there were ample outstanding projects to soak up whatever funds were made available.

'Then what you must decide', declared the Treasurer, 'is this: exactly what risk of loss is the club prepared to accept?'

There was silence for a moment, as nobody knew how to answer this question. 'I should have thought', said the Chairman, tentatively, 'that we want to be about 90 per cent sure that we will more than break even.'

'And accept a 10 per cent risk of loss?', queried the Events Organiser, doubtfully. 'I suppose we could afford a small loss, but I don't think we want more than, say, a one per cent risk of losing more than about $5000.'

'And I don't think we should accept any risk of a deficit as large as

$20 000 ', added the Treasurer, 'since we would never find large enough overdraft facilities to cover it and that would mean we would have to fold up completely. Now let me get this straight: the feeling of the committee is that the planned expenditure for next year should aim to give us a 90 per cent chance of making a profit. We are prepared to accept a small risk, one per cent, of making a loss of $5000 and no risk at all of making a loss greater than $20 000?' There were murmurs of assent. 'Of course', continued the Treasurer, 'the last constraint means we cannot contemplate more than $20 000 expenditure, for if we are avoiding any risk at all of a greater loss we must consider the very worst case, i.e. the possibility we will have no receipts.'

The Membership Secretary looked glum, but no one was prepared to counter this argument.

The Treasurer warmed to his analysis. 'It may be, when we dig deeper into the problem, that the expenditure which will give us a 90 per cent chance of making a profit is less than $20 000 - in which case we choose the lower expenditure, right? And if that expenditure gives us more than a one per cent chance of a $5000 loss, then we choose a still lower expenditure that will bring the risk down to one per cent?'

The Committee members fidgetted, many eyes on the clock, and nodded. 'Give me until the next meeting to think about it and I shall try to find the figure which meets these criteria', concluded the Treasurer.

The Chairman closed the meeting.

* * *

Considering the problem in the comfort of his study, the Treasurer was beginning to realize that his problem was not an easy one. He was not worried much by what was meant by 'a 90 per cent chance' of making a profit. His concept was that there was a certain level of expenditure which (if the same policy were applied every year thereafter and other circumstances did not materially alter) would tend, in the long run, to produce receipts exceeding payments in 90 per cent of the years. It did strike him that a very large number of years - more, perhaps, than his remaining years as Treasurer - might be needed before anyone could say whether his recommended figure had been right or wrong, but he found this more of an encouragement to continue than a philosophical obstacle.

No, his real problem was that the club had been running only five years and he did not have much past data to work on. From the preceding years' accounts, he constructed the chart shown in Fig. 2.1.

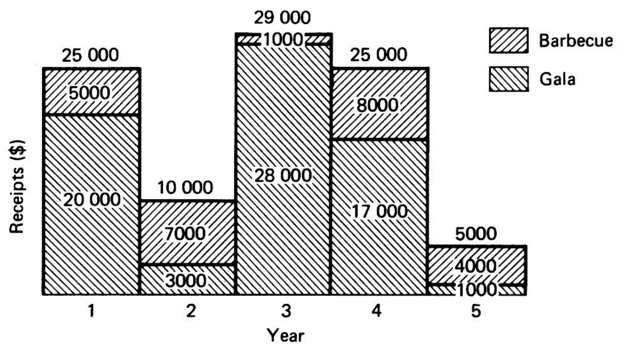

Fig. 2.1: The Treasurer's chart of the Club's receipts in the preceding five years, divided between the two events