New Studies in International Relations

THE FIELD OF NATIONS

Nigel Forward

An account of some new approaches to international relations.

New Studies in
International Relations

Series Editor: Anthony de Reuck
Research Fellow in International Relations,
University of Surrey

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The Field of Nations

An Account of Some New Approaches to International Relations

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Palgrave Macmillan

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N. S. F.

1 Introduction

When a theory is in its infancy it is sometimes difficult to be sure it is there at all. It is as if one were watching the waters receding after a great flood, to reveal at first no more than a few small isolated pieces of ground. Why should they be taken for anything more than the insignificant islands they seem to be? How should one believe that they are really the tops of great hills? By what power of insight is one to imagine the fertile plains that lie between, the clustered towns, the busy industry and thoroughfares that will spring into being when the waters have fallen away and the area has been peopled? It may be that the theory of international relations is in a similar state. Indeed the search for 'islands of theory' was put forward nearly twenty years ago as a rallying cry for students of international relations (Guetzkow, 1950).

A. N. Whitehead speculates somewhere that

> there are an indefinite number of purely abstract sciences, with their laws, their regularities, and their complexities of theorems – all as yet undeveloped. We can hardly avoid the conclusion that Nature in her procedures illustrates many such sciences. We are blind to such illustrations because we are ignorant of the type of regularities to look for. In such cases, we may dimly sense a sort of familiarity attached to novel circumstances, without any notion how to proceed in the analysis of the vague feeling. (quoted in Woodger, 1937)

I do not suppose he would have favoured an attempt to apply what he said to the social sciences, but I believe it expresses well the state of mind of the student confronted by the great complexity of world affairs and lacking, as one does, any formulated general theory of international relations.

Of course, the committed political scientist will not be

deterred by the infancy or fragmentariness of his science; nor will the enthusiastic mathematician with ideas for applications to the social sciences – two of the targets at whom this book is aimed. But I have also in mind a wider, more remote section of the reading public, some of whom might pick up a book like this and have a look at it; sceptical, thoughtful people with no specialised interest in international relations – or for that matter in mathematics – who might be intrigued by the title and wonder what on earth it could be about. I am sure that they would say, to a man, that looking to mathematics, to computers, to the scientific approach generally, for solutions to the problems of international relations was so much pie in the sky. And they would be right. But to look in those directions for *aid* – on a modest scale – in finding solutions is not necessarily mistaken; and it is to give colour to that assertion that this book has been written.

If ever there was a time when the study of international relations was in need of a helping hand from some new and untapped source of intellectual energy, that time is now. The brooding menace of nuclear weapons places an almost intolerable responsibility on the shoulders of a few statesmen; the degree of 'nerve' (or callousness) that is required of them is almost superhuman. We know enough of the drama of human conscience that was played out, within both the American and the Russian leadership, during the Cuba missile crisis of October 1962 (which I analyse in Chapter 6), to realise the risks and the strains that must have been involved.

There are two sides to the question. The first, to which the greater part of this book is devoted, is the attempt to detect a theory, or fragments of a theory, and in particular the search for an adequate model of the international system or of parts of it, as a vehicle for theory. The examples of other men's research that I have plucked out of their context to illustrate my thesis should be judged by reference to the original papers quoted, and not on my partial and perhaps impressionistic presentation which lays no claim to be a balanced critique. The only virtue – if it is a virtue – of these summary accounts is that they make it possible to relate together many things that are not often found between the same covers: we here encounter in quick succession states-

men and statisticians, cybernetics and Cubanautics, graph theory and game theory, Raymond Aron and random walks, mathematics and the Middle East. I hope that these juxtapositions, however inept in themselves, may here and there strike a responsive chord and – who knows? – perhaps even lead in some reader's mind to the Aha!-experience of intellectual discovery that we all long for.

But almost as important, if less generally recognised, is the practical matter of bringing the results, if any, of such theoretical work into effective use at the decision-maker's elbow. The gulf between academic and practitioner in any field is traditionally a deep one. Reasoning to a conclusion and reasoning to a decision, deciding what to think and deciding what to do, are different mental processes and those accustomed to one have as a rule little patience with those accustomed to the other. There is a chronic problem of communication between the two. Moreover it is not to be supposed that new techniques will be grafted on to the existing decision-making apparatus without changing it, any more than a computer can usefully be introduced into a business organisation unless the whole organisation has first been subjected to a rigorous systems analysis. Who can say what would be revealed by a systems analysis of the Foreign Office? Chapter 8 of the book is a squib of the imagination that the reader may regard as light relief, although it has a serious aim as well: to give an impression of the organisational fireworks, as well as the intellectual and imaginative effort, that will be called for if machine simulation and other theoretical aids are ever to be harnessed to practical politics.

I express in Chapter 7 (pp. 141-2) the creed of those like myself who believe there is something useful to be discovered by the scientific method. What is the consequence of taking the other view? It is to accept that there is no element of regularity or predictability, however complex, to be found in international affairs – even when all the leading nations of the world are expressly looking for it – beyond what can be revealed by traditional historical analysis and study of individual nations, individual situations, individual leaders each in their unique context. It is to say with Hans Sachs at his most despondent, in Act 3 of 'The Mastersingers':

Wahn, Wahn, überall Wahn!
Wohin ich forschend blick
In Stadt- und Welt-chronik
Den Grund nur aufzufinden
Warum gar bis auf's Blut
Die Leut' sich quälen und schinden
In unnütz toller Wuth!

(Madness, madness, all is madness, wherever I look in the history of cities and the world, seeking to discover the cause why people torment and flay each other to the quick in vain and senseless frenzy!)

We can take some comfort from the fact that by Act 5 Sachs had cheered up and was quite himself again. This book springs from the conviction that the international scene, though certainly no laughing matter, need not be 'überall Wahn'. It is written in the hope that among its readers there will be some who are ready to admit the possibility of the world's becoming a saner place and perhaps a few who will be encouraged, through theory and through practice, to help to make it so.

2 Approaches to International Relations

The Scientific Approach and Objections to it

Since 1950 the study of international relations has undergone a revolution. So, at least, the revolutionaries would have us believe. It is certainly clear from the periodicals that there has been intense controversy between those advocating a new, 'scientific' approach to the subject and those who defend the classical, 'historical' approach. What is at issue between them?

It may be unwise to try to capture the essence of scientific inquiry in a phrase, but I would say – following Karl Popper – that a scientist, in this or any other field, is one who in offering explanations of phenomena provides the criteria by which others may refute his results. If he states a hypothesis, it is in terms that make it clear what evidence would be sufficient to disprove it. I think any truly scientific statement must satisfy this condition. As the American historian Aydelotte (1966) put it, 'an imprecise or slipshod formulation is impregnable: a statement that has no exact meaning cannot be disproved'. It is the scientist's aim to make only vulnerable statements. It is a good test to apply to generalisations and pontifications of all sorts.

Mathematics is the language of science and so is likely to be encountered in the course of any scientific work. It is well to recall that mathematics is not exclusively concerned with number: all the modern descendants of geometry – topology, combinatorics, graph theory – are there to remind us that the logic of structure and relation are as much a part of mathematics as the logic of number. It is true, though, that the social sciences generally tend to be quantitative, and one of the main uses of mathematics has been to provide techniques – normally statistical – for the manipulation of

quantitative data. Political science is no exception.

Let us venture a comparative characterisation of the 'scientific' and the 'historical' approaches to international relations. The *scientist* holds that there are unperceived regularities in international affairs that would be laid bare if relevant data were available; aims to formulate with the aid of mathematics a deductive system of propositions about the international scene that are 'operational' and can be tested directly against events (i.e. verified or falsified) given the necessary data; to this end tries to render an account of the world in quantitative terms as far as possible; is wide open to analogies from other disciplines: and seeks a position of detachment from which to observe the human scene, the aim being to describe, perhaps to explain and possibly to predict – not to prescribe.

The *historian* holds that events are unique; and that the interactions of large organised groups of human beings, where there is no laboratory and no repetition of identical events, are the least promising field for the application of methods appropriate to the natural sciences; aims to provide by literary description and analysis the best possible account of past events from which, by inductive methods and study of human nature, clues can be derived for the future; and holds that the complex mixture of fortuitous events, human free will and the intervention of leaders with unique qualities makes futile any attempt to reduce the international scene to systematic, let alone quantitative, terms.

My aim in this book is to explore the possibilities of the 'scientific' approach. The title *The Field of Nations* may already have suggested as much, containing as it does a double meaning, almost a pun. The everyday meaning of 'field' is certainly intended, whether it is a battlefield or Piers Plowman's 'fair feeld ful of folk' that comes most readily to mind as a picture of the international scene; but there are also the special meanings of the word in mathematics and more particularly in physics – as in electrostatic field, magnetic field and gravitational field. In physics it is found convenient to distinguish those phenomena that have a local habitation and a name – mass, charge and so on – from those that are diffused in space, in other words the field, even though no boundary can logically be drawn between point

phenomena and field phenomena. The field of nations, therefore, is that part of the international situation that it is convenient to regard as distinct from the nations themselves and having no unique location, even though it both presses upon the nations and is called into existence by them, and is logically inseparable from them. But it would be a mistake to infer that because we have borrowed this general idea from physics it will be from physics that most of our inspiration will come in developing a science of international relations. That is not to be expected, for reasons that will be gone into in later chapters of the book.

Reverting to the scientific approach generally, the main objections of principle to be heard are:

(i) that the number of variables is unmanageable if all contributory factors are to be accounted for;
(ii) that there is no scope for controlled experiment;
(iii) that a theory of politics affects events, through the minds of men, and so can never 'catch up';
(iv) that human purpose, which is the motive power of politics, is beyond the scope of scientific inquiry.

Without going into a lengthy analysis I shall simply say that I regard (i) and (ii) as serious, if obvious, difficulties, though not such as to close the door on all possibility of useful progress. As to (i), the proof of the pudding is in the eating and the whole endeavour must clearly be to reduce the variables to a manageable few. As to (ii), the present, the future and the unexplored past are the laboratory; in this respect international theory is in much the same case as meteorology.

Objection (iii) should be if anything an encouragement to the scientist: let us hope his efforts do have some effect on events. Evidently any prediction about a given international situation will have to take into account the sophistication and self-consciousness of the principal actors. The economic theories of Keynes did not become self-defeating when they were published; nor was it a disaster for psychoanalysis when Freud came out in paperback. The logical difficulty would arise (and even then might not be insuperable) only if the theory were expected to furnish a complete universal forecast

of all future events, which is much more than is expected of any theory in the realm of international relations – and indeed in that of physics.

(iv), like (iii), would if it were true bring all the social sciences down in ruins. I recognise that, for some, this may not in itself be a sufficient refutation: let us go another way about. At the risk of being thought to quibble in answering a serious question, I must point out that (iv) does not itself qualify as a scientific statement, since it is not at all clear how one could ever be expected to disprove it. It is perhaps not so much an argument as a feeling of disquiet that is at work here – a feeling that these anonymous regularities in human affairs are an affront to the sanctity of the human individual. In recent centuries the species has received, and survived, a number of shocks of this kind: the shock of learning that the Earth revolves round the sun; the shock of learning that man is descended from the lower animals; the shock of learning that the total amount of money saved by individuals in a community over a given period is equal – not *approximately* equal but exactly equal if the terms are rightly defined – to the total amount of investment expenditure of the community during the period. And under these repeated blows to its dignity, the species still survives. A few more regularities will, one may reasonably hope, be absorbed without fatal effect. And of course the truth is that the personality and reality of the individual is not a whit diminished by any of this.

Maxi-theory and Mini-theory

Post-war academic work on international relations from the scientific point of view divides into two distinct branches which can be conveniently referred to as maxi-theory and mini-theory. Maxi-theory examines the structure of the international system and seeks to account for the broad movements and structural changes that may take place over a period of years or decades. Mini-theory, which is concerned with texture rather than structure, seeks to explain in detail how a situation develops in the short term: negotiation, bargaining, crisis, conflict, the options open to the various

actors, their choice of moves and of strategies and the relations between these choices – these are its stock in trade. This book will be almost entirely concerned with mini-theory. Before we take our leave of maxi-theory, however, there are a few things to be said.

The Olympian systematisations of Spengler and Toynbee were maxi-theories. So in more recent years are the attempts of Morton Kaplan, Rosecrance and Burton to apply general systems theory to international relations. Their subject-matter is the consideration of the laws governing the bipolarity or multipolarity of the international system, the alignment of nations, the relations between great and small powers, tendencies towards integration and disintegration and the working of social forces across and within boundaries, the means by which change can be brought about in the system, the relations between the inward condition of a nation and its outward reaction to its environment in the international system. Common to all this work is the idea, central to the general systems approach, that the sequence of international events may be studied on several different levels:

- the international system itself, where the nations are regarded as black boxes and their interactions are studied;
- the national systems, where the 'national' black box is opened up and the principal actors (sovereign, legislature, decision-makers, etc.) are studied in their relationships to one another, to throw light on the means by which a national response is provided to a stimulus from the international environment;
- the national subsystems and sub-subsystems, where the actors are normally individuals studied in their various aspects as members of organisations;
- the individual (especially the individual decision-maker) who is studied to account for the responses he makes to his environment and the contribution he makes to the groups to which he belongs.

Though these categories have been used in one way or another since time immemorial, it is an advantage to have them recognised as a standard framework for analysis, and we shall have occasion to use it once or twice later in the book.

There are of course many writers on maxi-theory who do not use the general systems approach and whose claims to be described as scientific have varying force. The best known among the American writers are Morgenthau, Hoffmann, Waltz and Claude, among the British, Hinsley. It would require another and different book to review this literature and I shall not attempt it here. But I find it necessary to mention Raymond Aron, whose monumental *Paix et Guerre entre les Nations*, now translated into English (Aron, 1966), has already laid claim to classic status. His findings are on the whole traditional and pessimistic; the novelty of his approach is that he uses the whole panoply of modern sociological theory to arrive at these familiar conclusions. His position, though not exceptional, is exceptionally clear; more of an anti-theory than a theory, as Hoffmann (1963) points out in a sympathetic review, it provides an admirable test-bed on which to try out the more radical theories of the day.

Aron's position may be summarised as follows:

(i) The diplomatic-strategic conduct of nations is essentially indeterminate since the aims of each are multiple and contradictory. It is no use hunting for *the* rational course of action in a situation; the best you can hope to do is to grasp its intelligible structure (pp. 88-93).

(ii) The history of conflict is open to many interpretations: the least fragile is that hostility between men in organised society is natural, and will not bow before a collective authority except in the face of a bigger menace that threatens to lead to wider hostilities (p. 755).

(iii) The system of nation-states contains an inherent competitive element and hence an inherent possibility of war; there is no set of crucial issues whose removal or adjustment would guarantee peace (pp. 706-8).

(iv) A legally pacified world – 'paix par la loi' – could be attained only by means of an imposed empire or a planetary federation. The conditions for federation would be:

  (a) a degree of mutual confidence and homogeneity in the world community;

  (b) a degree of common respect for juridical and constitutional notions;

(c) monopoly of armed force, which no federated member-state would be able to use to impose its will on others. This is philosophically not impossible but practically most unlikely, in the face of renewed proofs of 'tribal consciousness' (pp. 735-6).

(v) The moral is the disappointing one 'politics as usual, mais avec encore bien plus de prudence' (to use Hoffmann's words). The super-powers have a special responsibility: they have shown signs of a prudential tendency, and must take progressive steps to reduce the risks by such means as diversification of arms and limitation of political objectives (pp. 689-700).

It is true that Aron allows himself a glimpse of Utopia on almost his last page, a time when

> the nations will gradually surmount their prejudices and their egoism, fanatics will cease to incarnate their dreams of the absolute in political ideologies, and science will give humanity, grown conscious of itself, the possibility of administering the available resources rationally, in relation to the number of the living. The organisation will be universal, the communities of culture will be numerous and small. The (power-states), having completed their mission, will wither away into a pacified humanity. (p. 786)

For all that, he gives no inkling of how this divorce of administration and culture could come about in a heterogeneous world, and shows no sign of believing these changes to be possible. If he is right it is a bleak prospect. As the above summary makes clear, his main target is maxi-theory, though his clause (i) is destructive of hope for mini-theory too and we shall shortly have to take issue with it (see the section 'Critique of Game Theory as a Model of Political Situations' in Chapter 3, pp. 43-50).

Aron has cast a great rock into the sea which future navigators will ignore at their peril. It is a sobering landmark which will loom up before the reader from time to time as he cautiously proceeds, in the following pages, around the coastal waters of international relations theory. We must now point out to him a chain of small islands dimly visible on the horizon

which are to be the subject-matter of the main part of this book – the mini-theories.

Types of Mini-theory

I shall survey briefly the four main types of mini-theory which, I will remind you, is a theory that seeks to explain in detail how an international situation develops in the short term. We begin with the type of theory that observes nations exclusively from outside and then go on to probe progressively deeper into the body politic until we seem to reach the very core of the decision-making process. In medical or zoological terms the four headings correspond roughly to the study of behaviour, comparative anatomy, physiology and biochemistry. The categories are not very different from those used by Charles McClelland (1966) in his extremely readable introduction *Theory and the International System.*

(i) Interaction analysis (cf. Study of Behaviour)

Interaction analysis studies the pattern of interactions (usually political or military 'moves', though sometimes ordinary transactions such as trade and tourism may be included) between selected nations both in normal times and in crisis, the nations being 'black-boxed' so as to exclude all subjective interpretations such as why A was followed by B, or what was nation X's perception or nation Y's intention. A typical proposition is

> In crisis the 'inventory' of actions employed by a nation tends to broaden, not to narrow as might intuitively be expected.

McClelland himself has made most of the running with his studies of the Berlin crisis 1948-63 (McClelland, 1968) and the Taiwan Straits confrontation 1950-64 (McClelland et al., 1965) and his comparative study of the two (McClelland, 1967). States' reported acts towards each other are the variables, and are coded in various ways according to their degree of co-operativeness with or hostility to the adversary.

The work of Quincy Wright and Lewis Richardson on the statistics of wars could be regarded as early ventures in this field, limited to the case where the only 'reported act' con-

sidered is war (Wright, 1942; Richardson, 1960*a*). The early stages of the project by Singer and Small, 'The Correlates of War', fall into the same category – for example their study of 'Alliance Aggregation and the Onset of War' (Singer and Small, 1968) – although their plan for the later stages takes them into the decision-making process and therefore out of this category.

Some of this work is discussed in more detail in Chapter 4.

(ii) Comparative analysis of nations (cf. Comparative Anatomy)

Here the analyst collects and analyses hard data about individual nations (economic, political, cultural) and attempts to relate this to the interactions of the nations on the international scene. It is in this field that the big battalions of the United States research effort in international relations are concentrated and where great and sometimes extravagant hopes seem to reside. There is a close link with (i) above and indeed the two approaches may be regarded as complementary; but here the national attributes or the national 'factors' underlying them are generally regarded as the independent variables, and relationships are sought between them and some measure of a nation's external performance. Often enough the 'measure of external performance' takes the simple form of the presence or absence of war. Typical results are:

> There is no significant correlation (negative or positive) between the amount of internal violence within a nation and the amount of violence it displays externally.

> 70 per cent of the variance of a nation's behaviour in international relations can be summed up in seven independent attribute variables.

The chief workers in this field have been Bruce Russett at Yale (the Yale Political Data Program and the World Handbook), Rudolph Rummell at Hawaii (the Dimensionality of Nations Project) and Arthur Banks at M.I.T. (the Cross Polity Survey). Some of this work, too, is discussed in more detail in Chapter 4 (see pp. 82-4).

(iii) Foreign policy analysis (cf. Physiology)

This looks inside the black box and seeks to identify the internal processes that lead to external action, at their point of convergence. It must take account not only of acts but of how acts are perceived by other actors; of intentions and of perceptions of intent; of organisational constraints; of long-term aims, attitudes and codes of conduct. This is the field of traditional historical analysis; it is the most difficult of all in which to make 'scientific' statements and yet, because of its obviously central position, has attracted the most varied approaches. There are many contending models, but as McClelland candidly says, 'research contributions remain at a modest level'. Among the approaches are:

(a) direct inquiry among decision-makers, e.g. in the State Department (Pruitt, 1965);
(b) analysis of the documentation of past events, especially crises, to elucidate chains of cause and effect and try to see how each decision was arrived at (Holsti et al., 1964 and 1965, on the First World War and the Cuba missile crisis; see also Chapter 6 below);
(c) psychological study of the behaviour of small groups including theories of learning and the acquisition of 'role' (Rapoport, Flood and Messick; for references and discussion see Chapter 5, pp. 106-17).

Further discussion of work in many of these fields, together with the 'communication model' derived from communication theory that informs so much of it, will be found in Chapters 4 and 5.

(iv) Abstract theory of interdependent decision-making (cf. Biochemistry)

This is the study of the logic of decision-making, abstracted from its subject-matter, and leads directly to game theory and its derivatives which, since they seem at first sight to be at the heart of the matter, are discussed at some length in Chapter 3.

All four of the main categories I have listed have room within them for the analysis of conflict and crisis, although in order to keep the right perspective it is perhaps necessary to put this the other way round, and say that none of the above

categories is exclusively concerned with conflict. The main drift of American work at present – and all the writers I have cited are American – is to establish objective facts and acquire understanding of how the system works *normally*; only then will there be a sound basis on which to study the origin and course of disturbances in the system.

Statistical Analysis

Much of the work to be surveyed in later chapters, especially that classified under the headings (i) and (ii) above, consists of the manipulation of statistical data. I shall make no attempt to delay the reader with a short course in applied mathematics and statistical techniques, but a few remarks may not be out of place by way of preparation for what is to come.

A common form of statistical statement is one that speaks of the degree of correlation between two variables (for the measurement of which there are a number of different methods whose merits and applicability I shall not go into here). This confronts us straight away with the idea of the 'variable', which although – or perhaps because – it is so fundamental is usually introduced without explanation and may well perplex the non-mathematical student. When an object or set of objects is under observation, any recordable feature of the object or set that is not necessarily constant throughout may be taken as a variable: one may be interested in the variation *with time* of a feature of one object, such as a person's temperature, or in the variation *across the set* at a given time, such as the G.N.P. of all European countries in 1968. The variable is said to take 'values' which are the readings of the recordable feature whatever it is; they may be quantitative as in the two examples already given, or not. Examples of non-quantitative variables are the sex of a human subject which may take one of two values, male or female; or the answer to a question in a social survey questionnaire which perhaps may take one of three values, yes, no or don't know.

What does it mean, then, to say that two variables are highly correlated, and is there any inference that may be

drawn from a statistical statement to that effect? The meaning, simple to express as a mathematical relation, is rather difficult to put into words. It is, roughly speaking, that there is a strong similarity between the patterns of variation of the two variables over the set. For example, the following (invented) set of readings of temperature and wind speed shows a high correlation: as the temperature rises, the wind

Readings at noon	*Day 1*	*2*	*3*	*4*	*5*	*6*
Temperature (°C)	5	5	12	15	15	0
Wind speed (m.p.h.)	30	20	11	2	8	35

usually drops and vice versa. When plotted on a graph as in Fig. 2.1, the points lie fairly close to a straight line. It is not a completely linear relation and so the correlation is not perfect; but it is pretty good. One standard way of measuring its 'goodness' is to calculate how unlikely it is to come about purely by chance: this is the meaning of the 'significance level' often associated with a statement about correlation (e.g. 'significant to 0.05' means that as close a correlation would come about by chance only 5 times in 100 on average).

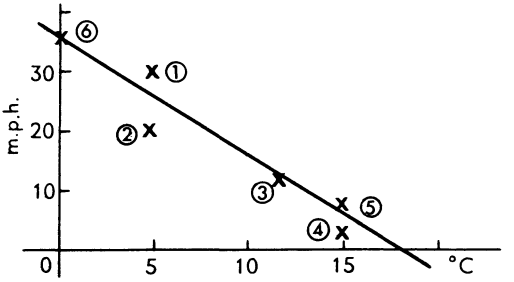


Fig. 2.1. Temperature and wind speed. Circled figures show day of observation

The better the correlation, the better the quality of the predictions that one can make *within the set*. That is to say, supposing one of the wind-speed readings was obliterated, a statistician knowing the degree of correlation could guess the missing value by a calculation based on the temperature observed on that day and the other pairs of readings and would not be far out. This is equivalent to taking a point corresponding to that temperature on the straight line drawn

to pass as near as possible to the other points on the graph (Fig. 2.1). The degree of correlation between the variables would determine the degree of assurance with which the statistician could estimate the missing value, in other words the probability of his estimate falling within any given range of accuracy. There is a strict logical correspondence.

But if we proceed to inquire what can be said about prediction *outside* the set of observed values, the situation is entirely different. (As will be apparent to those familiar with the terrain, I am here following the Bayesian line.) No logical inference whatever – not even in probabilistic terms – can be drawn, from the data alone, as to what the relationship will be between temperature and wind speed on some other day not included in the observations already noted. The observer may nevertheless have a shrewd idea that there will be a correlation, but if he has, it is because he has taken a view as to the intrinsic likelihood of, say, an approximately linear relationship holding between temperature and wind speed, perhaps as a general law or perhaps within some limited conditions of climate or season which are satisfied. He can then compare his feeling about this likelihood with the significance level given by the statistical analysis, i.e. the likelihood of the correlation cropping up by chance. Such feelings or judgments are the rennet without whose help the sour milk of statistics will never turn to cheese.

If a relationship is intrinsically plausible, then it will not take many readings to convince the observer that the correlation he has observed between them is 'real'. If on the other hand I observe that every time my neighbour's dog barks the page of the book I am reading is a prime number, then the intrinsic implausibility of a 'real' connection between the events is so great that an enormous number of observations would be necessary before I would begin to shed my incredulity and look around for a causal explanation.

But even if for some reason external to the data one believes a correlation to be 'real' or to have a 'causal explanation' – to pick up two phrases used in the last paragraph – what is it in fact that one believes? The only point I want to make here is that the correlation does not carry within itself any *specific* causal message. It cannot be inferred, for example, that one is the cause of the other. If

variation in *A* in fact causes variation in *B*, then the variables *A* and *B* will show a correlation; but the same would be true if the causal connection were the other way round, or if both *A* and *B* were subordinate to some unspecified third variable which was the common cause of both of them. If Jack Sprat and his wife both leap up from the dinner table at almost exactly the same time every evening, it may be that one of them is reacting politely to the other or that they have both looked at the clock and seen that it is time for their favourite television programme.

Statistical analysis, we conclude, will show up relationships between variables but will not confer any status on them as a basis for prediction except when combined with a judgment external to the data; and a correlation established is simply a correlation and not in itself a causal relation. These general considerations, which apply to statistical work in any field, have two consequences to which I draw attention in the context of this book. Both have to do with the enormous number of features of the international scene that are available to be chosen as variables. It is not uncommon for the number of variables observed and tabulated to go into hundreds. To mention two studies that are referred to at the end of Chapter 4, Barringer's 'The Conditions of Conflict' has 300 variables to describe the pair of nations in any given conflict situation; Singer and Small's 'Correlates of War' seems to be on a similar scale; and there are many others. With so many variables, the chance correlations that arise between them will be very numerous indeed and many will be at a high 'significance level' in statistical terms. This does not mean, as I have explained, that they really are significant. The first point therefore is that there is a great danger of importance being attached to spurious correlations, and it is necessary to test any conclusion to see whether the author has acknowledged and disposed of the danger. For this testing, sophisticated statistical techniques may be required.

The second point is that, notwithstanding the large number of measurable or observable features, so many of the really interesting data about a nation or situation are inaccessible or unmeasurable. This is a point that can only be fully argued by reference to a particular case; but to give an impression of what I mean: it is easy to state a nation's

population, not so easy to say what kind of people they are or to measure their attitudes at different times; it is comparatively easy to state how many people are killed in a war, not so easy to say how near to war two nations came at some time of tension that did not in fact lead to fighting. The reduction of a 'state of affairs' of this kind to a single figure on a scale, which is often the essential stage in determining a variable, is a highly subjective business and the scaling process may be hard to distinguish in the last resort from the methods of the traditional historical approach. Examples will be found throughout the book, but to show that I bear no ill-will I refer the reader to my own exercise of subjective judgment in the Appendix to Chapter 7 – 'The New Model Model'.

The typical situation, if my analysis is right, is for a large amount of precisely quantifiable data to be available – between which spurious correlations will spring up like poppies in the corn – while what seems intuitively to be the most interesting data either are not quantifiable at all and are thus left out of account to the detriment of the theory, or are reduced to quantitative terms by dubious scaling procedures which reflect on the objectivity of the whole process. Theories of international relations that are based heavily on statistical analysis start therefore with a handicap and their advocates must, to gain our respect, show that they are aware of these difficulties and have vanquished them. I do not often find this awareness, or this victory.

I add one or two points of explanation of statistical terms and methods. When one is testing a hypothesis to the effect that the variation in one variable x (say the G.N.P. across a set of nations) is wholly or partly accounted for by the variation in a, b and c (say the rate of investment, the working population and the productivity), then x is described as the dependent variable and a, b and c as the independent variables. The distinction can be important because one usually has to appoint one of the variables as the dependent variable for the purpose of applying statistical techniques of correlation.

It would be a pleasant surprise if it turned out that the dependent variable x was a constant multiple of the product of the three independent variables:

$$x = \lambda abc$$

There are of course all sorts of other possibilities, and it is the task of the statistician to see what likely-looking relations he can pick out of the data he has been given. One method that can be useful when there is a large amount of data and a computer is available, is called factor analysis – the daily bread of the comparative analysis studies referred to in the previous section (see p. 13). Factor analysis is the statistical manipulation of interdependent variables to answer the question 'What set of supposed underlying factors would most simply account for such-and-such observed variations?' The jargon may perhaps be penetrated by an example. A typical statement of factor analysis would be:

> 236 attributes were tabulated for 82 nations. Factoring by the Yale Computer Centre Factor Analysis Program 6S gave 13 independent factors (dimensions) leaving 15 per cent of the variance unaccounted for. Of these 13, two can be regarded as rank-dimensions and are labelled 'economic development' and 'power'; five can be regarded as value-dimensions and are labelled 'Catholicism', 'feminine rights', 'self-government', 'internationalism' and 'communism'. It must be remembered that these are arbitrary labels to aid discussion and memory and are not objective characterisations of the factors.

Factor analysis is a standard procedure in statistics for the methodical search for correlations in large masses of data. Some of the limitations of the method are brought out later, in Chapter 4 (see pp. 87-94).

Content Analysis

A special technique for handling data in the form of documents that has been developed very rapidly in the past ten years is content analysis. Much of the work under sub-heading (*b*) of 'Foreign policy analysis' (p. 14) depends on it. The technique consists of a mechanical analysis of the word content of the document, normally by computer, against a dictionary in which words are classified according to various criteria, so that one has at the end a statistical measure of the

kind of document it is – just as one might be given a statistical summary of a person in terms of his height, weight, blood pressure and I.Q.

A typical statement of content analysis would be: 'Table I shows the results of analysing the action/attitude content of all publicly available documents emanating from U.S. and Soviet decision-makers during each day of the Cuba missile crisis, using the Stanford General Inquirer programmed to measure positive and negative perceptions in each of three dimensions: potency (strong, weak); activity (active, passive); value (positive affect, negative affect).' (The sinister name 'General Inquirer' is given to more than one computer-based system of content analysis developed in the U.S. See for example Stone et al., 1966.)

As an illustration, we are told (Holsti, 1963) that President Kennedy's statement announcing the quarantine of Cuba on 22 October 1962 had the following score, which could have been worked out within minutes of seeing the text:

	Positive	*Negative*
Potency attitude	45·1	3·7
Activity attitude	25·7	7·7
Value attitude	18·7	16·8

It is hard not to smile when one first hears of such analyses, or of vast projects for the counting of angry or threatening words in the telegrams sent just before the outbreak of the First World War; it seems such a crude, inaccurate steamhammer of a method compared to the exercise of human judgment by someone who knows the background and who can surely assess much more accurately the hostility or friendliness of the document by reference to his knowledge of the actual meaning of the assertions, threats and innuendoes of which it is composed. Is there an answer to this criticism? Content analysis does *not*, certainly, assess the meaning of messages; it does provide an objective measure of certain characteristics of messages which, taken over a large enough sample, can be regarded as having a meaning of its own – though one that is subordinate, in my view, to the actual meaning of the messages themselves. The important word is 'objective'. It is now generally accepted

that in the days immediately before the outbreak of war in 1914 there were misperceptions on both sides: Germany and Austria (having originally underestimated the risks of British or Russian intervention) in the last days consistently overperceived the level of violence intended by the other side's moves; while the Triple Entente consistently underperceived the level of violence intended by Germany and Austria. In the heat of the moment, objective conclusions about the other side's intentions based on their messages may be very difficult to arrive at. Even the historian may have difficulty, as he seeks for the right phrase to denote a 'peremptory demand' or an 'abject apology', in discarding his preconceived ideas as to how one event led to another. Content analysis of a message is free of all such preconceptions. Nevertheless it is not a substitute for the message itself, and I think there are dangers in erecting theoretical constructions on the basis of content analysis alone. Whenever possible, other more direct data should be used. But there is a place, even if a modest one, for content analysis in that it provides an objective measure or set of measures as a corrective to be put alongside any other information or judgments about the sequence of events being studied. The method has been the subject of much methodological research and many of the early crudities had been removed by the time the General Inquirers came into being; no doubt it will continue to be refined.

Conclusion

A fundamental question remains unanswered at the end of this summary: is not all or the greater part of this scientific endeavour a misconceived and ill-starred attempt to find answers to unanswerable questions? And can we not expect twenty years from now that the General Inquirers, the behavioural studies, the dimensions of nations, the giant banks of data, the great overarching systems, the cloud-capped pinnacles and all, will be found working themselves deeper and deeper into the sand while decision-making and the historical study of events go on much as before? On the merits of the case I shall delay any attempt at an answer till

later in the book. Here I shall simply record a few facts: that this work is absorbing the attention of a large and increasing number of those engaged in the study of international relations; that those concerned are not in general feeble-minded, incompetent or unstable; and that any methodological argument that may persist within the profession must be regarded as having played itself to a standstill, in the sense that the scientific approach described here is being applied with increasing confidence to a widening range of problems. Whether it is right or not, it is certainly happening!

3 Game Theory, or The Theory of Interdependent Decisions

The branch of pure mathematics now generally known as game theory was christened 'the theory of games of strategy' by its creators, the mathematician John von Neumann and the economist Oskar Morgenstern (von Neumann and Morgenstern, 1947). The longer name is enlightening since it brings out the important point that the games in question require the players to choose among strategies. But it would have been even more helpful if it had simply been called *the theory of interdependent decisions*. That expresses precisely what the theory is about: it embraces all interpersonal decision-making situations, the only essential requirements being that there should be two or more players and that the outcome must depend on the strategies chosen by each of them and on nothing else. If one of the players can determine the outcome by his own decision alone, then the game is a trivial one, of no theoretical interest. In mathematical terms, a 'finite n-person game in normal form' (the subject of the major part of the theory) is characterised simply by a function F that relates the combination of choices of strategies $s_1, s_2, \ldots, s_n$ by each player to a set of values $(x_1, x_2, \ldots, x_n)$ defining the outcome, or payoff, for each player. In symbols,

$$F(s_1, s_2, \ldots, s_n) = [x_1, x_2, \ldots, x_n].$$

Once you have listed or indicated the strategies available to each player, and the possible outcomes for him, and defined the function taking you from one to the other, you have your game. I shall not be brandishing mathematical symbols in what follows, but it may be indicative to have at the outset this reminder of the extreme degree of abstraction of the foundations of game theory. We may also note for later reference the absolutely central position occupied by the payoff function.

The study of the logic of interdependent decision-making is, one might well suppose, at the heart of any scientific approach to international relations. It is not surprising that great – indeed extravagant – hopes were placed in game theory as a cornerstone of the new edifice. What a stroke of luck it must have seemed to be that this intellectual *jeu d'esprit*, already elaborated to a considerable degree of refinement, should be at hand just at the moment when it would come in useful to provide a framework for international relations theory. In the last chapter I drew the analogy with biochemistry; we may note from a recent textbook that 'One of the achievements of modern biochemistry is the demonstration in chemical terms of the essential unity of life which is often obscured by diversity of outward form' (Barker, 1968). What better biochemistry for revealing the essential unity of international affairs than game theory! No wonder the international relations literature of the 1950s is studded with enthusiastic references (usually by non-mathematicians, it is true). I shall try to explain in the course of this chapter how these expectations came to be disappointed.

To avoid any risk of confusion arising from the name, let me explain straight away that game theory has no connection with war games or political games. In contexts where there is any likelihood of confusion, it is usual to refer to those pursuits as 'gaming' or, in their more computer-bound forms, simulation. The reader who is interested will find some discussion of gaming and simulation in the first part of Chapter 5.

I give first some of the main ideas of game theory, in outline and with the minimum of mathematical terms, as put forward by von Neumann and Morgenstern twenty-five years ago and as developed by others in the short but active life that the theory has enjoyed since then.

Two-person Zero-sum Games

An especially important place is occupied by this class of games, which turns out to be particularly tractable mathematically and gives rise to one elegant theorem, usually called the Minimax Theorem. If it had not been for the Minimax

Theorem it is doubtful whether the interests of mathematicians would have been sufficiently aroused to recognise the existence of a 'theory of games' at all.

In the two-person zero-sum game 'in normal form' (a phrase that I will explain shortly along with the technical meaning of the word 'strategy' in this context), the two players have diametrically opposed interests: what is a loss to one is a gain to the other and vice-versa. If the 'payoff' or utility of every possible outcome is put in numerical terms, u_1 for the first player and u_2 for the second, then $u_1 + u_2 = 0$; hence the name zero-sum. I am assuming here for simplicity that the two players' utilities are comparable, although this is not logically necessary for the theory of the two-person game. Conventionally the game is looked at from the point of view of the first player and his payoff, and it is understood that the second player's payoff is the negative of it.

The game in normal form then consists of a set of strategies for each player: let us suppose there are three possible strategies x_1, x_2, x_3 for the first player and four possible strategies y_1, y_2, y_3, y_4 for the second player. For each combination of strategies x_i and y_j there is a payoff a_{ij}. The situation is conveniently summarised in a rectangular array, or matrix: in the following highly artificial example (Fig. 3.1) I have chosen some figures almost at random (but not quite, as will appear).

	y_1	y_2	y_3	y_4
x_1	7	4	2	3
x_2	−4	2	0	4
x_3	9	9	1	0

Fig. 3.1. The matrix of a zero-sum game.
The payoff for player 1 can be read off when the strategies of *both* players are given. The payoff for player 2 is the negative.

What is the best strategy for either player? The answer is not immediately obvious. Player 1 might be attracted by the high scores in the third row and plump for strategy x_3, player 2 might foresee this and choose strategy y_4 (to get the payoff zero which for him is the *best* score in that row). Player 1, in turn foreseeing this, might change his mind and choose x_2.

Player 2, still keeping one jump ahead, would change *his* mind and choose y_1. If player 1 foresaw that, he would be induced to play x_3 after all (in order to get the score of 9 which he had his eye on in the first place), and so on.

We seem to be caught in an endless regress, and this 'trial and error' is evidently an unsatisfactory way of setting about the problem. A more systematic way to proceed is as follows (the reader may like to follow the method through in this simple example):

(i) Consider each row in turn and mark the *lowest* entry. This corresponds to the worst that player 2 can do against the strategy chosen by player 1.

(ii) Find the *highest* of all the entries so marked. This is the *maximin*, the highest score that player 1 can guarantee to secure for himself by prudent choice of strategy. In the example it is 2, the payoff resulting from the combination of strategies x_1 and y_3.

(iii) Go through the corresponding routine from player 2's point of view, i.e. mark the highest (worst for him) entry in each column and then find the lowest (best for him) of the entries so marked – the *minimax*.

(iv) If the two values are the same (as they turn out to be in the example), then the correct play for each player is the strategy corresponding to this value – in the example, strategies x_1 and y_3.

There is in fact something special about this point in the matrix. It is called a *saddle point*, for reasons which are clear if one regards the matrix as the map of an area with the payoff representing the height of each point above sea-level. The point (x_1, y_3) is the lowest point looking right and left, and at the same time the highest point looking up and down the page. It can easily be shown that the matrix has a saddle point in this geometrical sense if and only if the minimax and the maximin procedures lead to the same value; this is then called the value of the game and the pair of strategies defining the point is known as the solution. A feature of it is that neither player can improve his position by a change of strategy; that is to say, it is an equilibrium point.

However, a matrix does not normally have a saddle point, and so the conveniently simple solution illustrated above

does not normally arise. Before going on to consider the general two-person zero-sum game, let us pause to reflect on the phrase 'correct play' in (iv) above. The play dictated by the theory is 'correct' on certain assumptions which underlie the whole of game theory and which need to be clearly stated. They are as follows:

(a) each player is fully rational and is capable of working out to any required lengths the logical consequences of his and the other player's decisions;
(b) each player has full knowledge of the matrix, i.e. all the strategies available to both players and the corresponding payoffs;
(c) each player acts to maximise his payoff, in the knowledge that the other player is doing so too.

The first two assumptions seem rather trite in the context of a simple example, but will be significant when we consider the application of the theory to more complicated cases. Assumption (*c*) is no more than a combination with (*a*) and (*b*) of the even more fundamental assumption that each player is able to express, in a consistent fashion, his preference as between any two outcomes. This is a basic formulation of rationality without which a matrix of payoffs could not be set up; indeed, without it it is difficult to see how any progress could be made with the theory. I make this point in order to forestall the criticism that is sometimes made of game theory that its initial assumptions are unnecessarily restrictive. Restrictive they may be, but they are necessarily so if the theory is ever to get off the ground at all, i.e. if the material is to be sufficiently rich to provide the basis for mathematical operations.

I now turn to the more general case of the two-person zero-sum game with no saddle point. As an example of this, take the game of 'matching pennies' which must be one of the simplest games of strategy ever devised. One would be forgiven for suspecting that it had been invented purely as an illustration for textbooks on game theory. The matrix is

	H	T
H	1	−1
T	−1	1

Fig. 3.2. Game matrix for 'matching pennies'

Each player has a penny which he conceals from the other player until at an agreed moment both are shown. If both show the same face (both heads or both tails) player 1 wins; if not player 2 wins. There is clearly no saddle point.

A solution is arrived at by introducing the notion of a 'mixed' strategy. This is a probability mixture of the listed, or 'pure' strategies. Instead of acting to maximise his payoff ((c) above), each player acts to maximise his *expected* payoff. It can be shown by elementary algebra that if each player uses the mixed strategy that combines his two pure strategies in equal proportions (the strategy on any particular occasion being chosen at random), then there is equilibrium like that of a saddle point: the value of the game is determined (it is zero in fact), and neither player can improve his position by a change of (mixed) strategy.

Von Neumann's first main achievement (von Neumann, 1928) was to prove that *any* finite two-person zero-sum game has a solution in this sense in mixed strategies. This is what is now known as the Minimax Theorem. What is easy to see for 'matching pennies' is by no means easy to prove in general, and the theorem has a respected place in the annals of twentieth-century mathematics.

The notion of a 'mixed strategy' and the assumption that a player will maximise his 'expected payoff' are not so readily acceptable as the earlier assumptions when regarded as a possible basis for a theory of decision-making. When there are repeated plays of the same game (as one imagines there would be of a game like 'matching pennies' or the rather similar game 'knife, paper, stone'), then a mixed strategy over a period will give a certain security of average payoff, within the bounds of probability theory. When the game is played only once, one cannot actually *play* a mixed strategy: at least there is no outward difference between doing so and playing the pure strategy on which the lot happens to fall. Whether a mixed strategy is being used becomes an academic question,

so far as that game itself is concerned.

However, it is common knowledge that in a game like poker the use of bluffing is beneficial. The man whose play is predictable from his hand (and therefore vice versa) will go down before the man who gives nothing away. Bluffing enables a player by bidding high to conceal the weakness of his hand, thereby increasing his prospects in that hand, and moreover making the opponent uncertain the next time he bids high whether it is from a strong hand and perhaps inducing him to 'see'. It would not be advantageous to reduce the signalling value to nil by bidding completely at random; but if a combination of tactics is found that increases the opponent's uncertainty to the point of maximum gain to the player, then the bluffing can be said to be perfect. What is this but a perfectly randomised mixed strategy?

A mixed strategy can, it seems, be held up as a plausible model of behaviour whenever similar (not necessarily identical) situations occur in which past play would otherwise be used to predict future play.

The mention of poker calls for a clarification of the previous theoretical discussion: what has it to do with *real* games – chess, poker, bridge, even noughts and crosses? The matrix of strategies seems to allow only for games like 'matching pennies' consisting of one move, made simultaneously by the two players – surely a limited class indeed. This is the point at which it is necessary to explain the meaning of a game 'in normal form'. In a sequential game such as chess it is possible to imagine two very inexperienced players considering each position from scratch as it is reached in the course of the game. There will be for the player due to move a number of possible moves, and his strategies will consist of just those choices. Then it is the other player's turn, and so on. At a more advanced level, the players might be deemed to be looking several moves ahead at least for some of the possibilities, and one might concede that, in theory, they could have compiled a list of all the combinations of moves possible for, say, three moves on both sides. Except in the end game, this would be a considerable list. Note that in any given position both players' lists would be the same. Now each player would be able to compile a separate list of strategies for this section of the game, a strategy being a

complete statement of his responses to every possible sequence of moves by the other – normally a rapidly ramifying tree of alternatives. Ultimately we arrive at the full definition of 'strategy', which is a complete statement by one player of what he will do in all the possible situations that may arise in the course of the whole game.

The device of the 'normal form' reduces the entire game, in this way, to a once-for-all choice by each player of his strategy which will then determine the play in any eventuality. These choices having been made (simultaneously) at the outset, the players would then be able to hand their chosen strategies to the umpire and go home, leaving him the tedious task of working through the actual sequence of moves to be played and discovering who had won. Fortunately, in a game such as chess not all the books in the British Museum would provide enough space to write down more than a fraction of a strategy, and so this simplified method of playing is unlikely to become popular. Even for noughts and crosses, I believe the number of strategies, allowing for symmetry, is of the order of 10^9.

The point is that, as Von Neumann discovered, it is only by making this conceptual reduction of the game to normal form that it becomes amenable to mathematical treatment. Although a certain amount of attention has been given to the theory of games in 'extensive form' – i.e. regarded as a sequence of alternate moves in the ordinary way – the great bulk of game theory is concerned with the normal form. Only one out of twenty-nine papers in a recent collection of contributions to game theory (Tucker et al., 1964) deals with the extensive form.

To illustrate the extensive form I use a very simple version of the game of Nim. Four matchsticks are arranged in two groups of three and one, and the players moving alternately may remove at one time any or all of *one* group. The game is to take the last match (alternatively, to make the other player take the last match – the analysis of the two forms of the game is fundamentally the same).

Fig. 3.3. shows the whole game in extensive form, every possible combination of moves being given. It can be seen that in last-taker-wins Nim player 1 can be sure to win by playing A2, and in last-taker-loses Nim he wins by playing A3.

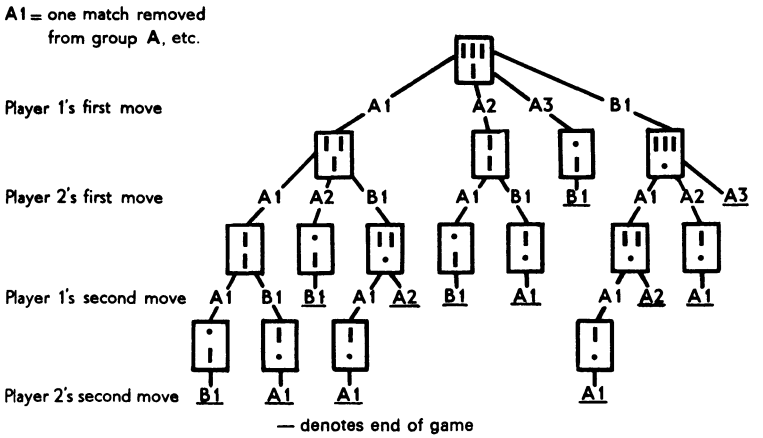


Fig. 3.3. A simple version of Nim in extensive form.

There is no guaranteed winning strategy for player 2 in either case, though of course he may win if player 1 makes a mistake. It is interesting to note that the mathematical theory of Nim, which determines among other things the conditions under which a player can be sure to win and the strategy he should follow, does not depend at all on the extensive form. Chess on the other hand, which has no general mathematical theory, can be discussed only in terms of the extensive form — that is, of specific moves made in specific situations or types of situations.

In the games discussed above, the number of pure strategies available to each player — though it may be fantastically large — is nevertheless finite. A good deal of advanced and interesting mathematical work has been done in recent years on *infinite games* (including what are alternatively known as 'differential' or 'continuous' games) where the number of strategies is infinite. This branch of the theory has practical applications to military problems of pursuit and evasion (e.g. submarines or aircraft) and to partitioning problems in economics, but seems to have no possible bearing on international relations and so I do not pursue it further here.

Other Games

As soon as game theory moves outside the cosy corner of the two-person zero-sum game — whether by increasing the

number of players or by allowing the sum to be non-zero – it begins to look quite different. There is no longer any simple procedure for identifying and arriving at a solution; indeed the whole notion of a solution, which before seemed to emerge naturally and uniquely out of the mathematics, now becomes nebulous and elusive.

The n-person zero-sum game, with $n>2$, can be reduced to something like a two-person game by moving the centre of interest from the individual player to the *coalition* of players. There are two features that distinguish the acts of a coalition from the acts of individual players: the members of a coalition can co-operate by co-ordinating their strategies to maximise their collective payoff, and they can make side-payments to one another. It follows that the allocation of payoffs at the end of the game, known in the literature as an 'imputation', will not necessarily correspond to the payoffs determined by the combination of strategies chosen according to the payoff function – though the *sum* of the payoffs must of course remain the same (0 if it is a zero-sum game). Concealed in the idea of coalitions with side-payments is the assumption of 'transferable utility', namely that units can be found in terms of which one person's gain may be compared in value with another's and that a transfer from one to another leaves the value unchanged. I have touched on the 'comparability' part of this assumption in the context of the two-person zero-sum game. In the n-person game it raises difficult issues which have been much discussed (Luce and Raiffa, 1957, pp. 168, 180, etc.), but they do not concern us here and I shall simply assume that all such conditions are satisfied when necessary.

This was von Neumann's approach, and with the help of Morgenstern he developed the theory of coalitions to an extraordinary extent (nearly half of a 600-page book) in the hope of providing helpful models for the study of economic problems, such as oligopoly. There can be few other occasions in mathematical history of a theory being set out so extensively on its first presentation.

It is assumed that for any coalition that forms in an n-person zero-sum game, all the other players will immediately form the opposing coalition. This is on the same level of gloom – or rationality – as the assumption in the

two-person game that the other player is perfectly rational and will do his best to defeat you. For any given coalition you can then set up the two-person zero-sum game of coalition A versus the rest, apply the minimax theorem and derive the 'value of the coalition', i.e. the payoff that coalition A can collectively assure itself of by playing a maximin strategy.

However, it remains an open question which coalition should form, and how the proceeds should be divided up between the members; and although von Neumann was able to specify a few conditions that a solution must satisfy in order that some other solution should not clearly be preferred to it by all players, these conditions are not strong enough to reduce the welter of permissible solutions to manageable proportions. The result is that even in one of the simplest games considered – a three-person zero-sum game – there is an infinite number of solutions, nearly all of which contain an infinite set of imputations (i.e. ways of dividing up the proceeds) with nothing to choose between them. Apart from the negative suggestion that coalitions are inherently unstable, it is difficult to think of any practical way of making use of these results.

In view of the unpromising state of the theory, it is with the greater admiration that one must record two substantial attempts to apply n-person theory to politics. In the first, the notion of the 'Shapley value' is developed from the value of the coalition as defined above. In order to assess the weight or voting power of a particular player P, the idea is to regard the forming of a coalition as occurring in a series of steps, at each of which one new member is added. If all the possible ways in which all possible coalitions could come into being are then listed and regarded as being equally likely, it is a matter of arithmetic to work out the average expected advantage that would accrue to the coalition by the admission to it of player P. It may turn out that some players often have a significant effect on the strength of the coalition they have just joined; the admission of others may have a negligible effect or none at all. The rating of each player can be regarded as an equi-probability mix of his influence relative to the other players on the final outcome of the game. One cannot help feeling some misgivings about the

arbitrary assumption that all sequences of coalition formation are equally likely, since it must surely be implicit in the results obtained in terms of player-power that certain coalitions will form more readily than others. However, the suspicion of circularity in the argument can perhaps be overlooked in the satisfaction of finding an objective method of calculating the players' weights. Some quite plausible results have been worked out by this method for the relative power of Senators, Congressmen and the President in U.S. legislation, and veto-holders and non-veto-holders in the U.N. Security Council. There is a wide measure of agreement that this method is valid for zero-sum games (Shapley, 1953*a*; Shapley and Shubik, 1954). I note in passing – though this is to anticipate the critique at the end of the chapter – that to the best of my knowledge this is the only practical result in political science that is properly attributable to game theory.

The second application of *n*-person theory is by William Riker (1962) in his book *The Theory of Political Coalitions*. He makes the assumption that political situations may be found that can be represented by an *n*-person zero-sum game with side-payments (rationality and perfect information assumed as usual), and asserts that for such situations participants create coalitions just as large as they believe will ensure winning and no larger – in contrast to the intuitive view that coalitions will always try to make themselves as powerful as possible and get as many players to join as they can. The general idea is that the proceeds of victory will then be divided up among the fewest possible people, so that it will not be in the interest of members of a just-sure-of-victory coalition to acquire any more members. Riker admits, however, that it is necessary to postulate a principle working in the opposite direction, namely that in times of conflict and change when information (e.g. about the intentions of politicians and the possibility of new groupings) is *not* perfect, winning coalitions will tend greatly to exceed the necessary minimum size because of uncertainty as to just what the minimum size will be. His first maxim is, in other words, likely not to apply in times of crisis.

This is also true for a second reason: that politics, domestic or international, in time of crisis cannot reasonably be assumed to be zero-sum. There will normally be some

outcomes that would be widely deplored and others that would be widely welcomed: the sum would be minus in one case and plus in the other – and not zero. Moreover it will be noticed that Riker uses the word 'winning' in a naïve way that cuts across the normal complexities of an n-person game. It is probable that he is thinking primarily of win/lose contests (known in game theory as 'simple games') such as elections, and the examples he quotes are mostly from nineteenth-century domestic U.S. politics.

My provisional conclusion is that neither Shapley's nor Riker's work seems likely to point the way to a clearer understanding of international conflict situations.

Non-zero-sum Games

Both in economic and in political theory there is a strong presumption that the interesting situations are non-zero-sum – assuming that a game-theory model of any kind is appropriate. It is not surprising that much mathematical firepower has been trained on this target. Unfortunately the neat interlocking of the two players' maximin strategies, which is the cause of those strategies' significance in the zero-sum game, disappears without trace when the sum is not zero. The idea of an equilibrium point – a pair of strategies from which it is in neither player's interest to depart – carries over to the non-zero-sum game; but maximin strategies do not as a rule lead to equilibrium points and indeed there is no longer any sense in a maximin strategy except possibly as a last resort or safety net.

As an illustration, consider the well-known example of the Battle of the Sexes. He and She have to decide independently whether to go to the play (P) or the opera (O). To keep in line with the notion of a game in normal form, we have to assume that they make their decisions simultaneously and without communication; but each has a clear idea of the other's preferences. He prefers the opera, she prefers the play, but each likes the other's company and going alone spoils their enjoyment. This is the matrix:

		She: *P*	She: *O*
He	*P*	2, 1	0, 0
	O	0, 0	1, 2

Fig. 3.4. Matrix for the Battle of the Sexes

The first figure in each cell is his payoff, the second is hers. The points (*P, P*) and (*O, O*) are equilibrium points since any deviation from one of these points by either player will lower his score; this goes for pure strategies and mixed strategies alike.

The miserable inadequacy of maximin strategies can quickly be seen. His maximin strategy is a mixed strategy of one part in three of *P* and two parts in three of *O*, or $\frac{1}{3}P + \frac{2}{3}O$ for short, which assures him of a payoff of $\frac{2}{3}$. Her maximin strategy is $\frac{2}{3}P + \frac{1}{3}O$, assuring her also of a payoff of $\frac{2}{3}$. But this is not the best strategy against his maximin strategy (why should it be? The two maximin strategies are independent of one another, being derived from consideration of two different sets of figures). Indeed the combined maximin payoff of $(\frac{2}{3}, \frac{2}{3})$ is not even as good as randomising half and half, which gives $(\frac{3}{4}, \frac{3}{4})$.

The other novelty is that whereas in the zero-sum game the whole point of a mixed strategy is to randomise independently of the other player (in matching pennies there wouldn't be much point in telling the other player that you were going to play heads), in the non-zero-sum situation there clearly is advantage in comparing notes. This can be seen from Fig. 3.5, which shows his and her payoffs in two dimensions. The shaded area represents all the possible combinations of mixed strategies: the horns at (2, 1) and (1, 2) correspond to the pure strategies (*P, P*) and (*O, O*). If the players are allowed to communicate, then they can co-ordinate their strategies and the effect is to give them access to the area enclosed by the dotted line joining the horns. If they are wise they will confine their attention to the points on the dotted line itself – known as the negotiating set –

and not bother about the interior of the unshaded area. There is an obvious symmetry in this case that suggests the mid-point of the dotted line as 'the' solution, in other words to go equally often to the play and to the opera, using jointly randomised strategies determined by, say, the toss of a coin. But in other cases the picture would not necessarily be symmetrical and there would be no obvious candidate as a solution that would commend itself equally to both sides.

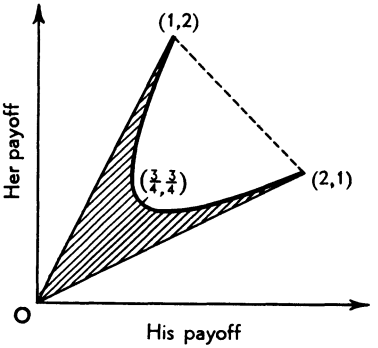


Fig. 3.5. Payoffs in the Battle of the Sexes

The main point of that example was to illustrate the breakdown of all, or almost all, the comfortable features of the zero-sum case. In spite of extensive efforts over the past twenty years by many mathematicians, among whom John Nash has been prominent, no rule of procedure for the players or rule for distribution of payoffs has been discovered that carries anything like the conviction of the zero-sum solution. As von Neumann pointed out, you can transform a non-zero-sum game into a zero-sum game of a sort by adding one more notional player whose payoff is calculated to make the total always come to zero. As a method of analysis of the non-zero-sum game this has not proved fruitful because the extra player is a dummy who cannot be treated as equivalent to the other players (he cannot effectively join a coalition, for example); but it does indicate that the same kind of arbitrariness is likely to be present in non-zero-sum theory as we have already found in the three-person zero-sum game.

A distinction has grown up in the literature between co-operative games, in which the players may enter into binding agreements (e.g. joint randomisations, or undertakings to play or not play certain strategies), and non-cooperative

games in which no such agreements are permitted. In the example of the Battle of the Sexes we have already seen what a difference this can make. Von Neumann maintained that once the negotiation set had been established, the co-operative stage was not further susceptible to mathematics and this (in my opinion) erroneous lead has died hard, so that the non-cooperative game has continued to receive the most attention. However, Nash (1953) developed what he called an 'extended bargaining model' in which a two-person non-zero-sum game is divided into two parts:

(*a*) a non-cooperative game in which each player chooses a 'threat' strategy with a view to staking out the best posture he can regardless of what the other player does: the two 'threat' strategies determine payoffs which establish a *status quo* point;

(*b*) a bargaining process, taking the *status quo* point as the origin and adjusting the payoffs so that they are all measured by reference to this new standard. Now work out the new payoffs (u, v) for all possible combinations of strategies and choose that for which the product uv is a maximum. This can be shown to be equivalent to a principle known to economists according to which, when a choice is to be made between two trades in any trading situation, the rule is that he who has less to lose proportionately (compared to a return to the *status quo* point) should concede.

Although the Nash solution is perhaps the most persuasive, it is only one among many that were put forward in the 1950s and justified on various grounds, both intuitive and axiomatic. They are compared at length in the very readable *Two-person Game Theory* by Anatol Rapoport (1964) and also by Luce and Raiffa (1957, pp. 124-50) in *Games and Decisions*. A recent paper in *Econometrica* (Harsanyi, 1966) and other papers by the same author introduce (for co-operative games) the interesting idea of a bargaining game preceding the game proper, in which the players jockey for position and form views about the subjective probability of particular strategies being adopted by other players. Based on these subjective probabilities, the player can then determine

his own initial 'bargaining strategy' and from then on a process of mutual adjustment takes place which is supposed to converge upon a final unique set of strategies for the game itself. It is said to yield the Nash solution for two-person co-operative games, and a generalisation of a modified version of the Shapley value for *n*-person co-operative games. However, the use of Bayesian ideas of subjective probability is puzzling and seems to fly in the face of classical game theory, one of whose achievements was precisely to escape from the necessity of guessing what the other fellow was likely to do and the unprofitable infinite regress that this could so easily lead to; and there seem to be other technical difficulties to be resolved before a clear solution concept can be erected on this basis.

Though all this work is mathematically rigorous and precise, I conclude nevertheless that the non-zero-sum game does not, in general, have a determinate solution that emerges naturally out of its mathematical structure; or if it has, no one has yet found it. There is no known rule of procedure that can be taken as a guide to arbitration, nor any prominent landmark defined by the data on which the players' interests can reasonably be expected to converge when it has been pointed out to them. And of course the indeterminacy is further compounded when there are more than two players.

Sequences of Games and Repeated Games

Much attention has been given to the 'supergame' that consists of a sequence of two-person zero-sum games linked together by a probability function: that is to say that when the first game is played, the strategies chosen determine not only the immediate payoff but also the probability that any one of a set of alternative games will be played next in the sequence, and so on. This appears to offer promise as a model for a developing political situation, although the zero-sum limitation is a disadvantage.

Shapley (1953b) showed that such supergames have determinate solutions, just like ordinary two-person zero-sum games from which indeed they do not differ very greatly.

Based on this and other work by Shapley, it has been shown recently that the players lose nothing by confining themselves to a very limited range of strategies – and rather primitive strategies at that (Hoffman and Karp, 1966). A *pure strategy* is defined as one that determines the players' choice at every move in the light of all the play so far; a *mixed strategy* is, as usual, a randomised set of pure strategies in some given probability mixture. A *stationary strategy* is a new concept in which the players' choices are determined at each move, as it were on the spur of the moment. The choice depends only on the game (i.e. the state of the supergame) at which the player finds himself, and no account is taken of the past history; also the randomisation is carried out at each transition separately instead of being done once for all at the beginning. What Hoffman and Karp showed was that for a solution to a non-terminating stochastic game (as this kind of game is called), one need not look beyond the stationary strategies. This, incidentally, is a characteristic 'result' of game theory; though far from obvious, it is very general and abstract, and not at all directed to the business of deriving definite solutions to a particular game.

If some real-life situation could be found of which the stochastic game was a model, the burden of this result would be that the players learn nothing from their play and need only observe where the game has got to and make an automatic (though randomised) response at each stage. Given the initial assumption of complete knowledge of the payoffs and transition probabilities and complete rationality on the part of the players, it is perhaps not altogether surprising that they have nothing more to learn. But this suggests that sequences of games, for all their promise as a model of a situation developing unpredictably, are every bit as unrealistic as ordinary games. Of course, if one of the players were to deviate from the rational track the other would not be able to take advantage of the fact unless he *was* paying more attention to the course of the game than is implied by a stationary strategy. This is equally true of the repetition of a single zero-sum game – for example, repeated plays of 'matching pennies' when one player fancies that the other player is not playing the two strategies with equal probability. But such deviations from full rationality are

frowned upon in the austere mathematical theory.

Learning theories based on a more realistic view of human behaviour are discussed in the second part of Chapter 5; but it is important to recognise that rejecting full rationality is the same as rejecting game theory. The cornerstones of game theory are the payoff function and the assumptions of full rationality and full information; as far as I can see there is no significant part of the structure that would remain intact if either of the cornerstones were tampered with.

To conclude this review, I give below a convenient cross-section of recent work, the contents of the latest in a series of volumes published every few years by Princeton University Press (Tucker et al., 1964). The twenty-nine papers it contains, all of which have a high mathematical content, can be very roughly classified as follows:

(1) Use of game theory to illuminate other branches of pure mathematics (e.g. combinatorics, topology, set theory, measure theory, transfinite numbers) 9
(2) Infinite games, if not already included in (1) 5
(3) Special aspects of n-person games (incl. Shapley value) 5
(4) Mainstream theory of n-person games 6
(5) Sequences of finite games 2
(6) Applications (to poker and economics respectively) 2

It is groups (4) and (5) that one would look to for possible applications to international relations.

As a general summary, I would say that game theory is in a thriving though hardly an expanding condition mathematically, with much good work going on in the United States and the Soviet Union and on a much smaller scale in Germany, France and Japan. There is hardly any original work being published in Britain. It seems that after a fruitful creative period in the ten years immediately after the war, there has been no major new advance since about 1955. (An excellent, thorough survey will be found in Luce and Raiffa, 1957.) I leave out of account here the various explorations of game-like situations and 'meta-games' to which reference is

made in Chapter 4 (see pp. 57-67). In game theory proper there is a tendency for the work to be more and more connected up with, and even to some extent absorbed by, other branches of pure mathematics and less and less oriented towards application to the social sciences. The special applications of infinite games have been mentioned; there is also a revival now of efforts (by Shapley and others) to make a contribution based on game theory to the foundations of economics, in the spirit of von Neumann and Morgenstern. There is, alas, no sign of fulfilment of the original hope that game theory would resolve the mysteries of interdependent decision-making and provide a skeleton key to many arcane departments of human affairs, international relations among them. But this is to anticipate the next section.

Critique of Game Theory as a Model of Political Situations

Notwithstanding these unfavourable portents, it is necessary to start our critique from the point already noted that, as game theory is an abstract theory of interdependent decisions, there are good *a priori* grounds for thinking that it might serve as a model for at least some political situations. This remains a permissible conjecture until it is tested; but even before it has been tested we can subject it to a critique to see whether its axioms and basic assumptions seem to bear a proper relationship to reality. Many writers on political science have tried their hand at this, as they became aware in the past fifteen years of the existence and potential of game theory. Most of these references are enthusiastic or at least optimistic in tone, although on closer examination many of them take the form 'Game theory would be splendid, if only it were something other than it is'. And some are uncompromisingly critical. Five main threads of criticism can be detected: that the theory is indeterminate, impracticable, static, preposterous and irrelevant. I am making these five words work hard; in what follows I shall set out the arguments that each is meant to represent, and discuss their validity. Some of the distinctions I draw may seem over-subtle, but game theory is after all the only sophisticated logical model that has been 'tried for size' by

international relations, and if it has been found wanting it would be as well to know exactly why.

1. Indeterminate

The criticism is one anticipated earlier in this chapter, namely the incoherence and ambiguity of the solutions of non-zero-sum games, which must surely be the model of political situations if any games are. We have here a sufficient reason for saying that as it stands now the theory does not provide us with a useful model; but we should remember that this conclusion is contingent on the state of the theory and might be overturned by a breakthrough, however unlikely this may seem in the light of the earlier dicussion.

2. Impracticable

It is asserted by many critics (e.g. Simon, 1959) that the fantastic information-processing and reasoning powers expected of the players by the assumptions about knowledge and rationality remove the theory from any possible connection with practical affairs. But I believe the point is put in better perspective by Snyder (1955), who suggests that one should not exaggerate the need for great complexity in modelling a political situation. For example, you don't have to list all conceivable strategies, only those that are 'relevant, permissible and . . . feasible'. It remains part of the decision-maker's art to list the effective strategies and assign payoffs. The theory 'simply assumes that values can be known . . . and . . . expressed in mathematical symbols'. This raises a separate, more fundamental, question discussed in (3) and (4) below. For the moment we note that complexity alone does not seem a sufficient ground for disqualifying game theory as a model for political situations.

But it may be wrong to assume that the decision-maker can absorb the complexities at no cost. It has been suggested (Deutsch, 1966, pp. 60-6) that game theory ought to find a way of representing the physical limitations on information-processing and decision-making. It *is* unrealistic to suppose that the decision-makers can bear an unlimited load, or that enormous increases in the amount of information received

can be absorbed without cost elsewhere in the administration. This suggests that some sort of boundary condition ought to be devised to control the amount of activity in a game, or to allow for a change in the rules when the activity rises above a certain threshold, corresponding to the effect of information overload in real life.

To summarise, I would resist any general criticism of the theory on the grounds that the knowledge and rationality assumptions are unrealistic; but would support the need for some element in the theory corresponding to the load on information-processing and decision-making.

3. Static

There is a widespread feeling that game theory cannot be of great practical use unless it enters the time dimension in one way or another. A game, in the present theory, is static, 'once for all'. The values in the matrix are fixed quantities, given from the start. Several writers favour the concept of a supergame or sequence of games in the course of which a player's evaluations of different outcomes evolve, as do his estimates of the other players' values: in other words the players are undergoing a learning process which affects their perception of the payoffs (Deutsch, 1966, pp. 58-9; Kaplan, 1957, pp. 219-39; Iklé, 1967, pp. 164-7). As another way of putting the same thing, it has been suggested (Kaplan, 1957; Coddington, 1967) that the normal form is unsatisfactory, but Coddington rightly goes on to point out that it is essentially in the concept of the normal form that the virtuosity of game theory resides. Shubik suggests that efforts should be made to develop the theory so as to allow for changes in value systems, in the levels of information and belief, in trust, in the perceptions of the bargainers and their feelings of security (Shubik, 1968). Some changes!

Iklé is particularly concerned that the theory should allow for the bargaining reputation of a player as established in previous plays (does he hold out? does he bluff?); Deutsch, thinking of the shorter-term development of a particular crisis, wants the theory to bring out the factors that determine whether a situation is likely to converge or diverge.

Von Neumann and Morgenstern were aware of their self-

denial in having eschewed any dynamic theory, merely remarking that 'The emphasis . . . seems to be shifted more towards combinatorics and set theory – and away from the algorithm of differential equations which dominate mathematical physics' (1947, p. 45) and although the work described above on sequences of games is a step in the right direction, the payoffs in each subgame are still fixed quantities and it cannot be said that anyone has yet found a way of developing dynamic games on the basis of the present static theory.

This defect must be regarded as a serious one, and is in my opinion a great limitation on the prospects of game theory as it stands ever being used as a model for international relations.

4. Preposterous

Bernard Cohen (1962), in a review of *Deterrence and Defense* by Glenn Snyder, puts forward the most direct statement I have seen of the point of view I represent by the word 'preposterous'. He asserts that the inputs (that is, the evaluation and ordering of all the possible outcomes by a player) are not in real life available until after the decisions are made. The decisions in fact precede the ordering, and the only way to deduce the ordering is by observing the decisions. He is not speaking of other players' orderings; he means that the political process is such that the only way for a player to discover what his own preferences are is by observing his own decisions. Game theory is therefore a topsy-turvy model since it represents things as happening in the opposite order from what they really do.

A less paradoxical version of the same point of view – but it *is* the same point of view – is given by Jessie Bernard (1954), who says simply that in social and political situations it is impossible to assess the costs and payoffs. Raymond Aron elaborates on this with specific reference to the international system, asserting that the aims of the actors in the system are diverse, multiple and contradictory: there is no counterpart in international relations to the universal aim of maximising gain that has been attributed to 'economic man' (Aron, 1967) – though it is worth adding that this concept

seems to be losing its usefulness even in the economic context. And in his recent book (Aron, 1966, pp. 772-80) he maintains that in political matters there are no stakes capable of being ordered in advance; the process of compromise through which political decisions are reached may well throw up preferences that are not transitive and so do not satisfy the axioms of an ordered set – for example, A may be preferred to B, B to C, and C to A. Moreover he suggests that as the stakes tend to infinity (e.g. in nuclear contexts), any residual rationality departs, so that strategy today is further away than ever from any rational model.

The effect of these criticisms is to deny the possibility of rationality, and not only rationality as it is understood in game theory, but the very heart of any rational approach at all, namely the assumption (quoting from earlier in the chapter) 'that each player is able to express, in a consistent fashion, his preference as between any two outcomes'. This complete denial of rudimentary consistency in politics I find hard to take as it stands, and as suggested in (5) below, it is possible to adopt a more plausible view of the processes of politics that is no less lethal to the game theory model.

However, to take the argument as it stands, we must distinguish between two assertions that may be made about the process by which decisions are arrived at:

(*a*) that a conscious ordering of the possible outcomes is actually part of the process;
(*b*) that the process can be represented as *equivalent* to one in which the possible outcomes are ordered: i.e. the actor acts *as if* he ordered his outcomes (a similar formulation was much discussed among economists in the 1950s).

These two assertions are not identical: obviously if (*a*) is true then (*b*) is true, but (*b*) could be true even if (*a*) were not. The point of making the distinction is that the validity of game theory as a descriptive political model depends on (*b*), and not on the more exacting (*a*) which is Cohen's target and which for the sake of argument I would concede is not always satisfied. Leaving (*a*) aside, the question is, have the critics disproved (*b*)?

I very much doubt whether they have. How could they? Only by showing in a large number of cases that no such representation is possible, or by proving in general that it is a logical impossibility. The former they have not done; the latter is a tall order and not really satisfied by a few general remarks on the nature of the political process. We may admit with Aron that political aims are diverse, multiple and contradictory; but who can say that it is logically impossible to make summations of them (and, consistently, of the aims connected with the making of other decisions by the same actor) in such a way that the chosen course comes out top of the list? As regards non-transitive preferences, it is difficult to see how the existence of such preferences at any given time could be detected by observation – for only one course is chosen at any one time; and if non-transitive orderings are envisaged as emerging from a series of successive decisions (A preferred to B on Monday, B to C on Tuesday and C to A on Wednesday), it must be remembered that the function by means of which the diverse aims are summated into one ordered set need not be constant and might well vary in some systematic way as the political pressures change. This suggests how very difficult it would be to provide a general disproof of (*b*).

I do not think these criticisms can be regarded as more than assertions; and my counter-assertion is that the political process, though complicated and often confused, is not devoid of reason. I take issue moreover with Aron's suggestion that rationality is not likely to persist when the stakes 'tend to infinity'. It is true that the subject-matter is then less accessible to reason; and I do not believe that nations make, or could make, specific calculations of the consequences of nuclear exchange and weigh up the probabilities of such events against the probabilities and consequences of other events. It is no explicit mental calculus, but rather a shudder of fear, that goes through the political system when such possibilities are evoked, which on the whole seems to make for caution and so leads back by a short cut to rationality. The relative cautiousness of the nuclear powers since 1945 is convincingly brought out by Hinsley (1963, p. 351) in his book *Power and the Pursuit of Peace*. A vestige of this thought will be found in the 'naïve deterrent effect' of the Appendix to Chapter 7 (see pp. 165-6).

5. Irrelevant

P. M. S. Blackett, in an article in *Encounter* some years ago, made this devastating comment:

> I think the influence of the Theory of Games has been almost wholly detrimental ... [it is] a branch of pure mathematics and almost wholly irrelevant to decision-making. (Blackett, 1961)

He did not vouchsafe his reasons, but I believe he was right, at least in the second of these judgments which is the only one that immediately concerns us here.

If the conclusions we have just arrived at are not mistaken, then it is possible for an actor in a political situation to evaluate the various outcomes that he can foresee. He may not necessarily do so but the possibility is there, and a rational decision will be based on the evaluation and not the other way round as Cohen suggests. What we can say is that once the list of possible outcomes has been established and evaluated, there is no problem left to solve. This *is* an empirical, not a theoretical observation. If it is true it means that there is nothing left for game theory to do. In real life, if there has been an evaluation, then the solution is visible in it, emerges from it: one can say that the evaluation is the solution. And so the analytical task is complete. It remains of course to act: it is one thing to select a policy and quite another to carry it out, and the paths to disaster are strewn with good policies that have been discarded or ineffectually applied. But for the decision to act, a different order of qualities is called for – qualities of will and steadiness of nerve. In terms of analysis the whole problem is in the evaluation, and arriving at an evaluation is equivalent to arriving at a decision. If the evaluation is imperfect, the decision may not be the best; but the remedy is to improve the evaluation, not to develop techniques for bridging a non-existent analytical gap between one and the other. It is precisely this gap that game theory is supposed to fill. The game theorist is found to be busily stitching down the middle of a seamless garment in the belief that he is holding the whole thing together. His contribution may be ornamental, but it is not structurally necessary.

The above is, to me, the most telling argument of all against game theory as a model of political decision-making. It is interesting to note that Carl Stevens (1963) comes to the same conclusion on game theory considered as a model for collective bargaining. The only way, he finds, of fitting a negotiation into the framework of game theory is to regard all the real bargaining as taking place before the game starts and as being concerned largely with determining what game is to be played. The play of the game itself is tautological and its analysis of no interest. These comments, as well as the arguments I have given above, suggest that it may be worth looking into the possibility of a meta-theory of games. This is examined, along with what I call 'game-like' models, in the next chapter.

Anatol Rapoport's emphatic rejection of game theory as an aid to decision-making can perhaps be put in the 'irrelevant' box, though it is specifically the attempt to use game theory prescriptively that offends him (Rapoport, 1964, esp. chap. 8). He maintains that the determination of values is the central problem, from which it follows (as we have just concluded) that a theory which starts with values as given objective facts cannot have much to offer. Moreover the performance of an act has an effect on the actor himself and may affect his values in the future; this is covered by the discussion at (3) above. As regards the question of game theory being used prescriptively, i.e. for claiming to tell people what they ought to do, I will only make this comment. Game theory itself – whatever hopes its founders may have entertained – is not a normative (or prescriptive) theory. It is not even a 'conditionally normative' theory as Luce and Raiffa suggest (1957, p. 63). It is an axiomatic theory, like Euclid's geometry. Game theory does not say 'Thou shalt do X'; it does not even say 'Do X if you wish to achieve Y' – for that would only be good advice if you knew the other player was going to follow the same rule; what it says is 'Players in a game with data D who follow a rule of rationality R will arrive at an outcome in the set $[O_1, O_2, \ldots]$; and if a real-life situation can be found that is represented by D and R, then the outcome will be among those represented by $[O_1, O_2, \ldots]$.'

There is little doubt that if the theory of games had never been invented, present-day thinking on political and politico-strategic questions would be different from what it is. The notation of the payoff matrix with outcomes at the intersection of two strategy choices is a neat way of describing something complicated and of organising data; this has stimulated a great deal of thinking and writing, especially via paradoxes such as the Prisoners' Dilemma and the game-like explorations described in the next chapter. The very indeterminacy of the *n*-person game – the fact that there are normally a host of possible solutions of which none has any particular claim to be preferred over the others – is illuminating in a political context and warns us that things are likely to be more complicated, and less stable, than we might otherwise be tempted to believe. Even the two-person zero-sum game, for all its inappropriateness to political situations, is instructive in showing how an apparently infinite regress of mutual speculation between two decision-makers can in some circumstances be telescoped into a unique perception by each of them; one of the fruits of this perceptual conjuring trick – outside the zero-sum field – is mentioned in Chapter 4 (pp. 62-7). On these grounds I conclude not only that game theory has influenced the theoretical approach to international relations, but also that some of the influence has been beneficial.

The above does not, perhaps, amount to a great deal. In any case this critique has been concerned with the narrower and more stringent question whether game theory – the theory itself as distinct from any metaphors or parallels that it may have inspired – can stand on its own feet as a useful model of the international system. The findings here can be summarised in two parts of which the first may be described as technical. From an analysis of earlier writers' critiques we conclude that there is no need to be troubled by allegations that game theory is impracticable in a general sense. But it is unlikely to have practical application to political situations unless technical advances in the mathematics can be made to provide for:

(a) unambiguous solutions of non-zero-sum games;
(b) changes in the rules of behaviour at high levels of activity corresponding to the physical limitations on the capacity for information-processing and decision-making;
(c) development over time, including changes in the players' assessment of values as the game proceeds.

Though recent work on sequences of games is a step in the right direction, there is no sign of a dynamic theory on the above lines emerging. On these technical grounds, therefore, the prospect of useful applications to politics is dim.

Secondly we have found a more fundamental objection. The central problem of political decision-making is to identify and evaluate the alternative courses of action; once this has been done, the decision has virtually been made. It follows that game theory, which takes over only after this stage, is beside the point as a model of political decision-making.

4 In Pursuit of a Model

In this chapter, after attempting a definition of a model in the context of international relations theory, I shall review the models other than game theory at present in stock, that is, those that have been put forward or seem to be implicit in recent theoretical writings as capable of representing short-term international situations – crisis, conflict, negotiation, bargaining. First, then, for the definition. A model is a deductive system whose elements, and the relations between them, correspond to certain identified elements, and the relations between them, of the system under study (in this case the international system). All that is necessary to set up the model is to define the correspondences.

But it is important to distinguish at once between a passive model, which simply preserves in a convenient form the theorist's present ideas and concepts about his subject, and a true or working model which gives access to a deductive system that was not previously available and can therefore be used to develop new theory. A passive model may be of use in focusing the mind and clarifying or popularising unfamiliar concepts, as does a diagram in geometry – indeed passive models often *are* geometrical diagrams or pictures. Most of those to be discussed below are of this kind. But a passive model does not contain any more than has been put into it in the original definitions; it ends there. A model will be truly productive in the development of theory only if deductions can be made in it that are not immediately obvious in real life, and which are testable. The deductive procedure is, in full:

(i) observe a set of empirical facts in real life: call them collectively A;
(ii) construct the corresponding set of propositions in the model: call them a;

(iii) deduce from them some new propositions in the model: call them *b*;
(iv) construct the corresponding propositions in real life: call them *B*.

If the propositions *B* are not obviously true, and yet when tested against reality turn out to be right, then the model has a good mark and the theory has taken a step forward; if wrong, a bad mark and a step backward. If they are blatant tautologies, or if they are not in a form in which they can be verified, the model also has a bad mark because its use has not advanced matters.

A Few Examples

A map is a model of the area mapped; it is a working model, too, with mensuration and geometry available to help in making deductions about the distances between places and their relation to one another. Consider also the model said to have been constructed by Euler about two hundred years ago to solve the problem of the Königsberg bridges. The problem is to find out whether it is possible, beginning at any of the four land areas of which the city is composed (see Fig. 4.1), to go for a walk round the city crossing each bridge just once and return to the starting point. It does not take long to check that many attempted solutions are unsuccessful and to conclude, perhaps, that the existence of a solution is unlikely; but that is not the same as proving that it is impossible. And it would be tedious as well as mathematically inelegant to go through all the possible permutations of the seven bridges.

Euler's model is shown in Fig. 4.2. This brings the problem within the scope of the branch of mathematics now known as graph theory, by means of a modest deformation that shrinks land areas into points and squeezes bridges into lines. The original question about land areas and bridges in the real town of Königsberg becomes a question about lines and points in the figure *ABCD*. Euler was able to prove, using the logic of lines and points in a plane, that this question was in turn equivalent to the question whether every point in the

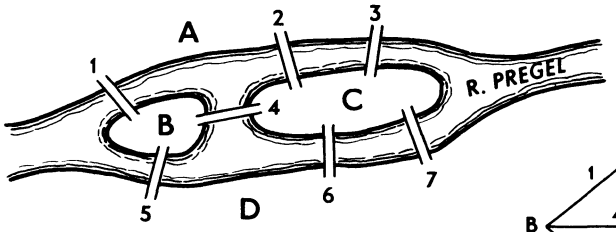


Fig. 4.1. The Königsberg bridges

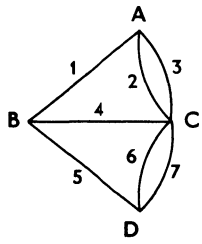


Fig. 4.2. Euler's model

figure had an even number of lines meeting at it. The answer to *this* question can immediately be seen to be no, and so the answer to the original question must be no, which was not originally obvious.

If I am perfectly honest at this point I have to admit that, though the answer in that example was perhaps not *quite* obvious originally, it was very nearly so. It is fairly easy to see directly from Fig. 4.1, and without benefit of any model, that if you take a particular land area – say A – the required tour of the town would be possible only if there were an even number of bridges having access to A, since a pair of bridges is required to get you to A and get you away again (or the other way round if A is the starting and finishing point); and similarly for the other areas. I chose the Königsberg example because of its historic interest; but the model is in fact no more than a visual aid in that case. However, it would be easy to construct an imaginary town with a much more complicated network of bridges where, shall we say, each land area *was* reached by an even number of bridges. If the same question were then asked, the man with the graph model in his mind would be able to assert with certainty that the round tour was possible as soon as he had verified the 'even number' rule for each land area – a routine task. Without this help it would be necessary by trial and error to trace out an actual route, which might be a lengthy business. In this case therefore the model has done what was required of it: it has translated the problem into a form in which it is accessible to a highly developed deductive system, more

powerful than any that was readily available to tackle the problem directly.

Cartesian geometry is another familiar mathematical example of a model. In this case a point in two dimensions is represented by an ordered pair of numbers (x, y), any two-dimensional geometrical figure is represented by an equation in two variables, and thus the resources of algebra (including the calculus) are made available for the solution of problems in geometry.

Other examples can immediately be found. In network analysis and linear programming, a plane network of lines and points may be made to stand for the various interlocking parts of a programme of factory production or a complicated administrative process. A commonplace model in physics is to represent electrons as particles. Another is to represent them as waves. Some phenomena are explicable in terms of one model, some in terms of the other. This shows that a model can sometimes be helpful up to a certain point and thereafter a hindrance. It is difficult, once having been introduced to the particle model, which is a useful shorthand in which to describe some of the phenomena of electricity, to avoid thinking of the electrons as if they *really were* particles. Of course they are not; nor is the north bank of the river Pregel really a point, but it is useful for certain limited purposes to represent it as such.

There is one important difference between the mathematical and the physical models I have mentioned. In the mathematical models the correspondence with reality is ensured by definition and grounded in known facts; there can be no doubt that one *is* a model of the other. In physics, on the other hand, the model usually represents a major assumption about unobservables (causality relations: logical constructs such as atoms and electrons) from which inferences can be drawn which will be observable and which can therefore be used to test the model. The models used in the social sciences are of the latter kind, and will commonly represent assumptions as to human, group or national attitudes, dispositions, etc. In science a model is always the vehicle for a hypothesis.

We have questioned and found wanting the assumptions implicit in taking game theory as a model for the study of

international relations. Now we turn to examine briefly some of the other models that have been put forward or which are implicit in recent mini-theories, starting with those that bear some affinity to game theory. I relate them as they come, and reserve a synthesis to Chapter 7. What follows should be regarded not as a full survey or critique of the research work in this field, but as an analysis of the underlying models. We shall find that hardly any of the models unearthed satisfy the criteria of a 'working model'.

Game-like Models

In his book *The Strategy of Conflict*, Thomas Schelling (1963) probably did more than anyone else to put game theory on the map – an ironic effect because it was his intention to debunk game theory as an aid to the analysis of conflict situations. That he was misunderstood was largely his own fault, for in the course of putting forward his penetrating criticisms of this application of the theory he retained the language of games, littered his pages with payoff matrices and insisted that the innovations he wished to see introduced were to be carried out 'deep in game theory'. But by the time he has finished, though the players are still there, and a game of sorts, the theory has departed.

Perhaps the clearest indication of this is his conclusion (p. 162) that 'the mathematical structure of the payoff function should not be permitted to dominate the analysis'. But that is as much as to say that the central concept of game theory is to be discarded. It is clear from my definition at the beginning of Chapter 3 that, pared down to its essentials, game theory *is* the theory of the mathematical structure of a payoff function. Remove that, and there is no theory left. Schelling later acknowledged as much when, in an understandably acid review (1964) of Rapoport's book *Strategy and Conscience*, he exclaimed: 'The game theory hypothesis was not a bad guess, but a wrong one.'

He concentrates his analysis on the strategic 'moves' by means of which a player can influence the actions of another player through his expectations – threats, promises and, more generally, combinations of statements of the form 'If

you do A, I shall do B'. What are the chances that such statements will be heard, believed and acted upon by the second player, and what is there that the first player can do to improve the chances? He concludes that such questions, which inquire into the relations between two or more consciousnesses, must be researched experimentally and cannot be derived by purely formal deduction.

> We cannot learn anything about those tactics [*sc.* threats, commitments and promises] by studying games that are already in normal form. The objects of our study, namely, these tactics together with the communication and enforcement structures that they depend on, and the timing of moves, have all disappeared by the time the game is in normal form. What we want is a theory that systematises the study of the various universal ingredients that make up the move-structure of games; too abstract a model will miss them. (Schelling, 1963, pp. 156-7)

Schelling outlines a large experimental programme in small-group psychology to fill the gaps. It is not easy to descry, even in outline, the sort of model of which he would approve and into which these numerous factors could be fed. Not 'too abstract'; but what abstraction are we to make and in what would it differ from the universal hurly-burly? It is evidently 'game-like'; but the game is in extensive form and the moves may consist either of real moves or of strategic moves in his special meaning of the term. Perhaps we can venture the following sketch of the necessary elements which I have divided into two, the objective part (A's) and the subjective part (B's).

A.1 A 'state of affairs' S defined between (say) two players X and Y.
A.2 A set of possible future states of affairs $S_1, S_2, \ldots$.
A.3 A set of real moves to be made by X or Y.
A.4 Rules to determine which real moves by either player are admissible in a given state of affairs, and to which new state of affairs each leads.
A.5 Rules to determine admissible strategic moves, that is, statements by a player restricting his own future real moves.

B.1 Sets of probabilities entertained by each player of admissable moves by the other player at any future stage. These will take account of the tendency to favour states of affairs that stand out as *focal points* because of symmetry or some other salient feature.

B.2 Evaluations (for each player) of every possible future state of affairs.

B.3 Rules to determine the effect of a real move by one player on the probability sets and evaluations of the other players.

B.4 Rules to determine the effect of a strategic move by one player on the probability sets and evaluations of the other player (including his assessment of its credibility).

They have only to be written down to show how 'wide open' the situation is and how very far we are from a model – even a helpful diagram, let alone a working model – and we begin to see what we may have lost in abandoning game theory. Having made this general point, I shall move straight on to my next example.

Fred Iklé's study of *How Nations Negotiate* (1967) can be regarded as a further working-out of the seam opened up by Schelling, though on the whole he avoids using the language of game theory. The restriction of scope, implicit in the title, to negotiation as distinct from any wider range of international events is more apparent than real and Iklé manages to include references to international crises such as Cuba, Suez and Hungary as well as to negotiations in the narrower sense such as the test-ban treaty and the Yalta and Potsdam agreements. The key to an Iklé negotiation is the 'agreement' that comes at the end, and I think the best way to link this conceptually with what has gone before is to regard all the real moves as being rolled up in the agreement. The negotiation, in which the real moves are contemplated, discussed and bargained over, itself consists only of strategic moves *à la* Schelling together with proposals, that is to say, statements of terms of agreement that would be acceptable to one of the players. Translating back into game terms, the agreement can be looked upon as the once-for-all classic 'moveless' game in normal form; the negotiation is a pseudo-game or bargaining game that precedes the game itself, and indeed determines

what game it shall be. This, as we have already noticed in the last chapter, is the language used by Harsanyi in his recent papers (1966); at a more practical level it is also, according to Carl Stevens (1963), the only way in which the particular kind of negotiation he was discussing – collective bargaining – can be expressed in terms of game theory. It is clearly the bargaining game that contains all the interest.

An important Iklēan concept is that of *bargaining reputation*, which is the mechanism by which the tactics used by a player in one negotiation may serve as a clue to other players in the next. He says:

> an international negotiation is never a self-contained 'game' but is a phase vaguely related to a never-ending 'super-game'. Although each phase yields its own payoffs, the tactics used in it affect the opponent's calculations in subsequent phases and hence influence subsequent payoffs. (Iklē, 1967, p. 77)

I make no attempt at a comprehensive statement of the desiderata for a model to fit Iklē's book. However, the new elements that can be seen emerging are as follows:

(i) A negotiation has a finite number of steps, and ends in agreement or no agreement.

(ii) At each step each player is confronted by a choice which he must evaluate between three courses: no agreement, agreement on certain stated terms that he regards as acceptable at that time, and further bargaining.

(iii) The evaluation of 'further bargaining' depends on the player's assessment of the probable course of events in the bargaining; this in turn depends in part on the bargaining position taken up by the other player and his inferred minimum position, and in part on his *bargaining reputation* based on knowledge of his performance in previous negotiations and so far in this one.

(iv) Any player may at any time make a strategic move, either proposing terms of agreement that would be acceptable to him or restricting the combinations of terms that he is prepared to consider.

(v) *Rules of accommodation* exist, which are a function of the long-term relations between the players, and the breaking of which entails a heavy long-term penalty, though it may bring advantage in the immediate negotiation.

(vi) It is not assumed that the criteria of evaluation are constant; on the contrary, during the course of bargaining a player may come to assign a different value to the same outcome. The rules must allow for re-evaluation in any new position of the game.

Knut Midgaard is a Norwegian philosopher who has published a number of papers in this field largely under Schelling's inspiration (Midgaard, 1965, 1966). He is another who maintains that game theory must be cherished and yet whose proposals for cherishing it run the risk of smothering it to death.

His contribution is to propose a meta-theory which discusses the possible relations *between* games, rather than the solution of the games themselves. The negotiation stage on which Schelling and Ikle focused their attention is regarded by Midgaard as a network of auxiliary games preceding the primary game and having a certain influence on it; and as he points out, this influence may be of many kinds. He distinguishes those which influence

the structure, or structural knowledge state
the analysis
the outcome
the occurrence

of the primary game, and further distinguishes them according to whether or not the players are conscious of the relationship and take it into account in their play of the auxiliary game. The relationship auxiliary-primary may also hold between one negotiation process and a quite separate later game (this is related to Iklé's concept of bargaining reputation).

His nomenclature is a complicated edifice, definition being piled upon definition, and I shall not elaborate it here. He gives no clear idea of how one could make use of it in practice, and there could be many difficulties in rendering some of the definitions operational. One significant idea is his

suggestion that the route by which a given outcome is reached (in other words the style of the negotiations) may have some value in itself, if only as an example good or bad for future negotiations. A player would therefore be justified in giving himself a bonus in anticipation for using the style that was 'good' in this forward-looking sense, over and above the value that he set as the payoff for the particular outcome. This is what happens when somebody who has been given too much change returns it, and when asked why replies 'Honesty is the best policy'. The 'score for style' is an alternative approach to the 'rules of accommodation' suggested by Ikle.

It may be possible, in developing some of the ideas so far mentioned in this section, to make use of a theory of 'metagames' put forward by Nigel Howard in a recent issue of the *General Systems Yearbook* (Howard, 1966). It is based on a highly abstract theory of behaviour, or rather theory of theories of behaviour, in the course of which we are introduced first to a theory of behaviour, or policy (Howard deliberately does not make any distinction); to a meta-theory in which a choice may be made between the primitive theories or policies; then to a meta-meta-theory in which the meta-theories are under review; and so on as far as we like. The *coup de grâce* is the axiom that no theory is complete without its meta-theory, from which it follows that only an infinite hierarchy of theories and meta-theories can be regarded as complete, and we seem to be lost in an eternal meta-meta-land. However clumsy this might appear at first sight, it turns out to be a surprisingly powerful weapon of analysis.

Suppose we take the game 'Chicken' as an illustration. The payoff matrix is shown in Fig. 4.3. The players are imagined to be driving fast cars directly towards one another, and as they approach the question is which one will 'chicken' and turn aside. Of course they both may; or more unfortunately they may both stick to the alternative play, 'dare', in the mistaken belief that the

	C_2	D_2
C_1	1, 1	−1, 2
D_1	2, −1	−5, −5

Fig. 4.3. 'Chicken'

other will 'chicken'. This is a game that is not taken very far by the methods of traditional game theory. There are two equilibrium points given by the strategy-pairs C_1 D_2 and D_1 C_2 – an equilibrium point being one from which it is in the interest of neither player alone to move. But there is nothing to choose between the two and therefore, as one would expect, no recommended policy for either player and nothing that one could describe in any convincing way as a solution.

The important thing in the theory of meta-games is to decide what 'state' you are in – i.e. from what level of meta-theory you are surveying the game. In the ground game G itself each player is simply in the primitive state of being 'in the game G' and has two primitive choices: to play C ('chicken') or D ('dare'). If we now consider the 2-meta-game, player 2 is to be regarded as being in the meta-state with regard to the game G. This means he is aware of the possible plays of player 1 and treats each as a separate state (even though they are in fact indistinguishable to him). In other words he considers what policy (or meta-play) to adopt, a policy being a set of decisions as to what to do in all the various combinations of plays and states that may confront him. There are four such policies, which I call $\underline{C}_2$, $\underline{D}_2$, $\underline{E}_2$, $\underline{F}_2$, defined as follows:

$\underline{C}_2$	If 1 has played C_1, play C_2 If 1 has played D_1, play C_2	i.e. always play C_2
$\underline{D}_2$	If 1 has played C_1, play D_2 If 1 has played D_1, play D_2	i.e. always play D_2
$\underline{E}_2$	If 1 has played C_1, play C_2 If 1 has played D_1, play D_2	i.e. tit for tat.
$\underline{F}_2$	If 1 has played C_1, play D_2 If 1 has played D_1, play C_2	i.e. do the opposite.

To make one point clear: player 2 does not actually know in advance what player 1 is going to do. Indeed this is intrinsic both to games in normal form and to the political situations for which we are seeking a model. What player 2 is doing is pretending that he knows what player 1 is going to do and seeing what happens; he is simply engaging in speculative analysis of the possibilities that arise when he considers as distinct the states that different plays by the other player give rise to, even though in physical terms they

are known not to be. The point is that this process of speculation – which is available to both players alike since they both know the payoff matrix – may itself provide clues to what the other player may do.

The second meta-level is the 1-2-meta-game, which is derived from the 2-meta-game by repeating the process of speculative analysis from the point of view of player 1, who may be assumed to have already gone through the above process on player 2's behalf and who therefore has to contemplate the possibility that he may be in any one of four meta-states, one corresponding to each of player 2's policies. We move from self-consciousness to self-and-other-consciousness.

There are 16 possible meta-policies (or meta-meta-plays) for player 1: $\underline{\underline{C}}_1$, $\underline{\underline{D}}_1$, $\underline{\underline{E}}_1$, . . ., $\underline{\underline{R}}_1$. We can conveniently set out in a table the consequences in terms of actual play to which each combination of policies gives rise (in Fig. 4.4 I have shown only four of the 16 rows):

Player 1's meta-policies	Player 2's policies: $\underline{C}_2$	$\underline{D}_2$	$\underline{E}_2$	$\underline{F}_2$
$\underline{\underline{C}}_1$	$C_1\,C_2$	$C_1\,D_2$*	$C_1\,C_2$	$C_1\,D_2$
$\underline{\underline{D}}_1$	$D_1\,C_2$*	$D_1\,D_2$	$D_1\,D_2$	$D_1\,C_2$*
.	.	.	.	.
.	.	.	.	.
.	.	.	.	.
.	.	.	.	.
.	.	.	.	.
$\underline{\underline{K}}_1$	$C_1\,C_2$	$D_1\,D_2$	$C_1\,C_2$*	$D_1\,C_2$
.	.	.	.	.
$\underline{\underline{M}}_1$	$D_1\,C_2$	$D_1\,D_2$	$C_1\,C_2$*	$D_1\,C_2$
.	.	.	.	.
.	.	.	.	.

Fig. 4.4. 'Chicken': the 1-2-meta-game

The asterisked policies indicate some of the equilibrium pairs in the enlarged matrix (there are 16 altogether). The significant thing is that the speculative analysis, formal and unreal though it may be, has thrown up a new equilibrium pair

which was not an equilibrium pair in the original matrix, namely $C_1 C_2$. Moreover this one, unlike all the others, is symmetrical and this gives it a certain salience. Howard is able to prove that in any two-person game, notwithstanding the infinite hierarchy of policies that he started with, all the additional equilibrium pairs thrown up in this way are revealed after only two steps – though it is necessary for completeness to go through the process again with the players the other way round. In this case symmetry indicates that there will be no significant new discoveries.

The $C_1 C_2$ solution, which appears twice, corresponds to the intersection of the following policy choices by the two players:

By player 2: the policy of tit for tat, which we have labelled $\underline{E}_2$, viz.

playing C if player 1 plays C
playing D if player 1 plays D

By player 1: the meta-policy which we can label $\underline{\underline{K}}_1/\underline{\underline{M}}_1$, defined as follows:

playing D if player 2's policy is always to play D ($\underline{D}_2$)
playing C if player 2's policy is tit for tat ($\underline{E}_2$)
playing D if player 2's policy is to do the opposite ($\underline{F}_2$)
(It does not matter what player 1 does if player 2's policy is always to play C ($\underline{C}_2$); the same equilibrium point arises)

It is noticeable that familiar expressions like 'tit for tat' which come in handy in the first-order meta-game can no longer be used with any clear meaning in the second and higher orders. Ordinary language is equipped to express policies, but not meta-policies.

$C_1 C_2$ is a square solution with a middle-aged appeal, and one that if generally followed would rob the game of Chicken of most of its practical interest. But it has *something* to be said for it in terms of common sense, and the interesting theoretical point is that it is not until we ascend to the theory of meta-games that it comes to light. The standard processes of game theory fail to throw up any real solution at all.

If we fall back on the usual, narrow, definition of rationality, the new solution that has been thrown up is strictly

'irrational' for each player given the other's choice. It comes about through player 1 asking himself whether he should be 'rational', or whether some other policy might be rational in a wider sense in view of the fact that player 2 can ask himself the same question.

There is no last-minute reprieve for game theory as a whole in this theory of meta-games. With its wider definition of rationality it certainly provides a more satisfying technical framework for the solution of some conundrums such as Chicken and the Prisoners' Dilemma, which is discussed in Chapter 5 (pp. 112-17). But the total number of solutions of non-zero-sum games has if anything increased and the general difficulties discussed in Chapter 3 are unaffected. The interest of this work for our present purpose lies in the possibility – briefly touched on by Howard in the paper referred to – that the same framework could be used to investigate ideas such as those of the other writers already mentioned in this chapter. For example, the process of deriving meta-games described above is all on the assumption that the payoffs remain the same from whatever height they are perceived, and it would be interesting to inquire what happens if a player attaches a value to a policy in itself, distinct from the value that he attaches to the outcome of the original game. This is precisely the 'score for style' idea of Midgaard's that we were considering earlier. Howard mentions this variant of the theory, only to terminate the discussion with the stern remark that it is 'uninteresting because there is not much more one can say about it'. Another example is the Schelling-type strategic move which is a conditional move in the primitive game and which can now be regarded as an unconditional statement by a player that he is in the meta-state and has adopted a certain policy; this in turn becomes an input to the other player's meta-game at the second level. The interesting questions are, as ever, whether an 'unconditional' statement is really unconditional: if both players make 'unconditional' statements – and if they both mean them it is not a promising situation – what determines whether one of them will back down; and so on. As a third example, there seems to be no reason why bargaining games should not be studied by the theory of meta-games.

All the writers considered in this section have been wriggling, more or less furtively, out of the straitjacket of game theory proper. The common feature which they share also with the work of Harsanyi mentioned in Chapter 3 is their interest in the bargaining, skirmishing or speculative acrobatics that precede the game rather than in the game itself. This change of focus may bring reality into better view, but at the same time it tends to dissolve the tight logical linkages of game theory that were part of its original attraction. With the writings of Schelling, Iklé and Midgaard there is no longer a model in the strict sense. However, we have acquired three important concepts: the strategic move, the bargaining reputation, and the rules of accommodation (or 'score for style') which will come in useful when we set about constructing a home-made model in Chapter 7 and its Appendix. In the case of Howard we have a new analytical procedure which may be a fruitful source of game-like models in the future.

The Communication Model

An increasing amount of theoretical writing in international relations today is indebted to communication theory and cybernetics. The lure of these new sciences is that they seek to abstract the common features of all systems that depend for their survival on information: the systems may be living organisms, guided missiles, business firms or nations. The central notion is information, and attention is directed to those properties that a system must have if it is to receive and handle information to the best advantage. Some of the more obvious of these properties are:

receptivity – the capacity of the system for receiving information and readiness to accept new information
selectivity – the ability to distinguish significant information from background noise
memory – the capacity for storing information and recalling it
cohesion – the fidelity of internal transmission

and of course

feedback – the capacity for making comparisons between what was intended and what was achieved and correcting future action accordingly.

Feedback, the most famous horse from this stable, has been ridden up and down for some time by political theorists and still seems to be going strong. It provides the essential link between the information circuit and the action circuit without which action is blind and information useless. Learning can be interpreted in cybernetic language as an advanced type of feedback in which information is used to change not only the future action but also the actual rules of operation of the system. Karl Deutsch (1966) in *The Nerves of Government* gives a stimulating account of how political phenomena can be looked at afresh through these new spectacles, and for good measure offers translations into cybernetic terms of free will, mind, dignity, integrity, meaning, faith and reverence.

It would be a correct usage to speak of communication models when the central feature is perception and cybernetic models when the central feature is control; but the two are not always easy to distinguish and I shall follow common practice in using communication models loosely as a general term. For a single nation the communication model would have to show the nation in relation to its environment, acting on the environment to make it more acceptable, receiving feedback from the environment and adjusting its intentions and its actions accordingly. For two nations a loop of interaction is formed between the two. It is clear – to refer back for a moment to our types of mini-theory in Chapter 2 – that we are well and truly inside the 'black box'.

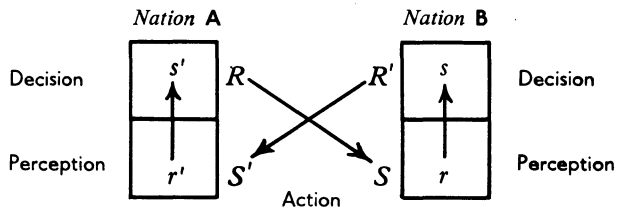


Fig. 4.5. The communication model for two nations

The diagram in Fig 4.5 shows how the links between the two nations can be envisaged in the communication model. Suppose we start with the diagonal 'action' line that goes from top left to bottom right and follow the sequence of events round the diagram:

An act *R* by nation A offers an external stimulus *S* to nation B.

S is perceived by nation B as *r*.

The perception *r* is transmitted to nation B's decision-makers on whom it acts as a stimulus *s*.

The stimulus *s* gives rise to a decision to act which can be regarded as a response *R* by nation B to the original stimulus *S*; and so on.

Note that the external acts are represented by capital letters and the internal acts by lower-case letters.

The model has been applied by Holsti, Brody and North at Stanford to the analysis of the period leading up to the outbreak of the First World War in the following way (Holsti et al., 1964). Actions by either side (R/S), including statements aimed by one side at the other, were scaled in terms of a dimension of behaviour such as conflict or hostility. All other significant documents that were publicly available were classified into *r* (internal appreciations, e.g. by ambassadors or Foreign Offices of statements made or action taken by the other side) and *s* (policy documents and statements of intention to act). Having been so classified, they were then subjected to content analysis on the lines set out in Chapter 2. The scores for each factor were correlated as between the various links with respect to both sides and this gave a number of indications of the points at which, for example, the 'hostility' factor was magnified or diminished. Behavioural characteristics of the two sides – such as a tendency to overperceive hostility on the part of the Dual Alliance ($r>S$), a tendency to underperceive hostility on the part of the Triple Entente ($r<S$), a tendency to overreact on both sides ($R>s$) – could be inferred. The parallel study of the Cuba missile crisis by the same authors (Holsti et al., 1965) showed that perceptions then were generally much more accurate and that, for example, efforts by either party to delay or reverse the escalation were perceived as such and responded to in like manner.

Richard Brody has elaborated the above elementary model in a conscious attempt to organise the numerous but elusive forces that must be deemed to be operating within the 'black box', in a recent publication (Brody, 1966) the essence of which is set out in Fig. 4.6. The stimulus is first characterised in four ways: according to intensity, to whether it is physical

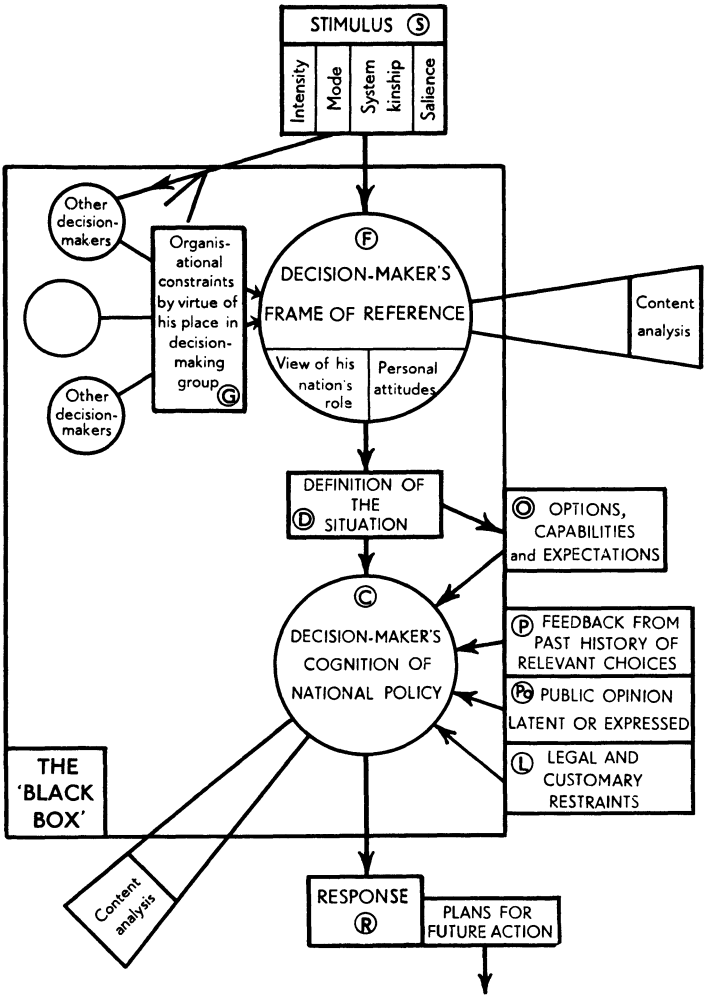


Fig. 4.6. A communication/cognition model

or verbal (mode), to the system relationship between the source and the target of the stimulus (system kinship) and to whether the source is one that the target nation will readily attend to (salience). This information, according to Brody, is then located by the decision-maker in a frame of reference F including the features shown in the diagram, which enables him to arrive at a definition of the situation – an answer to the question 'What have we here?' This goes through for action to the next stage, labelled 'cognition of national policy', which is a summary of the way the decision-makers look at their nation's behaviour towards other nations, and which acts as a generator of responses. Given a well-defined situation it will in turn bring forth its response, which will be capable of measurement and characterisation under the same headings as the original stimulus *S*. The cognitive apparatus is itself largely moulded and held in position by boundary conditions of various kinds as shown in the diagram.

Brody envisages that the necessary understanding of both the frame of reference F and the cognition of policy C will have to be obtained from content analysis of documents, in default of any prospect of being able to assess the psychological and organisational factors directly. Both F and C are regarded as entities that will in some way be measurable (in a number of dimensions), but it is not clear what these measures would mean. It may be that they are to be regarded as intervening variables, meaningless in themselves except as necessary bridges from the stimulus to the 'well-defined situation' and from the situation to the response. This is a common enough phenomenon in theories of individual psychology.

Can we say anything about the modes of the constraints O, P, Po and L that act upon the cognition process? It seems plausible that the modes of operation, and the priorities as between the various kinds of constraint, will vary from nation to nation and will be different in crisis from what they are in normal times. We do not know what mechanism Brody may have in mind for representing the action of these constraints in his model. It is possible that so far as P is concerned – feedback from past history of relevant choices – there may be an eventual contribution from theories of learning, as suggested in the second half of the next chapter.

It could well be necessary to postulate two sets of laws, one as a mini-theory to describe how the system uses feedback for incremental learning in the short term, and one as a maxi-theory to describe how the learning process cumulatively brings about system change in the long term.

The considerable constructive effort that has been applied here must not blind us to the fact that there remain a very large number of unknown relationships about which hypotheses will have to be made and tested before the model can come alive. This fact, together with the heavy and indeed excessive reliance on content analysis that is implied in current discussion of the model (though not logically demanded by it if other means of access to the 'black box' can be devised), must prepare us not to expect too much. As a general, if irreverent, comment on the communication model in any of its forms, I cannot help going back to first principles and asking whether any theorem of communication theory – which after all *is* a scientific theory and *has* theorems – has ever been translated into terms of international relations. If not, we must admit that the model is, for the moment, a passive one. It may have helped us, for example, to see more clearly the two kinds of learning laws that might be worth looking for – to pick up the conjecture at the end of the last paragraph – but it does not supply, yet, any additional deductive power for exploring the question.

Of the same general type is the simple mathematical model of the bargaining process, primarily designed for economic contexts, that has been put forward by John Cross (1965) and discussed and elaborated by Alan Coddington (1967, 1968). It is a communication model – more strictly, a cybernetic model – in that it is a closed loop in which each actor adjusts his actions in the light of feedback from observation of the environment, namely the other actor's actions. It is formally similar to the basic model just considered (Fig. 4.5), but simpler in that there is not supposed to be any problem of perception: the act by one party and the perception of it by the other are the same. The process of perception is replaced by another uncertain process, that of expectation as to what the other actor is going to do next. The two operations for each actor therefore become 'expectation' and 'decision' (see Fig. 4.7), and in Cross's

model the reaction round the circuit is considered to be continuous, more like an electric current than a game of catch.

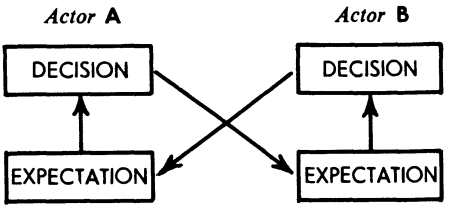


Fig. 4.7. The Cross bargaining model

The two actors A and B are assumed to be negotiating for shares of a certain 'cake' M. At time t their claims are p and q respectively and assumed to be changing continuously; moreover actor A estimates that B is modifying his claim at a rate s, and B estimates that A is modifying his claim at a rate r. The two continuous and connected operations of making decisions and adjusting expectations are accounted for in the Cross model as follows:

Decision-making. At any given time t actor A decides what claim to make, setting p at the most advantageous value (to maximise his utility) on the following assumptions, the first two of which are notably unimaginative:

(i) he, actor A, will stick at p for all time;
(ii) actor B will concede steadily from his present claim q at the rate (assumed constant) currently estimated by A (see 'Adjustment of expectations' below);
(iii) agreement will be reached at the moment when for the first time $p + q \leqslant M$;
(iv) the utility to A of agreement at the point P at some future time T is a known function of P and T (e.g. linear in P and containing a discount factor for increasing T as in most economic bargaining models). Incidentally, an important feature of the notation in this model is that P, Q and T – the values of p, q and t at which agreement is reached – are treated as variables in their own right.

This will give rise to a differential equation together with a boundary condition expressing (iii). Another similar equation can be derived for actor B.

Adjustment of expectations, or *learning*: A adjusts his expectation of B's concession rate (the variable s) not by choosing a new value of s but by amending the rate of change of s, written $\dot{s}$, to keep it proportional to the gap between s and the observable quantity of which s is an estimate, namely $\dot{q}$. The wider the gap the more quickly s will move in to bridge it. In symbols,

$$\dot{s} = \alpha (\dot{q} - s).$$

Again there will be a similar equation to express B's adjustment of his expectations about A. Thus we have two pairs of differential equations, one pair for each actor.

Communication models are noted for the care with which they make allowance for the actors' limited powers of information-processing and rationality; in this model the actors' reasoning powers are limited to the point where they border on the idiotic. That is not to say that they will necessarily give bad answers; but there is something unsatisfactory about what Coddington calls a 'self-replacing' decision rule, that is a rule that A expects B to adopt while himself adopting a different, presumably more subtle, rule (Coddington, 1968, p. 60). Another way of saying this is that A in making his decisions takes no direct account of B's decision-making process. There is no sense in which he puts himself in B's shoes. Such interdependence as there is arises only because A, in making his decisions, is taking account of an estimate of B's behaviour that is subject to correction by reference to B's actual performance.

Coddington has sought in his work on this class of models (1968, chap. vi) to find alternative decision rules that are not self-replacing and which bring in an element of direct interdependence between the two decision-making processes. As is to be expected with differential equations, neither author has been able to find general solutions to his sets of equations except under conditions of drastic simplification. It is therefore difficult at the present stage to breathe life into the

model by quoting typical interesting solutions. As yet there are none, though numerical solutions can be, and have been, explored by computer.

The Cross–Coddington model, with its discounting for time, with its blinkered decision rules, with its constant 'cake' reminiscent of our old and discredited friend the zero-sum game, seems remote from any political situation, unless it be one containing a large element of financial or commercial negotiation. But one cannot be too sure. There are structural resemblances to several of the other models discussed in these pages, especially the other communication models dealt with in this section and the linked-learning models in Part II of Chapter 5. We have here an important new departure, independent of game theory, in the effort to represent the behaviour of two self-steering mechanisms, their destinies inextricably linked but each regarding itself as largely independent. This is not a bad description of a political duel between two nations. If the model can be adapted to cater for varying amounts of recognition and awareness by each actor of the problems facing the other, it might have a lot to offer. For the moment, though, the theory is at such an early stage of development that we cannot say more than that it shows promise.

Lewis Richardson, the pioneer of mathematical models in political affairs, would be surprised to find himself laid out for inspection under the heading 'communication models'. And yet, as will be seen, his work embodies the essential idea of nation A perceiving a threat from nation B in one frame of reference – 'What is he up to?' – and reacting to it in another frame of reference – 'What am I now bound to do?' – just like Fig. 4.5. Richardson's main ideas on wars and arms races were worked out before and during the Second World War but were not published in fully extended form until 1960 (Richardson, 1960*a*, *b*).

I shall take his model of arms races as an example and show its similarity to the other models under discussion in this section. His main simplifying assumption is that each nation can be represented by a single variable which in some way corresponds to its external posture of threatening or co-operation. The equations he proposes to describe the interaction of two countries, with representative variables x

and y, are:

$$\dot{x} = ky - \alpha x + g$$
$$\dot{y} = lx - \beta y + h$$

where k and l are the defence coefficients of the two nations and measure the alacrity or the reluctance of each to react to provocation by the other: the bigger k is, the more rapidly x will increase for any given value of y. α and β are damping factors due to the cost of warlike preparations. g and h are constant terms representing long-standing grievances or ambitions that might be expected to cause x or y to increase with time even in the absence of any provocation from the other party.

By taking different arbitrary values of k, l, α, β, Richardson is able to show how the model will in some cases be stable and tend over time to converge on an equilibrium position, and in other cases be unstable and tend to diverge either towards ever-increasing arms levels and thus eventually, it must be supposed, to war, or towards complete disarmament. In fact if $kl < \alpha\beta$ the model is stable; if $kl > \alpha\beta$ it is unstable.

It is necessary to find a means of measuring or observing x and y if the model is to be tested. Richardson's first approximation is the defence expenditure of each country, reduced to a common monetary unit, though he later seems to prefer what he calls 'warlike worktime'. There is obvious difficulty in getting an acceptable measure, and Richardson tries to refine his original measures in various ways – among others by the dubious method of bringing the amount of trade into the calculation as if trade were perceived as a co-operative counterpoise to defence preparations. But the important thing is that Richardson found nothing insuperable in the measurement problem: the approximation one uses may be better or worse, but what cannot be doubted is that there exists some way of measuring the amount of threat that one nation offers to another. Nations do make these perceptions of one another, and in assuming that the perceptions have a basis in measurable fact we are making no more than a minimum assumption of consistency. Moreover, once a basis of measurement is announced and becomes the subject of general discussion, it tends to reinforce itself by its very

existence, and in default of anything better will become the actual basis of nations' perceptions simply because it is there. There is no reason why a measure of 'national menace' should not be as soundly established as a measure of national income – in which there are also inevitably some arbitrary or conventional elements. Von Neumann and Morgenstern (1947, p. 3) point out that in the history of physics precise measurements of the quantity and quality of heat energy and temperature were the outcome and not the antecedents of the mathematical theory. This supports the general view that measurement problems should not be allowed unduly to discourage the builder of a theory.

There is, on the other hand, a big assumption implicit in the simple form of Richardson's equations, in particular in taking k and l to be constants. It is of such assumptions that a model consists, and they can be substantiated only by testing. Richardson only partially succeeds in doing this, but produces one extremely good fit, for the path taken by the arms race in the years 1909-13. He also explores a number of variants of the equations to correspond to plausible assumptions about the behaviour of nations: for example by allowing the damping factor α to increase as x increases (choking off), or allowing k to decrease when $(y - x)$ is large so that a smaller nation will be cowed rather than provoked by a much more powerful one ('submissiveness').

There is no doubt that we have at last found a working model in the full sense demanded by the definition at the beginning of the chapter. We are told how to derive the variables from the real world; and, given a certain amount of data to set the parameters, the formulae can be used to deduce how the system should evolve, according to the model.

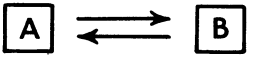
Nothing was further from Richardson's intentions than that he should be thought to be laying down his equations as laws of nature. His aim was to make his fellow-men aware of the distressing regularities of the past in order that conscious efforts should be made to avoid making the same mistakes again. 'The equations', he says, 'are merely a description of what people would do if they did not stop to think . . . if instinct and tradition were allowed to act uncontrolled' (Richardson, 1960*b*, p. 12). Indeed its sweeping simplicity, and

the difficulty that has been found in fitting later data to the formulae, suggest that the Richardson model may have had its day. In that day it has certainly had an influence, epitomising as it does the notion that an international system may be self-igniting in the absence of any specific *casus belli* or even initial hostility, given only certain ingrained attitudes of mutual distrust. It is essentially a vehicle for maxi-theory, for showing the broad sweep of events over the years after the wrinkles have been ironed out. It is not likely to tell us very much about the wrinkles.

It is interesting, as a footnote to the Cross and Richardson models, to know that work has been done analogous to both starting from the same assumptions of interdependent behaviour but using difference equations instead of differential equations (Bailey, 1962, chap. ix; Smoker, 1967, pp. 46-8). In these versions the process is regarded as a sequence of finite stages, the action taken in stage $(n + 1)$ being determined by the players' observations of what has happened in stage n and earlier stages, instead of being regarded as continuous and therefore a proper subject for the differential calculus. In economics with an (almost) continuous variable – money – the assumption of continuity may normally be made without qualms; in political interaction where the moves are usually discrete and well spaced in time it may not be justifiable. And, as Smoker shows, the solutions may be markedly different. It seems a reasonable guess, therefore, that difference equations will come into their own if these models prove worth developing in the context of international relations.

Simple Interaction Models

Are there such things as simple interaction models? If so, can they be any good? If the question is whether there is anything to be gained by setting down bravely on paper the pattern



the answer is probably no. Most of the researchers referred to under the headings 'Interaction Analysis' and 'Comparative Analysis of Nations' in Chapter 2 (pp. 12-13) would deny having any more complex model than that. But are they

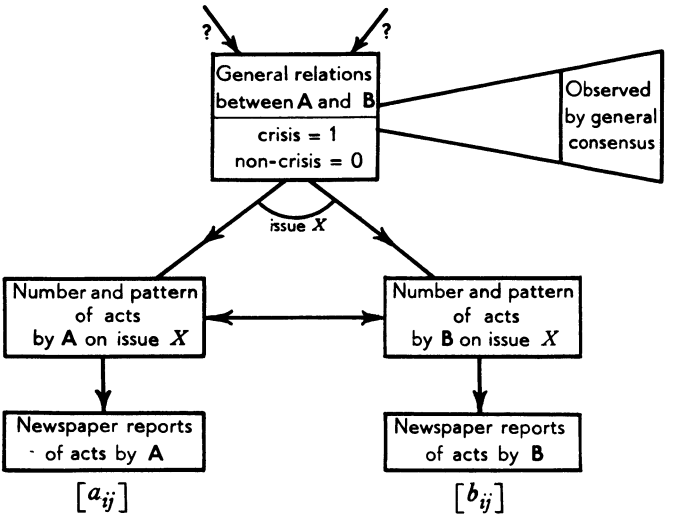


Fig. 4.8. A static interaction model

really as innocent as they make out?

McClelland's outlook in his work on the Taiwan confrontation and the Berlin crisis already mentioned in Chapter 2 can be set out schematically as in Fig. 4.8. In these papers (McClelland, 1965, 1966, 1967) he defines a number – say N: McClelland starts with 63 and brings the number down by stages ultimately to 5, but for most of his analysis uses intermediate numbers, sometimes 12, sometimes 18, sometimes 22 – of categories of action covering the spectrum of acts that one nation can perform towards another, ranging from the most conciliatory to the most aggressive and provocative. He then notes and sorts into these categories all the events – announcements, threats, military or political moves – taken by either nation towards the other in respect of the issue under study, say the question of access to Berlin, as reported in the newspapers for each period of a week or a month. He relies heavily on the *New York Times* index, but has been developing the use of other sources. He adopts the hypothesis that good indicators of the state of crisis between the two countries generally can be derived from this information, and tests two in particular: the *quantity* of events, which is simply the total number perpetrated by each nation during each period; and the *variety* of events, which is a measure (for each side separately) of the spread of events

over the spectrum. The 'variety' measure is chosen so that if all the events were tightly bunched in a very few categories the value would be small and if they were evenly spaced with exactly the same number in each category the value would be 1. It turns out that there is a good correlation between the state of crisis as generally recognised and the passing of these two indicators over certain well-defined thresholds.

By studying the actual pattern of actions – that is, what categories of action predominate, not merely the statistical contours of the distribution – he is able to make interesting observations about the trend of behaviour at special times, especially when the situation is mounting towards a crisis or falling away after one: for example that in the two major crises associated with the Taiwan confrontation (January to March 1955 and August to October 1958) 'crisis abatement was accompanied . . . by more than average amounts of harsh language directed at opponents. . . . One is led to suspect that conflict parties in international crises sometimes are inclined to cover their retreats from violent deeds with a barrage of complaints, protests, accusations, denials, rejections, warnings and threats' (1967, p. 24). These trends, though they persist in successive crises in one arena, do not seem to travel well and the patterns for the two arenas studied by McClelland are significantly different.

The diagram now needs little explanation. At the top the presence or absence of a crisis is noted, as a matter of consensus for any given time; at the bottom the number of events reported is counted, in each time period $t_1, t_2, \ldots, t_i, \ldots, t_M$, for each of the N categories of event 1, 2, . . ., j, . . ., N giving two $M \times N$ matrices of readings $[a_{ij}]$ and $[b_{ij}]$ for A and B. The 'quantity' and 'variety' measures are functions of these a_{ij} and b_{ij}.

We are not told where the crisis comes from nor where it goes to: there is no causal presupposition at all. We do not know whether the crisis caused the critical pattern of events, or the events the crisis; or whether they are the same thing and 'crisis' is simply a name for a class of patterns of events. We do not know whether there may not be an intervening variable, in the shape of the perception by the editor of the *New York Times* that there is a crisis, which would regularly lead to a greater number and a wider variety of reports being

printed. McClelland himself admits this possibility, at least as far as the 'quantity' of events is concerned (1967, p. 12); but it could equally well apply to the 'variety'. Indeed in the Taiwan study he reports a statistical chi-square test showing that the variables 'quantity' and 'variety' are not significantly distinct (<0.01), so that, whatever it is that they measure, they can be regarded as measuring the same thing (1965, p. 78).

So far, so good. But it is to be presumed that behind these variables McClelland sees dispositional forces working within each nation, forces going deeper than and separable from the actual visible presence or absence of a crisis. He studies with great care (1967, pp. 16-17; 1968, pp. 183-4) the periods for which the chosen variables say 'crisis' when outwardly there was none, from the point of view that perhaps there was 'something going on' just the same. He is now collecting data (the World Event Interaction Survey) with a view to identifying the dispositional characteristics of nations as they interact in normal times and comparing them with instances of behaviour under the pressure of conflict. Indeed, without some such intervening variables it would not seem possible to progress from the static step-by-step descriptions characteristic of the work already cited to a dynamic model having some predictive power.

If we relax the austere demeanour of the interaction analyst for a moment and admit that we should like to have a peep at the national decision-makers, it may be that the pattern of actions chosen by McClelland as a data base will turn out to be as good a window to look through as the content analysis of the communication/cognition model in Fig 4.6 or even better. Perhaps the (as yet) unavowed dynamic model would be something like Fig. 4.9. As well as showing the two nations' actions and behaviour states reacting upon one another, I have also inserted a probabilistic 'disturbance factor' to allow for the fact that there is never likely to be perfect correlation between the values of the Q and V variables and the existence of a real honest-to-goodness crisis. But the construction of this model is yet to come.

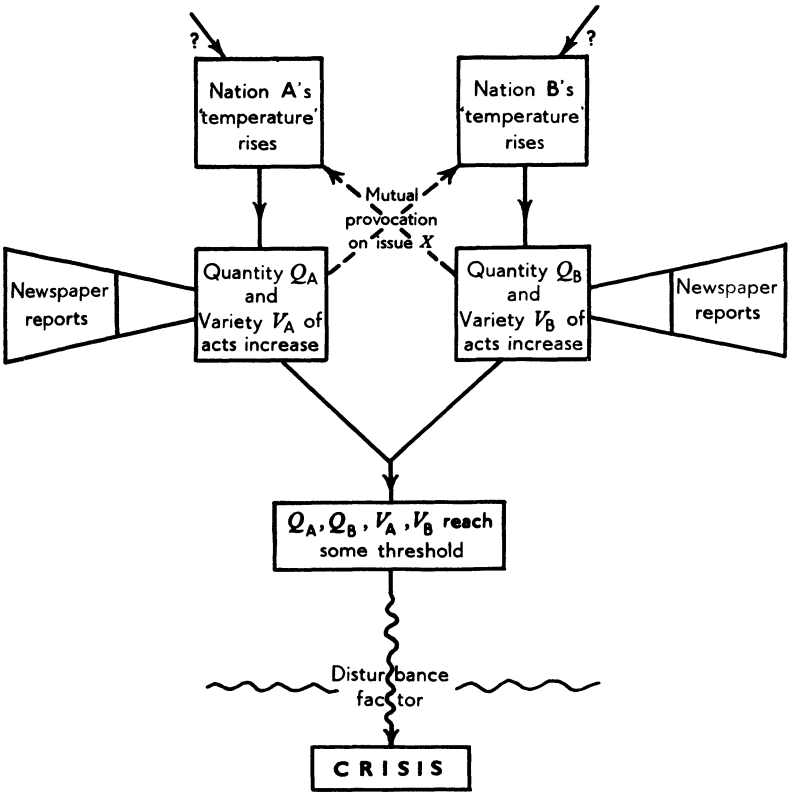


Fig. 4.9. A dynamic interaction model

Dimensionality Models

These can be briefly dealt with. Rummel (1963, 1964) and Tanter (1966) have explored the data provided by the projects mentioned in Chapter 2 (see p. 13) by means of factor analysis, working on the general assumption that a nation can be usefully characterised by a small number of variables ('factors') which change only slowly with time for any given nation and whose variation across nations accounts largely for the variation in the many indicators for which values are known. Indeed the assumption is a rather stronger one, that any indicator I is a linear function of the factors $a_1, a_2, \ldots, a_n$:

$$I = \sum_{1}^{n} \lambda_i a_i.$$

It may also be presumed that the authors believe all behaviour to be a function of these underlying factors, which are supposed to constitute the basic stuff of which the comportment of the nation is constructed – always allowing for the intervention of a probabilistic 'disturbance factor'. In its simplest terms all this throws up, so far as I can see, is an elementary fragment of a model (see Fig. 4.10) which is static and unformed in two major respects: there is no provision for change in the national factors with time (which would be basic to any maxi-theory), and there is no procedure laid down for constructing the external behaviour profiles by means of which short-term situations are translated into specific behaviour (which would be basic to any mini-theory).

Rummel's best-known result, which emerged not from any correlation with a dependent behaviour-variable but from the factor analysis itself, showed that there were virtually independent factors responsible for internal (social) conflict and external (warlike) conflict. From a study of 77 nations over the period 1955-7 he concluded (1963) that 'foreign conflict behaviour is generally completely unrelated to domestic conflict behaviour'. This result was slightly qualified by two other studies (Rummel, 1964; Tanter, 1966)

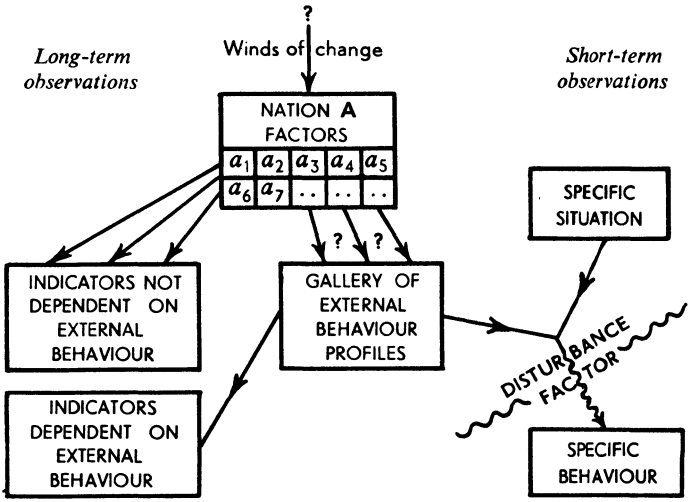


Fig. 4.10 A dimensionality model

of which the latter concluded that 'there is a small relationship between 1958-60 domestic and foreign conflict behaviour which increases with a time lag'. Nevertheless the general conclusion remains that there is no close relationship.

Models from Graph Theory

Graph theory, which is the branch of mathematics concerned with the formal properties of points with lines joining them, seems to be a 'natural' for the representation of the relations between states. We have already met it briefly at the beginning of this chapter in connection with the Königsberg bridges. However, there is hardly any trace of it in the literature and only one occasion known to me where a graph is used as a working model, as it was by Euler.

This example is from Frank Harary (1961). The mathematics of it is straightforward. Nations are represented by points. Between any pair of points there may be a positive link, a negative link, or no link at all (corresponding to the state of relations between the nations). The system collectively is known as a signed graph.

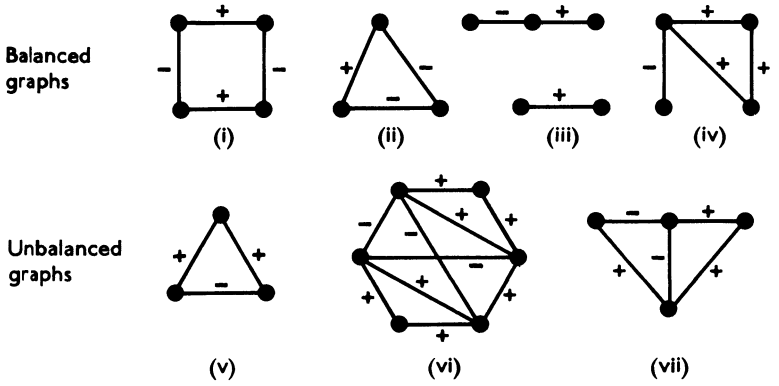


Fig. 4.11. Some signed graphs

Harary then introduces the idea of balance, corresponding to a sort of stability in the international situation. A signed graph is balanced if and only if all its cycles are positive – that is to say, if in going round any part of the graph starting

and ending at the same point the product of the signs of all the links is positive. Fig. 4.11 shows some examples. His theorem, which is not at first sight obvious, though it is not difficult to prove, is as follows:

> *Theorem*: A signed graph is balanced if and only if its points fall into two non-intersecting sets S_1, S_2 such that all positive links are within one or other of the sets and all negative links are between a point in one set and a point in the other set.

The second part of this theorem, translated into the referent world, corresponds to a situation of two blocs or bipolarity. What does 'balance' as here defined correspond to in the international context? It means, roughly, that your friends' enemies are your enemies; that your friends' friends are your friends; that your enemies' friends are your enemies and that your enemies' enemies are your friends – unless in each case you are indifferent to them (i.e. there is no link in the model). And so on to any degree of remoteness. It is hard to say, knowing the answer, whether this state of affairs is obviously equivalent to bipolarity: I am afraid that if we are honest we have to conclude regretfully that the result is almost obvious in the referent world even if it was not in the model.

The more seductive part of Harary's article shows *how*, according to graph theory, the Middle East situation became unbalanced and then balanced again in 1956. But he admits disarmingly the weaknesses in the theory: that there is no way of showing intensity or durability of links, and hence in an unbalanced situation there is no way of telling which link will be the first to go, or change sign, to bring about the desired balance. Also each of the points could, if one were prepared to look inside the 'black box', be inflated to an internal graph or graphs representing the nation's own decision system.

Johan Galtung (1964) has taken matters a little further in his definition of degree of polarisation of a system. Total polarisation is equivalent to 'balance' in the Harary sense and zero polarisation is a completely random situation in which

no blocs can be found that show any significant difference in the distribution of positive and negative links. But it remains true that the use of graph theory to provide models for the international system has so far been rudimentary.

Of course one is not doing graph theory when one draws a diagram consisting of points and lines – even if they are directed or signed lines. Nevertheless, although he has done just this, I include here a reference to Alain Joxe's interesting essay (Joxe, 1966) in an entirely different field – the analysis of the relations between competing or conflicting objectives within a decision-making system. I believe that even if it is not itself graph theory, this work might lead to something that will be. It is perhaps to be regarded as a sketch of a possible 'internal graph' of the kind hinted at by Harary.

Joxe takes the political objectives of West Germany and breaks them down into the following list:

(*a*) Security in the status quo.
(*b*) Indirect access to nuclear weapons.
(*c*) Absorption of the D.D.R.
(*d*) Obedience to the U.S. and NATO.
(*e*) European integration.

He then examines them in pairs to see which can be joined by directed lines. The directed line (a)→ (d) means (a) could be a stepping-stone to (d), thereby implying that (a) and (d) are compatible and also that (d) will be more easily attainable once (a) is attained. This is referred to for short as a relation *oui* between (a) and (d). The relation *non* has two distinct meanings: (a) is incompatible with *or* irrelevant to (d). There is also a third relation *oui, si* . . . where a certain amount of bending of normal meanings or interpretations is required before the *oui* relation can be said to hold. In addition to these logical links there are emotional links such as that between (d) and (e) which provide a secondary network of connections to complicate the internal policy debate. The resulting graph (not including the emotional links) is shown at Fig. 4.12.

Joxe's proposal is that by taking thought and looking at the diagram – which he regards as a visual aid, no more–one can find the most promising pattern to choose as one's

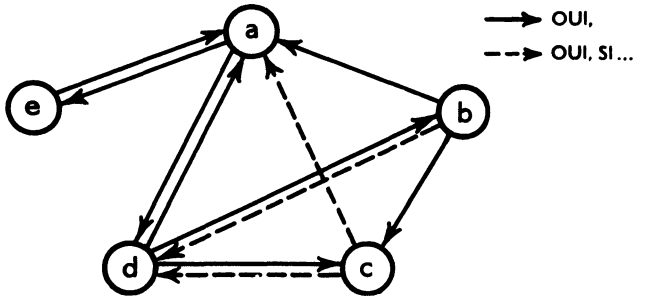


Fig. 4.12. Political objectives of West Germany

political framework, and that international politics will then consist in a process of mutual reconciliation between the national patterns. Though some system of weights or payoffs not yet devised would seem to be necessary to express the cost of abandoning an objective or putting it into cold storage, there might be scope here for a development of graph theory to investigate reconciliation procedures and solutions that would cause the minimum disturbance to the international system.

Statistical Stratagems

This section describes a method or methods of analysis rather than a kind of model. The reader will recall the brief account of factor analysis in Chapter 2 (p. 20). It seems that in many fields – psychology, sociology, political science – theorists have found reasons for being dissatisfied with factor analysis which have driven them to seek alternative means of handling data, especially where a number of interdependent variables are concerned. Without going into the statistical theory I shall simply state the two features that seem to be in issue here:

(i) Factor analysis assumes that all the variables are linear functions of the factors: this is a stringent condition and might sometimes be unfounded. For example, one variable may be proportional to the square of another, or to the product of two other variables. Or to take a

more complicated example, in a chemical context the amounts of two substances A and B that are present may determine the amount of a compound AB that is produced, provided that a third substance C (a catalyst) is also present in some sufficient quantity. But if some smaller amount of C is present, or none at all, then the quantity of AB produced is zero. It is not possible to find a linear function of the quantities of the three substances present that will express the quantity of AB produced.

(ii) There is an element of arbitrariness in the choice of factors, especially in the later stages of the factorisation, and in the subsequent 'rotation' of factors to reduce the factorisation to the simplest form; though this does not often give rise to serious difficulties, one cannot say that factor analysis gives a unique factorisation of any given heap of data.

With adequate data the skilled statistician would have no difficulty in spotting the true relationship in cases like those quoted in (i) or in a variety of other sorts of departure from linearity, using one or another of several standard devices. But as the number of variables increases, the practical problem of seeing which trick to play may become formidable.

Typal analysis has come into use as an umbrella phrase to cover a number of methods that get round the difficulties inherent in factor analysis by handling the data in a different, though still systematic, way. In its most elementary form it consists of a step-by-step grouping of the variables, at first in pairs and then in larger groups. Suppose for a number of 'cases' 1, 2, 3, 4, . . . (they could be nations, or subjects of a social survey) you have readings of a number of variables A, B, C, D. . . . The procedure is to work out the correlation coefficients for each variable on each other variable, giving r_{AB}, r_{BA}, r_{AC}, r_{CA}, etc. Find the largest of all the rs and link together the two variables in question: if for example r_{AC} is the largest, then A and C are linked. Look then for the next largest r, link *those* two variables together, and proceed in the same way up to the point where the variables are sorted into two connected chains. A typical analysis of

Rummel's indicators of internal and external violence (Smoker, 1968*b*, p. 55) shows them neatly divided into two chains corresponding almost exactly to the two types.

There is no assumption of mutual dependence here; all that is assumed is that the variables have affinities with one another and are in a general sense 'typable'. It seems that in this form typal analysis normally gives the same results as factor analysis and can be regarded more as a short cut than as a fundamentally different method.

A more sophisticated form of sequential procedure, programmed for computer use and having something in common with the above although its authors do not call it 'typal', is described in a recent paper (Shure et al., 1968). Here it is the individuals that are 'typed', not the variables. In the example quoted, one dependent variable is picked out – in this case participation (Yes or No) in the 1965 riots in the Watts area of Los Angeles – and there are four independent variables: age, sex, religious observance (more or less than once a week) and education (number of years). The computer programme, known as Inductive Data Exploration and Analysis (IDEA), searches every partition of all the independent variables to find which gives the best resolution of the dependent variable, and repeats the process successively with each branch, and then with each sub-branch, and so on until the investigator tells it to stop. In the example the first partition chosen is by religious observance, which throws up one strongly differentiated type straight away. It shows that nearly all (24 out of 25) of those who went to church more than once a week participated in the riots. This is good enough and no further analysis is needed. The remainder (i.e. the 139 who went to church less than once a week, of whom 52 participated and 87 did not) are carried over to the next stage, where it is found that the best partition is according to sex. And so on until the total number has been broken down into seven types of which all but one show a strong preponderance either of participants or of non-participants. To use statisticians' language, the same result could be obtained by inspection of a four-dimensional contingency table, although of course the grouping into seven types is more enlightening than the four-way statistical table with its large number of cells; and with more variables the 'inspection'

would become increasingly difficult. Moreover a feature of IDEA is that at each node the investigator is presented not only with the partition recommended by the computer but also with a digest of the analysis so far, on the basis of which he may intervene. For example, he may decide to override the computer's recommendation in favour of some different partition that he prefers, or to move to another branch to see what happens there, or to back-track to an earlier node: in this way he is free to follow up any inductive ideas or hunches that he may have about the underlying types. The tree-like structure that results is shown (for the Watts riots example) in Fig 4.13.

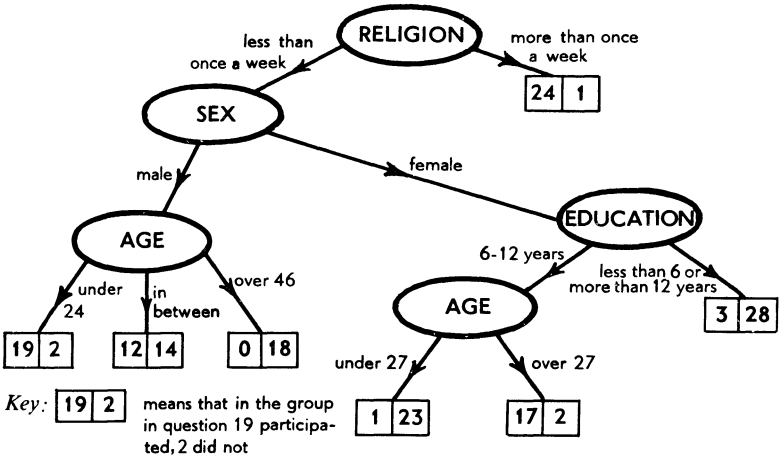


Fig. 4.13. Sequential analysis of participation in the Watts riots

This is an illustration of a general principle put forward by Meeker and Shure (1968) that brutal factor analysis assaults on data should give way to a more subtle conversation with the computer in which the relationships concealed in the data may be teased out as economically and effectively as possible. Returning to the Watts riots example, the basic assumption behind the method is that people fall into types in such a way that:

(*a*) suitable combinations of information about a person's score on the independent variables determine his type,

and

(*b*) determination of type goes a long way towards determining his score on the dependent variable (i.e. whether he took part in the riots or not).

A point to notice about the sequential method is that the same variable may occur in more than one place on the tree and may have a different – even an opposite – significance in the different occurrences depending on the values of the other variables that accompany it. This is true of the age variable in Fig. 4.13: on the left, in the context of men, it indicates that the young participated much more than the old; on the right, in the context of women-with-average-education, it indicates the opposite. Factor analysis makes no allowance for this phenomenon, which is known to statisticians as interaction between variables. It is distinct from correlation and deserves a little attention on its own account. It has been much studied by psychologists in recent years in the course of their efforts to find good indicators of the presence or absence of some suspected condition (such as schizophrenia) from a number of observations of a population. Instead of schizophrenia, let us take an imaginary condition call symbolophobia. The characteristic behaviour of symbolophobes, or symbos for short, is to acknowledge their religious beliefs while denying or rejecting the symbol for those beliefs. The subjects are chosen from a population half Christian and half Moslem, and so all the Christian symbos would admit to being Christian and deny the Cross while the equal number of Moslem symbos would admit to being Moslem and deny the Crescent. The results of the tests considered separately are shown in Fig. 4.14, as applied to a total population of 100.

	Normal	*Symbo*
Qu. Are you Christian A_0	25	25
Moslem A_1	25	25
Qu. Do you venerate the Cross B_0	25	25
the Crescent B_1	25	25

Fig. 4.14. Symbos concealed

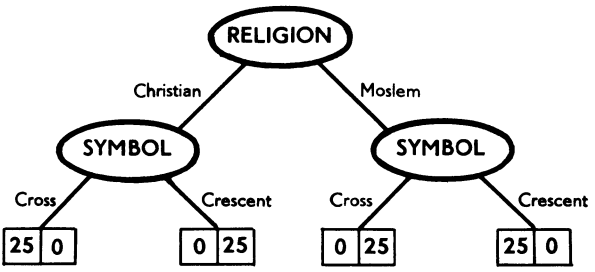
Clearly no statistical investigation of these results as they stand will yield a useful indicator of symbolophobia.

As soon as the answers to the two questions are considered together the symbolophobes stand out 100 per cent, as is clear in Fig. 4.15.

Combination of answers	*Normal*	*Symbo*	*t*
$A_0 B_0$	25	0	1
$A_0 B_1$	0	25	0
$A_1 B_0$	0	25	0
$A_1 B_1$	25	0	1

Fig. 4.15. Symbos revealed

Incidentally, the sequential method (IDEA) referred to earlier and illustrated in Fig. 4.13 would also have done the trick, once the computer had been induced to decide which of the two totally uniformative single variables to take first (see Fig. 4.16). The four types that emerge are of course precisely those revealed in Fig. 4.15. A neat way to represent the interaction is to record for each subject, not just his codes scores for question A (x=0 for answer A_0, x=1 for answer A_1) and question B (y=0 for answer B_0, y=1 for answer B_1) but the function $t=1-x-y+2xy$, which can be seen to take the values shown in the right-hand column of Fig. 4.15, thereby picking out the symbos (score 0) from the normal subjects (score 1). And in general any interaction between two variables — where the significance of one



Key: 25 | 0 means that of the group in question, 25 are normal and none are symbolophobes

Fig. 4.16. Sequential analysis of symbos

variable depends on the values of the others – can be revealed by using a general measure $T = a + bx + cy + dxy$. This result can be further generalised to any number of variables. Analysis on the above lines in the psychological context will be found in Meehl (1950), Horst (1954) and McQuitty (1956, 1968), and there is a voluminous recent literature in psychometrics on the development of techniques for the analysis of personality.

I am indebted for most of the above references to a monumental study 'The Conditions of Conflict' (Barringer and Ramers, 1967) in which an elaboration of this kind of analysis ('configural' or 'pattern' analysis) is applied to eighteen recent international disputes. Barringer identifies a number of critical points for each dispute – points at which it passes visibly from one stage to the next – and for each critical point has tabulated no less than 300 observable variables characterising the dispute itself, the two nations principally involved, the economic and political background, the military situation and the actions and attitudes of third parties. He then seeks among these variables for configurations common to two or more disputes in order to establish species or 'types' of critical point.

Validation is as so often a problem. When there are few variables and many individuals the results of this kind of analysis speak for themselves: one is intuitively satisfied by the results conveyed in Figs 4.13 and 4.15, though I do not know whether it would be possible to attach statistical significance to them. On the other hand when the variables are numerous and the cases are relatively few, as in 'The Conditions of Conflict', the procedure is bound to throw up 'types' and it is necessary to consider with the greatest care whether they are significant ones or mere bundles of coincidences.

I hope that these examples have been sufficient to show how researchers in international relations theory, along with those in the other human sciences, are finding it necessary nowadays to experiment with do-it-yourself data analysis. Alker makes a brief reference in a recent article to mixed additive/multiplicative and fully multiplicative models in the international relations context but without giving any recipes for their use (Alker, 1966): only the sketchiest account is to

be found in the standard statistical textbooks (see, for example, Coleman, 1964, chap. 6). It is not only a familiarity with standard statistical methods that is called for, but an ability to improvise new ones to fit new problems. It may well be that the theoretical underpinning of statistics is in need of review and that some new mathematics is required to draw the various kinds of data analysis together into a coherent system, so that the social scientist can be confident that he is using the best tools available.

Correlates of War

Finally, I refer to a large research project (already mentioned in Chapter 2) under David Singer and Melvin Small which cuts across many of the approaches that have been outlined above. The aim of the project is, by a study of all wars (suitably defined) between 1815 and 1945, to discover those measurable factors that are best correlated with the outbreak of war; that account for why they broke out when they did and why other conflicts did not become wars. Singer has listed nearly a hundred wars in the period, half of them wars between fully-fledged nations and half colonial wars, and so there should be a good quantitative basis for correlations – and it will be even better if he is able *à la* Barringer to distinguish significant phases of crisis for each war and correlate for each phase separately.

Singer's first preoccupation in this herculean task is to identify and record the more obvious 'ecological' variables, i.e. those that describe the relations of the nations with their environment. Studies have already been published on alliances (Singer and Small, 1968) and on diplomatic status and representation (Singer and Small, 1966). Trade relations will also be recorded and work is in hand on certain internal variables – among them an index of political stability and a measure of the religious and ethnic make-up of nations.

The general categories under which it is intended to collect information (Singer, 1968) are:

1. *Attributes of the actor* – physical, structural and cultural (the last to include simply the preferences,

predictions and perceptions of the actor on matters relevant to international relations).

2. *Relationships between actors* – for any two actors, the difference between the cultural profiles, and the links between the actors as far as they are perceptible: formal treaties, trade, tourism, etc.
3. *Behaviour and (if any) interaction* – the sequence of moves and strategies adopted by the actor.

Moreover he is prepared to descend below the national level of analysis – i.e. to enter the 'black box'.

There is no doubt that as a data-collection project, spanning as it does nearly all the kinds of data mentioned in the earlier examples discussed earlier in this chapter, this will be of great interest. It will not be possible to assess the prospects of theoretical advances until Singer puts forward some explicit general hypotheses for testing; this may not be until a much later stage. For the moment, he lays claim to no general theory, and to no particular model.

Conclusion

It is worth recording that our survey of the present-day research effort has brought to light precisely one genuine working model. That one – the Richardson process as applied to arms races – for all its significance in the history of model-building is, as we have noted, a vehicle for maxi-theory, and whatever role it may have to play there is almost certainly too sweet and simple to throw much light on the evolution of situations in the short term. Apart from the Richardson process we have unearthed plenty of sketches, plenty of pictures, plenty of fragments – but no models in the strict sense. We continue the pursuit in the next chapter, and at least the first part, dealing with simulations, may bring us some cheer; but there is a price to pay. It is only to be expected that the more comprehensive a model is, the wilder and more unrealistic will be the assumptions on which it is based.

5 Further Pursuit: Simulations and Linked Learning

I. SIMULATIONS

War gaming, political gaming and simulations of international processes have much in common. The range of representation extends from the entirely human at one end to the entirely computerised at the other, with man–computer mixtures of various kinds in between. Following the normal convention, I use the word 'simulation' to mean those representations where some of the processes are simulated according to predetermined formulae (usually built into a computer programme without human intervention. The rest are games. I repeat here the reminder that the games in question have nothing to do with game theory, which is a branch of mathematics, as explained in Chapter 3, and not an experimental technique.

In the entirely human political game, individuals or teams are appointed to represent nations, are given a scenario and are briefed by the control team on the resources and kinds of 'move' open to them. At intervals during the play the control team intervenes, for example to keep the game within the rules, to correct moves that are 'offside', to arbitrate difficult situations or to announce a new situation that has arisen as a result of the moves that have just been made – the consequences being assessed by the control team in the light of their own experience and the rules of the game.

In the mixed man–computer simulation, some of the functions of the control team are taken over by a computer. For example, a computer might well be used to work out the economic consequences for a nation of its recent moves – say a programme of arms spending followed by a war – which will be presented to the team who have been making these decisions at the beginning of the next period of play. This would require of course a computer programme in which

explicit rules are incorporated for translating the range of possible moves into economic terms.

At the extreme end of the range comes the fully computerised simulation in which the entire process, political, military and economic – or as much of it has been chosen for study – is simulated by a computer which has been programmed to accept data of an initial state of affairs and simply takes it from there.

Where there is simulation, there must be theory. In fact, theory must be present precisely to the extent that simulation is present, whether this takes the form of definite rules of play or of a computer programme. On the other hand the only theory contained in a completely human political game is that implicit in the rules. The national teams play according to their own personal theories or instincts, whatever they may be; to the extent that causes and effects are assessed by the control team (within the limits imposed by the rules) the game will reflect *their* personal theories.

It is not surprising therefore that whereas political games are almost exclusively used for training and mind-broadening purposes there is an increasing tendency, especially in the United States, to look to the computerised simulation as an aid to theory-building. There is a clear logical equivalence:

$$\text{Theory} \equiv \text{Model} + \text{Hypotheses} \equiv \text{Simulation}$$

For all that, it is as well to bear in mind at the start the somewhat chilling conclusion of a recent survey of gaming and simulation (Banks et al., 1968) that these methods have so far led to no major insight into international relations.

The Inter-Nation Simulation

The best-known example, which has been in operation since 1957, is the Inter-Nation Simulation (INS) programme organised by Harold Guetzkow at Northwestern University, for which an internal model is used to represent the domestic evolution, both economic and political, of each nation taking part. For a full description, see Guetzkow et al., (1963). I shall not explain it in detail but will enumerate the factors that are

used to determine national behaviour, the values for which are set for each nation before each 'run' of the simulation:

Economic:
Initial grant of basic economic units (which must be allocated between the three possible end-products – consumption, investment and defence).
Generating rates (three different figures) determining how much of each end-product is produced by the allocation to it of one basic economic unit.

Constitutional:
Decision latitude, i.e. how much or how little attention the governor normally has to pay to public opinion.
Conservation period for which a government is assured of power and at the end of which its prospects are reassessed to allow for the possibility of orderly change of government.

Behaviour-political:
Two *security factors* which, given the physical state of one's own and one's opponent's defence preparations, determine the degree of satisfaction of the people with their security.
Revolution threshold, a value below which the general satisfaction of the people may not fall without risk of revolution.

Simulation runs are usually 'content-free', that is to say the nations are not labelled with real names even though the investigator may have programmed them with real nations in mind. What happens is that the players (two per nation were originally used) allocate economic resources within certain restraints and may also increase temporarily their decision latitude, at some cost in popularity, if necessary in an emergency to give them a freer hand. The economic consequences, including the resources available for defence, can then be computed, period by period, and the political evolution of the nation can also be followed through. The central political assumption is that the satisfaction of the people – and therefore their willingness to carry on without either changing the government constitutionally or attempting revolution – is made up of a certain function of the level of consumption enjoyed plus another function of the force

levels. Chance intervenes in a number of ways: through variations in the depreciation rate of basic economic units and in the obsolescence of the forces; through limited chance variations in the decision latitude (perhaps intended to correspond to fluctuations in the charisma of leadership); and through the fact that the formulae provide only probabilities of change of government, whether by orderly process or by revolution, at times when such changes are allowed by the rules of the game.

For example, in its simplest terms, the basic formula for change of government by orderly process is

$$1 - R = (1 - S)(1 - \alpha)$$

where S is the satisfaction of the people, on a scale from 0 to 1. α is the decision latitude of the governor on a scale from 0 to 1. R is the retention-of-office function on a scale from 0 to 1. If the value of R is high (which will happen if S or α is high enough), then the government is unlikely to change. R is worked out at the end of each play-period, and at the end of the constitutional 'conservation period' the average value of R over recent times is calculated. A throw of dice with this average as probability determines whether the governor stays in office or is replaced. Thus a rational governor will play so as to maximise R.

This model is instructive in showing how, in the evolution of policy, decision-makers may be constrained by the long-term political effects of their decisions both on the economy and on the security of the nation. But many of the real-life constraints are conspicuously absent – the cultural amber in which the decision-maker is embedded as a result of his nation's history, its role, its symbols and its myths (though something of this can be provided by apt choice of players); the legal and customary restraints; the immediate pressures of public opinion; not to speak of the personal fear of death and devastation, especially where actions are contemplated that may affect the risk of the use of nuclear weapons. One would expect such constraints to have particular force at a time of acute international crisis when the long-term economic and internal political considerations, so heavily provided for in the simulation, would tend to be forgotten. The substitute for all this is the 'game culture', in other words the pattern of

behaviour and beliefs about the behaviour of others that grows up as a result of the players' experiences in a particular simulation run or series of runs.

Paul Smoker (1968*a*) has elaborated a new version which he calls the International Processes Simulation (IPS) in an attempt to overcome some of the defects that critics have drawn attention to in the INS. Apart from improving the verisimilitude of the internal economic and political models, he has tried to enrich the international scene by giving each nation an International Representative who can meet privately with the other nations' International Representatives, and a Citizen who can consort with the other national Citizens in non-political activity, can contribute to a World Opinion index and may perhaps obtain resources that enable him to be politically subversive in his own country. He also introduces the concept of the international economic organisation with economic interests in several countries. Two systems are postulated and can be run alternatively: the 'nation-state' system in which these international activities are minimal (resembling the old INS), and the 'international' system in which they are potent. Although these changes give rise to a wider variety of results, the central difficulty remains and many theorists including Guetzkow himself believe that the mixed simulation as an aid to theory-building is no more than a stepping-stone to the fully automatic simulation.

TEMPER

A fully computerised model of the international system is attempted in an ambitious project called Technological Economic Military and Political Evaluation Routine (TEMPER) developed in the last few years at the request of the U.S. Joint War Games Agency (see useful descriptions in I.C.A.F., 1966, and Alker and Brunner, 1966). This consists of a set of interlocking computer programmes from which I will extract the features that bear on the perception of aims and on the political moves that take place in crisis before war breaks out. I leave on one side the extensive economic and

war subroutines that are an important part of TEMPER.

The computer is programmed to accept initial data on thirty or more nations in the form of values set for a large number of variables – about a dozen for each nation individually and one 'positive relationship' (alliance or alignment) variable for every pair of nations except those in opposite blocs. Once the values of the variables have been fed in, the simulation proceeds by stages and the computer will print out a situation report whenever it is required to do so. The variables can be grouped as follows:

Structural:
It is necessary to define the group structure of the nations to be simulated. A nation may be in either of two blocs or neutral. Conflict regions are defined, embracing a certain number of nations.

Cultural:
These represent five deep-seated characteristics of the nation – taxability, propensity of G.N.P. to increase, inclination to use military coercion to attain its ends, readiness to spend money on defence, and 'external dynamism' or zeal in the search for allies. They are regarded as capable of variation but only in one highly specific way – a predetermined linear relationship with the external threat; and so each has to be set one value corresponding to zero threat and another corresponding to maximum threat. For intermediate threats the values can then be calculated.

Situational:
These represent features of the immediate situation. Initial values are set by the investigator and the variables may vary as the simulation proceeds. Moreover, the value of a variable at any one time for a nation may be variously perceived by other nations. They include the sensitivity or readiness to react, land hunger, budget allocation pattern, level of military operations, tactical military strength on the spot, and perception of the likelihood of a nuclear exchange. There is also a 'crisis threshold' for each nation against which the index of every problem can be tested: if it is higher than the threshold, the nation is in crisis and its behaviour changes.

At the risk of greatly oversimplifying, the main processes affecting nation i can be portrayed as in Fig. 5.1.

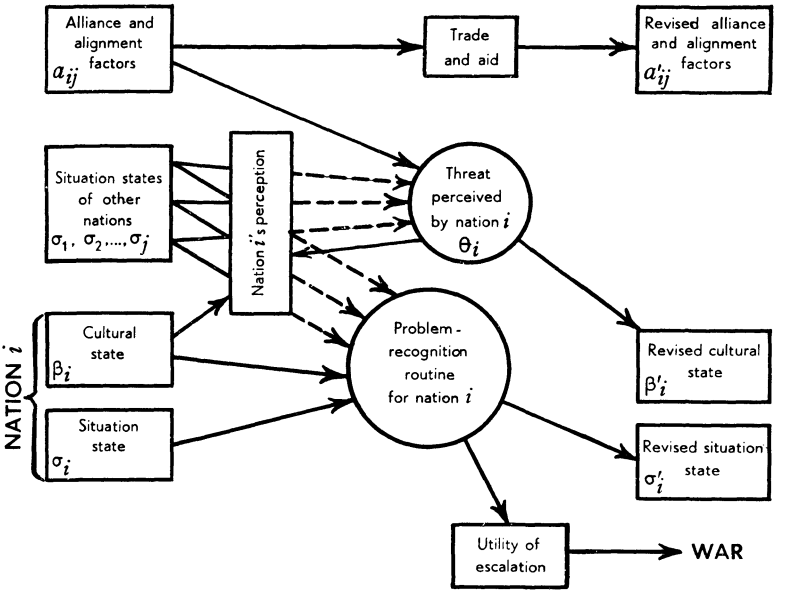


Fig. 5.1. Some features of the TEMPER model

The diagram shows the importance of the two central operators, the threat function and the problem recognition routine. The *perception of threat* is a complicated function of the massed variables of the other nations as perceived by nation i – the perception itself being largely a behavioural thing but also affected by the threat, as perceived at an earlier stage. The *problem recognition* routine is the heart of the simulation and illustrates the potential power of the computer in this field: it is an operation that could not possibly be done by any other means. The computer scans, for each nation in turn, every one of a series of potential problem areas and gives it an index of importance. Each nation asks itself, as it were, how pressing is the need to do something about each problem area. The questions are:

Am I keeping up with others in various aspects of military expenditure?

Am I keeping the total defence spending within the desired limit?

Am I under pressure to retaliate in a conflict area?

Do I fear retaliation?

Is there a tactical imbalance?

Do I suffer from land hunger?

The nation also considers how its behaviour will appear to the others and in this way possibilities of bargaining may emerge – e.g. nation *i*'s problem No. 1 (for which nation *j* is responsible) being traded with nation *j*'s problem No. 1 (for which nation *i* is responsible). When bargains are exhausted, the nation may have indicated to it a range of further steps: seeking new trade and aid relationships in exchange for political support (a possibility that I have not been able to show in Fig 5.1), taking or threatening to take military action, or altering its goals, for example by reallocation of resources.

One other feature I should mention shows how the model represents the deterrent threat of nuclear exchange. For each bloc a 'nuclear exchange factor' is calculated as a function of technological, weapons and population data; it is characterised as the 'perceived population advantage in nuclear exchange'. This factor contributes to the threat function, but only to a small extent except when the level of military operations is high. The choice of a military option is determined by the 'utility of escalation', a game-theory type of payoff function which assesses and takes account of the probability of counter-escalation both nuclear and conventional and injects a probabilistic term.

TEMPER makes no attempt to model directly the internal political structure that is such an important feature of the INS. This structure has all been shrunk down to the cultural variables marked β in Fig. 5.1, and no internal political events are allowed beyond the strictly limited variations in the βs as permitted by their dependence on the threat.

TEMPER is, as yet, an elaborate toy with no political verisimilitude. The problem recognition routine with its deliberative scanning of all problems in sight and its extraordinary caution and anxiety to strike bargains if possible; the absence of anything to correspond to the external

constraints of moral commitment to policies, or to the internal constraints of domestic political pressure inside or outside the decision-making circle; these render it *a priori* implausible and unlikely to illuminate. But is is a model, a genuine working model that satisfies our full specification, and we have to salute it as a remarkable constructive achievement, recognising that it is the first real model we have met since we said farewell to Richardson's model of arms races in the last chapter (pp. 75-8). Being a chip off the scientific block we ought therefore to be able to establish, if we do not like it, exactly what it is that we do not like, to suggest improvements and also to devise ways of refuting one version or the other. But this last point needs a little further consideration.

Validation

Either to refute or to validate a simulation would be a difficult undertaking and I do not think there is a clear body of opinion on how to set about it (Hermann, 1967; Smoker, 1968, chap. 11; Alker and Brunner, 1966). Evidently if again and again a likely-looking situation is put in and an incredible sequence of adventures ensues, we can give the simulation a low mark for plausibility. But in any particular case there would be so many possible explanations to account for the simulation having gone off course: to take the structure of TEMPER as an example, it might be that the cultural variables had been wrongly chosen and did not correspond to those found in real life: it might be that the simulation was valid over a certain range of situations and that the case in point was outside the range. How do we test for all the possibilities? There can be nothing for it in the end but a large number of runs, cunningly devised to ring the changes on the pre-set variables and observe the consequent changes in the course of events: an important result, as Hermann points out, will be to discover which are the sensitive variables, the ones in which a small change has a big effect.

It will always be easier to compare two simulations, or two runs of a simulation on different values of a parameter, than to come to an absolute opinion about either. It seems likely that simulations will grow like coral reefs, by continual small

accretions of theory as each small piece in turn is compared with the ways it might be and a judgment is made by the investigator. However that may be, if simulations are ever to get beyond the stage of playthings, if they are ever to be admitted to the prestigious precincts of the decision-maker, they will need to have passed a severe entrance test. As simulations wax and multiply – and they will – the theory of their validation will have to be greatly developed.

II. LINKED-LEARNING MODELS

At the opposite extreme from machine simulations with their ruthless elimination of the human element from international relations, we find many workers toiling in the vineyard of small-group psychology. There must be few American undergraduates in the sociology, anthropology or psychology departments of the many universities working in this field who have not turned an honest penny by taking part in a mass programme of small-group tests of one sort or another. These are now being conducted on a very large scale, and some with an emphasis on conflict situations are explicitly regarded by their authors as having a bearing on international relations theory. Of the two main theories I describe below, one is so regarded, the other not. Both seek through experiment to gain insight into the learning process which, as we have seen from the communication/cognition model in the last chapter, must be an important part of what goes on in the decision-making 'black box'. They are learning models of a particular kind, in which there are two linked learners each of whom learns from the performance of the other or from the joint performance of both – i.e. the learners are not exclusively self-regarding. I make no attempt to survey the voluminous literature on learning theory generally.

Perhaps the reader should be warned that this section of the chapter contains, inevitably, more mathematical symbols and close argument than other parts of the book.

Markov Learning Models

Stochastic models of learning based on ideas first put forward by the psychologist William Estes have been developed over the past ten years. 'Stochastic' is simply another word for probabilistic, that is to say, containing a chance element. Learning includes learning to interact with other people, and a number of models are studied from this point of view by Suppes and Atkinson (1960) in their formidable book on *Markov Learning Models for Multiperson Interactions*.

The starting point is a conjecture as to the mechanism between the stimulus a learning subject receives and the response he makes. He is supposed to have a number of stimulus-elements $s_1, s_2, \ldots$, one of which will be sampled at random on a given trial. Each element may be in either of two conditioning states and we may crudely regard them for the purpose of this account as being equivalent to light bulbs which may be 'switched on' or 'switched off'. Sampling an element means giving it a stimulus to which it will respond either in the 'right' or in the 'wrong' way, depending on whether it is switched on or off. The experimenter then 'reinforces' the trial by a reward or punishment of some kind that can be recognised by the subject and which may affect the state of the element – i.e. cause it to be switched on or off ready for the next trial.

In a deterministic model of the simplest kind, the reinforcement is always in the direction favourable to learning: that is, a right response is rewarded and a wrong response is punished. Also the subject always gets the message, that is, if rewarded he leaves things as they are and the successful element remains switched on; if punished he identifies the offending element, switches it on, and so ensures that it will not offend again. The learning consists simply of switching on one element after another as they are successively activated in the random sampling; the probability of a correct response increases steadily and after a finite number of steps all the elements will be switched on and the learning process will be complete.

In a stochastic model this happy progress is interfered with in two ways in an attempt to approach more closely the imperfections both of the outside world and of the learning

subject. The reinforcement is assumed to be favourable to learning only in a proportion π of the trials: we may assume $\frac{1}{2}<\pi<1$. In the other cases a right response will be punished or a wrong response rewarded. Secondly it is assumed that punishment is heeded only with a probability θ, where θ may take any value between 0 and 1. So there are two variables, one describing the performance of the environment (π) and the other describing the performance of the player (θ). The learning process is now a doubly chancy affair and one could expect many hesitations and backslidings on the way; indeed the process will never end, since a certain proportion of the information supplied will always be misleading and, when heeded, this will cause switched-on elements to be switched off.

The basic model used by Suppes and Atkinson is as described: one point to notice is that according to it the subject never makes the mistake of switching an element off when it has been rewarded. A more complicated model (necessarily introducing a third variable) allows for this too: the authors call it the generalised conditioning model. They mention – but do not develop – a further refinement in which the conditioning parameters vary in the course of time depending on the history of reinforcement that they encounter. They suggest that this might be used to good effect to compare the learning of something unfamiliar or unwelcome (e.g. going against the trend of reinforcement in earlier trials) with learning where reinforcement is 'traditional' (in line with the earlier trend). It would obviously be useful to have a means of testing this possibly important historical phenomenon in a still quite simple model.

An important feature of the basic model (which is discussed at length for the cases with one and two stimulus elements) is that the probability of the right response can be shown to increase steadily with the number n of trials and tend to π as n tends to infinity. This gives an easily observable measure for π when the model is being tested. The rate of learning, i.e. the rate at which the probability approaches the limiting value, is a function both of π and θ; obviously the nearer θ is to 1 the more efficient the subject is at learning. These results are obtained by making use of the fact that the sequence of trials is a Markov chain – a well-known

type of relation in probability theory whose mathematical properties have been extensively explored.

So far we have a one-man model only, and it is necessary to go a step further before we can discuss 'multiperson interactions'. Suppes and Atkinson's achievement is to develop from this learning model a behaviour system for two subjects, each possessing his own learning coefficient θ and, as it were, providing a 'π' for the other subject by acting as his environment. They confront one another in a series of win/lose situations – a sort of elementary game – where each player has a choice of two strategies (two buttons to press on a black box) and the result is determined by a chance mechanism depending on the combination of strategies chosen. For example the mechanism might be set so that if both players press button No. 1 it is evens whether A or B wins (probability ½), and if A presses button No. 1 and B presses button No. 2 it is a win for A every time (probability 1). The actual determination of who wins each time is made by a random device giving the required probability. The whole mechanism can conveniently be shown in a 2 x 2 matrix such as Fig. 5.2, the entries in the matrix being the probabilities of a win for Player A.

		Player B	
		B_1	B_2
Player A	A_1	½	1
	A_2	½	¼

Fig. 5.2. Mechanism for testing a learning model

Starting from any given combination for the first play, it is then possible to follow each player's learning process and to compute the probability of different possible second plays, and so on. For any given matrix, various statistical predictions can be made about the pattern of successive plays, including limiting values for the proportions in which the two strategies should eventually be chosen by each player, in terms of the unknown learning coefficients θ_A and θ_B.

It is worth pointing out how utterly different this

approach is from that of game theory, despite the game-like device that is used to provide the stochastic link between the players. Here there is nothing to correspond to 'putting yourself in the other person's shoes'; the players are building on their common experience but neither is supposed to be taking account of the fact that the other is doing so. This is acknowledged in the design of the experiments. In the first series described, the pair are told that there is no connection between them, and that they are merely being tested together for convenience; nor are they shown the matrix or given any of the inner structure of the black box. Later series allow for a greater and greater degree of interaction between the players and finally they are shown the matrix so that the trial is an acknowledged zero-sum game. The authors suggests, however, that even in the 'blind' games the players may learn enough about the prospects of winning and losing to approximate to the strategy that they would have employed in the equivalent 'eyes-open' game.

The testing of the models through a long series of trials with different matrices and varying degrees of openness occupies most of Suppes and Atkinson's book and I cannot summarise it here. Both the multi-element Markov model and the generalised conditioning model give a good account of themselves, though the authors admit that the difficulties of computation with these more complicated models are daunting. The theory is still at a very early stage.

Incidentally, the results are also tested against a game-theory (pure rationality) model and show conclusively that this is *not* how people behave. To quote: 'There is little if any evidence that game theory is an effective descriptive theory of actual behaviour for any of the experiments we performed' (p. 282). A similar conclusion has been reached by many other experimenters (Simon, 1957, chap. 16; Messick, 1967; Bush and Mosteller, 1955). This is shown to be true not only of games but also of even simpler situations where decision theory dictates a rational strategy, for example binary choice experiments with probabilistic payoffs (or the 'two-armed bandit'). If 'red' wins two-thirds of the time and 'blue' one-third of the time the subject, instead of plugging red every time which will maximise his gains, as a rule does something rather less successful such as playing red

two-thirds of the time, i.e. 'matching' in a certain sense the performance of the machine.

But let us pause and consider whether this is necessarily a failure of game theory. Might one not as well say that because the average mark in an examination is only 50 per cent, the examiners' model answers do not correspond to human behaviour and are therefore at fault? Game theory may still, on this analogy, deserve retention as an ideal of rationality towards which human behaviour will continue to aspire. One can conjecture that the falling-short on the part of the average subject has two components: (1) inability to draw the full legitimate conclusions from probabilistic information, and (2) a canny eye for the unforeseen, in other words a doubt whether the observed probability really is stationary – and of course if it were to change, the average subject might be in a better position to cope than someone who thought it was a (stationary) game. The first component is perhaps open to improvement as a result of a cultural trend. There is nothing inherently difficult about the understanding of probability in contexts in which one has a personal interest, as is evident from midday conversation in any betting shop; and if it were to become a staple of life to play two-armed bandits or to draw balls from bags, there is good reason to suppose that familiarity would bring improved performance. Component (2) is, however, less accessible and probably goes deep into the springs of human adaptability; one suspects, thinking for a moment of international relations, that a statesman who did not have this quality would not be very successful.

A comment made in 1954 still holds good: 'At present, mathematical game theory and statistical decision theory provide no generally accepted prescription of optimal behaviour in the non-stationary case: indeed it is quite conceivable that organic behaviour will be found to represent such a solution if and when it is understood' (Flood, 1954). Life is a 'non-stationary case', whether we are thinking of biological or of cultural evolution, and there is bound to be something defective in a theory that expects us to act as if the environment will continue to perform in the way it always has. If game theory has this defect, the stochastic learning models appear to avoid it and this must be held to their credit.

Some experiments by David Messick on zero-sum games (Messick, 1967) not only reinforce the belief that game theory itself is not a good descriptive theory of how people play, but also throw doubt on the Suppes and Atkinson models. Subjects were made to play against a computer which played a variety of strategies – sometimes minimax, sometimes minimised loss based on play (i.e. taking advantage of the subject's observed tendencies), sometimes minimised loss based on recent play (i.e. the same but with a limited memory). Messick's central finding is that the strategy used by a player depends critically on the strategy used by the machine: strategies are essentially counter-strategies. He believes this points to a learning mechanism different from the stimulus-sampling of Suppes and Atkinson. Instead of imagining that the opponent's behaviour overturns the subject's previous pattern of behaviour (switching off or on), it is more promising to think of the subject as governing his play by a set of rules, weakly ordered, which he keeps under constant review. Each new play gives him the opportunity to generate new rules; he scans the available rules and chooses a play for the next trial; the result of *that* play is used to reassess the rules, promoting some, de-activating some; and so on. This is consistent with the concept of learning as a special kind of feedback, more advanced than simple steering, which is current in the literature of communication theory (see pp. 67-8, 'The Communication Model'). Messick asserts that given a set of *a priori* rules, the reassessing of rules and choice of play according to this scheme could be formalised and put into a computer programme.

There is a gleam of encouragement in this account of learning, which seems to be less remote from human behaviour than the austere stochastic models of Suppes and Atkinson. It offers at least a distant prospect of being connected up with some of the approaches to international relations that we have been considering: for example, the game-like and communication models reviewed in Chapter 4. The task of making the connection awaits the attention of some intrepid researcher.

Prisoners' Dilemma, or the Price of Prunes

Anatol Rapoport has given much attention to the non-zero-sum game or class of games known as the Prisoners' Dilemma (Rapoport and Chammah, 1965), which he regards as the key to the understanding of human conflict, the paradigm of human relations and the touchstone of theories. The essence of the game is as follows. Each player has the choice of two strategies, C and D. If both players play C, which stands for Co-operation, they each receive a modest reward: either player can do better for himself and dish the other if he plays D for Defection; but if they both play D they both receive punishment, though less than what one suffers when the other defects. A typical payoff matrix would be:

		2nd player C	2nd player D
1st player	C	1, 1	−2, 2
	D	2, −2	−1, −1

Fig. 5.3. The Prisoners' Dilemma
(1st figure in each box is 1st player's payoff;
2nd figure is 2nd player's payoff.)

The original story that gives rise to the name Prisoners' Dilemma has grown somewhat musty in the telling. I prefer to think of it as the Price of Prunes Problem, with a simple economic scenario: two merchants between them control the market in prunes and either can put the other out of business (−2) to his own great advantage (+2) by cutting his price, unless they both do in which case both suffer but survive (−1).

Each player, if he assumes that the other player has made up his mind what to play, must conclude that D is the better strategy since it gives the higher payoff – whether the other has settled for C or for D. Rapoport and others have concluded that this 'dominant' strategy is the prescriptive advice given by game theory, and that since it gives the unsatisfactory result (−1, −1) compared to the modest satisfaction of (1, 1), game theory is damned. In fact the idea of the dominant strategy applies fully only in zero-sum games,

where it is a particular case of the saddle-point solution: a dominant strategy is bound to contain a saddle-point, which will be the solution of the game. Although the idea of dominance for each player separately can be carried across to non-zero-sum games, the saddle-point cannot and there is no assurance that dominance alone will give a rational solution, as indeed the Prisoners' Dilemma shows. The real trouble with non-zero-sum game theory is not that it gives bad solutions but – as we have noticed already in Chapter 3 – that it is reluctant to give any distinct solution at all. And in any case the paradox of rationality that may seem to arise here has since been solved by Howard's theory of meta-games (Howard, 1966; see also the account in Chapter 4, 'Game-like Models', pp. 62-6).

Rapoport has carried out extensive experimental trials in which players are made to play this game repeatedly, in various versions, and uses a number of models in an attempt to explain the results. His work marks a further step towards a theory embracing both the individual learning process and the interaction process that is superimposed on ordinary learning when each player knows he is in the same boat with another player who faces the same dilemma. The most striking feature of the results – though by no means a surprising one – is the strong correlation between the play of the two players, and indeed a strong tendency if the play goes on long enough for them to 'lock on' to CC (both co-operating) or DD (both defecting). The tendency to co-operate is greater when the matrix is shown to the players, so that they see the scheme of reward and punishment clearly from the start, than when they have to deduce it from the results of the trials.

Rapoport constructs two alternative series of models:

(*a*) what he calls first order models, where the player's decision what to play is deemed to bear a probability relationship to what he played on the previous trial, and a separate probability relationship to what the other player played;

(*b*) second-order models, where the player's decision is related to the *joint* play of the two players on the previous trial. The joint play on the previous trial is

known as the 'state' of the game and the variables in this case are called 'state-conditioned propensities'. These second-order models avoid the overlapping of variables that seems to be inherent in the first-order models and can be shown also to avoid a tendency that those models have to throw up bogus correlations between the propensities of the two players.

It is this second-order series that I shall discuss here. In them four propensities are attributed to each player:

The probability of playing C when the last play was CC (so that the player won 1)
The probability of playing C when the last play was CD (so that the player lost 2)
The probability of playing D when the last play was DC (so that the player won 2)
The probability of playing D when the last play was DD (so that the player lost 1)

Symbols and nicknames can be given to them as follows:

P(CC–C) = x	'trust'
P(CD–C) = y	'hope'
P(DC–D) = $1-z$	'lust'
P(DD–D) = $1-w$	'disgust'

I am afraid I have been unable to think of a rhyming nickname for y. For mathematical consistency each of the symbols x, y, z, w, is applied to the probability of a given state being followed the play of C. But in the last two cases it is the propensity to play D (whose probabilities are therefore $1-z$, $1-w$) that can most easily be characterised by a nickname.

The model can now be used to make deductions, assuming first that the propensities remain constant for the two players – a *stationary four-state process*. It is not possible to take the mathematics very far in complete generality, and Rapoport's first simplification is the special case of the 'two simpletons' who have the convenient habits of always changing their play when they get a minus score (CD or DD), always sticking

when they get +2 (DC), and sticking with probability x when they get +1 (CC). So of the four propensities only x remains a true variable; $y = z = 1 - w = 0$. Suppes and Atkinson's players are a different kind of simpleton; they stick when reinforced (CC or DC) and change when punished with probability θ (CD or DD), though their scenario differs in that the two rewards are equal and so are the two punishments. So for them $x = 1$, $z = 0$, $y = 1 - \theta$, $w = \theta$. In both cases the number of variables has come down to two (one for each player) or one if they are assumed to be identical simpletons.

Rapoport does also work out the asymptotic values for the four states derived from the more general model, but making the different simplifying assumption that the propensities are the same for the two players, so that there are four variables. I shall not reproduce these results, which are complicated. Generally they are found to give a tolerable account of the long-term results of many trials, but cannot be made to account for the course of play, which approaches the asymptotic value much more slowly than the model predicts. Another interesting discovery is that the behaviour of women in these trials is strikingly different from that of men.

He introduces briefly what he calls a *stochastic learning model* (though all his models in fact answer to this description), based on the same propensities but assuming that they can be adjusted according to the experience of the previous trial – each propensity being corrected, so to speak, every time it is used. This corresponds to the intuitive notions that trust abused, or hope disappointed, will fade; and that the unco-operative propensities will also be modified under the dull blows of punishment. But to express this requires 36 variables and it is not surprising that no attempt has been made to test it.

A *homeostatic model* is also described in which the players' propensities (x, y, z, w) are adjusted, not to their experience of the last trial but to their observation of play over a period and especially of the value of the average payoff. More exactly it is not the average payoff actually observed but that to which the game seems to be tending – the asymptote of the stationary process which the game is at first thought of as being – that the player is expected to

assess before deciding how to adjust his propensities. He may then be thought of as cautiously making a small adjustment and seeing whether things get (asymptotically) better or worse – which again he will not be able to judge until a further period of play has elapsed. There is no attempt (as I understand Rapoport's book) to introduce what one may call a 'telepathy' factor, corresponding to the fact that one player may realise, as he is contemplating an adjustment in his propensities, that the other player is doing the same.

	C	D
C	1, 1	−A, A
D	A, −A	−1, −1

Fig. 5.4. The Prisoners' Dilemma: another version

Even without this, the homeostatic concept of how people play may seem at first sight to attribute a curious intricacy to the mental processes of the players. However, it is quite easy to express mathematically by means of differential equations. In the 'two simpletons' case, but taking a more general payoff matrix (Fig. 5.4), Rapoport has shown that if A, or the temptation to defect, is not too big ($A < 3$, in fact) there will be unstable equilibrium and the game will tend to lock on to CC or DD – which way it goes depending on the initial values of the 'trust' factors x_1 and x_2 (the values of x for the two players). This is reminiscent of the unstable equilibrium in one case of Richardson's arms-race model (see Chapter 4,

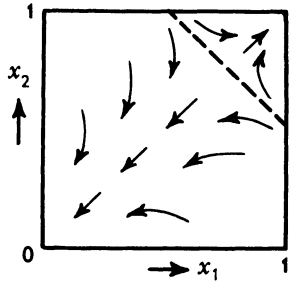


Fig. 5.5. A homeostatic model

p. 76) and indeed the mathematical structure of the two models is the same. The situation can be pictured as shown in Fig 5.5 for a given value of A less that 3. The arrows show, for any particular combination of values of x_1 and x_2, what the resultant effect will be of the adjustments that each player will be disposed to make as a result of his observation of the play. If the game starts towards the upper right-hand corner (i.e. if x_1 and x_2 are sufficiently high), the adjustments in the xs will all tend to improve 'trust' and the game will lock on to CC. Otherwise the adjustments will be downwards and towards DD.

There is a profusion of ideas in Rapoport's book, but as he himself admits many of the proposed models are difficult to test operationally and he does not claim that his experimental programme at Michigan has done more than begin to explore the field.

Conclusion

The samples of learning theory that I have summarised above provide one of the necessary features of a model as required by our original specification – but not yet the other. They do give access to a deductive process enabling one to make probabilistic predictions about the outcome of a sequence of a special kind of decisions. What they do not immediately tell us is how to make these decisional bricks or modules correspond to elements in the real world. Is it possible to break down the real decision-making process into components to which a stimulus-sampling, rule-revising or prisoners'-dilemma stochastic model can be applied? For the moment we must note that the theories suffer from a lack of hooks to hang them on to the referent world; and also that their progress seems to be impeded at present by problems of handling and mathematical manipulation.

These two last chapters will have left the reader wondering 'Where does theory go from here?' This thought will be taken up again in Chapter 7, after an interlude in which we see whether there is anything to be learnt from the analysis of a particular case history – the Cuba missile crisis of 1962.

6 Anatomy of a Crisis

The two crises that have been subjected to the most intensive analysis by theorists are the outbreak of the First World War in the summer of 1914 and the Cuba missile crisis of October 1962. For my anatomy lesson I shall take the Cuba crisis because it was fast-moving, recent and short. The chapter is open to criticism from purists both of science and history: by scientists because I am taking 'events' in the everyday sense without going out of my way to seek 'objective data'; by historians because I have not sifted the published material in any serious way but have uncritically accepted the versions that happened to be at hand (Pachter, 1963; Abel, 1966; Hilsman, 1967; Robert Kennedy, 1969). However, the aim is not to find out something new about the Cuba crisis, but to throw light on the problems of analysing a crisis – just as the medical student dissects the corpse not to find out what is wrong with it but to learn dissection. We are students, not pathologists, in this case.

Using hindsight, I have started from the assumption that there were two principal actors, the United States and the Soviet Union – although this was not at first evident at least to one of them, as the narrative shows. I have divided the significant events as far as possible according to the familiar scheme (Fig. 6.1), although this has required some imagination for many of the 'Soviet decisions' and for all of the

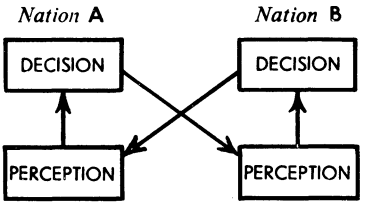


Fig. 6.1. The communication model again

'Soviet perceptions'. On the American side we have a fairly full account under both headings. Fuller definitions are given below, but broadly speaking the 'perception' is the actor's answer to the question 'What is the situation I am in?' and the 'decision' is his answer to the question 'What shall I do about it? (i.e. about those elements of the situation over which I have control)'

What follows is a two-stage reduction of the crisis, first to a manageable sequence of 89 events showing the immediate relations; then to a skeleton of some two dozen critical points.

The First Reduction

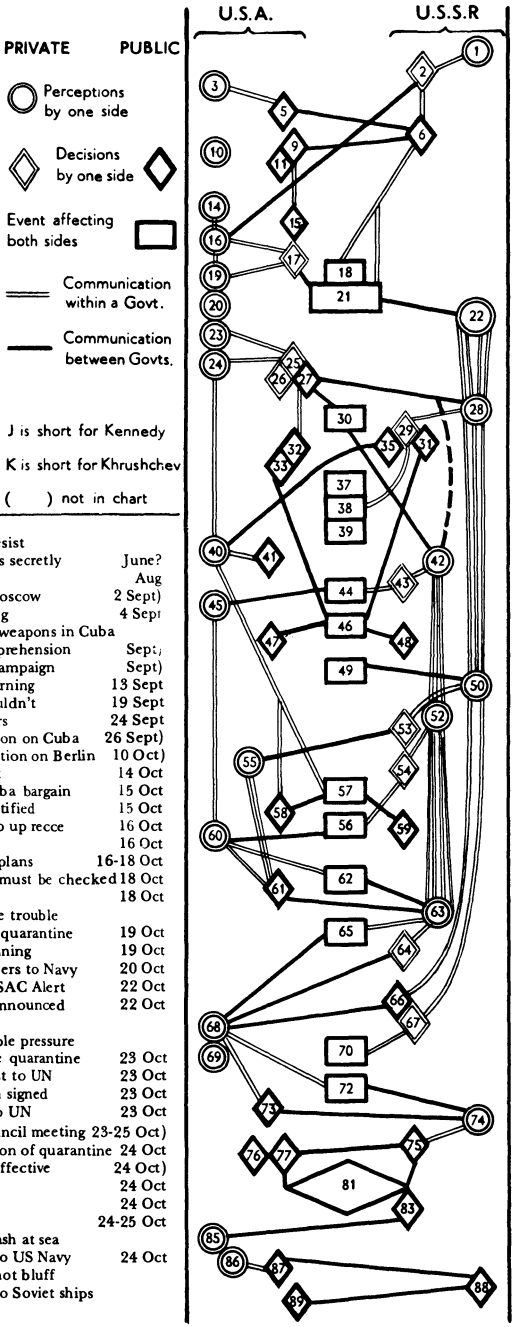
The chart in Fig. 6.2 is the 'first reduction' of the 1962 Cuba missile crisis and portrays the events and presumed relations between events over the period from the (uncertain) date of the Soviet decision to station ballistic missiles in Cuba until 20 November when the United States Government terminated the quarantine. The events are arranged in five columns as follows:

1. *U.S. perceptions* (numbers in circles) – significant developments in the perception of the situation by the U.S. Government, including the facts and their interpretation, likely future developments, likely consequences of U.S. actions, and the practical possibilities of action for the U.S.

2. *U.S. decisions* (numbers in diamonds) – both private decisions (with open border) and public acts or announcements of intention to act (with solid border) by the U.S. Government.

3. *Other events* (numbers in rectangles) – significant events affecting both sides, including developments in the physical situation affecting the crisis (especially at sea), also important meetings and other opportunities of communication such as were provided by the U.N. Secretary-General.

4,5. *Soviet decisions* (diamonds) and *perceptions* (circles) – columns corresponding to those for U.S. decisions and perceptions. The Soviet perceptions are entirely speculative, the Soviet decisions partly so to the extent that items not made public (those with open borders) are included.



1 US will not resist
2 Install missiles secretly June?
3 SA2s seen Aug
(4 Guevara in Moscow 2 Sept)
5 J's 1st warning 4 Sept
6 No offensive weapons in Cuba
(7 US public apprehension Sept)
(8 Republican campaign Sept)
9 J's second warning 13 Sept
10 USIB : K wouldn't 19 Sept
11 Call-up powers 24 Sept
(12 Joint resolution on Cuba 26 Sept)
(13 Senate resolution on Berlin 10 Oct)
14 Key U2 flight 14 Oct
15 No Berlin/Cuba bargain 15 Oct
16 BM sites identified 15 Oct
17 Secrecy : step up recce 16 Oct
18 K/Kohler 16 Oct
19 Options and plans 16-18 Oct
20 K it is & he must be checked 18 Oct
21 J/Gromyko 18 Oct
22 J will not give trouble
23 Recommend quarantine 19 Oct
24 Detailed planning 19 Oct
25 J agrees : orders to Navy 20 Oct
26 Allies told : SAC Alert 22 Oct
27 Quarantine announced 22 Oct
28 It's bluff
29 Apply multiple pressure
30 OAS endorse quarantine 23 Oct
31 Soviet protest to UN 23 Oct
32 Proclamation signed 23 Oct
33 US protest to UN 23 Oct
(34 Security Council meeting 23-25 Oct)
35 Soviet rejection of quarantine 24 Oct
(36 Quarantine effective 24 Oct)
37 K/Knox 24 Oct
38 K/Russell 24 Oct
39 Ivanov/Ward 24-25 Oct
40 Danger of clash at sea
41 New orders to US Navy 24 Oct
42 Perhaps it's not bluff
43 New orders to Soviet ships
44 Some ships change course 24 Oct
45 'Other fellow blinked'
46 U Thant No 1 24 Oct
47 J : No 25 Oct
48 K : Yes 25 Oct
49 Lippmann article 25 Oct
50 Is Lippmann a kite?
(51 *Bucharest* interrogated 25 Oct)
52 Increasing doubt
53 Carry on building at sites
54 Avoid showdown at sea
55 Building seen to continue 25 Oct
56 Some ships turned round 25 Oct
57 U Thant No 2 25 Oct
58 J : Yes, if . . . 25 Oct
59 K : Yes 26 Oct
60 K's resolve weakening
61 'Further action' 26 Oct
62 *Marucla* boarded 26 Oct
63 US invasion imminent
64 K No 1 26 Oct
65 Fomin/Scali No 1 26 Oct
66 K No 2 27 Oct
67 Use SAMs when operational
68 K No 1 it is 27 Oct
69 Tomorrow : grim decisions 27 Oct
70 U2 shot down over Cuba 27 Oct
(71 U2 astray over Siberia 27 Oct)
72 Fomin/Scali No 2 27 Oct
73 Reply to K No 1 + 27 Oct
74 The game's up
75 Will withdraw 28 Oct
76 Welcome withdrawal 28 Oct
77 Quarantine suspended 28 Oct
(78 Castro proposes terms 28 Oct)
(79 U Thant/Havana 30-31 Oct)
(80 Mikoyan/Havana 2-25 Nov)
81 Verification agreement
(82 US mid-term elections 6 Nov)
83 Proceed to withdraw
(84 Castro will shoot down U2s)
85 All BMs gone mid-Nov
86 Ilyushins still there
87 Threat to bomb Ilyushins
88 Ilyushins will leave 20 Nov
89 Quarantine terminated 20 Nov

Fig. 6.2. 89 steps: first reduction of the Cuba missile crisis

The open lines represent communication links within a government and show presumed connections between one perception and another, between one action and another, or between perception and action. The solid lines are links between governments, and normally lead from one government's decision to the other's perception, though sometimes an 'other event' such as a meeting in the middle column is an intermediary. In some of the exchanges in early September and also after the crisis was over there was no perceptual problem and so the successive decisions are linked directly to one another. All these connections are matters of interpretation and open to challenge.

The events are numbered chronologically as far as possible and represented in the key on the right of the chart by a shorthand phrase and a date where one is known. A number of events, important to the narrative but not in themselves the immediate cause of any other event, are listed in the key in brackets and do not appear in the chart. Fuller accounts of all the events are given below, grouped where convenient to make an intelligible narrative. I have given references to sources only for the speculative items and for those where there is some obscurity in the public record.

1,2. It is not known when the Soviet decision to install long-range nuclear missiles in Cuba was taken, though it is conjectured (Hilsman, pp. 159-61) that it can have been no later than June. The decision has often been described since as 'irrational' and the Soviet aims have been the subject of endless speculation; but it seems a fair presumption that behind the decision lay a judgment that the U.S. would not, in the last resort, stand firm and resist the installation of the missiles. It is also likely that whether or not the U.S.S.R. expected to 'get away with it', they did expect that political advantage would accrue to them in some way when the dust had settled: in particular that a Cuban manoeuvre would make it easier for them and more difficult for the U.S. in subsequent confrontation over Berlin (Pachter, pp. 23-7; Abel, pp. 36-41; Hilsman, pp. 164-5).

3,5. Some time in August, U.S. intelligence reports identified Russian S.A.2s (defensive surface-to-air missiles) in Cuba. Kennedy at his Press conference on 4 September

announced this, and the presence of 3500 Russian technicians. He said there was no evidence of any offensive missiles. 'Were it otherwise, the gravest issues would arise.' He warned that 'aggressive moves by the Castro regime would be opposed by the United States by whatever means might be necessary'.

4,6. Che Guevara went to Moscow late in August and on 2 September a Tass communiqué reported agreement on the delivery of armaments for the defence of Cuba. In a riposte to Kennedy on 11 September, Tass said that so far as retaliatory weapons were concerned 'there is no need to search for sites for them beyond the boundaries of the Soviet Union'.

7,8. Public apprehension in the U.S. about the Soviet build-up in Cuba grew throughout September, and as the mid-term election campaigns got into full swing there were unsupported assertions by Republicans about I.R.B.M. sites in Cuba and calls for resolute action, including invasion.

9. Kennedy at his Press conference on 13 September attacked 'loose talk' about unilateral military intervention by the U.S. but repeated his warning of the week before (5). He continued for the next month in campaign speeches to denounce Republicans who called for invasion of Cuba.

10. 19 September. The U.S. Intelligence Board concluded that the Soviet Union would not install long-range missiles in Cuba but called for continued alertness.

11. 24 September. Congress gave authority for the call-up of reserves.

12. 26 September. Joint Resolution of Congress to prevent Cuban aggression 'by whatever means may be necessary'.

13. 10 October. Senate Resolution that Allied rights in Berlin would be defended, if necessary, by the use of arms.

15. The State Department had more than once during the summer found it necessary to reject any proposal that the U.S. would be ready to abandon Berlin if Russia agreed to abandon Cuba; the last occasion was on 15 October, following a week of intense diplomatic activity on the subject of Berlin.

14,16,17. The crucial U2 flight over Cuba – one of a series of regular surveillance flights that had been undertaken in early September – was made on 14 October; the evaluation, showing I.R.B.M. and M.R.B.M. sites (though no missiles)

and Ilyushin bombers was made the following day and Kennedy saw it on the morning of 16 October. The immediate appreciation was that *some* U.S. reaction would be necessary and that while planning proceeded at top speed complete secrecy should be observed (Pachter, pp. 13-16; Hilsman, p. 198). Reconnaissance of Cuba should be greatly intensified. An *ad hoc* Executive Committee was formed to steer the planning.

18. Khrushchev saw U.S. Ambassador Kohler on 16 October and assured him that Soviet purposes in Cuba were defensive. There had been many similar assurances through many channels (Hilsman, p. 166; Abel, p. 50).

19. During the course of 17 October the possible directions for U.S. action that emerged were:

diplomatic action – public (through U.N.) or private (e.g. through Gromyko);
invasion of Cuba;
pinpoint bombing of sites;
blockade;
no action (Abel, pp. 60-2; Hilsman, pp. 195-6).

20. During this and the following day (18 October) the Executive Committee may be regarded as having come up with a crucial perception that can be divided into two parts:

(i) Khrushchev was the adversary, not Castro, and if he was not shifted from his course the consequences for the U.S. would be grave.
(ii) He was unlikely to be deflected by anything less than a physical demonstration by the U.S. involving a public showdown (Pachter, pp. 26-7, 92; Abel, pp. 59-60. There is an element of imaginative reconstruction in this item, which does not emerge too clearly from my sources).

21,22. Gromyko, who had a long-standing appointment with Kennedy, told him on 18 October that the Soviet Union proposed to sign a peace treaty with the East German Government. In reply to a reminder of Kennedy's warnings on Cuba (5 and 9), he said that the military assistance from

the Soviet Union was entirely directed to the defence capabilities of Cuba. Kennedy did not rise to this; what Gromyko's conclusion may have been is conjectural. He may have thought he had successfully pulled the wool over Kennedy's eyes; if on the other hand Kennedy's harping on offensive missiles was interpreted as indicating knowledge, this would also support the Soviet view that the U.S. were not going to make a fuss (Pachter, pp. 20-3; Hilsman, p. 199).

23,24. The serious options for the U.S. had after further discussion been narrowed down to two: pinpoint bombing and blockade (Abel, p. 80). The Executive Committee finally recommended on 19 October a strictly limited blockade, to be called a quarantine, in consideration both of the immediate consequences of bombing which was thought likely to leave Khrushchev with no choice but to retaliate violently – a 'spasm' reaction – and of the moral argument that unprovoked violent attack was not the way the U.S. behaved nor the way they would wish the world to behave in future. Detailed plans for quarantine were to be urgently worked out (Pachter, pp. 27-30, Hilsman, pp. 203-6; Abel, pp. 76-8, 84-6).

25,26. Kennedy agreed to the recommendation at a meeting of the Executive Committee on the morning of 20 October, ratified by the full National Security Council the same afternoon, and secret orders were issued to the Navy (Abel, pp. 25-7). He satisfied himself at a further briefing the following day that even a major air attack could not be certain of destroying all the sites and missiles. Special arrangements were made for telling close allies (U.K., Canada, France and Germany). Preparations for air attack or invasion of Cuba if necessary were completed. The deterrent force was placed at maximum alert.

27. At 7 p.m. on 22 October, Kennedy announced the quarantine on all ships bound for Cuba carrying offensive military equipment and demanded the immediate withdrawal of the weapons already in Cuba, in default of which further action would be justified, and stated that any nuclear missile launched from Cuba against any nation in the Western hemisphere would be regarded as an attack by the Soviet Union on the United States, requiring a full retaliatory response upon the Soviet Union.

28,29. The first series of reactions by the Soviet Government showed every sign of their determination both to carry on with the missile preparation in Cuba and also to make the maximum political capital out of the U.S. action. This could only be based on the judgment that the U.S. were bluffing and would not in the last resort enforce the quarantine. A number of opportunities were found for harassing diplomatic activities.

31,33,34. In particular the U.N. Security Council were summoned to consider a Russian protest at the U.S. quarantine as well as the U.S. resolution calling for withdrawal of the missile bases. The Security Council met for three days (23-25 October) without passing any resolution and did not meet again during the crisis.

30,32,35,36. The Organisation of American States endorsed the U.S. action unanimously at a meeting on 23 October and Kennedy signed the Proclamation the same evening. The quarantine was to be in force from 10 a.m. on 24 October, the day on which some Soviet ships were expected to reach the area defined in the Proclamation. The Soviet Government announced their rejection of the Proclamation.

37,38,39 (see 29 above. Khrushchev conveyed through William Knox, a U.S. businessman, a threat that if the U.S. stopped Soviet ships, Soviet submarines would have to sink a U.S. ship. He also responded enthusiastically to a telegram from Lord Russell suggesting a summit conference. The idea that Britain should make a political initiative in this direction was floated by Captain Ivanov (naval attache at the Soviet Embassy in London) to Dr Stephen Ward and Sir Godfrey Nicholson, M.P., who passed it on to the Foreign Office (Hilsman, p. 214; Abel, pp. 133-40, 154-5).

40,41. It remained a key U.S. objective to avoid confronting the Soviet Union with a choice only between humiliation and violent retaliation. A direct clash at sea would have this result and was to be avoided if possible. An order was sent in clear to the U.S. Navy not to make any interception until absolutely necessary (Pachter, p. 43; Hilsman, pp. 213, 215).

42,43,44,45. The Soviet leaders seem to have had the same feeling that it was important to play for time and avoid a direct clash at sea if possible. New orders must have been

given to those of the Soviet ships that were approaching the quarantine zone and they were observed to have stopped at the edge of the zone or changed course. Whether (as the scheme in Fig 6.2 suggests) this was attributable to the first Soviet inkling that the U.S. position might be firm and not a bluff, or was simply an instinctive prompting to caution, it would be hard to prove; but this was the occasion for the celebrated remark attributed to Secretary Rusk: 'We're eyeball to eyeball and I think the other fellow just blinked' (Hilsman, p. 215).

46,47,48. After inconclusive and stormy discussion in the Security Council, U Thant addressed to Kennedy and Khrushchev on behalf of 'a large number of member governments of the U.N.' a request for temporary suspension of arms shipments to Cuba and of the quarantine. Khrushchev agreed; Kennedy declined, insisting on the withdrawal of weapons.

49,50. An article by Walter Lippmann in the *Washington Post* pointed the parallel between the Soviet bases in Cuba and the U.S. bases in Turkey and suggested that both be dismantled. This familiar notion, put forward at this moment, may have been regarded by the Russians as a trial balloon and strengthened the hand of those in the Kremlin who were still looking for political advantage from the crisis.

51. The Soviet tanker *Bucharest* was interrogated on the high seas at 8 a.m. on 25 October and allowed to proceed to Cuba after declaring that her cargo was petroleum.

52,53,54,55,56. It seems reasonable to impute to the Soviet Government at this stage an increasing doubt about their original assessment of U.S. intentions. I have represented this as a widening wedge of anxiety down the left-hand side of the Soviet 'perceptions' column. At the stage now reached the 'decision' seems to have been the temporising one of continuing at full speed with work on the missile sites (a fact known to the U.S. through reconnaissance, and referred to by Kennedy in his letter of 25 October to U Thant) while avoiding physical confrontation at sea. This was confirmed by the news that twelve (or fourteen) Soviet ships bound for Cuba had turned round on 25 October (Abel, p. 150; Hilsman, p. 216; Kennedy, p. 74).

57,58,59. At U.S. instigation, U Thant sent a second round

of messages dated 25 October appealing to Kennedy and Khrushchev to avoid a direct confrontation at sea and asking Khrushchev in particular to instruct Soviet ships to stay away from the quarantine area. Kennedy replied on 25 October that he agreed if Khrushchev did too, and on 26 October Khrushchev also agreed.

60,61. The U.S. now had reason to believe that there was some weakening of resolve in the Kremlin and that a successful outcome would depend on convincing them that the U.S. were unshakable, and that time was pressing (Pachter, pp. 54-5; Abel, pp. 161-2). This policy was pursued during 26 and 27 October. Low-level flights over Cuba were intensified (Kennedy, pp. 78, 83-4). The White House issued a statement about the continuing and indeed accelerated work on the sites; the State Department reminded the Press of Kennedy's reference (in 27) to 'further action' being justified in these circumstances, though this reference is said to have been made without Kennedy's knowledge and to have aroused his displeasure (Hilsman, pp. 213-14). There have been allusions to messages that were exchanged 'almost daily' between Khrushchev and Kennedy, though these have not been made public in detail (Kennedy, p. 79).

62. The *Marucla* (Lebanese flag, under Russian charter) was boarded on 26 October by U.S. officers, searched and allowed to continue to Havana (Pachter, p. 44).

63. The pressure from the U.S. side must be presumed to have had its effect on the Russian leaders and there is some evidence that they were convinced that a U.S. invasion of Cuba was imminent. The F.B.I. learnt on the night of 26 October that certain Soviet personnel in New York had been ordered to prepare to destroy sensitive documents (Kennedy, p. 91); Khrushchev, in a speech made later (12 December) said that on 27 October he learnt from Cuba that they were to be invaded within two or three days.

64,65. A long emotional message from Khrushchev (K. No. 1) dated 26 October was received that evening. It appeared to propose withdrawal of the missiles in return for a U.S. undertaking not to invade or support any invasion of Cuba. Only excerpts and paraphrases of this letter have been made public (Abel, pp. 166-9). A precise proposal, similar but including a reference to U.N. supervision of the withdrawal, was put by

Alexander Fomin, Counsellor at the Soviet Embassy, to the television reporter John Scali at lunch that day, who after clearing with Rusk (and through him Kennedy) replied that the proposal would interest the U.S. Government (Hilsman, pp. 217-18; Abel, pp. 163-5).

66. It seems that almost if not quite at the same time another more formal message from the Soviet Government (K. No. 2) was being drawn up. Dated 27 October, and published in Moscow before it was received in Washington, it proposed a trade of bases in Cuba for bases in Turkey.

67,70,71. In the morning of 27 October an American U2 reconnaissance plane was shot down over Cuba and its pilot killed. This must be presumed to have been preceded by a specific decision to use the surface-to-air missiles as soon as they were operational. Another U2, said to have been on a routine air-sampling mission, went astray and overflew Siberian territory, causing Soviet fighters to scramble, although it was allowed to return safely to base. Khrushchev later reproached Kennedy for allowing this provocative flight at such a time and Kennedy apologised.

68,69,72,73. The Executive Committee recommended that the proposal in K. No. 1 – enhanced with the precision of the Fomin message – should be responded to and that K. No. 2 should be ignored. The U.S. reply was sent on the evening of 27 October (Hilsman, p. 223; Abel, pp. 82-3; Pachter, pp. 54-5). At the same time it was recognised that such was the progress with the missile sites that if the situation was no clearer by the following day a new decision stage would then have been reached when ugly options would have to be faced: to extend the quarantine to include oil, bomb the sites, or invade (Abel, p. 178). Scali had seen Fomin again that afternoon on instructions and told him vehemently that there was no future in anything but unconditional withdrawal of the missiles (Hilsman, pp. 222-3). Robert Kennedy said the same to the Soviet Ambassador Dobrynin when he gave him a copy of the letter, but added a hint that the Turkish missiles would in fact be gone before long (Kennedy, p. 106).

74,75. Khrushchev in a letter of 28 October accepted Kennedy's proposal. Whatever the climate of opinion may have been in the Kremlin, the decision itself must have been

made on the lines 'They are not going to budge; we shall have to withdraw and make the best of it'.

76,77. The message was immediately welcomed by Kennedy and orders were sent to the quarantine ships to remain on station, taking no forceful action.

78,79,80,81,83,85. Castro had independently announced his terms for a settlement on 28 October. U Thant went to Havana in an unsuccessful attempt to get his agreement to proposals for inspection of the withdrawal. Mikoyan was despatched by the Soviet Government to Havana with no greater success. In the end agreement was reached between the U.S. and Soviet Governments for the cargoes to be inspected at sea. By mid-November the U.S. were satisfied that all the ballistic missiles had been removed (Abel, pp. 193-6).

82. The U.S. mid-term elections took place on 6 November.

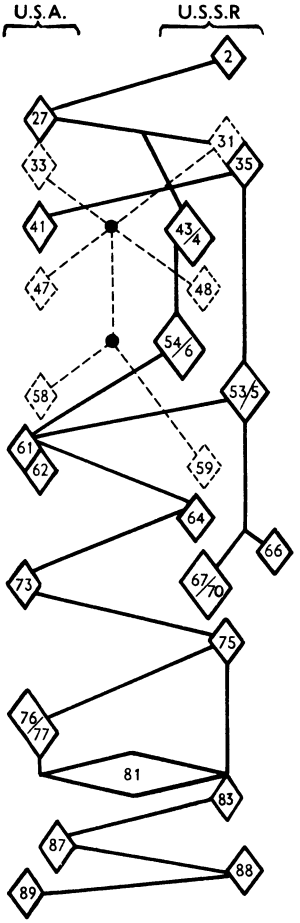
84. During the withdrawal Castro threatened to shoot down a U2 if reconnaissance continued.

86,87,88. The Ilyushin bombers were still there. Khrushchev had said he could not remove them because they were a gift to Castro. Kennedy insisted, threatening to destroy them on the ground if they remained. Finally Castro's agreement was obtained and Khrushchev agreed, on 20 November, to withdraw them too and permit their departure to be observed from the air (Hilsman, p. 225).

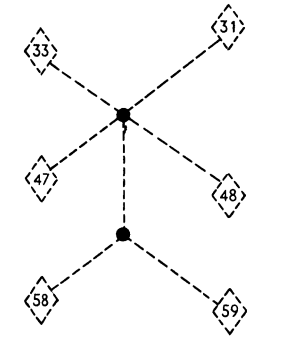
89. The quarantine was terminated the same day, twenty-seven days after it had come into force.

The Second Reduction

In the 'second reduction' shown in Fig. 6.3 we ignore perceptions and secret decisions and show simply those overt acts or decisions on both sides that constitute between them the main action of the crisis. 'Overt' is taken to include acts that began as secrets and became known later – such as the original decision to install the missiles. These main items are chosen subjectively, as were the original 89 events in the first reduction; but I do not want to give the impression that I have been altogether unsystematic – the diagrams have this



2 Install missiles secretly	June ?
27 Quarantine announced	22 Oct
35 Soviet rejection of quarantine	24 Oct
41 New orders to US Navy	24 Oct
43/44 Soviet ships change course	24 Oct
54/56 Some Soviet ships turned round	25 Oct
53/55 Building at sites continues	
61 'Further action'	26 Oct
62 *Marucla* boarded	26 Oct
64 K No 1	26 Oct
66 K No 2	27 Oct
67/70 U2 shot down over Cuba	27 Oct
73 Reply to K No 1 +	27 Oct
75 Will withdraw	28 Oct
76/77 Quarantine suspended	28 Oct
81 Verification agreement	
83 Proceed to withdraw	
87 Threat to bomb Ilyushins	
88 Ilyushins will leave	20 Nov
89 Quarantine terminated	20 Nov



UN MOVES

31 Soviet protest to UN	23 Oct
33 US protest to UN	23 Oct
47 J : No to U Thant No 1	25 Oct
48 K : Yes to U Thant No 1	25 Oct
58 J: Yes, if . . . to U Thant No 2	25 Oct
59 K : Yes to U Thant No 2	26 Oct

Fig. 6.3. 20 steps: second reduction of the Cuba missile crisis

degree of coherence, that every sequence

A's overt act – B's perception – B's overt act

that is linked in the first reduction also appears in the second reduction, although it will appear there in the abbreviated form

A's act – B's act

since the perceptions have been suppressed. The U.N. branch of activity has been picked out as a matter of interest and shown in a separate diagram.

The Dissection (Methodological)

I believe the events fit quite well into the five categories of the first reduction without doing violence to the narrative. These categories in fact make formal what is a natural way of accounting for international phenomena and require, as does any such 'accounting' process, some imaginative reconstruction of the perceptions of each actor, so far as this is not made evident in his acts. It is not necessary for a perception to have been clearly formulated at the time, much less written down – though I would say that the more this is done the better. Hilsman is interesting: 'even though no one analysis [of Soviet motives and purposes] was singled out at the time for formal approval as authoritative, as time went on knowledgeable opinion tended to converge' (Hilsman, p. 201).

Though the second reduction is much simpler to read, it is a laconic document and does not seem to take one very far in attempting to make sense of the sequence of events. However, it does bring out some structural features that are less easy to see in the first reduction form (see the next section 'The Corpse', p. 135).

A causal link in the first reduction is designed to connect an event with its immediate cause; but of course it would be a gross oversimplification to think of it as 'the' cause, or to suppose that the matter for each step in the train of events resided wholly in the previous one. This can best be discussed

for perceptions and decisions separately, since the considerations are not the same.

Every *perception* is a function not only of its immediate cause or causes, but also of the actor's way of looking at things in general: his views on how people behave, his experience in assessing the reliability of intelligence, his knowledge of the personality of his adversary, and the particular symbolic precedents in which his and his adversary's outlooks are embedded. Many of these precedents were referred to explicitly in the course of the crisis. Kennedy mentioned the thirties, the years of appeasement, as an object lesson in his broadcast of 22 October; on the other hand he is reported as having quoted from the outbreak of the First World War the helpless cry of Bethmann-Holweg when asked how it all began – 'Oh, if I only knew!' – as a warning against letting things get out of control. We are told that Khrushchev poured out his own war experiences in his letter of 26 October. And, nearer in time and place, the Bay of Pigs episode (of April 1961) was in both their minds: in Khrushchev's perhaps as a sign that if the Americans were wobbly then they would be wobbly again, in Kennedy's on the contrary as a goad not to let the Western Hemisphere down a second time.

Every *decision* is a function not only of its immediate cause or causes but also of the actor's way of doing things in general: his personality, values, and particular aims, his rules of behaviour and the role that he reckons he is playing. The U.S. Government would in the context of Cuba have to bear in mind their role in the Western Hemisphere harking back to the Monroe Doctrine, and also their standing with NATO – their reputation with their allies and the effect on it of concessions to the Russians over Berlin or in Turkey. The role includes the moral role, the ultimate question 'how to behave' without reference to particular aims that Robert Kennedy is said to have raised to such good effect in the debates over an air strike (Abel, pp. 62-3; Hilsman, p. 203).

None of these factors appears in the diagrams: all of them may be regarded as habits of perception and behaviour that remain fixed over a period and are therefore theoretically predictable on the basis of previous behaviour except to the extent that they may be clouded or affected by the crisis itself,

e.g. by the level of tension under which the perceptions and decisions are made.

Two points stand out that present serious difficulties to the reductionist – by which I mean anyone who is seeking to by-pass detail and find out the essential form of the crisis:

(i) The extent to which the course of events was affected by the *day-to-day success or failure of intelligence* – both in the technical sense of U2 reconnaissance and agents' reports, and also in the more general perceptual sense of distinguishing the significant from the background noise or of recognising the 'right' channel; it seems that a large chance factor must be allowed for on this account. What would have happened . . . if the key U2 flight had been flown three days earlier? three days later? . . . if the Fomin channel of communication had been (wrongly) disregarded by the U.S.? . . . if the Lippmann article had been (rightly) disregarded by the Russians? In each case – except perhaps the last – one can be confident that the course of events would have been different. The question is, how different, and in what way a model of the course of events would have been able to accommodate the different possibilities.

(ii) The importance of the process of *identifying practical options*. The point is that the options cannot be derived in any mechanical way from one or two coefficients of effective power; they have to be hammered out of a most intractable material composed of relative military capabilities, dispositions and timings. If an economic or political move is contemplated, there will be only a certain number of possibilities. Or, to return to a military example, before assessing the likely political and military consequences of an air strike on the missile sites in Cuba, it was necessary to define what kind of air strike was practical: what sites exactly could be attacked, what proportion were likely to be destroyed, how long it would take and whether surprise would be achieved. Kennedy's last-minute briefing on these matters on Sunday, 21 October – though it may have been a political gesture to appease the Joint Chiefs of Staff –

suggests that even at that stage he had not ruled out a truly 'surgical operation', if such a thing had been possible; but it wasn't. Another military example: a quarantine of Cuba could not be considered seriously until it had been shown that there were the ships and that they could be deployed to cover the approaches to that long island – not an operation one expects to do every day. Put another way, there would be little comfort in a calculation indicating that a response of, say, Military Force 6, Political Force 4 (on some well-tempered scales from 0 to 10) was just what the situation required to achieve the greatest chance of success and the least risk of escalation – if no response at just that level was to be found. One has to look and see what shots are in the locker before deciding what to do. Moreover one must look further at the options open to the opponent: a Force 4 move on my part may seem a modest move, but if it leaves my opponent with a choice between Force 0 and Force 8 it could well be disastrous. It stands out from the narrative that this consideration was constantly in the minds of the U.S. leaders.

When planning an elopement it is necessary to have regard not only to the mutual feelings and perceptions of the two actors, but also to humdrum practical details. If the ladder doesn't reach, if signals at the wrong window are responded to, if plans for evading parental reconnaissance are based on faulty intelligence, no amount of affection will save the operation from failure. If it is true that love will find a way, then the forecaster can assume safely that all the practical difficulties will somehow be overcome provided that the love factor is above some well-defined theshold. But experience suggests that there is some uncertainty even here. Undoubtedly the designer of models and simulations of crisis must show how he is going to allow for the constraints of imperfect intelligence on the one hand and the discontinuities in the strategic gamut on the other.

Though this is a point of interpretation and one that would be difficult to prove objectively, it seems to me that the crisis was remarkable for its freedom from domestic political pressures. Public opinion in the Soviet Union was not active on this issue as far as we know. In the United States the policy was forged by the decision-makers in secret, each one of course aware of domestic political considerations – some no doubt more aware than others – but these considerations were in the background and did not obtrude as a separate force to be reckoned with. This bears out the view that at least in intense short-term crisis public attitudes can be ignored as a factor separate from the attitudes of the decision-makers. The shorter and sharper the crisis, the truer this is likely to be. Hilsman comes to this conclusion after a survey of a number of crises with which the Kennedy administration had to grapple (p. 575). The decision-makers make their decisions without reference to their constituents, and have to carry within themselves the necessary minimum of representational constraints, so that what is done is within the bounds of what can afterwards be made acceptable to those in whose name it was done – to the extent that they care. Indeed, even in the wake of the extraordinary international crisis that we have been studying, it seems clear from surveys that local and social issues were decisive in the 1962 U.S. elections (Pachter, p. 28).

One of the most striking features of the final stage of the crisis, however one looks at it, is the bifurcation of Soviet response ultimately leading to two almost simultaneous messages with incompatible contents – the letters of 26 and 27 October. I have tried, using the apparatus of the 'first reduction', to trace the bifurcation back as far as the evidence will support it, and have represented it graphically by a 'wedge of doubt' which as it widens down the time-scale gradually displaces the original conviction that the U.S. would yield (see Fig. 6.2). The two threads of action are also clearly seen in the second reduction (Fig. 6.3).

It is quite uncertain whether there was real rivalry in the Kremlin – real in the sense that there were one or more factions who might have taken over from Khrushchev if his

policy had not given them satisfaction. Rusk seems to have believed at the time that this might be so, but to Hilsman it seemed clear in retrospect that Khrushchev had been 'in full control throughout the crisis', and he says moreover that even at the time, apart from momentary doubts, 'it was generally assumed in Washington that Khrushchev remained in charge' (p. 201). This makes it all the more interesting that the Soviet Government, whatever the reality may have been, behaved for a time as though it was two actors and not one. One line of conduct was seeking to extract every ounce of political advantage from the situation, on the assumption that the United States would not resist if they were pressed hard enough; the other was seeking to avoid an unacceptable physical confrontation that would lead to incalculable developments and probably to a rapid escalation. These two attitudes can be ridden together for a while, but there clearly comes a time when the galloping events move too far away from one another and the rider has to decide which horse to choose. There is nothing very surprising in this. One does not diagnose schizophrenia when a man holds conflicting beliefs; much less need one diagnose schism when inconsistent behaviour is observed in a nation, consisting as it does of a number of imperfectly co-ordinated men.

Returning to the Soviet behaviour on 25, 26 and 27 October, it may be regarded in any of at least three ways as representing the oscillations and mental struggles of a single man, or the divergent output of two or more minds with not enough time or energy to obtain consistency, or a deliberate 'widening of the repertoire'. One can discuss at length whether, in the 'first reduction', it would be better to link together items 53 and 54 into a single item 'carry on building in Cuba but avoid a showdown at sea' – a synthesis of 'hard' and 'soft' lines deliberately and collectively arrived at; or to show them as they are, representing the two tendencies, each leaving its distinct trace on events and giving rise to its distinct reactions on the other side. Perhaps some Moscow reader would care to comment on these speculations and correct the right-hand part of the chart in the interests of political science.

Chance also plays a part here: the United States appeared two-faced for a short time when the U2 went astray over

Siberia at the most critical moment of all. Again, without its being necessary for us to make any judgment as to the *real* internal situation, we must acknowledge that the outward behaviour of the nation seemed for a short time to have been that of two actors, not one. Robert Kennedy, commenting on the situation on that day (27 October), says 'The change in the language and tenor of the letters from Khrushchev indicated confusion within the Soviet Union; but there was confusion among us, as well' (Kennedy, p. 94).

It is perhaps a virtue in these charts that they make it easier to discuss such questions. However, one should beware of using circular arguments and reading conclusions into them that were already implicit in the way they were drawn up. For example, it would be tempting to draw conclusions about the role of the U.N. from the fact that the U.N. branches of activity – see items 47, 48, 58, 59 in the 'second reduction' – hang limp and without issue, and can virtually be treated as a separate branch of the diagram (see Fig. 6.3). But this is so because we say so to begin with. It would be open to anyone to maintain that the experience of having to turn down an U.N. initiative (item 47) had a deep effect on U.S. perceptions of world opinion which influenced later U.S. actions (e.g. items 58, 62 and 73). This would, I think, be hard to sustain and was not suggested by any of my principal sources; but you could make up a different chart in which these links were shown. In other words, the charts may help to make explicit some point of interpretation on which an implicit assumption has already been made; they are unlikely to lead to new conclusions on such points.

Conclusion

And so we withdraw, a little chastened but I hope not down-hearted, from the contemplation of this case history. A disappointing feature of the 'first reduction' is that although derived directly from the communication model, it does not bring out as one would wish the intertwining of the action circuit and the communication circuit that is the central unifying concept of that model. Why is this? Partly perhaps because of the ever-present but invisible habits of perception

and behaviour which permeate the action (see 'The Dissection', p. 132); partly again because of the limitations of a two-dimensional diagram. How can one be expected to portray more than a single cycle of events on the model shown in Fig. 6.2? The communication model cries out for three dimensions to do justice to the passage of time: a cylinder or spiral would answer well, but to these the publisher raised unaccountable objections. Indeed a double helix like that of the DNA molecule would have been helpful to represent the double signalling that was taking place at one stage on the Soviet side.

In this chapter the reader may have looked in vain for a glimmer of new light on the events of October 1962: but that was not its purpose. What the discussion does perhaps suggest is that there is almost endless scope for experiment with visual aids of this kind – not as ends in themselves but as means to improve dissecting skill and to clarify theoretical concepts.

7 The Scope for Theory

What Sort of Mathematics?

The mathematics we have encountered so far has been of a limited kind: some two-dimensional Cartesian geometry, some linear differential equations usually in two variables, some statistical methods, some rudimentary graph theory and one more fundamental branch of mathematics (I mean game theory), grounded in set theory and probability theory, which we have found unsuited to our present purpose. Hardly an exciting catch, considering the variety of good fish there are in the sea. Does the review we have carried out bring any insight into the prospects of a more profitable use of the resources of mathematics in the future?

First, a general caution. It would be vain to expect the mating of mathematics and international relations to bring forth some one or two heroic concepts (on the analogy of force and energy in physics) that will transform our understanding. Indeed the comparison with physics obscures more than it illuminates and had better be forgotten. Von Neumann and Morgenstern pray it in aid more than once, for example when they say:

> The decisive phase of the application of mathematics to physics – Newton's creation of a rational discipline of mechanics – brought about, and can hardly be separated from, the discovery of the infinitesimal calculus. (von Neumann and Morgenstern, 1947, p. 5)

But the lesson they draw is unexceptionable: that if a major contribution is to be made to the explanation of social phenomena, the advances required in mathematical technique may be very considerable, that no 'mere repetition of the tricks which served us so well in physics' is likely to suffice.

Thus far we may agree, only adding that there is another profound difference. Whereas at least a first approximation to physics was possible using only two leading concepts, and only two great laws, it seems as certain as anything can be that even the simplest model of international relations will need many variables, and that these will have to be worked into the mathematics right from the start. We shall find no Law of Conservation of International Momentum to supply us with information about the state of affairs after a series of national collisions, given the state of affairs before; no Law of International Gravitation with which to predict the rotation of the nations in their alliances. As McClelland says, 'the international system as a whole apparently lacks the coherence, the direction, the relatedness, and the organisation that many of us are constantly inclined to read into it' (McClelland, 1966, p. 97).

Perhaps biology gives a better lead. In the spectrum of the sciences it is much nearer the human end than physics is. According to Peter Medawar, 'Biology is complex, messy and richly various, like real life. . . . It should therefore give us a specially direct and immediate insight into science in the making' (Medawar, 1969).

The question is, how does biology use mathematics? J. B. S. Haldane (1932, p. 240) referred to 'statistical methods, which have been developed, largely by biologists, to enable us to deal with cases where we cannot get information as complete as the physicist can sometimes obtain, and which offer one of the few hopes of introducing scientific method into politics'. There is no doubt that statistics received a great stimulus from the challenge of biological problems. Outside statistics, biology has made use of existing mathematical equipment which has required little if any special jigging to meet biological specifications – probability theory for genetics, differential and integro-differential equations for the study of epidemic and predator cycles ('rabbits and foxes') and so on.

If this is a good parallel, then we may expect the uses of mathematics in opening up new terrain to be many and various: not a matter of one magnificent equation that carries all before it like a bulldozer, but a whole range of tools of different shapes and sizes, some specially jigged, some old

and tried, with which to reveal the contours of the theory.

Returning to international relations, what is the general advice of the experts? Mackenzie says 'Relax the rigour and precision of the theory' (1967, p. 126). Knorr and Verba look for 'approaches that lie somewhere between the precision of mathematical models and the confusion of historical reality' (1961, p. 3). 'Come down off the mountain', says Schelling, and avoid 'too abstract a model' (1963, p. 157).

With all respect to these distinguished authors, I think there is a fallacy in supposing that there is some comfortable semi-precise way in which mathematics can be used. You can use it or not use it: there is no half-way house. Once you have adopted a mathematical model you are stuck with it and have to take the consequences according to the rules of mathematics, and follow wherever it leads, until such time as you feel able to discard or amend it. That, indeed, is the whole point of using mathematics, which is a vast and ever-growing treasure-house of patterns of deductive inference waiting to be used by anyone who can pass the guardians at the gate. The password is 'organisation of data'. If, and only if, you have assembled your data in a logically systematic form (not necessarily quantitative, though it often will be) will you be able to gain entry. As an example of organised data that is not quantitative, many of the results of game theory require only that the players should put the results of the possible outcomes of the game in order of preference: there is no need to scale them quantitatively. Again, Harary's model of international relations (see Chapter 4, 'Models from Graph Theory', p. 84) requires no more than the information whether between each pair of nations there exists a positive link, a negative link, or no link at all: there are no numerical data of any kind.

It is my personal conviction that international relations theory cannot afford, any more than any other branch of social science, to pass by the mathematical treasure-house, and that advantage is likely to come from the use of mathematics in the handling of material derived from the study of international relations – granted that the material will have to be logically organised in the sense that it has been reduced either to quantitative form or in some other way that renders

it amenable to mathematics. I believe that there are unperceived regularities – perhaps very complicated ones – in the behaviour of the international system that no amount of verbal analysis can hope to bring to light and which might be revealed in the fulness of time by the sort of approach that has been discussed in the earlier chapters of this book. This, the reader must understand, is a matter of personal judgment, or faith, and I cannot offer a proof.

It is to be hoped that any necessary mathematical developments will keep pace with the needs of the theory; this will require the close attention not only of those who regard themselves as social scientists but also of those who regard themselves as mathematicians. So far, at least in this country, I doubt if they know of one another's existence.

It would be foolish to speculate in which branches the useful developments are most likely to take place. In previous chapters, in addition to the ubiquitous statistical analysis, which as I have said in Chapter 4 seems not to be entirely adequate to the needs of the social sciences and is perhaps ripe for a major advance, we have had occasion to mention graph theory (Chapter 4), difference equations (Chapter 4), and the probabilistic sequences known as Markov chains (Chapter 5). Any or all of these branches of mathematics could find themselves propelled into new developments by the challenge of international relations theory. But it may be that some new and quite unexpected kind of mathematics is round the corner, as is hinted in the quotation from von Neumann at the beginning of this chapter. John Kemeny has drawn attention (1959, p. 249) to the fact that mathematics has taught us how to cope with situations where there are a few objects – using algebra, geometry, set theory, combinatorics – and with situations where there are millions of objects – using statistics and the calculus. What we lack is a mathematics of the numbers in between, of situations where one has left behind the stark simplicity of small numbers and not yet reached the countervailing simplicities of continuity and statistical regularity that reside in very large numbers. It is these in-between numbers that occur all the time in international relations.

Of course it may be that there is no such mathematics, and that the gap is destined to be filled by the computer. The two

are in a sense complementary. Faced with a problem posed in mathematical terms, the computer will never find a general abstract solution, as mathematics may do (if there is one); what the computer will do is to try a large number of values of the parameters and give you a set of numerical solutions, which may for practical purposes be just as useful as a general answer. For example, it is when you cannot solve your differential equation by mathematical methods that you put it on the computer to give you an approximate result by taking finite differences.

To the extent that the relations between elements of the international system can be modelled in neat parsimonious equations – like the Richardson process for an arms race described in Chapter 5 – which can be solved by mathematical methods, it would be foolish to proceed in any other way. But it is inconceivable that this will be true of more than small parts of the system. Stanley Hoffmann has referred to the 'dilemma' of social science analysis in the following terms:

> While single-cause analysis is invalid, multiple causation is valid but too complex for scientific treatment, since it is not possible to follow in all their meanderings the interrelations among a large number of factors. (Hoffmann, 1965, p. 273)

If he is right, surely this is where the computer comes in. While one must be constantly on the lookout for fragments of theory that can be expressed and manipulated in purely mathematical form, it seems inevitable that machine simulation techniques will be needed for anything approaching a comprehensive statement of an international situation.

A final point is that any descriptive model of the relations between political events, whether in the short term or in the long, will be probabilistic: that is, it will aim not to determine future or unknown events but to establish probabilities for them. Probability will enter on the following counts:

(a) To allow for fortuitous but significant events that could not conceivably be accounted for in any scientific analysis: examples are natural disaster (drought,

flood, etc.); the coincidence of two unrelated events; the death of a statesman or indeed any unpredictable feature of personality.

(b) To allow for imperfections in the information available to an actor either on the facts of the situation or on the intentions or policy of another actor.

(c) To allow, even assuming that all fortuitous events are catered for and all uncertainties of information made good, for the uncertainty of the decision-maker's decision – that is to say for error and free will.

These things can be allowed for in any model by introducing what Kenneth Arrow has described as a 'disturbance factor': a random variable with a constant probability distribution which accounts for the unmeasurable and unknowable features of the system and for the errors in those that we do know and can measure (Arrow, 1951). With regard to free will, D. M. MacKay pointed out many years ago that its operation is behaviourally indistinguishable from that of a random variable, since the essence of free will is that decisions made according to it are not in any determinate or predictable relationship to other events (MacKay, 1951). The same idea was implicit in the project of the mathematician Alan Turing, reported in a memoir by his mother, to fit a roulette wheel into a computer to simulate the action of human choice (Turing, 1959).

The use of mathematics will, it seems, be multivariate (no great unifying concept), piecemeal (no all-embracing formula) and probabilistic (no determinism). It does not follow that it will be of no account.

What Sort of Framework?

In the search for models in Chapter 4, although we did not find many actual models we found a number of analytical frameworks, which can be compared. Looked at not as defective models but as useful frameworks, we can characterise them by the elements in the international situation on which they focus. I am here arranging some of them from this point of view (in a different order from that in which they appear in Chapter 4):

Focus on *events*	Interaction analysis (McClelland, Barringer)
Focus on *nations*	Dimensionality studies (Rummell, Tanter)
Focus on *decision-makers*	Communication/cognition model (Brody) Game-like models

As we go down the list we go deeper into the black box, that is, into the internal workings of the national actor, as can be quickly seen if we compare Figs 4.8, 4.10 and 4.6. The learning models discussed in the last section of Chapter 5 can be regarded as the final stage, with the individual decision-maker on the couch. This is a focus on 'inside the decision-maker'. And so there are big doctrinal differences between the main theorists today on the depth to which analysis should go.

The theory as a whole is at far too early a stage to say confidently 'This approach is right', 'That approach is wrong', and it would be very unhealthy (and very surprising) if everyone was found to be working on the same lines to a carefully co-ordinated plan. Nevertheless, one can make one or two tentative comments which bring together points that have been made in earlier chapters.

1. It is hard to see how interaction analysis can develop into a dynamic model of a particular crisis, much less into a model with any general predictive power, unless some variables representing the external behaviour norms of the nations are allowed in (Chapter 4, p. 81).

2. On the other hand, it is hard to believe that it will ever be possible to fill out the unabridged communication/cognition model with sets of explicit variables to describe the many nodes of the model and formulae to express the relations between them. Also, its heavy reliance on content analysis seems to place a limitation on the use of this model in its present form (Chapter 4, p. 72).

3. Any general model, if it is to give a plausible account of a specific situation, must leave room for the insertion of 'stop press' factors from outside the model, corresponding to the unpredictable effects (on perception) of last-minute intelligence and (on action) of last-minute analysis of military options (Chapter 6, pp. 133-4).

4. There is a presumption that machine simulation will be a necessary part of any model approaching in complexity a

comprehensive statement of an international situation (Chapter 5, Part I, esp. p. 102, and 'What Sort of Mathematics' in this chapter, pp. 142-3).

5. Linked-learning models cannot be applied until some way is found of breaking down the complex decision-making processes of real life into the simple components that are these models' fodder (Chapter 5, Part II, p. 117).

From the first two comments we arrive at the presumption that a model is to be sought somewhere between the extremes of interaction analysis and communication–cognition. This means finding some set of dispositional variables to represent the style of the actor while not attempting to express the whole mechanism of perception and decision explicitly in symbolic form. Together with the presumption from (4) that machine simulation will be a necessary aid if the model is at all complex, I believe that is as far as we can go in indicating the probable direction of future model-building within mini-theory.

Mini-theory versus Maxi-theory

Having introduced in Chapter 2 the distinction between mini-theory and maxi-theory, which I find a useful one, I should like to deal briefly here with the relations between the two. In particular, can mini-theory exist without maxi-theory; in other words is it possible or sensible to study the fine structure of events and national behaviour without regard to the presumed underlying forces that are going to show themselves in the longer term? Ought we not rather to think that long-term prediction is simpler than short-term prediction for the same reason that classical physics is simpler than nuclear physics – because things tend to even out over large numbers: the accidents and individualities that loom large for the moment will not have the same influence over a period of years? This is a familiar line of argument. George Orwell's way of putting it was 'I could not say exactly what wars and revolutions would happen, but they never surprised me when they came'. One feels sure that a certain government *must* fall; that women *will* get equal pay; that there *will* be five more nuclear powers; without being able to say exactly

how these things will come about, or exactly when. This feeling of historical inevitability suggests that maxi-theory contains the fundamental laws that will provide the broad explanation of the unfolding of events, and that mini-theory must be regarded as a refinement of maxi-theory to enable the researcher to explain the fine structure of events, such explanations being necessarily more complicated, introducing more laws and also being more heavily laced with chance factors than the seven-league explanations of maxi-theory. According to this way of thinking, it would be useless to start by examining short-term sequences of events, where the simpler regularities will be overlaid with a mass of confusing detail and one will be unable to see the wood for the trees.

This line of argument has a certain appeal, but I do not believe it to be true. In the first place physics once again provides a misleading analogy. There is nothing in international relations corresponding to the statistical laws of large numbers. International events are not like molecules in a gas, individually chaotic and collectively coherent. The scale is different, the interdependence is different; the comparison cannot be sustained for a moment. Moreover, without wishing to go deeply into the philosophy of history, I have grave doubts whether, in fact, historical prediction is any more reliable in the long term than in the short term – though I recognise that it would be extremely difficult to put that statement into a scientific or testable form.

Unpredictable events, it seems to me, are simply unpredictable events and cannot be encompassed in any theory except perhaps in terms of probabilities. Sometimes – let us take the case of Votes for Women – it could have been said by a perceptive student of politics that the probability of the event occurring sooner or later was very high, though he would not have been able to tell when it would occur. But there are many important scene-setting events for which even that much could not have been said. It would have been impossible for anyone, however well informed, to predict with any assurance before the event that President Kennedy would be assassinated, or for that matter that General de Gaulle would come to power in France. Contingent prophecies could, however, have been made in both cases; a student of the American scene would have been able to draw

conclusions as to how some features of American policy would be likely to develop *if* Kennedy were to die; and certainly no one with a good knowledge of France and of de Gaulle would have had much difficulty in predicting, in 1958, the course of French policies *if* he were to come to power. But it would be quite fanciful to suppose that, had de Gaulle not come to power, some other event or set of events would have taken place that in the long run would have tended to put France and the world on to the same course as that actually taken. Maxi-theory has therefore to contend with these unpredictable absolute changes of course, and the longer the period it purports to cover the more unpredictability it must contain. Two possible futures of France – France A and France B – allowed to develop under the influence of different chains of unpredictables, will not converge but rather diverge with time, like two men doing random walks. (A random walk goes according to the toss of a coin – one step forward for heads, one step backward for tails. You might expect that since the proportion of heads and tails tends to level out, a random walker would oscillate around the starting point never getting very far away. But in fact the actual *number* by which heads exceed tails or vice versa tends to become bigger with time and so the man gets further and further away. If two men are doing independent random walks starting from the same place, they will tend to get further and further apart as time goes on, both from the starting point and from one another.)

However that may be, there are solid grounds on which it may be easier to account for events in the short term than in the long – particularly if one is dealing with the *very* short term. In acute crisis a whole host of considerations that are normally present to the political consciousness fall away into insignificance. This should greatly simplify the analytical task. Complications that can be disregarded in the very short term include the following:

1. Immediate pressures of public opinion – as distinct from the attitudes of the decision-maker, which must be regarded to some extent as representing public opinion (this point was discussed in the context of the Cuba crisis; see Chapter 6, 'The Corpse', p. 135).

2. Long-term considerations of internal politics. 'Effective handling of the crisis' is likely to be the touchstone of future judgment of the decision-makers who can safely concentrate on *that* and leave the long-term politics to take care of themselves. In the language of the INS (see pp. 98-9), the decision-makers can assume a greater 'decision latitude' in a crisis.

3. Long-term economic considerations, which at other times are one of the most important elements of national policy-making.

4. Changes in national rules of behaviour (style). These take place slowly and, it may be conjectured, according to some cumulative process of learning that is distinct from the ordinary short-term incremental reaction to events. We have already met this distinction in discussing Brody's model (see 'The Communication Model', pp. 71-2). Rules of behaviour must ultimately be regarded as derived from the national goals which also change, though very slowly, and can safely be taken as constant in the short term.

Armed with this impressive list of simplifications, I do not have any difficulty in concluding that the study of mini-theory is legitimate in its own right. We can now outline a programme of theory-building based on mini-theory. If regularities are revealed that can be explained in terms of a mini-theory valid over short periods of time only, it will then be possible to try, by adding in successive complications on account of (1), (2), (3) and (4), to develop theories that are valid over progressively longer periods of time, perhaps finally approaching the broad expanses of maxi-theory. I am sure that the approach from the opposite direction, starting from maxi-theory, will have its devotees. It would be a happy situation to have work in progress on both lines, and a matter of some interest to see, as in the building of a long tunnel, whether they eventually meet in the middle.

To show that it is possible, I have set out the end of this chapter the sketch of a very elementary model of conflict situations – entitled the New Model Model – which could, I believe, be used as a research toy and which could, I am sure, be improved out of all recognition by anyone who took the trouble to make a programme of it and discuss it with a friendly

computer. Out of consideration for those readers who will not want to wade through the details, I have put it in an appendix rather than in the body of the text, but those who do tackle it may find that it throws some light on the next section.

What Sort of Answers?

After all this general discussion there must remain the nagging question: will *any* deployment of factual research, mathematics and mother wit produce useful answers at the end of the day; or, since early in this chapter I have already made a personal expression of faith that it will, the question can be rephrased in the optimistic form: what sort of answers will they be? We cannot really know what is on the other side of the hill until we get there; but have we any idea of what we expect to find or what we are looking for? I think that much of the writing on international relations today suffers from the lack of enunciated aims, which diffuses effort and also invites detraction since it is easy to ridicule a programme of discovery to which no limits have been set.

I give in this final section a list of personal suggestions as to the aids to understanding and prediction of international relations in the short term that we might have to hand fifteen to twenty years from now. They are no more than imaginative guesses based on the research work with which I am acquainted – work which for the most part has been under way for only a few years and which must therefore provide a most unsure base for such a long extrapolation.

The first need is to calibrate the world, and national views of the world, so that a situation can be truly 'situated' therein. Using the 'field' metaphor of the book's title, this will be a calibration of the field of nations. An important part of the process is the building-up of a case-book of past international events analysed in retrospect to show the realistic alternative courses that were perceived at each stage and the perceived 'naïve probabilites' of further escalation (see item 7 below). This can be done graphically with the help of a 'tree of options' as defined and discussed in the Appendix. It will demand on the one hand meticulous recording by Foreign Offices and Cabinet Offices at the time,

and on the other hand the exercise of super-hindsight to fill in gaps in the knowledge of choices as perceived by other nations, and could I believe be a subject of immediate research effort: I do not know of any published work in this field above a rudimentary level. Blechman (1966) has analysed and quantified the options open to Israel (but not the other nations involved) in the 1956 Suez crisis; and Cushen (1966) has shown how it is possible to make a useful analysis of the options open to players in gaming situations, in the series known as the Polex-Dais games at M.I.T. In the Appendix I give a fragmentary analysis on these lines of the 1962 Cuba missile crisis simply as an example to illustrate the use of the New Model Model.

What follows is a speculative list of the main features of a possible calibration of the field of nations. A summary of those features that would tie in with the model appears in the Appendix.

1. *Levels of action* will be defined unambiguously, so that any international act can be given an index on a fixed scale. This will apply both to physical moves (mobilisation, deployment of troops or weapons, reconnaissance, economic blockade, military attack) and strategic moves, using this phrase as Schelling does to cover any kind of conditional commitment that restricts the range of future action – a promise or a threat. The scale will be an objective basis on which all nations concerned can judge in the midst of a conflict just how much escalation is contained in a move by one of them.

2. *Hostility level* of any statement made by a party to the conflict – even a statement of one's own – will be measurable, as indeed it is now, by content analysis (see Chapter 2). At a time when susceptibilities are high and well-intentioned or neutral messages may be interpreted as deadly insults or injuries, this scale should provide a useful corrective against hasty judgments. The computer might not see all the subtle innuendoes, but the harassed official or politician in mid-crisis might see more than were really there. This and item 1 together should reduce the risk of misperception of the amount of heat in the situation.

3. *Effect of possible situations on perception of national identity* will be scaled with many examples from history. Attack on the heartland or attempt to colonise a nation would have a large negative score; achievement of separate existence or freedom a large positive score. (This and Nos. 4,5 and 6 are directly related to variables in the New Model Model.)

4. *Effect of possible situations on perception of national security* will likewise be scaled: particular features of a nation's history, such as repeated invasion from a certain quarter, or a traumatic experience that has left a scar on the national consciousness, must be taken into account here as well as the objective nature of the threat.

5. *Effect of possible situations on perception of national role* is a much more difficult nut to crack but must also be attempted. One method would be to develop a set of contour maps for each of a number of fundamental political issues, from which one would be able to read off the answer to any reasonable questions of the form 'What sort of situation would constitute a score of x for national role on this issue?' or 'What would be the score of situation so-and-so?' The concept of role is in analytical terms essentially backward-looking: it is the ability to maintain the position in the world that the nation is regarded as having occupied in the past, and to continue to do the things that were done in the past in accordance with that position. Being a portmanteau for all kinds of national aspirations-in-action, expectations as to the behaviour of others, and myths, it is going to be particularly hard to objectivise. It will be necessary to decide how to assess a situation that advances one goal and retards another. Joxe's analysis (see Chapter 4, 'Models from Graph theory', pp. 86-7) may be relevant here.

6. *The international standard score* of a sequence of moves will be measurable. It will be a principle that escalation of the level of action (item 1) has a negative score here, de-escalation a positive score. But more than this, perhaps it would be possible for the international standard score to act as an index of observance by a nation of the current operating

code of international society. This operating code, or set of rules of accommodation (to use Iklé's phrase; see p. 62) is a constraint that is observed by most nations most of the time and can be recognised by the fact that it restricts the immediate freedom of action of the nation but if observed by all will assure better results in the long term. All walks of life bear witness to the usefulness and power of such rules. A striking example – where so far as I know there is no direct sanction against failure to conform – is the mutual trust of members of the Stock Exchange which enables the system to work much more quickly and efficiently than if security were always required. Conversely, when a long-established social rule is once broken, it is usually not long before it is broken again. Imitation is a potent force in human society. So far as international relations is concerned, the assumption is that any nation contemplating an overt act will raise its eyes from the immediate consequences and consider the act also in its context as a possible long-term influence, for good or ill, on the behaviour of nations. If Great Britain has nuclear weapons, France must have nuclear weapons, and it becomes progressively more certain that other nations below super-power level will follow suit. If Russia long ago claimed twelve miles of territorial sea, it was the easier for Iceland and now many other countries to do the same. If (to return to the Cuban missile crisis) the U.S. imposes a 'quarantine' for its own compelling reasons in 1962, it becomes marginally easier for any other nation to impose a similar quarantine or even a full blockade on some future occasion when it wishes to do so. But if – to continue the example – the U.S. had decided to bomb the missile bases without any overt military act or even provocation from the other side, the ugly example would have cast a much more sinister shadow over the future conduct of human affairs – quite apart from the incalculable immediate effects.

Any rules under this rubric must be in simple, compelling terms – focal points, to use a phrase of Schelling's – such as will gain the support of public opinion. One must be able to imagine the attitude spreading among the more politically active part of the population: '*That* is the way we do things in this country.' Some of this is embodied in international law. But many rules that are generally recognised and often

observed are not written down because nations wish to reserve the right to infringe them from time to time, and conventionally any rule established in international law has an absolute quality. What we are saying is that such rules should be regarded as having relative, not absolute, force and that nations behave (to a limited extent) as if they subtracted or added marks in evaluating a proposed course of action according to whether it infringes or reinforces an article in the code. A complication is that nations may be willing to observe rules of accommodation with friendly nations but not with others; the rules could then be regarded as of universal effect but with a varying penalty for infringement depending on the other nation concerned. Note that we are seeking to describe not how nations ought to behave but how they do behave, how they do operate within their self-imposed systems of values.

The task for theory under this item is to list the rules, the logical connections between them, and the penalties.

7. Rules will be available for determining the *naïve probabilities of escalation* inherent in a situation, to any level higher than the current level. These probabilities, based on a crude comparison with the profiles of previous crises in which the nation has been involed, can be applied to the situation as a first appreciation before it has been analysed with respect to specific possible future sequences of events, or when such analysis is too difficult. Some means will have to be found of giving weights to previous crises according to whether they are recent, relevant, deeply rooted in the national consciousness, etc.

So much for the situational or field variables. The other general requirement is a series of readings of *dispositional variables* chosen to represent the characteristic behaviour of each nation. The number and nature of these is yet to be revealed and will no doubt vary with the model being used and the period for which validity is claimed: the longer the period, the more variables there will have to be. The short-term model outlined in the Appendix has a set of eight or ten variables for each nation. It is to be supposed that whatever they are they will for the most part be constant in the short

term; to the extent that they vary, the variation will have been correlated with observable indicators and will therefore be predictable to within a probability range at any given time. Take as an example one of the parameters in the Appendix – that which determines the mode of the probability distribution of β. This parameter is of importance in determining the weight to be attached to perceptions of bargaining reputation and precedent as features of a situation. It will be related in some way to the records of actual bargaining reputation of the nation based on what other nations have said about it historically, which in turn will presumably be found to correlate with hard data of threats carried out, promises broken, etc. Content analysis might provide a direct cross-check to the value of the parameter found by this roundabout method, true to our policy of using content analysis whenever possible as a secondary technique rather than basing our whole edifice of theory upon it.

There will be an index of *inter-nation responsiveness* for each ordered pair of nations; to construct these indices it may be necessary to depend on content analysis of official documents and the history of past interactions weighted according to remoteness in time and significance, and corrected by reference to samples of elite (active) and general (passive) opinion. The index would not be reflexive; it could not be assumed that A's responsiveness to B was the same as B's responsiveness to A.

The above is a scratch list of measurements that in fifteen or twenty years' time it may be possible to make with some confidence and with little room for argument, in place of the highly subjective and unscaled guesses that are all we can muster today. The next question is, will it be possible, by feeding the 'situation' and the 'nations', suitably treated, into a computer, to predict the course of events a day, a week, or a month ahead like a weather forecast? I think the answer must be No: not even like a weather forecast. It does not seem remotely possible that any mechanical process will enable us to look even one step ahead with any precision in the evolution of an international situation.

But let us be cheerful; for we shall have aid. All the measures I have listed, if they come to pass, will help us to

see the situation that little bit more clearly and talk about it that little bit more precisely than we can now. We shall still have to judge the situation, to size it up so to speak; but the gap across which our judgment will have to leap will have been narrowed and our foothold will perhaps be surer. As we found in Chapter 6, the judgment of the decision-maker will loom large in the identification of the realistic options between which a choice must be made, whether by his own nation or by another. If (as supposed in the Appendix) a tree of options is worked out and fed into the computer, that will be one way – the only way that I can think likely for a very short-term study – of providing the human judgment in a usable form.

It must be assumed, finally, that the aids provided by theory will be in the form of probability statements; and the ultimate 'output' of the combination of judgment and calculation is likely to be probabilistic too.

I hinted at the end of the previous section (see p. 149) that it might be possible, starting from mini-theory, to develop a theory that was valid over a longer period. It would be unprofitable to speculate at any length on how this might be done, but there are one or two pointers. The application of a successful mini-model to a large number of crises should make it possible to work out a typology of crises and identify the necessary minimum of data in the pre-crisis situation that will 'type' it and enable us to predict something about its course, though obviously something that falls far short of full detail. One would hope in particular to learn from its type whether it is likely to converge or diverge in the longer run. But we have to remember at this point the many factors that mini-theory can afford to disregard, and which will now have to be considered, and probably worked in somehow before any long-term validity is to be expected: the degree of integration of the society and other features of internal politics, the pressures of public opinion in general, longer-term 'learning' by the decision-making apparatus, and economic consequences, to name the most obvious. Their incorporation will represent a major second stage in theory-building.

APPENDIX: THE NEW MODEL MODEL

Levels of Action

Given: two nations A and B between whom a situation of conflict exists. The action consists of a series of moves by one or the other party, each move having ascribed to it a level of action on a scale from 0 (normal background noise of international abuse) to 5 (nuclear attack). Rather than define each step explicitly on this and the various other scales in the model, I shall try to show by example how they would be calibrated. There is room for much difference of opinion both on the number of steps necessary and on their calibration.

Moves may be physical (mobilisation, deployment of troops or weapons, reconnaissance, economic blockade, military attack) or strategic. A strategic move is a commitment in one of the following forms:

(i) If you go to level x, I shall go to level y ($y>x$).
(ii) If you continue to act at level x, I shall go to level y.
(iii) As (ii) . . . . and moreover I shall go to level y unless you reduce your level of action so that my perception of the situation reaches p (see 'Evaluation of the Field' below).

Note that the quarantine of Cuba was a strategic move of type (iii) equivalent to the threat: 'If you send a ship with a missile in it to Cuba, we shall stop it (and – unstated corollary – if it doesn't stop we shall if necessary sink it); and moreover we shall go on like that until the missiles are withdrawn.' It was only when the *Marucla* was boarded on 26 October that a physical move took place. A conflict sequence could very well consist entirely of strategic moves, and be played with paper and pencil.

It is necessary to define the level of action of a strategic move unambiguously, and it would be convenient if this could be done in terms of the levels of the action that it refers to. A first approximation for case (i) might be the formula $b + \frac{1}{2}(y-x)$ where b is the present level of action of B. For cases (ii) and (iii) $b = x$ and the formula becomes

½ $(y + b)$, i.e. half-way between the present level of B's action and the threatened level of A's action.

The course taken by the Cuba missile crisis can be represented as follows:

$$B_1 - (A_2) - (B_2) - A_2 - B_0 - A_0$$

A represents the United States, B the Soviet Union. The suffixes are levels of action. Brackets denote strategic moves. To explain further:

B_1 Soviets send missiles to Cuba. In terms of international behaviour, not in itself a high level of action – though it was perceived as a serious threat by the United States, but that is another matter. It can be assumed (and it would be necessary to do so when calculating the danger to bargaining reputation in future stages of the crisis – see next section) that behind the physical move lay a strategic move, namely a promise to Cuba to supply the missiles. According to my formula above, this preliminary strategic move would have a level of action of ½; unfortunately it seems necessary to allow half-sizes for strategic moves.

(A_2) U.S. quarantine of Cuba. 'If you go on with action at level 1, we shall respond by action up to level 3 if necessary' – in other words stop (level 2) and if necessary sink (level 3) any ship that tries to run the blockade. If it is thought that I am too severe in insisting on the threat of sinking, which was not explicit in President Kennedy's broadcast on 22 October, then the level of action of this strategic move should be 1½.

(B_2) Khrushchev's muffled threat 'If you stop (let alone sink) one of our ships, we shall sink one of yours'. Could be regarded as being at level 2½, but the unofficial channel through which it was communicated (see Chapter 6, narrative item 37) seems to call for some downgrading. This example suggests the need for a correction term in the formula.

A_2 Boarding of the *Marucla*.

B_0 Withdrawal of missiles.
A_0 Termination of quarantine.

Evaluation of the Field

Six variables $a_1, a_2, a_3, a_4, a_5, a_6$ represent A's perception of the situation, each with values that can range from −5 to +5, with 0 in the middle representing 'no change'. They measure A's perception of the impact of the situation, and the sequence of events that has led to it, on the following counts:

a_1 on *national identity*: attacks on the heartland or attempts to colonise would have large minus scores; achievement of separate existence or freedom large positive scores. Territorial aggrandisement or colonising activity by nation A would rate a small positive score; extensive use or loss of resources a small negative score.

a_2 on *national security*: mobilisation of hostile forces or any increase in military threat from outside would have a negative score; absorption of a tiresome neighbour-state would have a positive score. A negative score for a_2 usually indicates that a negative a_1 is expected at a later stage.

a_3 on *national role*: ability to maintain the position in the world that the nation regards itself as having occupied in the past, and act as that position requires. Note that this includes keeping up the nation's real position, and also sustaining any current national myth.

a_4 as a *precedent*: significance of the sequence of events as a model for future sequences affecting the interests of nation A — except in the specific way covered by a_5.

a_5 on *bargaining reputation*: a positive score if specific promises have been kept, specific threats carried out; otherwise a negative score. The size of the score to depend on the importance of the issue on which the commitment was given.

a_6 as an *international standard*: significance of A's performance measured by the standards of international morality, law and good practice, i.e. regarded as an

example for the behaviour of other nations; raising the level of action would incur a negative score, acting to de-escalate would have a positive score.

A similar set of six variables b_1, b_2, b_3, b_4, b_5, b_6 represents B's perception of the situation. The sets can be referred to collectively in vector notation as **a** and **b**, and in terms of the 'field' metaphor we can regard these as the values or strengths of the field at the points A and B.

The sets of variables constitute a sort of multiple payoff in the language of game theory: if positive they measure the advantages that the nation sees in the way things have gone, if negative they measure the costs to the nation, or the threat – i.e. the cost that will be incurred 'if something isn't done about it'.

Dummy Decision-makers and the Behaviour Function

From these materials we assemble three universal dummy decision-makers, perhaps recognisable as representing extreme attitudes but having, it must be admitted, definitely subhuman profiles. The three universal dummies will have to be yoked together in a second integrating process before we can arrive at the behaviour function. The three are:

Hasty $\mathrm{H}(\mathbf{a}) = \Sigma \lambda_i a_i$ where $\boldsymbol{\lambda} = (\frac{1}{3}, \frac{1}{3}, \frac{1}{3}, 0, 0, 0)$

in other words an amalgam of the first three variables with equal weights. His nation's interests and position are paramount and he is sensitive to any menace to them in the immediate situation.

Worrit $\mathrm{W}(\mathbf{a}) = \Sigma \mu_i a_i$ where $\boldsymbol{\mu} = (\frac{1}{3}, 0, 0, \frac{1}{3}, \frac{1}{3}, 0)$

in other words, a concern for the national identity (which I do not think can be denied this second appearance) and for the posture in which the nation will emerge from the mêlée to face the future (bargaining reputation and precedent). This character is worried about how things will turn out and is prepared to give ground now in the interests of a sounder position in the future.

Dreamy D(**a**) = a_6, in other words the standard-bearer of international virtue.

Clearly there is much scope for choice in the manner of moulding these dummies. It would be possible, for example, to have six dummies, one corresponding precisely to each of the original variables. But this would provide five degrees of freedom in the next stage — i.e. in deciding their relative weighting — instead of the two degrees of freedom in the present model, and it seems to me that such a large range of possibilities would be an *embarras de richesse*. A nation does not, I believe, have such a very great range of behavioural styles that it can call on. Though this is a matter of instinct, I have a feeling that if one is going to use dummies in the model then three is about enough. As for the way the basic variables are linked together in the construction of the dummies, I can only say that the way I have chosen seems as natural as any (but see (*c*) in the next section, p. 169).

To construct the profile of a particular nation, we now make a further linear combination F(**a**) by means of probability distribution functions applied to the dummies:

$$F(\mathbf{a}) = \alpha H + \beta W + \gamma D$$

This is the behaviour function.

I propose two cases. The simpler is the *fixed international*, where γ is given a fixed value, say $\gamma = 0.2$, and α is determined by a probability distribution function in the remaining range, that is $0 \leqslant \alpha \leqslant 0.8$. β is determined by the equation $\alpha + \beta + \gamma = 1$. A simple probability distribution function would be:

$\alpha = 0.3$ with probability ¼
$\alpha = 0.4$ with probability ½
$\alpha = 0.5$ with probability ¼.

This would have the following effect on the behaviour function F(**a**):

With probability ¼ $F(\mathbf{a}) = \frac{3}{10} H + \frac{5}{10} W + \frac{2}{10} D$
.. .. ½ $F(\mathbf{a}) = \frac{4}{10} H + \frac{4}{10} W + \frac{2}{10} D$
.. .. ¼ $F(\mathbf{a}) = \frac{5}{10} H + \frac{3}{10} W + \frac{2}{10} D$

The probability element corresponds to the chance fluctuations of internal politics. Subject to that, the function defines the nation's perception of any given situation, present or future, in numerical terms.

It is conceivable that to represent the behaviour of a nation that is controlled by a single highly dominant and highly consistent decision-maker, the pattern of combination of the dummies could be regarded as fixed once for all, so that α, β and γ are all constant. But for the normal case where agreement has to be reached between a number of people of differing outlooks, a probabilistic spread on the above lines seems to be indicated. The underlying idea is that a national style of cognition and behaviour consists essentially of the striving for expression of a number of distinct styles, each of which tends to obscure the others.

The more complex case is the *floating international*, in which α and β both vary independently according to probability distribution functions and γ is determined by $\alpha + \beta + \gamma = 1$. For the purposes of illustration in what follows I shall use the fixed-international model, although I should expect the floating-international, if it turned out to be manageable as computer fodder, to give better results in practice.

There would be nothing to be gained by leaving a large degree of freedom in the construction of these distribution functions. Since the calculations would be computerised, the functions would provide probabilities for discrete intervals and not a continuous function; and probably a small number of intervals – say about six – would be ample. The modal value or peak for each distribution having been fixed (0·4 in the above example), and the range of possible values (0·3 to 0·5), one more parameter would suffice to fix a suitable symmetrical bell shape for the probability diagram. That means four parameters for the fixed-international model (three for the probability distribution and one for the fixed value of γ), and six parameters for the floating-international (three for the probability distribution determining α, another three for that determining β). Of these four or six, it seems likely that two will be major – those that determine the modal value of α, β and γ – and the others will be minor. But this is something that would only become clear with practical experience of the model.

It is now time to recall one of the conclusions of Chapter 6, namely the importance in the evolution of a crisis of identifying the practical options. I know of no way of side-stepping this process if we are determined to study the fine structure of events, and so the next stage is an explicit enumeration of the various courses of action open to nation A, the range of responses in each case open to nation B, and so on up to a certain point where the mists of prediction grow too thick. When this point is reached, no more than a crude indication can be looked for of the further developments; what I suggest is that this can usefully take the form of an assessment of the probabilities, for each value of x, that the crisis having taken the course indicated will reach the level of action x before it is finished. These will be known as the *naïve probabilities of escalation*. An assessment on these lines seems to give a plausible account of the mental processes that decision-makers would go through at such times.

It does not seem too much to ask, therefore, that before a 'situation' can be accepted by the model a tree of options should be worked out, each branch of which terminates either in a new situation which is believe to be stable and which can be evaluated for the two antagonists, or in an unstable situation where the naïve probabilities of the crisis going to new heights of action can be estimated.

Returning once again to the inexhaustible Cuba missile crisis for inspiration, Fig. 7.1 shows a very simplified and fragmentary tree of options at the stage where the United States know that the missile sites were being erected but had not decided what to do about it. As before, A represents the United States, B the Soviet Union. In the interests of clear exposition, and to keep the tree and the consequential table within bounds, I have not pursued the branches labelled X or Z, though in any conscientious analysis they would clearly have to play a part. With these exceptions the terminal points are evaluated for each actor in Fig. 7.2. The evaluations are based on values for the as and bs that I have inserted 'by eye' as it were. It would be open to anyone to propose other values. Where necessary, estimated probabilities have been inserted in the tree of options as figures in

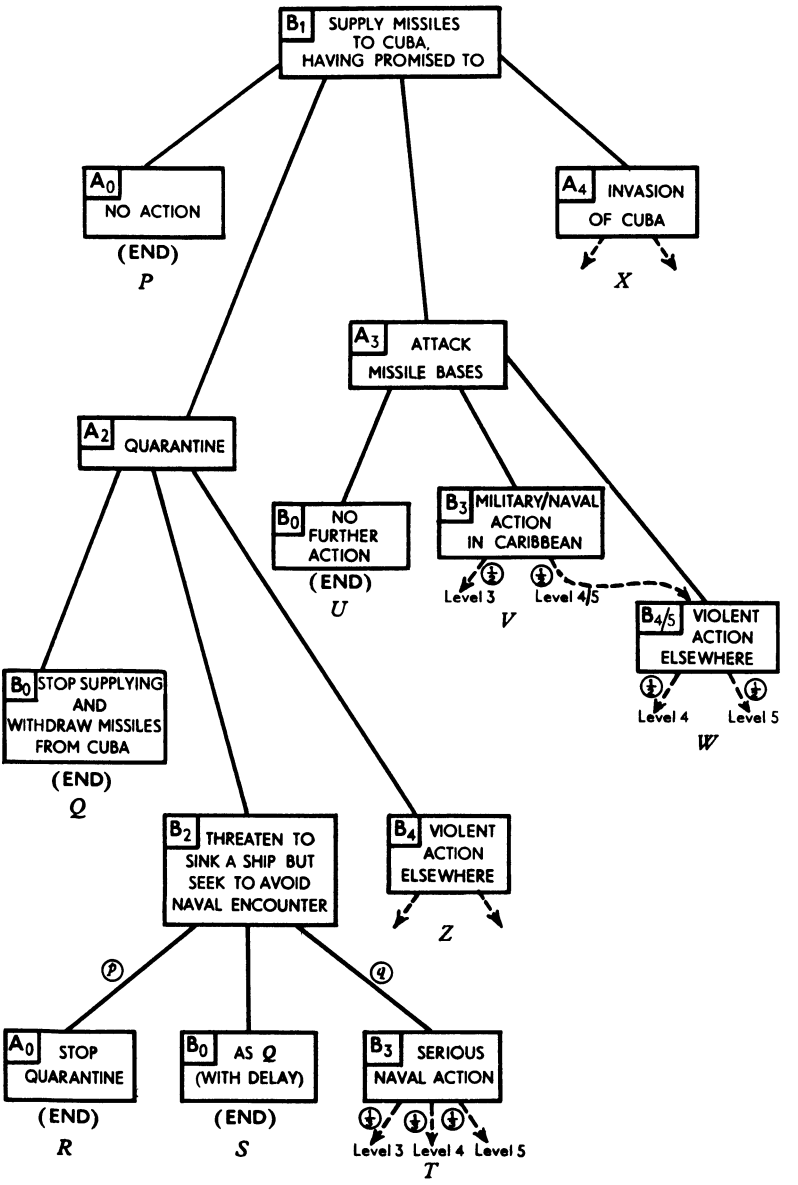


Fig. 7.1. Tree of options
A = United States; B = Soviet Union
Suffixes denote level of action
Figures in small circles denote probabilities

	P	Q	R	S	T	U	V	W
a_1	0	0	0	0	0	0	0	0
a_2	−2	0	−2	0	0	0	0	−3
a_3	−4	2	−4	2	0	2	2	0
a_4	−4	3	−4	2	0	3	0	0
a_5	−2	2	−4	3	0	0	0	0
a_6	0	−1	−1	−1	−1	−3	−3	−3
b_1	0	0	0	0	0	0	0	0
b_2	3	0	3	0	0	0	−2	0
b_3	3	−3	3	−3	0	−5	−5	2
b_4	3	−2	3	−2	0	−5	−5	0
b_5	3	−3	3	−3	2	−5	−5	4
b_6	0	3	−1	3	−2	0	0	0
Modal value of F(**a**)	−1.6	0.7	−2.1	0.7	−0.2	0.1	−0.3	−1.0
Modal value of F(**b**)	1.6	−0.5	1.4	−0.5	−0.1	−2.0	−2.3	0.8
Naïve deterrent effect	..	..	..	..	−1.3	..	−1.0	−2.0

Fig. 7.2. Evaluation of terminal points

circles. Note that apart from the naïve probabilities of escalation already referred to for unstable situations, only two probabilities are involved in this example, and as explained below no serious estimate has to be made of either.

Included at the bottom of the table are the calculated modal values of $F(\mathbf{a}) = \frac{4}{10}H + \frac{4}{10}W + \frac{2}{10}D$

$= \frac{4}{15}a_1 + \frac{2}{15}(a_2 + a_3 + a_4 + a_5) + \frac{3}{15}a_6$

and equivalent modal values for F(**b**), the evaluation for B. I have used the same behaviour function for the two nations in the interests of simplicity. The last line of the table shows, where it applies, the *naïve deterrent effect*. This is a function that comes into play in those unstable cases where a possibility is seen (according to the naïve probabilities that we have already met) of the crisis reaching level 4 or 5. Certainty of

reaching level 4 gives a score of m (to each party); certainty of reaching level 5 gives a score of n. To provide a concrete example, I take $m = -1$, $n = -3$. Probabilities of reaching these levels score proportionately: so for example course W, where there is an evens chance of going to level 4 or level 5, scores $\frac{1}{2}(-1) + \frac{1}{2}(-3) = -2$.

It is important to notice that the scale is a very sensitive one, in that a small movement in it makes a lot of difference. A change by one point down (e.g. from 0 to -1) would be a severe blow for the country concerned. We see in the evaluation of course P that the challenge that the United States decided was unacceptable and had to be resisted amounted to $-1{\cdot}6$. Even differences of quite small fractions – perhaps arising from a change of the order of a whole point on one of the original six variables – would be matters of some anxiety. One could of course make the scale seem larger by multiplying all the figures, say by ten, but this would change none of the results and seems a pointless piece of arithmetic, once the sensitivity of the scale has been understood.

We can now analyse the crisis as seen by A, whose first task is to estimate what B is likely to do following each of the possible next moves for A in the tree of options. We follow this through first on the simplifying assumption that the modal values of F(**a**) are *the* values, and ignoring all the variation that is caused by the probability distribution functions, which in practice would have been calculated for us by the computer. From Fig. 7.2 it is clear that the only moves among those we are considering that offer any choice to B are A_2, leading to the branches Q, R, S, T, and A_3 leading to the branches U, V, W.

To assess Q, R, S and T from B's point of view it may seem that we must first have B estimate the chance of A giving up at this stage (the move A_0 leading to R). But in fact all we need is the assumption that this chance is greater than zero. For then it will necessarily be in B's interest to move B_2, since B will then have some chance of the favourable outcome R to look forward to and S (≡ Q) to fall back on if A does not give up. This is on the assumption that B has complete control over the choice between S and T. And even if he feels he has to consider the risk of T arising through accident, a little arithmetic will show that the move B_2 is still

to his advantage provided that $1.9 \times p > 0.9 \times q$ where p is the chance of A_0 and q is the risk of B_3. With this proviso, we can therefore assume that A_2 will be followed by B_2 and (as and when it is seen that A is not going to give up) subsequently by B_0: this is S, the course that events actually took. There has been no regress of subjective probabilities here; the only assumption is embodied in the above relation between p and q, which is satisfied, broadly speaking, provided that p is not too small and q is not too large.

The other group of choices U, V and W can be analysed as follows. V is disastrous for B: a score of −2.3 in itself representing the failure of a local non-nuclear attack on an area defended by A's nuclear umbrella, already worse than capitulation (U), and a total score of −3.3 when the naïve deterrent effect is taken into account. So B is forced to consider W, a major escalation, whose score, notwithstanding the enormous naïve deterrent effect, is greatly preferable to course U. So we conclude that A_3 will be followed by an imprecise $B_{4/5}$ — in other words course W.

It is now for A to look at the situation from his own point of view, considering the probable practical options that have emerged from the first stage of the analysis, namely, P, S and W. There is no doubt where his preference lies as between scores of −1.6, +0.7 and −3.0. Ergo, his move will be A_2. Q.E.D.

I do not suppose any reader has been misled by these paragraphs into thinking that we have here a miraculous demonstration, requiring only a few minutes' work, of something that it took the entire brains of the U.S. Administration nearly a week to decide with sandwiches in the office and little sleep. If so, I must disappoint him. What I have done is to proffer a model of the processes that were then gone through — vastly simplified and crude though it may be — and to present it I hope in such a way that it does not seem altogether ridiculous and deformed. The figures in the table in Fig. 7.2 are nothing if not renderings into number form of explanations that have been given in public of the analysis that took place at the time. The hard work and the difficult part of the decision-making resides in drawing up these priorities, whether in the form I have suggested or in some other more literary form, as was done by the Excom on 19 October.

I can hear others cry 'But this is arrant game theory as ever was committed!' and turn for comfort to the form of burial service at the end of Chapter 3. Now it is true that the tree of options bears some resemblance to the representation of a game in extensive form (see Fig. 3.3). Perhaps, allowing for the probabilistic element in the distribution functions (about which I shall say a little more later), it would be better to say that it resembles a sequence of games where the payoffs may be different but the probabilities of moving from one game to another are given. But even so this is not game theory. Not a single theorem in all the 600-odd pages of von Neumann and Morgenstern, nor in the tens of thousands of pages that have been written since, finds a place in the model. All I have done is to borrow some of game theory's clothes.

The analysis carried out above was done using a simplified behaviour function which takes only its modal values. To generalise it, two elaborations in the procedure would be necessary: in the analysis of moves open to B, the distribution function appropriate to B would be applied to the evaluation of $F(\mathbf{b})$ in the various cases, and *these* sets of values would have to be compared. It is possible that this would lead to an ambiguous indication about B's course of action; if so, this would be in the probabilistic form that he would make move X with probability x, move Y with probability y, and so on. To this more complicated but still manageable result the analysis of moves open to A would then be applied, using at this stage A's probability distribution function to widen his possible range of behaviour. It would not seem too difficult to devise a computer program that would follow this procedure when presented with any tree of options that was drawn up in the proper form.

Many variants on the above rules for selecting a move immediately suggest themselves. I shall do no more than mention four, to give some idea of the versatility of the model:

(*a*) The analysis includes (when it is A's move) writing in A's perceptions of various alternative situations, and A's perceptions of B's perceptions of them. When it is B's move it will be necessary to write in B's real perceptions and also B's perceptions of A's perceptions. If accuracy of perception is taken for granted,

this will be straightforward; alternatively it will be possible to postulate *errors* of perception, e.g. that B's perception of A's perception always exceeds A's perception, whether of individual variables or of the resultant behaviour function F(**a**), by an amount δ. Allowing for errors of perception on both sides would require two parameters δ and ϵ. Hypotheses could be studied that related these errors to, say, the level of action.

(*b*) The distribution functions determining α, β, γ can be held constant during a run or varied as the run proceeds – e.g. the range of values widened (thus increasing the chance of freakish behaviour) in some relation to the level of action or the perception of the situation.

(*c*) Instead of universal dummies it may be found convenient to elaborate the model to provide alternative sets of dummies, if it is found that the patterns of behaviour of different nations are too various to be built up from the same basic elements.

(*d*) Instead of aiming simply to maximise its own payoff in classic game-theory fashion, a nation may be influenced by the size of the opponent's payoff. Suppose A is inclined to prefer outcomes that do not punish B too severely. This is catered for automatically by the decision process in cases where severe punishment of B would lead to a sharp reaction by B which would lower the probable scores all round, raise the level of action, etc. But in other cases one could study the effect of adopting a rule of behaviour that maximises not F(**a**) simply but F(**a**) plus some fraction of F(**b**), say F(**a**) + θ F(**b**) where θ is a sort of 'community coefficient'. If vindictiveness, or tendency to throw one's weight about, are to be studied, the same expression could be used with a negative value for θ.

To sum up, the inputs required for the programme are, for each of the two nations:

two major and two or four minor parameters determining

the probability distribution function that allots weights to the dummy decision-makers;
two parameters m, n determining the naïve deterrent effect (the bigger these are, the less inclined the nation will be to adopt a course that has even a modest risk of escalation);
possibly two other parameters, a perception error and a community coefficient;

and, before each move, a 'situation' in the form of a suitably evaluated tree of options. It would be an early target, when the model had been tried a little, to increase the number of nations. This would of course introduce a host of new structural complications, which I shall not attempt to enter into here. The account of TEMPER in Chapter 5 (pp. 100-4) indicates at least some of the many new variables that would have to be introduced.

Validation

I would guess that the first aim of any serious work on the development of this or any similar model would be to reduce the large amount of subjective scaling that has to be done in characterising the situation. In the example given above, some four probabilities and a matrix of 12 × 8 values of the basic variables have to be provided by the investigator before the computer can start. It should be possible to generate a large part of this, or at least to check it, by reference to objective data when some of the current data-collection and analysis programmes (see references in Chapter 2, esp. p. 13) are further advanced. The advantage of the model is that one *can* start to use it before that time, by taking the risk of using subjective scaling. But in the course of time it should be possible:

(i) to arrive at unambiguous definitions of the various levels of action;
(ii) to scale possible situations under 'national identity' and 'national security' – with enough examples from past history and imagination to make it difficult to put any future item in the wrong slot;

(iii) to do the same under 'national role', though this will be a much more complicated task;
(iv) to develop a precise rule for deriving the value of a_6 from the changes in the level of action foreseen in the sequence of events and from the nature of the events themselves;
(v) to build up a data bank of past crises as a basis for estimating in a current crisis the naïve probability of escalation to higher levels;
(vi) to use content analysis to cross-check perception levels and assumptions made about perception error.

In these ways (which are discussed at greater length in the body of Chapter 7) much of the evaluation of the situation will be removed from the area of dispute; but in the structuring of the tree of options, in the evaluation of the more unpredictable courses of action, and in applying stop-press information to correct the estimates of naïve probability of escalation, there will always remain the need for judgment – by the investigator in a simulation run as by the decison-maker in the real world.

The validation process would be a constant shuttling between theory-building and testing. I could imagine a sequence such as the following:

Construct a number of stock 'two-nation/situation' sets, some made up, some based on past history.
Run them through many times on the simplest version of the model, varying one parameter at a time, to get the feel of the parameters and find which are the sensitive ones, and what is the extent of chance variation that arises in practice.
Introduce model complications one by one.
Try out the model as a whole on a new situation to which it is not conditioned, and see whether the outcome is plausible. If not, back to the drawing-board.
Develop the 'calibration of the world' techniques listed at (i) to (vi) above, and test hypotheses linking the dispositional variables to hard data.
Have mixed man–computer runs with a written scenario, the teams being required to evaluate the situation, observe

the consequences as predicted by the computer, and comment. This would test both the calibration (do different teams produce the same evaluation of a given situation?) and the model itself (would the teams have acted in the way that the computer predicts?).

Develop a typology of model situations according to how they ride the model, and seek a corresponding typology in the real world.

Test the model in real time on a current situation as it develops, if possible as seen from inside a Foreign Office.

etc., etc., etc.

In conclusion, I should not be altogether surprised if for some reason not immediately apparent the model described here turned out to be unworkable, internally inconsistent or otherwise useless. Stranger things have happened. But I should be surprised if there was not to be found near-by a workable model not too unlike the one that I have outlined. Indeed there are probably many. The New Model Model has the advantage that the infinite complexity of cognition and communication within the decision-making system has been subsumed in a limited number of variables – not too many to be unmanageable, but enough to give a wide range of variation in behaviour. It uses the concept of the payoff in a down-to-earth way without ascending into the high realms of game theory. It is heavily conditioned by the military facts of life and the practical choice of options. It situates the ultimate deterrent in a haze of naïve probabilities which I think gives a better picture of how it operates on the minds of men than the over-precise calculations of Herman Kahn, remarkable exercises though these are in penetrating beyond the point where most people stop thinking. It can be used without benefit of huge organised systems of universal data on international affairs, but will greatly benefit from them as their results become available.

8 The Foresight Saga: A Dramatic Interlude

Author's Apology

It has been pointed out to me that nothing really happens in these two scenes – no bombs, no spies, no love interest – and that perhaps a better sub-title would be 'An Undramatic Interlude'. Moreover the international scenario is unimaginative and I have made no attempt to speculate on the changed nature of world politics and Britain's position in the world in 1986 – or whenever it is supposed to be. My only purpose is to suggest by illustration some of the difficulties of communication that must arise between the priests of a high cult – simulation – and the practitioners of political action who alone can provide the correct setting for the simulation.

SCENE I

The Foreign Secretary's room in the Foreign Office. The Secretary of State for Foreign Affairs, the Rt. Hon. Arthur Fox, M.P., is at his desk reading. At a long mahogany table sit two officials, one young, one old, conversing in low tones. They are Christopher Fly, the Private Secretary, and Sir James Crumble, Deputy Under-Secretary for Middle Eastern Affairs.

Crumble: Who else is coming?

Fly: Only three besides ourselves – it's terribly hush-hush at this stage and No. 10 have told us to be very strict. There'll be the Minister of State of course; the Head of the Foresight Office –

Crumble: Ha! The prophet Hezekiah himself!

Fly: The very same; and the Head of Middle Eastern Department, your Mr Smith.

Crumble: Hm. Thank goodness there's one sane person among us. Richard Smith knows a great deal more about the area than the rest of us, certainly more than your prophetic whiz-kids. Computers indeed! Palmerston and Grey must be turning in their graves.

Fly: Well, the S. of S. is ready to give the Foresight Office a fair crack of the whip. They did a good job on the Honduras case. And what he says is, if they make a boob it's plain for all to see, whereas if the trad. side of the Office makes a boob it somehow gets all wrapped up in the minuting on the file and there's nothing he can put his finger on.

Crumble (*suffusing slightly*): You're not suggesting that we tamper with the files? I should hope not. And another thing – I wish you would not encourage the Secretary of State in this habit of referring to the 'trad.' side of the Office, as if we were some sort of Old Time Jazz. Just because we use pen and ink, and don't converse exclusively in mathematical symbols. But of course if you rate a fellow according to how far and how often he sticks his neck out, then I grant you the Foresight Office ranks high. *We* have a little more respect for the uncertainties, and brief accordingly. (*Glancing at the S. of S.*) Has he read the papers? I hope you put our brief on top?

Fly (*primly*): I arrange the papers according to my Minister's instructions. As a matter of fact I think he's reading the Foresight Office brief at this moment – but you needn't worry, he reads them all.

Crumble: Hm. You'll let me have a copy of the Foresight brief, I hope?

Fly: Naturally. Here is a complete set (*hands Crumble papers*). I think I hear the others coming. (*He goes to the door and lets in Eustace Fogg, M.P., Minister of State; Sir Hesketh Brain, Head of the Foresight Office; and Richard Smith, Head of the Middle East Department.*) Would you please take your places round the table? The S. of S. will join you in a jiffy.

Fogg (ignoring Fly: as soon as he speaks it is clear why he is

known in Whitehall as Foghorn): My dear Arthur, you don't mean to say you're actually reading your briefs? I never read my briefs, it makes the meetings so much more exciting to pick the subject up as one goes along and follow the cut and thrust of debate –

Fox: Yes, I remember you cut and thrust to good effect at the Overseas Communication Committee last week: if what I hear is right you thrust in about seven words that weren't in your brief and as a result the Department's overseas information vote was cut by £350,000. Not bad going, that, £50,000 a word. I wish I got the same value for money.

Fogg (*not at all disconcerted*): You should not be so narrowly departmental, Arthur. After all, Bonzo also has his point of view.

Smith (*aside to Fly*): Who or what is Bonzo?

Fly: Alonso Boniface, Chancellor of the Exchequer.

Fogg (continuing): You see, I thought if I gave in gracefully on overseas information which we don't really mind too much about, then I should have his support over the level of diplomatic representation where we always have such a fearful battle. Rather subtle, I thought.

Fox: I'm afraid, Eustace, Chancellors are not renowned for that particular kind of gratitude. In any case, to return to the point, I like reading my briefs because they give me something to disagree with. The worst thing any office can do is to serve up a brief that one cannot take exception to: it adds nothing, and might as well not have been written.

Fogg (*impressed*): I say, that's rather good, Arthur. You should have it framed for all to ponder.

Fly: Excuse me, Secretary of State, but your meeting is ready for you.

Fox: Ah, to be sure. (*He collects up his papers and brings them over to chair the meeting at the mahogany table.*) Thank you, gentlemen, for coming at such short notice. You all know the latest position in the Middle East crisis. Russian troops are, have been for some time, in Lebanon and Syria. The Israelis have shot down a recce aircraft and apparently (though this is still not certain) the Russian pilot was killed. And this morning the President of the United States has issued a very stern statement that the integrity of Israel is a vital American interest. Cabinet is

meeting at eleven o'clock, half an hour from now, to consider what overt political action if any we ought to take and whether there is any behind-the-scenes role of mediator that we might usefully play and in particular whether our alleged skill in prediction could be put to good use.

Now I've read the briefs and very useful they are; but this could be a critical meeting and I shall need all your help in preparing for the difficult questions. First, of course, I shall be asked for my appreciation of the situation. The questions are, what are the Israelis up to, what are the Russians up to, and what are the Americans up to. By 'up to' I mean what their present intentions are, and how far we think they are prepared to escalate, to the extent that this is in their control. Sir James?

Crumble: I'll ask Smith to make the running on Israel and Russia, and chip in at the end on the United States.

Smith: Secretary of State, the Israeli position is clear-cut. They are ready to fight for their existence and I do not see any limit to what they might do. It is of course in their interest not to provoke any higher level of action than necessary, but their backs are to the wall and one cannot look for too much restraint. The Russians have in the last few years become steadily more and more of a protecting power in these Arab states and are now virtually in occupation, this being the only way they can give an adequate deterrent to the old-style Israeli punitive raid and at the same time keep the Arabs under control, who would otherwise be pillaging and foraying over the frontier almost every night. They have no wish whatever to escalate, but on the other hand they are bound to react in some way to the shooting down of their plane. On my reckoning they will now be contemplating a range of moves at a slightly raised temperature – around Force 2 on the U.N. scale.

Fox: And if I were to ask you what form that action might take?

Smith: Anybody's guess, Secretary of State, within that order of magnitude. We know the Russians use much the same scaling system as we do. I think I should put my money on a strong non-military move, economic as likely as not, backed up by the threat of military action if there

was another peep out of the Israelis.

Fox: Does this mean you are sure they are not contemplating anything more violent: any wholesale attack or desire to do harm to Israel?

Smith (*after a pause*): Yes, I'm quite sure about that.

Crumble: I'm sure you're right on that, Richard. Now as to the United States, we've had an intensive examination of their position in the Office this morning and I think the consensus is that their move was a very surprising one. The forcefulness of the President's statement took us all quite by surprise.

Fox (*Waits for a moment, expecting Crumble to say something more, but nothing comes*): Quite so. But having recovered from our surprise we now have to decide whether they mean what they say. Is it credible that they have suddenly overnight decided to commit themselves – something they have avoided doing for all these years – decided to give a firm commitment that they were ready to go to all lengths to defend Israel, up to and including a nuclear exchange? What I'm asking is, haven't they overshot the mark?

Crumble: What do you think, Richard?

Smith: Well, Secretary of State, I'm not an expert on the United States. But what strikes me is this. In Cuba in 1962 the U.S. gave a firm commitment to resist by all necessary means and they were able to make themselves believed – after a few days of great anxiety while the world held its breath. There were the successive crises over Berlin and Germany, and every time the U.S. stood firm and was believed: the Hong Kong débâcle in 1975 – I need hardly remind you of the uncomfortable posture both our Governments got into at that time – and then the Japan issue that has been with us since 1979 when the U.S. has been both firmly committed and entirely credible. Now the worrying thing about the position the Americans have just taken up is that Israel, although very much a concern of the U.S., is definitely not thought of as part of their super-empire – in the way that Berlin and Japan are – nor as part of the geographical area in which they are intensely interested, her security zone, so to speak – as Cuba was. So it seems as Sir James said highly surprising that she

should jump into Israel with both feet; and being so surprising, it is likely to be 'incredible' in the technical sense. And we have always regarded an incredible threat or commitment as one of the real sources of danger in the world. Not only in itself, I might add, but also because it will confuse counsel the next time the Americans want to strike a posture.

Crumble: Quite so. Just what I meant to imply in my earlier remarks, S. of S.

Fox: Has the Foresight Office anything to offer on this? There was nothing in your brief about the American position.

Brain: Quite honestly, no, Secretary of State. We have of course a whole series of indices of bargaining reputation for the U.S. over various kinds of situation – but as soon as you get up to the level where nuclear weapons are even being thought about, there aren't enough cases to go on and the special circumstances of the case deserve more weight than any index we could produce. And in any case there's the nuclear fuzz factor to put in, which means that any attempt at rational evaluation of options at this level has a greater margin of uncertainty than ever. What matters as Smith says is the credibility of the commitment, and in assessing this it is the symbolic weight of the last few cases that is all-important. If we are going to do any simulation runs this time, that will be one of the issues of political judgment on which we shall need the Cabinet's view.

Fox: So, nought out of ten for the Foresight Office, on this vital item?

Brain: Do you want me to rise to the bait, S. of S.? We're not universal soothsayers, you know.

Fox: No, I was only teasing. The conclusion I'm being driven to is that the Government has one useful role to play, and that is to rap the Americans smartly, in private, and see if we can't persuade them to cover up a little and withdraw from the very exposed place they've got themselves into. It might work: we stand high with them just now, by good fortune. I'll propose that. Perhaps the Department would be drafting something and pass it to me at No. 10 – Sir James? (*Crumble nods.*) Anything else on the President's message?

Brain: In anything we say to the Americans we might possibly add that, leaving aside the meaning of the President's message, its hostility content came out at 10·5 on the positive–negative affect scale that we use, and this is well above what we regard as the threshold for Middle Eastern situations in which the super-powers are concerned. We could make something of this. And we know also that the Russians measure these things on the Brest-Litovsk scale which would tend to give an even higher reading in this case.

Crumble: You'll give me a draft paragraph, Hez?

Brain: If the S. of S. wishes.

Fox: Yes, all grist to the mill. Though the U.S. Feedback and Control Agency will be perfectly well aware of this, and the fact that they haven't been able to make it register suggests that they are under a domestic cloud of some sort, in which case technical arguments will fall on deaf ears. We'll see what the colleagues say at Cabinet. Now let's see, my other main problem – I must go in a few minutes – is on the simulation inputs. I confess I am appalled, Sir Hesketh, at the suggestion in your brief that I should ask the Cabinet to assent to no less than twenty input values for what you prettily call the judgment-dependent variables. Are you aware what a Cabinet meeting is like? I assure you if I started in on your Twenty Questions programme I should very soon be switched off. Definitely a low TAM rating.

Brain: I'm sorry, S. of S., it wasn't my intention that you should formally ask your colleagues' approval of each one of the twenty inputs one by one. But as you know, the Cabinet have defined very specifically the class of political assumption that my office may not make without their authority. This is just a check-list; I think you will need the Cabinet's agreement to waive the rule if you feel that it's not necessary to bother them with all the details.

Fox: Then why the dickens . . . surely the Department can give us estimates of most of these?

Brain: We–ell (*he looks at Crumble, who says nothing*). (*Under his breath*) It wasn't for want of asking.

Fox: What's that? You two knights haven't been jousting again, have you?

Crumble: Of course not, S. of S., Sir Hesketh and I are on the best of terms. But naturally I would not presume to meddle in his *simulations* (*he says the word as though it were some unspeakable medieval practice*). I mean, we never have . . . . (*he tails off*)

Fox (*Gives him a long sad look*): Fly, please write on a large sheet of paper the words 'Knock Knights' Heads Together' and give it to me tomorrow morning. (*Embarrassed laughter.*) Now, joking apart, let us quickly decide what to do about these inputs. Sir James, your estimate of No. 1?

Crumble: Hm, ah, 'present degree of threat perceived by Israel'. Pretty high, after all she's surrounded. . . .

Fox: 6, Sir James? 7? on the usual scale of ten.

Crumble: I must say it seems meaningless to me to have to put these things into numbers. Do we really have to?. . . (*As no one else says anything*) Very well – 6.

Fogg (suddenly and loudly intervening): I do see Sir James's point, Arthur. I can imagine it wouldn't go down well in the constituencies if people thought we were deciding these matters by arithmetic as though we were playing bridge, instead of taking a suitably robust line in defence of British interests.

Fox: It will go down well enough if it works; and we shall get stick in any case if things go wrong. Ministers have accepted the principle of simulation of international processes as an aid to navigation these five years, and that much is public knowledge. I don't think we need worry too much about the grass roots of democracy, Eustace; the Government must expect to find them trailing a little from time to time. Now, No. 2. Smith, any views?

Smith: 10, Secretary of State. That's 'Soviet perception of Israel's bargaining reputation'. I don't think they are under any illusion.

Fox: No. 3?

Brain: May I suggest, S. of S., that most of these can be filled in objectively by the Department (*looking sharply at Crumble*) provided that you direct them to do so; but there are three that I think you will have to take on board yourself, and may wish to put to the Cabinet.

Fox: Why didn't you say so in the first place? Yes, I certainly so direct. Which are the three?

Brain: The last three in my list. One is 'labels for the tree of options'. This is the standard three-dimensional model that the Foreign Office and Ministry of Defence have been working on overnight, which will be on the Cabinet table, representing the physically possible chains of military action and reaction on each side. And we need from the Cabinet as usual any strong views they may have on likelihoods to correct the norms that we have put in, and – most important – their views on our estimates of perceived or intended degree of escalation of the conflict.

Fox: You mean I have to take the Cabinet through all the possible hoops?

Brain: It's not quite so bad as that, S. of S. The Defence Secretary will make the running on the military sequences. He will probably ask you to chip in on the main questions of political judgment. As you know, the model is a rather garish affair: threads of different colours trace out what seem to be the most likely courses of events, and the height of the thread above the table represents our estimate of the temperature, so to speak, that the conflict has reached. So you can quickly focus on the points where we've assumed that the temperature rises sharply and see if you agree.

The other two are the 'firmness of U.S. intentions' that we've already discussed and the 'status of the Grautsky conversation'.

Fogg: Oh, that's the fellow who had that extraordinary conversation with Lord Akenhead and said the Russians had taken a policy decision to exterminate Israel as soon as a pretext was offered, and woe betide anybody who tried to stand in their way! A likely story! I must say I'm surprised at them trying to hand us the old phonus bolonus like that at our time of life. Rather an insult to our intelligence, don't you think, Arthur?

Fox: Eustace, you are absolutely right; as you know I always – well, almost always – agree with you, and this is one of those occasions. The Grautsky story has all the marks of an attempt to divide counsel and weaken resolve, and suggests to me that the Russians have, on the contrary, set clear limits to their action, as one would expect. (*Looks up at Fly, who is hovering around making pantomime gestures*

at his watch.) I get the impression that it is time to go. Thank you, gentlemen; we will assemble again after Cabinet. (*He hurries off with Fly in a flurry of papers. The others disperse, murmuring, to their offices.*)

SCENE II

The Foreign Secretary's room again, two hours later. The Foreign Secretary is just returning from Cabinet. The others are standing around waiting for him.

Fox: Right ho, gentlemen, let's sit down and see what there is to do. (*They seat themselves as before round the table.*) The message to the Americans was agreed – the draft was most timely, thank you, and was accepted with a few very small changes. (*To Brain*) We decided to keep in your hostility bit. (*To Fly*) Would you see that it goes off right away?

Fly: May I have your copy, Secretary of State, if that is the agreed text? (*He obtains it and leaves the room.*)

Fox: The rest of the meeting was not too satisfactory, for reasons that I'll explain. One or two colleagues who shall be nameless seem to think the U.K. has miraculous powers in the Middle East and wanted us to be horsing around all over the place. But the Defence Secretary was sound and between us we managed to avoid the larger lunacies. The options tree went across quite well – incidentally the Foresight Office really lived up to their name by giving that briefing to Ministers last month; this meant there were enough people at Cabinet who had seen that sort of thing fairly recently and felt they knew all about it. Otherwise we should have had to start from square one and might well not have got the guidance we needed on the political inputs for the simulation. As it is, here they are. (*He hands Brain a sheet of figures.*) We agreed that there remained a big uncertainty in the U.S. intentions, which indeed we hope to influence, and that we must try at least three well-spaced values of that parameter to span the range. That all right so far?

Brain: Quite all right. There would have to be about a dozen

runs for each value, so as to get a good spread of probability. That will not be unmanageable. (*He makes some notes.*)

Fox: The bad news is that we have to consider a range of situations in which China intervenes.

Crumble, *Brain*: (*in unison*): China!

Fly (*who has returned*): You'd have thought China had enough on her hands, with both her southern and south-eastern borders in chronic uproar.

Fox: I know, I did my best to resist, but to no avail. The Cabinet think these simulations are a nice toy and they want to put their penny in and watch the wheels go round.

Brain (his head in his hands): But this is awful, S. of S. The structure of the simulation would be completely different and we should have to use one of the P.T.P. models. . .

Fogg: What's a P.T.P. model when it's at home?

Brain: Sorry, Minister: a Powerful Third Party model. Up till now we've had the situation modelled as essentially between two powers, Israel and the Soviet Union. Of course other nations are involved but no military intervention by them is seriously envisaged in the usual model. Then of course there was the American statement this morning, which was bad enough – and now this! Oh dear, oh dear! Of course one can't foresee everything, but it does seem reasonable to assume that the political inputs will be consistent with, er, . . .

Fox: With common sense, were you about to say? I don't wish to pull rank, Sir Hesketh, but the requirement in fact is that the inputs should be consistent with the political realities of the time. I must ask you to reflect that, in discerning those realities, your highest authority is the Cabinet, to which the Foresight Office must, from time to time, defer. I may have been a little too keen to apologise just now, out of consideration for what I know is going to be a stiff twenty-four hours for your office – but damn it all, why should I apologise for the Cabinet?

Brain: Touché, S. of S. We guessed wrong, that was all. (*He is the picture of gloom.*)

Fox: Now, don't let's get maudlin about it. Here we are, with these new inputs, and the question is can the Foresight

Office take the job on? Because if not I shall have to go back to the P.M.

Brain (*after thought*): I think we can take it on. But it will be a crude effort if we are not to hold up the main simulation on the model that we've got wired up already. May I have your authority to go ahead on that basis?

Fox: Well, the Cabinet will want results. What do you mean by a crude effort?

Brain: I'm thinking aloud, but firstly we shall have to make a simplifying assumption about the United States: in other words China will become the chief variable 'third party'. Presumably we take a middle position for the U.S., and assume that she retreats from her present extravagant posture but retains an active interest in the dispute.

Fox: Yes, I think that's acceptable.

Brain: Secondly, there won't be time to do the full range of exploration of the model. I don't want to get too technical, but as you know from our briefings, S. of S., there are three internal parameters that occupy key positions in the P.T.P. model and which can be very sensitive: we should normally want to take ten readings of each, making 10 × 10 × 10 = 1000 runs. Taking account of checks and print-outs and interpreting the results, one has to allow a minute to a run on the average. . .

Fogg (*reviving*): A run a minute, eh? Not bad going on a sticky wicket. (*He and Crumble have a private chuckle.*)

Brain: . . . and that's just too many minutes between now and tomorrow. We could either fix one of the parameters – make it a fixed-international job for example – and bring down the number of runs to 10 × 10, or we could make it coarse-mesh throughout and take, say five values of each parameter. 5 × 5 × 5 we could just about manage – yes, about two hours . . . (*scribbling as he talks*) or let's see we could compromise on the third parameter. Hm, 6 × 6 × 3 . . . (*He loses himself in calculation.*)

Crumble (*Clears his throat loudly*): H'rm!

Brain (*Looks up and sees everyone is waiting for him*): Er, oh, sorry, S. of S., I was just trying to make up my mind whether. . . . (*pulling himself together*) The proposal I would make, sir, is to throw a coarse net into the water and hope that the behaviour-shapes are fish of a decent

size. What I am trying to say is, we might be unlucky and find that an area of variation of critical interest occurred inside one of our coarse-scale intervals: if so the simulation will be much less informative than we should like. But if we did the other thing, and narrowed the ranges of the parameters – or even went so far as to fix one of them, which we sometimes do when we're fairly confident of it – then again we might be unlucky and in that case the results would be not just less informative, but quite useless. So I recommend coarse fishing. (*He nods his head, internally approving his own recommendation, as if he had just heard it for the first time. The others all look blank.*)

Fox (*after long silence*): I have a feeling the meeting is expecting me to say something.

Brain: To have your agreement to go ahead with the supplementary runs on the lines I suggested, S. of S.

Fox (*tentatively*): Coarse fishing?

Brain: Coarse fishing.

Fox: I don't think we can take the matter further round this table. I'm prepared to go along with your proposal, unless anyone else wishes to comment. Sir James? Smith? Fly? – Oh, I'm sorry Eustace?

Fogg: I've been doing my best to follow (*almost audible groan from Fly*) and I was wondering how we are going to explain these assumptions that we're being asked to make to the Cabinet. The shapes of behavioural fish may be all very well for those accustomed to the jargon, but I would like to ask – Can Sir Hesketh put what he has just said about his internal parameters into terms of a political choice?

Brain (*Rubs his brow thoughtfully, and is raising his head portentously to speak when he sees Fly grimacing hideously and shaking his head across the table, unseen by the two Ministers*): I – er, N–no, I wish I could but I don't think I can, Minister. We'll try to work something out – a written explanation of the limitations – and put it up with the results of the simulation in the morning.

Fogg (*greatly relieved*): In that case, Arthur, I think we must agree with you and leave it to the experts. (*Sharp release of breath by Fly.*)

Fox: Fine. Well, there we are, gentlemen. A hard day's night

for some of us, I fear. (*Fly whispers something in his ear, smiling.*) My Private Secretary says that remark dates me. Never mind, we all have our weaknesses. Thank you. (*The meeting breaks up into knots. Fox goes back to his desk with Fogg, deep in conversation.*)

Brain (*in an undertone, cheerfully*): Abortionist!

Fly: Thank you. (*Looks modestly at the floor.*) Or should I be offended?

Brain: Just a statement of fact. I had conceived a splendid brainchild for Fogg's benefit, really quite a neat way of answering his question; and you caused it to be aborted by making one of the most unpleasant grimaces that it has ever been my misfortune to see, in all my twenty years in the Office. This sort of thing *can* be explained in political terms, you know, ultimately, and it's terribly important to put this across to Ministers and not let them believe it's some inexplicable sorcery. Very likely the Minister will now go off and tell his cronies in the House that our work has no relation to political reality. I can just imagine him giving evidence to the Select Committee on Political Techniques. . . . (*He assumes a Foghorn voice and manner.*) 'I have it on the authority of none other than the Head of the Foresight Office himself – yes, I put the question to him personally – that some of the assumptions that have to be made to set up the internal structure of the model *cannot* be expressed in political terms. This only bears out my contention that all this model-building and simulation is so much balderdash and flapdoodle. It's un-British, it's unethical, it's un-Parliamentary, to represent one's allies – yes, and one's own beloved country – as though they were so many rows of cyphers and digits. . .', etcetera, etcetera, etcetera. And before we know where we are all the old shibboleths will be trotted out and our Estimates will be attacked – and all the good work of the last five years will have been undone.

Fly (*looking anxiously round at the Ministers, who still have their heads together*): Sh! I had no idea you were such a mimic. But seriously, I don't think Fogg will take it that way. He was longing for an excuse to bring the meeting to an end: he has several hot political potatoes to discuss with the S. of S. – as you can see. I'm sure he won't have taken

down what you said and use it in evidence against you.

Brain (ruefully): I sincerely hope you're right. Well, off to the coalface.

Crumble: Good luck with Ernie. (*Brain goes. Crumble makes for the door.*)

Smith (*Suddenly, producing a piece of paper from his pocket*): My God! I nearly forgot! The Redhot Rejected Options Routine – oughtn't we to do it now while we're still together?

Crumble: Ugh. I suppose so.

Fly: Heavens, yes. The Secretary of the Cabinet has been heard to say that he would personally shoot at dawn any Private Secretary guilty of omission, I mean of not tying people down to recording the rejected options at the moment of decision. We're explicitly advised not to allow people to disperse and write it all up, because of the mysterious differences that creep in between making a decision and explaining it afterwards.

Smith (*reading*): 'Options for action by Her Majesty's Government which have been specifically considered and rejected'. Well, those are courses (*a*), (*b*), (*c*) and (*d*) in the Foreign Office brief, I suppose: in other words, no action at all; economic measures; stopping arms supplies; and an initiative in the U.N. to get the Russians to withdraw from the Arab states.

Crumble: I must say this routine always strikes me as a crashing waste of time. As if it wasn't blindingly obvious what the rejected options are. You've only to look at the minutes on the file.

Smith: The only other conceivable option, I suppose, though we didn't even bother to mention it in our brief, would be a military gesture of some kind by the U.K.

Crumble: What? Thirty years after Suez? Don't be ridiculous. That wouldn't be a starter.

Fox (*who has been eavesdropping, chips in quietly*): Military intervention by the U.K. was considered and rejected by the Cabinet this morning.

Crumble: Good God! You don't say! Excuse me, Secretary of State, but that's the most astonishing thing I've heard in years. Intervention on what basis, might I ask?

Fox: Of course not a major military adventure. But one or

two of the colleagues have quixotic ideas about a return to intervention on a world scale by H.M.G.: that being a second-class power gives us a licence to make gestures of a kind that a super-power could not afford to make.

Crumble: But, S. of S., what effect is looked for from such gestures?

Fox: The same, I suppose, as the effect of someone quite unimportant lying down in front of the Prime Minister's car – a token of the exasperation of the man in the street with national policies over which he has no control and which may have a profound effect on him. And the car does stop – at least for a moment while the body is moved out of the way. On the international scene it would be a similar stylised gesture by the nation-in-the-street exasperated at the international policies of other powers over which they have no control but which may lead to the incineration of all of us. So what do we actually do, you will ask? We lie down in front of the Russian, or the American, or the Israeli juggernaut, whichever seems likely to roll – or perhaps we just lie down in the street. This complicates the consequences, and may change the payoffs for the principal parties – and, who knows, might even affect the course of events. That's the general theory.

Crumble: Well I'm . . . – I said just now, thirty years after Suez. What I should have said was, six hundred years after the Crusades.

Fox: Relax, Sir James, you forget I am describing a rejected option, for the benefit of your Cabinet Office questionnaire. Of course I agree with you that the idea was a wild one; and it was loudly shouted down with me doing a fair amount of the shouting. But I think Smith must certainly put it in his little list. (*He rummages among papers and picks one out.*) If you don't believe me, here it is in my notes.

Fly: I didn't know you kept notes, Secretary of State.

Fox: I shan't be a Minister for ever, you know. Must be able to turn an honest penny writing my memoirs when I'm out of office.

Fly: But you'll not be able to put that sort of thing in your memoirs? I mean, the thirty-year rule . . .

Fox (*as to a child*): There, there, Christopher, of course I

shan't. But if I didn't keep notes I shouldn't know what not to put in, should I? (*He has returned to his desk; the others leave.*) What's my next appointment?

Fly: Here's the programme for the afternoon. Your first appointment after lunch is at 2.45 and then it's P.M.'s Questions in the House. (*He sets a large tray of files and papers before him.*)

Fox: Right ho. No telephone calls or interruptions for half an hour, then I'm off to lunch.

(As the curtain falls, the Secretary of State is starting to work methodically through his in-tray.)

9 Conclusion

The science of international relations is a rudimentary science. The attempt to describe, let alone explain, in a systematic manner the ever-changing international scene has baffled mankind down the ages and will continue to baffle them for ages to come, if there are ages to come. But in the world today the wages of error is death, and to discard any possibility of better understanding would be inexcusable. That is why even unlikely routes to better understanding are worth exploring. It has been the aim of this book to familiarise the reader with some of the modern scientific and quantitative approaches and to suggest that they are perhaps not quite so unlikely or unpromising as might be thought at first sight.

The enigma of international relations, as presented here, is a challenge alike to mathematicians and statisticians, to political scientists and historians, and to governments. I have argued that there is room for advances in mathematics and statistics as a contribution to international relations theory; but the mathematically inclined political scientist, while being encouraged in his inclination, ought at the same time to bear in mind Charles McClelland's dictum: '. . . one must have a firm grasp on descriptive history in order to develop a realistic setting for subsequent quantitative analysis' (McClelland et al., 1965). On the other hand the historian who is ignorant of the possibilities of modern statistical analysis and deaf to mathematics must realise that he is cutting himself off from one of the advancing fronts of his subject. There is a need for much intermingling of the two ancient disciplines of mathematics and history, traditionally so remote from one another and even now only dimly aware that they can make common cause together.

The new approaches are naturally regarded with extreme scepticism by the professional decision-maker. At worst the

science of international relations is denounced as an absurdity, a contradiction in terms. At best it is seen as an expensive and pretentious way of arriving at the same results and the same uncertainties to which experience and practical wisdom lead. And being an infant science there is no difficulty in proving how little it has to show by way of results – that is, of tested theoretical propositions that are of practical use in increasing the decision-maker's understanding of international affairs. Why, therefore, should busy men be expected to have the patience to read a lot of strange stuff and acquire unfamiliar skills to so little apparent profit?

These are the very reasons why there is a challenge also to governments, especially to Foreign Offices. The first need is to keep an open mind – no small matter in a field that one regards as one's own professional preserve to which one has devoted all one's talents, perhaps for many years. The second need is to keep up with the hunt – to know what is published in the technical periodicals and to be able to communicate with the theorist on his own ground. But more than this, it is surely incumbent upon governments to find funds for academic research in fields related to national interests, even where the prospects of an immediate return in applicable results are small. And finally, if there were to be any successful development of theory along the lines suggested in Chapters 7 and 8, there would have to be a very great organisational effort in absorbing the new methods into the decision-making structure. This is many years off, but it may not be too soon to start thinking whether there are any unobtrusive changes that could be made now which, beneficial in themselves, would also make less violent any subsequent reorganisation. But however well the ground is prepared, the task of bringing everyone concerned up to the pitch of acceptance of new methods shown by the extremely well-trained characters in Chapter 8 will be a herculean one, and it would be foolish to suggest otherwise.

One 'unobtrusive change' that is worth mentioning on its own account is the day-by-day recording of situations in which the Government has been faced with a choice of actions. It is as certain as anything can be that to be useful as case histories for subsequent scientific analysis such records should include an untouched account of how the situation

looked to the decision-makers at the time: particularly, what the practical options were then seen to be and what moves then seemed open to the other parties to the situation. It is not so important to have a record of the reasons justifying the Government's decision as to know what the field of choice was reckoned to be, i.e. what were the expectations on the strength of which one course was adopted, and what were the options and expectations that were explicitly rejected. Present-day Foreign Office minuting no doubt goes a long way towards fulfilling the requirement at the official level, but there would not normally be any way of knowing whether the Government, at the political level, had endorsed all the analysis or had merely endorsed the conclusion. That is the point of the incident at the end of Chapter 8.

Is it fanciful to see, in the task of reconciling traditional foreign affairs practice with the use of aids based on scientific analysis, a problem of human engineering peculiarly suited to the British genius? In this field it will not be the biggest computer that wins the day, but the team with the best working understanding with their mechanical aids, those who are best at cutting corners, improvising hypotheses and arriving at a workable compromise between logic and common sense, those who have made most progress with the organisational conundrum of pooling the brainpower of academic theorists, the pragmatic power of decision-makers, and the machine-power of computers. It may well prove to be more difficult for a nation like the United States, with its almost universal interests, to build up an objective picture of the world than for one like Britain with a long tradition of wide-ranging diplomacy but a more restricted current area of direct national interest.

If there is anything in this last argument, perhaps we should pursue it to its logical conclusion: that those best placed to explain and predict international behaviour are those with no national interests at all. If so, we can look forward to the emergence of a class of international relations consultants and research foundations, independent of national governments but ready to offer advice and predictions to all for a fee; ready also, no doubt, to advertise their skills by publishing international indices just as the opinion polls, the reports of the Economist Intelligence Unit and the

Dow-Jones index are published today. It may be a few years before we hear of International Factors Inc., or Universal Conflict Situations Ltd, and in the meantime we can expect Swiss banks, Greek shipowners and other international business networks to develop research units in their own interests to take advantage of any new theoretical developments that show any promise of predictive power. After all, the outlay is not great: with the help of research toys such as the model outlined in the Appendix to Chapter 7, a small research team with access to a computer and with the skills necessary for historical casework and model-building might be able to make valuable progress.

However the race may be run as between government-sponsored and private research, it will be through the action of governments, if at all, that mankind will benefit from any better understanding of human affairs that a scientific approach may bring. I should like to leave the last word with Harold Guetzkow, one of the most influential teachers and writers in the field, who has said (Guetzkow, 1968) that if the use of quantitative methods and scientific analysis were to bring about an improvement of five per cent in the performance of nations in their relations with one another, he for one would be well pleased.

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