

An Agenda for Comparative Government*

Jean Blondel, European University Institute

The study of comparative government is in a state of relatively limited development. Questions are still being raised about the specificity of the subject matter and of the methodology; the enthusiasm with which comparative political scientists went into grand models in the 1960s and early 1970s has been followed by a degree of scepticism. This scepticism is not justified. It was imprudent to move so quickly towards grand models, while what was critical for the development of comparative government was and continues to be twofold. First, there has to be a substantial improvement in our knowledge of political life all over the world; this has begun to occur, but empirical studies must be pursued more systematically and in a true comparative manner. Second, the major theoretical thrust has to relate to the understanding of structures, which are the key elements in the life of political systems; this means in particular that we have to understand systematically and dynamically such questions as institutionalization, integration, legitimacy, as well as the relationship between values and structures. If this were to occur, as it can, and if empirical studies develop at a rapid pace in parallel, there is no reason to be despondent about the future of comparative government.

I have often wondered what feelings one must have when one belongs to a discipline whose boundaries are clear, which has a well-defined and well-articulated body of theory, and in which the methodology arouses little controversy and does not raise difficult choices. Perhaps no discipline has such characteristics; but some seem to come closer to them than others. In the many moments when one wishes to be able to advance more regularly and more swiftly, it has more than once occurred to me that it would be pleasant if political science, and the study of comparative government in particular, could benefit a little from some of the advantages which other disciplines seem to have in terms of their boundaries, their underlying theory, or their methodology.

It is true, on the other hand, that some sense of security can be drawn from the fact that comparative government is concerned with topics which are intriguing and important. It is fascinating to examine the many ways in which mankind is ruled and to be involved in assessing the extent to which successful efforts are made to 'improve' – to improve, that is, in the minds

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of those who make these efforts – the characteristics of governments as well as, unfortunately, the extent to which these efforts fail or are thwarted. It is also true that some sense of security can be drawn from the fact that these observations have fascinated some of the better minds across the centuries although, up to very recently, the number of those involved in studying comparative government had remained rather small; but it is none the less reassuring to try and follow the direction which had been taken by Aristotle, by Hobbes, by Montesquieu, by Rousseau, by Tocqueville, even if the preoccupations of Hobbes and Rousseau extended well beyond the study of comparative government and even if the preoccupations of Aristotle extended well beyond political science in general. In the more recent period, the constitutional lawyers of the nineteenth century and the founders of modern comparative government, Bryce and Lowell, also provide both guidance and a sense that one belongs, to an extent at least, to an illustrious family of scholars.

Yet it remains true that the study of comparative government raises in the minds of scholars many serious questions about the specificity of the subject-matter and the ways in which this subject-matter can best be handled. It is not so long ago that Macridis stated that the study of comparative government was non-comparative and parochial; it is not so long ago either that Holt and Turner complained that comparative government was not a field of study *per se*, that it was a method, an approach, since much, perhaps all, that we study in social science is in essence comparative. One has to remember that the study of comparative government emerged in universities and colleges primarily to find a way of studying 'foreign' governments, as it seemed unreasonable – 'uncultured' – not to know at least 'something' about governments other than one's own. So that it seems sometimes that the study of comparative government resembles the study of comparative literature: a field of study which has developed out of a feeling of dissatisfaction resulting from the parochial manner in which government was studied when it was restricted to the examination of the processes of politics in one's own country.

While this makes for a worthy effort, while it makes a field, or sub-field, important as a subject for teaching, it does not make for a coherent, intellectually satisfying discipline or sub-discipline. It seemed that either comparative government had to develop its own intellectual foundations or become a part of political science without any pretensions at being *sui generis*; and if it were to be reduced to the level of a methodological approach, as Holt and Turner seemed to suggest, it would also have to improve its tools, make its methodology tighter, as these authors indeed advise. By and large, those who had dabbled in comparative government had been amateurs, so it seems: they had to become professionals or turn over their subject to those who were professionals.

To some extent at least, the history of the last quarter of a century, as

far as comparative government is concerned, has been that of an attempt at seeing how comparative government could be, so to speak, professionalised. It is a history based on a series of attempts at straightening the subject-matter, at improving the methodology and, perhaps above all, at discovering the theoretical underpinnings on which a more robust 'science' of comparative government could be based. These attempts have been worthwhile; they have unquestionably improved the scholarly and technical character of the work. But they have not been fully successful – indeed some might say that they have been very limited in that, by and large, the problems which existed in the 1950s continue to loom very large. Theory in the field is partial and general theory is so contested that it has not been able to direct the activities of most scholars. Methodology has improved, but it remains varied; indeed, which method is being used often seems to depend on the circumstances of the case.

Yet, meanwhile, studies of comparative government have flourished in many directions. There have been probably many more works than ever before, not just on 'foreign governments' but on comparative government in the strict sense, that is to say looking at a particular aspect of government in a number, indeed a large number of countries: groups, parties, legislatures, executives and in particular the military, bureaucracies, outputs of governments have begun to be studied systematically; so have problems of stability and instability, of consensus and conflict, of development, and, perhaps most important of all, of the role of government – does it make a difference or not? Whether or not there is a subject-matter of comparative government in the theoretical sense of the word, that is to say a discipline defined by a problem rather than by a series of objects which need to be studied, there is now a vast body of literature and this body of literature has increased our knowledge of how governments behave; it has also enabled us to relate much better than in the past the characteristics of government to the social and economic structure and to begin to discover the internal dynamics of governmental arrangements.

Yet the question still arises perhaps as strongly as in the past: what precisely is the study of comparative government as an academic discipline? Is it really a field, a sub-set of political science, to be sure, but with special theoretical characteristics which would suggest particular problems and a specific methodology? If this is not the case, why is it so resilient and why can it not be integrated easily within the main body of political science? In order to give some answers to these questions, it is worth exploring first, even if briefly, the efforts which have been made to build theories designed to help to give a general structure to comparative government in order, in the second place, to ponder about the limitations of these efforts. One will then be able to see more clearly what needs to be done to ensure that comparative government progresses in such a way that theory and empirical findings are linked in an integrated manner.

I

Comparative government has flourished in the last quarter of a century, but it has not flourished in the way many predicted around 1960. It was then widely expected that the future of the sub-discipline would depend on the elaboration, first, and the gradual application to comparative analyses, second, of a 'grand theory' which would provide the framework which would organise and structure what was viewed widely as having been hitherto a set of dispersed, unconnected observations about a number of problems of government. The first substantial move in this direction was constituted by structural-functionalism, which was thought for a while to be able to provide a truly satisfactory basis for the analysis of all governments, liberal and authoritarian, capitalist and socialist, by means of 'functions', which all political systems were alleged to fulfil.

In fact, structural-functionalism, as it was first devised by Almond and Coleman in 1960, was primarily a taxonomy designed to replace other taxonomies which, under the umbrella of constitutional law, had helped to classify the 'modern' political systems of the time; but the liberal constitutional model on which this classification was based was no longer applicable in a world in which authoritarian and totalitarian regimes, from the 1920s onwards, had been on the ascendant and, even less, in a world in which, from the 1950s, developing countries were searching for new governmental arrangements: the arrival of these new countries on the scene confirmed the demise of the universalistic pretensions of the constitutional model. This is why Macridis could note the disarray within comparative government: there was no longer an overall framework which, it seemed, could realistically cover all types of political systems; comparative government was, no longer truly comparative.

It is at this point that structural-functionalism seemed to provide an answer to the need for a universal taxonomy. The model was based on the shrewd recognition that all political systems had structures and that these structures had some effects – or could be expected to have some effects: they were involved in some operations which presumably resulted in some accomplishments. In fact, this approach was in many ways only an amendment to the old constitutional framework, but an amendment which generalised this framework beyond liberal constitutional countries. In the constitutional model, bodies such as legislatures or executives had 'powers'; in the structural-functional model, bodies such as tribes, associations, parties, but also legislatures or executives had 'functions'. A solution to the difficulty seemed to have been found.

There were, however, three major problems, two internal to the model and the third concerned with the overall aim of the taxonomy. The first internal problem resulted from the fact that a major change had occurred as one moved from the constitutional taxonomy to the structural-functional

scheme: in the first case, 'bodies' had 'powers' in some objective sense, at least to the extent that the constitution was applied, as the construction stated what these powers were and observers could note them. In the structural-functional model, the structures could perhaps 'objectively' be pointed to – tribes, associations, parties, and the like had an 'existence'; but the 'functions' had to be discovered through an analytical operation involving the scholars themselves. This meant at least some room for argument as to what these functions might be.

Second, moreover, that taxonomy could only become operational after a long and detailed empirical inquiry had taken place, by which the 'functions' of each structure would be 'discovered' by the observers. Hence it seemed that the model did not constitute perhaps such an advance after all: it did not by-pass the problem of having to examine with considerable care, and through a lengthy operation, what specific structures in each country did or did not do. One might state in principle what the functions of some structure could be; but one would not be able to relate a given structure to specific functions unless one had found out what the functions happen in practice to be. If such an empirical investigation was not undertaken, the taxonomy would remain an empty shell: Almond must have realised the enormity of the task, as little was done by him to operationalise the model.

What he did try to do, on the contrary, was to endeavour to meet the third difficulty, which was not empirical but conceptual. For the model was attacked for being fundamentally static – and indeed for being also based on a Western conception of political systems. As a matter of fact, it was neutral in terms of being pro- or anti-Western; but because it was merely a taxonomy, it was indeed static. It had no dynamics – that is to say, it had no explanatory value of future events in terms of past situations; it merely helped to describe (with the difficulties which we mentioned) the effects which structures may have in various situations. But Almond wanted to do more, as indeed others who were concerned to elaborate a general framework. They wanted to move from a taxonomy to a general theory which would account for the ways in which governmental systems 'evolved' or 'developed' in various types of societies. In order to achieve this goal, a taxonomy was not sufficient: an 'engine' of evolution had to be found which could be shown to provide an explanation for the characteristics of political systems.

Hence the second wave of general theories, which of course had had antecedents in nineteenth-century sociology, but which were modified and modernised in particular in order to take account of developing countries. These theories did indeed, to a greater or lesser extent, attempt to tackle the problem of the reasons for the evolution of societies, although their individual aims were different and their ideological underpinnings were sometimes diametrically opposed. Almond and other system theorists

attempted to explain political change in terms of the ability of political systems to endure and overcome conflict, stability being often the best goal which could be achieved; others started, on the contrary, from the premise that societies went through a number of successive stages and the moves from one stage to the next were ascribed to the effect of forces in conflict within society. 'Explanations' of the broad characteristics of political systems were given in this way: a framework (or different competing frameworks) was elaborated thanks to which it seemed possible to understand how societies were characterised by a particular type of regime.

Yet, while these theories provided an explanatory framework, they did not provide, any more than Almond's earlier taxonomy, the specific tools which were needed to relate the stages of the evolution of societies to the arrangements of political systems or, if they did so, this was in such a broad and general manner that it was not possible to account for the specific characteristics of a given political system by means of these theories. Moreover, the problem was further complicated by the more general question of the relevance of political arrangements, as it seemed often to be believed that many such arrangements were to some extent interchangeable from the point of view of the global characteristics of the society. This was logical, as these theories were in essence concerned with the evolution of society at large and aimed therefore at explaining how social (or socio-economic) change occurred and how it might impinge generally on political life; but this was not sufficient if what needed to be accounted for were the detailed arrangements of the political system.

It may of course be that detailed political arrangements are of little consequence from the points of view of the society at large: but this is a hypothesis and a hypothesis which needs to be thoroughly tested before it can be adopted, even in principle. This means that, even if political arrangements and political configurations are viewed exclusively as dependent variables, they must be described and classified in order that we be able to find out whether, indeed, they are, first, a dependent variable and, second, interchangeable. Thus the emergence of a developmental theory did not provide a substitute, any more than structural-functionalism could, to the detailed examination of political arrangements in various societies. The study of comparative government remains therefore necessary, whatever philosophy of society one might proclaim. What seemed to be the case was that the models – whether taxonomic or explanatory – remained above the specific characteristics of political systems and consequently 'above' the problems which comparative government had to handle, namely the problem of comparing governments in order to assess how similar or different they are, for instance given different types of social or socio-economic systems.

There was yet another general approach which could be viewed as a means of solving, but in a very different way, the problems of comparative

government analyses. If a general theory based on a global societal approach had many limitations, perhaps the reason was that one attempted too quickly to look at these global aspects while forgetting that, in one sense at least, the engine of political life is at the micro level, that of individuals, at least of those individuals who are influential in the political system. Theories of the social contract were based on individual behaviour: while an individualistic basis may have been discredited in political science and sociology, it continued to play a major part in economics and it seemed to have helped to produce a strong, contested perhaps, but none the less powerful, basis for the development of that discipline. If individuals played a part in political life, perhaps it would be valuable to see whether, correspondingly, a political theory based on individuals could help to account for collective decision-making.

It is of course true that individuals are in large part constrained by the situation in which they find themselves, by the problems which they have to solve, and indeed by the institutions which make certain modes of action available and close other avenues. But individuals can also affect situations, solve problems, indeed set up institutions, or at least so it seems; and, ostensibly at least, politics can be markedly shaped by the decisions of some leaders. As a matter of fact, again perhaps on the surface but surely in a truly concrete manner, the decisions of individual politicians and of groups of politicians are the stuff of political life: a general theory cannot simply by-pass the problem.

Naturally enough, therefore, the most appropriate approach to the study of comparative government would be one which would put individual decision-making squarely at the centre of the analysis. This is probably why models borrowed from economics seemed to many to provide a markedly more satisfactory answer than sociological frameworks. The aim was to describe politics, in the first instance, by coming to collective decision-making by way of individual preference patterns and to build up gradually from this core. While such an approach is not specifically comparative in its origin, it might be expected to by-pass, in a sense, the comparative issue as, if successful, it would explain politics from the universal standpoint of decision-making by means of increasingly complex models which would relate to larger and larger decisions. Moreover, the model would be fundamentally dynamic while some sociological approaches, and structural-functionalism in particular, were not. The dynamics would come from the very fact that collective decisions have to be taken: the different preferences of individuals would lead to efforts made by individuals to come to a joint conclusion, this conclusion being a change over the previous situation. Thus, if it were possible to build a whole model of political life by gradually aggregating preferences of individuals, first within a small group context, and, ultimately, over the whole polity, it might be possible to provide a satisfactory theory of the political process which might account for the

realities of individual decision-making while also explaining the diversity of outcomes in various types of regimes.

The idea of applying economic theory to political life is thus intrinsically attractive; some developments were indeed successful and provided interesting insights into decision-making, for instance in the realm of small groups or in some aspects of mass political behaviour. But economic theory has not so far provided a truly general model and it is unlikely to be able to provide one. First, the analysis has to wrestle with the complexities of the preferences of individuals in the political arena: these preferences cannot easily be ranked or even broadly compared, as they can be in the economic field, where money is the common currency and where profit is (assumed to be) the overriding motive. Second, inequalities between participants in the political game make it often impossible to find a satisfactory theoretical solution to the decision problems. Micro-economic theories applied to political life have always tended to be more powerful when the political relationships which are analysed are based on bargaining among equals or near-equals than when they are based on various forms of authority, power, or coercion.

Most important of all, however, micro-economic theories seem unable to provide satisfactory means by which to move from small group decision-making analysis to the analysis of whole political systems. They can of course enable us to study the relationship between groups, provided they are treated as individuals. But the theories do not account satisfactorily for the reasons why groups exist at all: decision-making analysis of this type has little to say about the permanent or near-permanent allegiance which individuals have for groups. The inability of these models to provide a strong basis for the existence and maintenance of groups has often been noticed, whether in the context of voting (since it is hardly rational for almost any individual to participate at general elections) or in the context of associations (since it is so often more rational to be a 'free-rider'). Micro-economic models have thus had to posit the existence of the State as an historical and extraneous accident, as they could not deduce it as a logical consequence of the preferences of individuals faced by the problem of collective action.

This drawback is not only serious in itself: it is serious also inasmuch as it makes it impossible to derive a model of comparative government from micro-economic theories. These are bound to limit their explanations to situations which occur within a given institutional framework, not across institutional frameworks. They may explain to some extent how individuals are likely to behave, especially where bargaining is the main mode of operation; but they do not provide a basis for comparing different types of institutional arrangements and configurations. While 'grand' sociological theories could be said to stand too high above political reality, micro-economic theories are pitched in a sense too low: they operate too much

within the institutional framework of a given system to provide a basis for the understanding of the characteristics and evolutions of whole political systems.

II

The study of comparative government seems confronted with a major problem, at once a problem of substance and of methodology. How can one bring together, by means of some analytical tool, the different experiences which the various political systems which exist in the world represent? Whether merely to describe or in order to explain, an operation has to take place which will help to grasp within a common frame what seem to be very different pieces of many disparate systems.

In order to understand better the failings or limitations of comparative government analysis so far and, more importantly, to see how one might proceed in the future, the best is to examine systematically the specific characteristics of the field and discover in this way the elements which need to be investigated and which so far have either been ignored or insufficiently covered. At this point, let us not concern ourselves as to the feasibility or current practicability of such analyses: we first should find out what needs in theory to be done and examine later what can be undertaken at this point. Despite the claims made by D. Easton, in the *Political System*, that he was reviewing the needs of comparative government, I suggest that it is because a full investigation was not undertaken in the 1960s that many difficulties were experienced subsequently, not just by D. Easton, G. Almond, and the structural-functionalists, but by developmental theorists as well (while those who concentrated on 'micro-political' analyses were never truly concerned, *per se*, with the idiosyncrasies of comparative government).

Let us therefore start from the most obvious starting point, namely that one of the essential characteristics of comparative government is that it is concerned with *structures*, with institutions, groups, and organisations. This should by now be a non-controversial and undebated matter. In any political system, there are structures of various types, such as traditional groupings, interest organisations, parties, assemblies, governments, bureaucracies, courts, through which decisions are taken; these bodies are the units on which comparative government analysis should be based: they are, so to speak, the atoms which, by combining in different ways to each other, give governmental life its characteristics.

To say that the structures are the essential starting point is not to claim that the behaviour of individuals within these 'atoms' is not of crucial importance. It is; and, as the electrons in an atom, individuals give these

bodies their shape and character. It is therefore worth pondering a little as to what the nature of structures is and as to what is the relationship between structures and the individuals who belong to them. A structure – whether it is an institution, a traditional grouping, or an interest organisation – can be defined as a privileged pattern of interaction among certain people; a structure exists when some individuals are more likely to relate to each other, at any rate on some matters, than to relate to others. Families, associations, parties, assemblies, can be characterised in this fashion. The structure can be more or less durable, more or less large, more or less general, more or less tightly organised; but what characterises all structures is the fact that they privilege the relationship among certain people rather than being involved with everyone at random.

Two important consequences follow from this definition. The first is that structures change continuously in character, albeit usually very slowly: one sees a structure as being ‘stable’, even static, but this is only because, to the observer who looks at the structure from a distance, the many movements which take place within it (changes in membership for instance) cannot be perceived, at any rate at every moment in time. As a matter of fact, structures are continuously modified in composition, as well as in strength. This is because, in the second place, the strength of the structure – the extent of its ‘institutionalisation’ – depends on the extent to which the privileged pattern of relationships which the structure constitutes is tight or loose: if the structure is very inchoate – if its members are very loosely attached to it, come in or move out often, have very few contacts with each other –, the structure cannot be said to be very ‘institutionalised’; it is weak. On the contrary, the structure is strong if converse characteristics obtain.

From this follows what might be called the central paradox of comparative government (but a paradox which is not unlike what might be called the paradox of atoms and electrons in chemistry): the main elements of the analysis are structures; these give shape to political life and force actors (at any rate up to a point) to behave in a certain way. Yet the structures ‘exist’ only to the extent that they constitute a strong set of privileged patterns of relationships among certain members of the population. Thus, in order to have a real effect, a structure ultimately depends on the extent to which it is ‘recognised’ by those who belong to it.

Yet structures remain the units of comparative government, as it is through the structures that the activities of government take place. As a matter of fact, the individuals who ‘lead’ these structures and who use them in order to make a mark on the polity depend on the structures to multiply their influence and their power (and these actors are well aware of this multiplying effect, since political agents are involved in building structures if they find that they cannot use pre-existing structures to their advantage). It is very refreshing to note that the crucial role of structures is now well-

recognised, as the 1984 APSR article of March & Olsen or the *New Institutionalism* clearly indicates. We have moved away from the belief that the study of structures was traditional and *passe* to the recognition that structures are central to comparative government analysis.

It is important to note, however, that structures have special characteristics in the field of comparative government: this distinguishes sharply the study of government from most, and perhaps all, other social science fields and thus makes comparative government both a unique and a difficult field of study. The two most important among these special characteristics are, first, that some types of structures are very few in number and, second, that they resemble each other without ever being identical (that is to say that there are rarely sufficiently similar to be treated, in practice, as identical).

It is because of these two characteristics that it has been alleged by some (fortunately mainly in the past) that comparative government was not feasible and that only in-depth single-country studies could reveal the reality of government. This is manifestly not true. Not only do we practice comparisons all the time, naturally, both explicitly and implicitly, in day-to-day discussions of politics, but the structures which compose the government of each country tend to resemble each other to a substantial extent. Indeed, in a large number of occasions, elements have been adopted from one country to another – and of course adapted to some extent in the process – because it was felt that these elements would be valuable for the country adopting them. Without much exaggeration, it could be said that ‘progress’ or at any rate change in the political arena – as in other arenas of society – has been markedly dependent on efforts made to discover arrangements existing in other countries and to reflect as to whether these arrangements could be applied, albeit in a modified manner, in another country. The result is that there is similarity without there being identity: this makes comparisons both necessary and difficult; but we have to live with this problem and find means of tackling it.

We have also to live with the other problem, namely that the elements which compose the various segments of the State are small in number, both in each country and across the world. There are for instance only a few parties and sometimes there is only one; there is only one military or, at most, a few branches in the military; there is only one legislature, etc. Thus there will be relatively few opportunities to assess the broad range of similarities and differences: institutions are like large rocks in a landscape, each with a somewhat different shape and size. There are relatively few landscapes, moreover, – the different countries – in which these similar structures can be examined and among which comparisons can be made. In comparative government, with respect to many structures and indeed with respect to the most important ones, the problem is not to draw a satisfactory sample of the universe which is being examined: it is to find a

universe in which the number of observations is sufficient for generalisations to be drawn with a reasonable degree of accuracy.

The analysis of structures is obviously difficult and it has to be detailed (a point which was often forgotten by those who, in the 1960s, were concerned with the placing structures in the context of a general analysis). But it has not only to be detailed: it has to be set in a comparative context and it has to be dynamic. These two questions raise in practice the same problems of measurement. As we saw, structures can be more or less strong (which leads to comparisons) and this strength can vary over time (which leads to a dynamic analysis). The analysis of the relative strength of structures calls for an examination of the process of *institutionalisation*, to which we alluded earlier; the analysis of this process is complex and it raises theoretical as well as empirical questions. Comparative government – and specifically the comparative analysis of structures – will not progress truly until we have a means of handling this problem.

Thus comparative government analysis has to start from structures which are relatively difficult even to describe accurately, let alone to compare because, while they are often large, they are also often few in number and are similar only up to a point. But comparative government is not confined to the study of structures; or, more precisely, if structures are the ‘atoms’, the basic units, the next step which has to be taken is that of the examination of the linkages and relationships among these ‘atoms’. It is indeed with these linkages that structural-functionalists and developmental theorists have been concerned; as a matter of fact, they faced serious problems by moving too quickly to study linkages before having obtained a sufficient knowledge of the structures which were being linked: but these theorists cannot be blamed for having been concerned with these linkages, as these are manifestly one of the most important steps in comparative government analysis.

A survey of the field suggests that these linkages are of three types. The first – and most obvious – is the linkage between the structures. We rightly talk of political or governmental *systems*, that is to say of sets of structures which are related to each other in specific ways and form specific configurations as a result. Thus some groups are linked to some parties in a specific fashion; thus the relationship between parties and the executive or between the legislature and the executive has specific characteristics in each country. If the structures are the ‘atoms’, the configuration of structures which characterise a given government are the ‘molecules’ grouping the atoms in an idiosyncratic manner.

The study of comparative government necessarily entails an analysis of these configurations and, therefore, of the type of linkages which relate all the structures which characterise governments. The nature of these linkages is unclear, however: these are often difficult to describe, let alone measure and compare. The linkage can be one of dominance or dependency; it may

be reciprocal influence; it may take many intermediate forms. For instance, the relationship between trade unions and social democratic parties in some countries is considered to be close, but the exact nature of the links is difficult to describe: there is indeed considerable room for disagreement among specialists with respect to each case. Yet this type of linkage is one of the most explicit and best documented.

Whatever the difficulties posed by the description of these linkages among structures in a particular government, comparative government has none the less to grapple with this problem and be concerned to discover with precision the nature of the configurations which these structures form. In a general manner, the examination of these linkages among structures raises the problem of the extent of *integration* of the various elements of the political system. This problem has been examined under various aspects, one of which led recently to studies of 'corporatism'; but it has to be examined as a general concept which needs to be considered and indeed measured if comparative government is to make substantial progress.

Yet the extent of the integration of the structures within a system is only one of the linkages which have to be identified. A second type concerns the relationship between structures and governmental outcomes. We are obviously not concerned to study configurations of structures for their own sake: they are the central focus of the analysis, but only because it is assumed that the structures have some effect on the outcomes which emerge from the government. By outcomes, one means any type of development which may occur, from the most detailed policy to the most general norm of behaviour which the government may foster. Whether many outcomes are truly affected by the political system is of course a debatable, and indeed debated point. It may be that these outcomes emerge mechanically and automatically from the society and thus by-pass the governmental system; but, as the point is debatable, as it is *prima facie* the case that governments do have some impact and indeed as all of us behave as if we believed that governments have some impact, the question cannot be given a blanket answer. We need to investigate, if not each outcome, at least a representative sample of these outcomes; we need to do so in a comparative manner, as it can at least be hypothesised that different structures and different structural configurations will have a different impact.

This means that comparative government must ascertain what the outcomes are, rank these outcomes in terms of importance, and explain how these outcomes are associated with different structural arrangements. Part of this undertaking corresponds to what policy analysis has been attempting to do in recent years, but the inquiry has to go further: it has to be concerned with the extent to which governments elaborate or support sets of general norms for the polity. To this extent, the examination of the linkage between structures and outcomes also comes close to the type of analysis with which classical theorists were concerned when they were expressing preferences

for particular institutional arrangements on the grounds that these embodied certain types of norms, for instance liberal or democratic norms.

As in the context of configuration of structures, the nature of the link itself is rather obscure. Descriptions of policy developments suggest complex sequences where both structures and individual actors play a part; they are often not revealing about the specific character of the link. As a first approximation, it seems permissible to suggest that the types of outcomes which are likely to emerge will depend markedly on the values which actors, and those who can put pressures on actors, tend to have; but values obviously play a general part in the life and development of political systems. The analysis of outcomes is more specifically related to the question of the ability of the structures and of the system to 'deliver' certain policies, that is to say of the *effectiveness* of these structures, as the outcomes are also more likely to occur if the structures are able to 'deliver' rather than if they are not.

Comparative government needs to concern itself also with a third type of linkage, namely that between structures and the system as a whole and society. One of the purposes of comparative government must surely be to attempt to explain why certain structures exist rather than others: this is indeed a point which theorists have long attempted to investigate; it is even the central concern of developmental theorists (to the extent at least that these are concerned with governmental structures at all). Clearly the linkage between structures and society involves primarily values, but whose values are critical in this context does vary markedly: traditions, passivity, or unconcern usually inhibit large segments of the population from being involved in shaping the framework within which structures relate to society. Yet values as such do not provide a direct link, as what counts is not only the establishment of structures, but their strength and credibility. The link between values and structures is provided by the extent to which structures and the system as a whole are regarded as 'credible' or *legitimate* by those who are involved in the political system. It is therefore the extent of acceptability of the system in terms of the society, rather than the values themselves, which provide the link between society and the structures; it is also in this context that an 'explanation' of the relationship between structures and society can be provided, if such an explanation can be provided at all.

Comparative government analysis must therefore relate to four distinct aspects if it is to account fully for the characteristics of political systems, namely the individual structures, the configurations which these structures create, the links between structures and outcomes, and the links between structures and society. These aspects are summarised by the concepts of institutionalisation, integration, effectiveness, and legitimacy. In order to assess, in specific cases, the impact of these concepts, inquiries have to be made into values held in the society and among the political actors. This is

a major task, which is manifestly beyond what comparative government could expect to achieve at present. We have to be somewhat more modest in our immediate goals.

III

If comparative government needs to proceed by stages in order to be able to develop gradually a comprehensive understanding of the field which it aims at covering, we need to discover which lines of inquiry are most profitable and in what order these inquiries should best be undertaken. Clearly, comparative government must both be an empirical and a theoretical discipline; indeed empirical investigations and theoretical analyses must be closely connected, more perhaps than they have been in the past. Yet one must be more precise: on the basis of the examination of the problems which we have just undertaken, three types of inquiries seem to be essential. The recent evolution of studies in comparative politics suggests that there are movements in all three directions.

First, we must acquire a better knowledge of structures of government and of behaviour of actors within these structures. We must explore the characteristics of these structures on a cross-national basis and, in the first instance, do so on the basis of descriptions which provide gradually a detailed anatomy of these structures. To use a different analogy, we must determine the composition of the rocks in the landscape of the politics of the countries of the world; and we must monitor how this landscape changes over time. The need for such a knowledge was grossly underestimated at the time when grand models were on the ascendant; somewhat surprisingly, it seemed not to have been realised how ignorant we were at the time about the specific characteristics of institutions and groups. Problems of comparison were often – and still are – the direct result of this ignorance, as it is obviously difficult to compare meaningfully institutions and groups about which we know very little. In contrast with many aspects of the economy and of social life, political characteristics are markedly poorly recorded: not just the size and activities of groups and parties, but the composition of elites, even the activities of legislature and legislators, the composition of executives and of top bureaucracies are difficult to know. Thus, while secrecy surrounds many political phenomena and make investigations difficult, there are many areas where detailed inquiries can provide an appreciably more concrete picture than we have currently and improve comparative analyses.

It is because this particular veil of ignorance is being gradually lifted that there is ground for optimism in this regard. For the vitality of comparative politics has been remarkable in the last decades in the form of what has

often been defined as middle-range analyses. Despite the criticisms which may be levelled at some of the studies, these have opened up our knowledge of whole areas. Party studies may have not moved quite as fast as might have been expected after the publication of Duverger's classic, but there have been many developments. The field of legislatures has been opened up in the 1970s: we are now able to go beyond simple statements about the ineffectiveness of these bodies and move to the elaboration of realistic typologies, while the examination of the role of individual members and of committees gives more scope to the understanding of what legislatures actually do. Work on bureaucracies, in both developed and developing countries, has given rise to a large number of empirical and theoretical studies which have been concerned both with the nature of these elite groups and with the structural characteristics of the decision-making process. The field of elites has also given rise to cross-national studies which have helped to link findings on legislators, members of national governments, top bureaucrats and other influentials. Executives are also now beginning to be analysed systematically, while work on military governments and military regimes have come to be, since the 1960s, an area in which empirical and theoretical studies have led to the important findings. Finally, developments in policy analysis in the 1970s have markedly increased our knowledge of the way political systems operate though these studies are often so detailed that true comparisons do not always emerge. This is not the case, however, of some of the more systematic cross-national studies of policy outcomes; these have indeed often provided a new dimension to middle-range analyses, both because these are undertaken on a global world-wide basis and because they have shown the value of using statistical techniques in the field of comparative government.

Middle-range studies of specific structures need to be paralleled by studies concerned with the interconnections among structures and the dynamics of these structures, however. This means undertaking a second type of middle-range analyses, both on an empirical and on an analytical plane. We noted earlier that linkages and problems of dynamics were among the major questions which comparative political scientists had to explore: this is not so much because these problems had not been perceived in the earlier period, but because they had been examined in too vague a manner. In the first formula of structural-functionalism, for instance, linkages were subsumed under the generic term of function: this was too confusing a term to be of much practical use. Successive elaborations of this and related models identified, under the idea of 'capacities', a number of other linkages: these too tended to remain imprecise, while also fostering the view that societies were self-regulatory – a hypothesis which needs to be tested and cannot be postulated. In developmental approaches, on the other hand, the emphasis was on the ways in which conflicts could lead to the emergence of new arrangements provided, however, these efforts were not frustrated

by the weight of traditional authority patterns. Such generalisations about types of linkages were wholly unsatisfactory as it was in practice impossible to grasp the exact manner in which such linkages operated. Why should there be a link between groups and parties? No 'law' of nature suggested that this linkage should exist. While Marx may have suggested forms of 'automatic' societal change which could theoretically be accepted as explanatory, the fact that societal changes did not occur in practice the way Marx had suggested meant that 'developmental' approaches incorporated many other factors and thus remained vague about the true nature of the links.

Comparative government analysis must therefore be, in the first instance, a detailed and systematic investigation of structures and of the relationships between structures and among structures and the environment; these middle-range studies must be accompanied by an examination of the behaviour of the actors and of the role which actors may have on structural maintenance or structural change. But these analyses cannot be satisfactorily undertaken unless they are paralleled by another type of study, which is devoted to a formal theory of the process by which structures are setup, develop and decay. These studies must focus, as we saw, on four main problems, those of institutionalisation, integration, effectiveness, and legitimacy. No systematic development of comparative government will occur until theory-building takes place in relation to these concepts.

The literature on legitimacy or integration is vast: yet it is, by and large, most disappointing from a theoretical point of view, because it is vague, wordy, and scarcely formalised. Everyone recognises that legitimacy is crucial: governments and leaders are said to 'need' legitimacy in order to succeed or fail. But no-one is able to state by what amounts, at what rate, under what conditions, legitimacy grows or declines. We have here a strange paradox: here is a concept that all of us use, and use often, and yet one about which little can be said that can remotely be considered as scientific. Moreover, what is true of legitimacy is true also of integration, of effectiveness, of institutionalisation. A glance at the literature is sufficient to make it abundantly clear that political scientists have never tried to measure, or even circumscribe rigorously the nature of these concepts.

Yet these concepts should lend themselves to formalisation, as they attempt to identify variables which are continuous and which are closely related to other variables which are in principle at least measurable. Legitimacy, for instance, is related to support: support is measurable; it is indeed often measured in large numbers of societies. A formal theory of legitimacy could therefore be tested, at least up to a point, with the help of data which we now have. Similar comments could be made about the other variables. What seems therefore to be missing is the desire, on the part of political scientists competent to undertake formal analyses, to undertake a formal analysis of these concepts. The reason may be that

most formal analyses have been related to individual actors and to 'micro-politics' rather than to the analysis of structural development. The contribution which economists have made to political science in recent years has been at this level, Downs' and Olson's work being clear examples, as well as the work of other mathematically-inclined political scientists; perhaps such a development is natural as economics has been based on postulates relating to individual behaviour. This may or may not have been a useful strategy for economics; but in view of the role of structures in political life, such an approach has probably prevented theory from developing in the field of comparative government.

Thus, besides descriptive analyses of structures undertaken on a comparative basis, comparative government to-day should include studies of a formal-cum-empirical character of the key concepts of political life which legitimacy, integration, institutionalisation, and effectiveness constitute; these studies could follow the direction which a very small number of pioneering works have opened up, such as that of Rogowski (1972) on *Rational Legitimacy*. They should be at the middle-range level, in the sense that they should aim at explaining, in the context of specific situations, processes of legitimisation or of institutionalisation. For instance, it might be valuable to assess separately, to begin with, problems of institutionalisation in different types of structures (parties or parliaments) or in different types of contexts (liberal polities, developed societies). The rate at which institutionalisation may increase or decrease, indeed the context within which these processes occur are manifestly different, as are different the processes of economic interaction in societies at different levels of development. Given that little or no theory has been constructed so far, it would seem imprudent to pretend to operate immediately at the highest level of generalisation. In undertaking theoretical-cum-empirical studies of these concepts at a middle-range level, one can hope to be able gradually to provide for comparative government the operational tools which are required to enable us to handle precisely and therefore meaningfully the questions of dynamics which have eluded us so far.

There is a third level of inquiry, however: this is the level of the 'grand' or 'general theory' which political scientists have been so anxious to develop in both the distant and the more recent past. Such a desire is both natural and valid: comparative government needs a general theory if it is to provide an overall analysis of national political life. But the fate of the grand models of the 1960s and 1970s does suggest caution. Only if we proceed step by step can we hope to avoid the pitfalls of extreme vagueness and indeed uselessness which characterised the models of the past. Descriptive empirical work should be of major help in this regard, as those who engage in general theory-building will thus be reminded of the fact that these theories are helpful only if they enable us to account for the detailed variations of the real-world; the gradual formalisation of the major dynamic concepts of

comparative government should also provide a clearer and sounder basis for the elaboration of the grand models. But, if it is to fulfil its role, general theory must go one step beyond what the analysis of concepts such as legitimacy or institutionalisation can provide. General theory must use these concepts in order to account for differences and similarities among all national political systems. There has therefore to be a leap; but in this leap lies the danger that general theory may lose touch with reality.

The defects inherent in past general theories provide some clues as to what a sounder strategy may be in this respect. Such a strategy can be discovered if we think of general theory in terms of three distinct levels. The first of these, which is closest to the 'middle-range' or subordinated concepts which we have just described, relates to the way in which the political system as a whole tends to operate: at this level, the debate relates to *stability v. change*, both stability and change being better regarded as opposite poles of a continuous dimension than as two clear-cut and distinct categories; if one does so, one avoids the lengthy discussions about the difference between reform and revolution, as well, at the other end, the distinction between stability and 'staticness'. Stability and change can help to bring together some of the consequences for the polity of the effect of institutionalisation, of integration, or legitimisation; thus the concepts of stability and change are broader than the 'middle-range' concepts; but these concepts do not account for the characteristics of the political system: they are tools helping us to describe how the political system has behaved or is behaving.

Some element of explanation can be obtained by the second level, which relates to *values*. We encountered values earlier and we noted that they were inherent in the development of legitimacy, of integration, indeed of institutionalisation. This is because they help to account, at any rate in a proximate fashion, for the extent to which a given structure is more or less institutionalised or legitimate. The point can and must be generalised: values help to account, in a broad fashion, for the way in which the political system is shaped as a whole unit. This is indeed recognised in everyday language, as we characterise systems by their liberal or authoritarian character, by the extent to which they are democratic, etc. What needs to be done, however, is, first, to become more precise about the categories within which values can be described and classified and, second, to determine whose values need to be taken into account in different systems.

Yet values need in turn to be accounted for. It is at this level, the highest but also the most abstract, that the battle has tended to take place among the *paradigms*. Yet because these paradigms have been elaborated without having built gradually on the basis of the first two levels of general theories – those of political conceptualisation and of value categorisation – it has been impossible to relate these paradigms to the reality of political life, a reality which, as we know, was not well-known empirically at the middle-range

level. Thus the agenda for comparative government does not consist in replacing general theory by middle-range analysis; it is that paradigms will provide a satisfactory answer – constitute a properly-built roof to the whole structure – only if and when these paradigms are linked to middle-range analysis through instrumental political concepts and through the analysis of values and indeed if and when middle-range analysis is itself sufficiently developed. It can arguably be claimed that empirical developments at the middle-range level are now sufficient for new efforts at paradigm-building to be undertaken; but, even if this were so, other requirements are as yet not fulfilled: as we saw, little has been done to formalise the concepts which should help to provide a dynamic basis to middle-range analyses, while only vague generalisations can be made with respect to the broader political concepts of stability and change and to the values which characterise whole political systems. It is therefore probably premature to move quickly again to paradigm-building; at the most general level, the best pay-offs can probably be obtained if we concentrate on what we have described as the first two levels of general theory.

The study of comparative government is in a state of rapid development. The greater realism of the 1980s with respect to the immediate pay-offs of general theories is gradually bringing closer the moment when a robust general theory will emerge; but this will have to be based on the recognition of two essential aspects of the field. One is that comparative government has characteristics of its own, stemming essentially from the consequences of the existence of the State and from the small number of bodies and people at the top of the political pyramid. The other is that these diverse experiences need to be brought together within a common field. There is no recipe for a quick solution of these problems; in particular, one cannot hope to by-pass the need for detailed empirical investigations and for the gradual build-up of middle-range analyses. To use the image of the fortress which is being besieged, one has to erect gradually a number of positions from which it will be possible to attack the high wall of the dungeon. But such a development cannot be successful unless empirical studies are backed by theoretical work: but formal-cum-empirical analyses need to be conducted at the middle-range level as much as, and indeed almost certainly prior to model-making at the most global level. If developments do occur in this manner, if, specifically, empirical analyses and theoretical advances occur in parallel in the course of the coming decades, there is every reason to believe that, in the not too distant future, a realistic general model of comparative government will eventually come to be.