INTRODUCTION

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I can read anything which I call a book. There are things in that shape which I cannot allow for such. In this catalogue of books which are no books—biblia a-biblia—I reckon Court Calendars, Directories, the works of Hume, Gibbon, Robertson, Beattie, Soame Jenyns, and, generally, all those volumes which 'no gentleman's library should be without'.

Lamb's prejudice against 'things in books' clothing' must be shared to some extent by anyone who has suffered from the outpouring in recent years of collections of papers in the social sciences; and so the rationale of this book ought as far as possible to be explained. It is by British writers; and it has to do with the so-called 'revolution in political science' as it bears on the study of development.[1]

The existence of a long record of work by British writers on the politics of countries in Africa and Asia needs no advertisement; it is an important part of the standard literature on these areas. At the same time it is obvious that it has now been greatly surpassed in volume by the work of the very large numbers of American scholars, backed by impressive resources, who moved into these areas, partly as a result of the American decision to become deeply involved in Third World problems after the Second World War. This work has to a very large extent been based on quite different methodological presuppositions, those of 'behaviouralism'; and between these two bodies of political literature there existed, until quite recently, an abysmal gap of mutual incomprehension: a situation perfectly expressed by the remarkable fact that David Apter's classic monograph, *The Gold Coast in transition*, published in 1955, was not even discussed in the only other study of comparable quality on modern Ghanaian politics, Dennis Austin's *Politics in Ghana 1946–1960*, published in 1964.

The passage of time has done something to bridge this gap. The brilliant research successes of what Professor Mackenzie has called 'partial theories', and generous American financial support for British students to study in the U.S.A., have deepened understanding in Britain of the sources of 'behaviouralism', and made possible some discrimination between its different varieties and their different aspirations.[2] This has been reflected in teaching.

[1] I am very grateful to Prof. H. S. Bienen, Prof. B. D. Graham, Dr B. B. Schaffer and Mr T. F. Mars for invaluable comments on an earlier draft.

[2] W. J. M. Mackenzie, *Politics and social science*, Harmondsworth 1967, p. 77.

At first the new territory mapped out by behaviouralists was added to existing syllabuses based, so to speak, on maps drawn by cartographers using quite different projections (not to say flat-earthers)—thus 'political culture' or 'elites' would become marginal new topics in an old course on 'political institutions'; but later whole new courses began to be offered, with titles like 'political sociology', which began to convey some understanding of the point of view which makes sense of books on political culture like *The civic culture* or books on elites like *Politics, personality and nation building*. The last, unfinished phase is a re-shaping of the whole approach to political studies in Britain in which some of the gains made by behaviouralism, both in general and in particular, begin to be reflected in the work of British political scientists themselves.

This phase could obviously be interesting and productive. There are some particular intellectual resources on which British students can draw in coming to terms with this almost wholly foreign-made methodological revolution—the British tradition in political philosophy, for example, and in political history. A fusion of the theoretical and empirical preoccupations of behaviouralism with these traditional British modes of political study could be fruitful, especially in the study of developing areas, where the strengths and weaknesses of both approaches have been clearly revealed. But certain conditions need to be met. There must be no supercilious British parasitism, and above all, the inquisitive and speculative drive which is the secret of behaviouralism's vitality must not be smothered.

This brings us back to the behavioural movement, about which a great deal has been written.[1] Much of this has, unfortunately, been highly

[1] The best statement of the behavioural position known to me is Heinz Eulau's *The behavioural persuasion in politics*, Random House, New York 1963. The following summary gives the essentials of the general position.

'The orientation to the study of political science that I identify by the term political behaviour (1) rejects political institutions as the basic unit for research and identifies the behaviour of individuals in political situations as the basic unit of analysis; (2) identifies the "social sciences" as "behavioural sciences", and emphasizes the unity of political science with the social sciences, so defined; (3) advocates the utilisation and development of more precise techniques for observing, classifying, and measuring data and urges the use of statistical or quantitative formulations wherever possible; and (4) defines the construction of systematic, empirical theory as the goal of political science' [Evron M. Kirkpatrick, 'The impact of the behavioural approach on traditional political science', in Austin Ranney (ed.), *Essays on the behavioural study of politics*, Urbana 1962, p. 12.]

A useful list of writings on behaviouralism in politics is given in the same article, p. 3, n. 3. See also R. A. Dahl, 'The behavioural approach in political science: epitaph for a monument to a successful protest', *American Political Science Review* 55 (1961), 763–72; and James C. Charlesworth (ed.), *The limits of behaviouralism in political science*, American Academy of Political and Social Science, Philadelphia, October 1962; especially the article by David Easton, 'The current meaning of "behaviouralism" in political science', pp. 1–25. For a hostile view see the famous 'Epilogue' by Leo Strauss in H. Storing (ed.), *Essays on the scientific study of politics*, New York 1962, pp. 305–28; and the review by J. H. Schaar and S. H. Wolin and the reply by Storing and Strauss and others in *APSR* 157, 1 (March 1963), 125–60. For the impact of behaviouralism on the study of development politics see especially David E. Apter, *The politics of modernisation*, Chicago 1965, and David E. Apter and Charles Andrain, 'Comparative government: developing new nations', *Journal of Politics* 30, 2 (May 1918), 372–416.

polemical, and the associated argument about the nature and purpose of political science has at length begun to bore even political scientists. This is unfortunate because an extended analysis by a sympathetic student of scientific history is badly needed; especially because so many attempts to 'adopt' behaviouralism without understanding the many (and possibly, in some cases, contradictory) strands of theory and methodology that produced it lead to a peculiarly awful kind of formalism. But not even a crude attempt will be made here to classify or analyse the different elements in behaviouralism. Taking a good deal for granted, we will instead offer a few opinions about some of the most general characteristics of behavioural political science as revealed in the study of development.

Of the many springs from which behaviouralism flowed, three stand out in this context; the belief that the concepts in terms of which what is studied empirically should be organized must be derived from explicit theories about political behaviour; the view that political behaviour is intimately related to social and economic behaviour; and the particular influence of Max Weber. In the study of development, these influences seem much more important than some others (e.g. the drive for quantification). This is due partly to the nature of the subject-matter. Political behaviour in decolonizing countries was less accessible and less easy to measure than in developed systems; or rather, before the question of measurement could be tackled at all there was a fundamental prior problem of characterizing what the significant elements in behaviour were. For this the behaviouralists turned to theory, and especially to the rich stock of analytic concepts provided by general sociology, and especially Max Weber. It may also have been important—it is a matter for research—that many of Weber's major ideas had been introduced to American students by Talcott Parsons in whose writings they were strongly associated with the behavioural aspiration towards a unified science of society, transcending both cultural and disciplinary boundaries; early behavioural work on the politics of new states was often a conscious expedition across cultural boundaries which, it was hoped, would prove the worth of the cross-cultural analytic equipment carried by the expeditionary by enabling him to bring back findings fit to be added to the corpus of *general* social scientific knowledge.[1]

Parsons also taught a particular form of functionalism; and whether from this source, or from Robert Merton, or from a more direct encounter with structural-functionalism in anthropological writings on the developing countries that was entailed by the determination to see political behaviour only as one dimension of social behaviour, an important group of behaviouralists embarking on the study of new states adopted a

[1] See especially Talcott Parsons, *The social system*, Glencoe 1951, and (with E. A. Shils) *Towards a general theory of action*, Cambridge, Mass. 1951.

structural-functionalist position.[1] The merits and demerits of functionalism do not matter for the present discussion; but it does seem to be true that functional explanations do not rest on quite such a straightforward relationship between hypothesis and evidence as is required, in theory at least, by some other models of scientific explanation.[2] A further aspect of Weber's work, which is possibly even more emphatically reflected in Parsons and very marked in the behavioural literature on new states, is the use of 'ideal types', which need not be associated with a functionalist approach, but very commonly is. At all events, these schools of thought within the behavioural movement have been extremely influential in the study of development politics and have been associated with an unusually pronounced tendency towards theoretical experimentation and typology-construction.[3]

These ideas led to two kinds of literature on developing countries: theoretically oriented political monographs (the studies of Binder, Apter, and Pye stand out among early examples of this type),[4] and attempts at very general theory (the most influential being, of course, Gabriel Almond's, but proliferating year by year in the pages of *World Politics* and other journals, and promising to culminate in the long-awaited final volume of the distinguished series on political development published by the Comparative Politics Committee of the American Social Science Research Council.)[5] Neither kind of work has had any significant British

[1] For an account of Parsons's functionalism, see William Mitchell, *Sociological analysis and politics, The theories of Talcott Parsons*, New Jersey 1967, pp. 67–68. See also R. K. Merton, *Social theory and social structure* (2nd edn), Glencoe 1957, chapter 2; the locus classicus for the doctrine of functional equivalence applied to the politics of developing countries is Gabriel Almond's Introduction to G. A. Almond and J. S. Coleman, *The politics of the developing areas*, Princeton 1960.

[2] An excellent bibliography of the literature on the problems of functionalism is provided by the footnotes to R. E. Dowse, 'A functionalist's logic', *World Politics* XVIII, 4 (July 1966), 607–22.

[3] 'Even an empirical study of politics and government in a single little-known country is no longer complete without its general theory, or suggestions for one, as prologue or epilogue.' Ruth Ann Willner, 'The underdeveloped study of political development', *World Politics* XVI, 3 (April 1964), 470. Dr Willner drew attention to the limiting effect of influential general concepts on the pattern of research, an effect due to factors other than the scientifically established value of the concepts, and therefore to be distinguished from the 'paradigms of normal science' which form the turning points in scientific revolutions as described in T. Kuhn, *The structure of scientific revolutions*, Chicago 1962 (see below, p. 7, n. 1).

[4] L. Binder, *Iran: political development in a changing society*, Berkeley 1962; D. E. Apter, *The Gold Coast in transition*, Princeton 1955; L. W. Pye, *Politics, personality and nation-building; Burma's search for identity*, New Haven 1962.

[5] In the early 1960s hardly an issue of *World Politics* did not contain a largely theoretical discussion of some aspect or other of development politics. Among the most frequently cited contributions on this subject are: D. A. Rustow, 'New horizons for comparative politics' (July 1957); Martin L. Kilson, 'Authoritarian and single-party tendencies in African politics' (January 1963); Robert E. Ward, 'Political modernisation and political culture in Japan' (July 1963); S. N. Eisenstadt, 'Modernisation and conditions of sustained economic growth' (July 1964); Robert A. Packenham, 'Approaches to the study of political development' (October 1964); Samuel P. Huntington, 'Political development and political decay' (April 1965); C. S. Whitaker, Jr., 'A dysrhythmic process of political change' (January 1967). It seems almost superfluous to list the titles of the S.S.R.C.'s Committee on Comparative Politics series, published by Princeton

counterpart until very recently, as a glance at the contents of the British political science periodicals will confirm. The first type resulted from a prolonged graduate training (unavailable in Britain) in the relevant fields of sociological theory and method, and a particular way of pursuing research, quite alien to British traditions, by looking for facts to illuminate a theory rather than the other way round (to put it crudely but—as will be seen—not unsympathetically). The second type, very high-level theorizing, required a faith in a particular scientific ideal, and a consequent willingness to suspend the demand for normal standards of clarity or consistency during the supposedly pioneering stages of theory-building, for which again nothing in the British intellectual inheritance provided the slightest support.

The most immediately important point about both kinds of literature, for present purposes, is perhaps that they already seem dated. It is doubtful if anyone of intelligence is now pursuing either field research or theory-building with the same unreserved commitment to the original behavioural ideals that infused, say, Apter's early work on Ghana or even his much later theoretical work on modernization.[1] In field studies, especially, there has been a reaction against the procrustean spirit of the mid-1950s; the work of people like Zolberg on the Ivory Coast or Kilson on Sierra Leone or Brass or Weiner on India shows a continuing preoccupation with theoretical problems—their interest is in no sense parochial, but constantly seeks to interpret findings in terms of general and comparative concepts—but they have been much more willing to allow 'the situation' to determine what is central in their studies, and are content with quite modest or even with negative contributions to the existing stock of theory.[2]

But before considering this as a welcome trend with fruitful possibilities for a *rapprochement* with British political scientists working in developing areas, we should consider what may be lost, both in the research ethos of early behavioural field work, and in the realm of theory.

[1] David E. Apter, *The politics of modernisation*, Chicago 1965.

[2] A. R. Zolberg, *One party government in the Ivory Coast*, Princeton 1964; M. Kilson, *Political change in a West African state: a study of the modernisation process in Sierra Leone*, Cambridge, Mass. 1966; M. Weiner, *Party building in a new nation: the Indian National Congress*, Chicago 1967; Paul R. Brass, *Factional problems in an Indian state; the Congress party in Uttar Pradesh*, Berkeley 1965.

University Press: Lucian W. Pye (ed.), *Communications and political development* (1963); Joseph LaPalombara (ed.), *Bureaucracy and political development* (1963); Robert E. Ward and Dankwart A. Rustow (eds.), *Political modernisation in Japan and Turkey* (1964); James S. Coleman (ed.), *Education and political development* (1965); Lucian W. Pye and Sidney Verba (eds.), *Political culture and political development* (1965); and Joseph LaPalombara and Myron Weiner (eds.), *Political parties and political development* (1966). The final volume (edited by Coleman, LaPalombara, Pye, Weiner and Leonard Binder) is announced as *Crises in political development*. This casual way of alluding to the development of development theory in American political science is bound to do unintentional injustice to the leading role of others; names such as Edward A. Shils (especially his *Political development in the new states*, The Hague 1960) and F. W. Riggs (e.g. his *Administration in developing countries*, Boston 1964) come immediately to mind.

To take theory first, it seems rather obvious that the value of the theoretical work done by behaviouralists on new states is substantially independent of the validity of the claims actually made for it. The ideal of a unified science of society or even the less ambitious goal of a science of comparative political analysis may not have been brought much closer to realization by this work, yet its contribution has been of first-class importance. This is partly due to the organizing power of some of the analytic concepts that have been used, such as Parsons's achievement/ascription, universalistic/particularistic 'pattern variables', Weber's authority types, and so forth; but above all, I suggest, to the fact that the behavioural commitment to theorizing restored speculative political thinking as an intellectually legitimate activity—something which, for most political scientists, it had ceased to be for at least half a century.[1]

This last remark cannot be properly documented here, but perhaps this much will be agreed; even where (as in Britain) the idea of a 'value-free' political science had few supporters, a fairly clear distinction existed in practice between students of government (or 'institutions') and political theorists. Political theory appeared to most students of government or institutions as a quite separate kind of activity, concerned primarily with 'great books' written in days when speculation and exhortation were not disentangled from observation and comparison. The virtual disappearance of this class of literature from current publishers' lists was noted as a striking fact, deplored by those who found it stimulating to read;[2] its reentry into political science from general sociology through behaviouralism was less noticed.

Yet this is, surely, a fundamentally important aspect of the behavioural movement. Talcott Parsons's complete apparatus may have been too complex and obscure for most people; what mattered, however, was the identification—by him and other behavioural theorists—of formulae for speculative thinking with the ideal of a science of society. By a 'formula' I mean an intellectual procedure, a programme of discussion, description, abstraction, and so forth, in relation to a given subject-matter, that is to some degree accepted as a valid or reasonable way of treating it; in this sense the various schools of behaviouralism may be seen as providing formulae for speculative thinking about politics similar to the famous formulae of classical political theory such as Natural Law or the Social

[1] Leonard Binder's Introduction to his *Iran* is most frequently, and with justice, cited as an example of almost impenetrable abstraction. For another type, see Apter's *Politics of modernisation*, p. 249. But both are saying some important things which, though they could be said in plain English, have not on the whole occurred to plain English speakers.

[2] See, e.g., A. Hacker, 'Capital and carbuncles: The "great books" reappraised', *APSR* 48, 3 (September 1954), 775–86; also 'Political theory and the study of politics: a report of a conference', Rapporteur H. Eckstein, *APSR* 50, 2 (June 1956), 475–87; and a symposium: D. G. Smith, 'Political science and political theory'; D. E. Apter, 'Theory and the study of politics'; and A. A. Rogow, 'Comment on Smith and Apter, or whatever happened to the great issues?', *APSR* 51, 3 (1957), 734–75.

Contract.[1] Behaviouralists not only felt free to theorize, they felt an obligation to do so, and had a good range of conceptual building materials to hand, and a variety of approved ways of going about it.

The results were sometimes vacuous, often confused and rarely very readable; but they were, quite simply, the means by which an exceptionally talented group of scholars were able to lift the study of politics out of the rut, in which it was preoccupied almost exclusively with institutions and very largely with their forms, at a level of concern and understanding determined almost wholly by the policy-concerns of political actors.[2] Professor Oakeshott's diagnosis was exact: '. . . a curriculum of study of unimaginable dreariness. . . which could have no conceivable interest to anyone except those whose heads were full of the enterprise of participating in political activity or to persons with the insatiable curiosity of a concierge';[3] and he correctly perceived that to add newly fashionable items to the curriculum, without a fundamental reorientation of intention and the acquisition of an appropriate method, was only to make the rut deeper and greasier. But whereas he did not see any such possibility, and recommended only a retreat back into history and philosophical criticism, the behavioural theorists were utterly convinced that a new synthesis of ideas and knowledge was not only possible but imminent; and in the process of trying to create it, they asked entirely new and interesting questions, and developed new and challenging canons of what would count as answers.

It may be objected that the behavioural movement in political science proper can be sufficiently explained without reference to its general theorists, and that its main stream consists of the succession of field

[1] Formulae are not, I think, the same as the 'paradigms of normal science' (see above, p. 4 n. 3); at any rate they have occurred when the discipline is in the state discussed by Kuhn in *The structure of scientific revolutions* at pp. 13–16, which appears to be pre-scientific (in his usage). His description of the effect on a discipline of acquiring a paradigm (pp. 18–21)—i.e. the movement away from books towards articles, written in esoteric language for the circle who take the paradigm for granted, and other associated tendencies—is interesting in this connection; the phenomenon has occurred in behavioural political science, but seems to have been rather deliberately manufactured. The hoped for convergence of the next generation of scholars around the paradigm does not seem likely to occur, at least not in the clear-cut way some protagonists have wished. One is inclined to think that we are still in the run-up stage towards the emergence of paradigms in most branches of the subject.

[2] For example, the preoccupation with the viability of British constitutional machinery for British colonies after independence which led to such enterprises as the Legislative Council series edited by Margery Perham; or the curious faith in the significance of electoral studies of the Nuffield variety, extended to newly independent countries (e.g. D. C. Mulford, *The Northern Rhodesia general election, 1962*, Nairobi 1964, or G. Bennett and C. G. Rosberg, *The Kenyatta election; Kenya 1960–1961*, London 1961). This tendency has almost certainly been reinforced and rationalized by the demand (not unreasonable when other things are equal) for 'good', 'simple' English; as Peter Nettl aptly remarked, it helps to ensure that 'our nose is grimly kept a bare inch above the squiggly furrow of facts'—facts conceptualized and selected by certain of the political actors, as a rule (J. P. Nettl, *Political mobilisation*, London 1967, p. 23).

[3] Michael Oakeshott, 'The study of "Politics" in a university', in *Rationalism in politics and other essays*, London 1962, p. 324.

studies, in developed and underdeveloped countries, in which theory plays on the facts in a particular way. This view seems unhistorical, however; although there has been some division of labour, even the best-known general theorists such as Gabriel Almond or Karl Deutsch have been closely involved in empirical studies and in the field of development this is especially true. The fact is that for more than 10 years a particular group of academics, and a very large body of imitators and students, have been dedicated to the proposition enunciated by David Easton in 1953, that empirical research and general theory-building are essential to each other.

The kind of empirical work which they and their research students attempted can only be understood as an expression of this point of view. General theory provided a sense of participation in a major programme of discovery, and the hope of contributing, by the hours of 'fanatical boredom' devoted to some aspect of the politics of some obscure state, to our understanding of politics in general; a sentiment which was essential for sustaining the peculiar demands of fieldwork carried out under the behavioural canon.

This approach, which I have already called procrustean, in which the researcher brought to the field a set of theoretically derived hypotheses to be investigated in relation to appropriately selected facts, involved a phenomenal effort of self-denial; facts not relevant to the hypothesis had to be ignored, or at least set aside, however much more interesting or accessible or in a vulgar sense 'important' they might seem to be. Often far from any professional companionship, the researcher had to pursue his chosen course supported entirely by his own—or his supervisor's—conviction that his true contribution to knowledge would be made, not by reporting and interpreting things that seemed on the spot to be 'central', but by faithfully accumulating just those specific data which would confirm or disconfirm the hypotheses which he had travelled so far (and so expensively) to test.

This is, of course, an idealized and somewhat exaggerated account of the behavioural research canon in its application to developing areas. The starting point, in most cases, was the obligation to produce a 'dissertation proposal' for a behaviourally oriented dissertation committee; once in the field, the more intelligent researchers would, after a period of unsuccessful struggle, fasten on to some more tractable set of problems and emerge with a publishable study bearing little relation to the original proposal, although the level of theoretical concern in it might be equally great. But not all researchers had the independence and ability to adjust in time or completely; the results could be pathetic and barren (few of the many 'negative returns' have, in fact, been published); and it was also not surprising that many researchers were tempted to look for facts which would confirm 'their' hypotheses, albeit perhaps with qualifications,

rather than those which might disconfirm them.[1] Nonetheless we should again count the cost of the reaction against this approach.

The beauty of the best work which it produced is its single-mindedness. Seldom, perhaps, has it either established or destroyed some key theoretical proposition about politics with the finality and clarity which the behavioural ideal would imply. But there is a depth and tension in the best of such writing that is absent from other equally learned and often better-written books about politics; a sense of the persistent pursuit of something important and elusive, by means which may be faulty or even crude but which are still methodical and explicit and not wholly inappropriate, which makes the result stand out as at least a sustained attempt to penetrate to levels of order and regularity in political life, without denying the existence and importance, at another level, of its diversity and uniqueness. This is to some extent a personal matter; one either senses and responds to this dimension of works like, say, *Who governs?*, *Leaders, factions and parties* or *The political kingdom in Uganda* or one doesn't; although it is perhaps beginning to be an observable fact that such books are, in the end, the ones to which most of us return in preference to other sorts of books about more important and attractive cities than New Haven, more edifying and hopeful political systems than the Philippines, or more topical encounters between traditional and modern structures than those of pre-independence Uganda.[2] At all events few students who came to David Apter's *The Gold Coast in transition* from previous books on the Gold Coast, or indeed on other colonial areas generally, could fail to grasp that here was an entirely new, ambitious and extraordinarily exciting approach, which lifted the discussion out of the parochial plane, and that the study of African politics at least was about to be revolutionized, as indeed it was. Engels's retrospective comment on the effect on the Young Hegelians of Feuerbach's new philosophy—'One must himself have experienced the liberating effect of this book to get an idea of it. Enthusiasm was general; we all became at once Feuerbachians'—could fairly be applied to the effect of the early behavioural political monographs on graduate students in the late 1950s.

The causes of the partial retreat from both general theory and the 'hard-line' field research ethos are well known. The construction of comprehensive theoretical systems can degenerate into an uninspired and pointless scholasticism; like all social theory, behavioural theories rest on ideological presuppositions which may become less fashionable[3]; at the same time, the

[1] Pye's important and deservedly influential *Politics, personality and nation building* can be faulted on this ground, for example.

[2] R. A. Dahl, *Who governs? Democracy and power in an American city*, New Haven 1961; Carl H. Landé, *Leaders, factions and parties; the structure of Philippine politics*, New Haven 1965; D. E. Apter, *The political kingdom in Uganda*, Princeton 1967.

[3] This view rests, I would argue, on quite general arguments about the relation between social experience and the language of social analysis. I do not subscribe to the frequently expressed view that structural functionalism is an inherently conservative method of analysis, nor even that it has always been used as a matter of fact to support a conservative viewpoint.

ideal of completing, by sheer deductive effort, an 'over-arching' social-scientific conceptual web which would then require only to be 'filled in' with the findings of appropriately conceived research, strikes more and more people as based on a methodological and psychological error—a once fertile one, perhaps, but no longer capable of inspiring useful work.[1] Theoretically inspired research, for its part, *can* easily become trivial or indeed meaningless; and it is characteristic of a science to try to reduce its methods to rules and consequently to acquire a large fringe of people who can follow the rules, but without inspiration or insight, leading to mediocre results, the more dispiriting for being presented under the banner of 'science' or 'analysis' ('blessed word', as Oakeshott scathingly remarks) or—worse still—'scientific rigour', meaning, sometimes, the faithful counting of what can be counted.

But these are risks attending the behavioural approach; they are the price that has been paid for its achievements. For this price it has established the study of politics on an independent footing, provided it with a set of methods, and in a number of directions has set in motion a very impressive accumulation of reliable scientific knowledge at a quite high level of generality.[2] Above all, and especially in the study of developing countries, it has enabled political scientists to ask questions that were of general interest and importance; so much so that we have possibly learned more about politics from studies of underdeveloped countries than from studies of developed ones in recent years.

This creativity and innovation and capacity for comparison and accumulation are the things which have been so conspicuously lacking in the institutional and historical tradition of British political science, and which restricted its work in developing areas for so long to the cultivated writing of history too recent to be guided by criteria of relevance furnished by a reliable historical perspective, or to the reporting of institutional changes of real significance only to local actors; which, in short, made it parochial. American political scientists may be moving towards a less ambitious and more pragmatic conception of behaviouralism, but they will certainly not abandon its commitment to speculative theory, and theoretically inspired field work; these are now part of a deeply entrenched tradition, and are supported by the great strength of American sociology. But in Britain these are still very new ideas and they could conceivably be stifled, at least partly, unless the real gains of behaviouralism and the

[1] This view, which I personally share, is certainly debatable; and it refers specifically to 'overarching', not to 'partial' or 'middle-level' theorizing.

[2] A good example of this process in current work on developing areas is the study of factionalism; see R. W. Nicholas, 'Village factions and political parties in rural West Bengal', *Journal of Commonwealth Political Studies* 2 (November 1963), 17–32; the work of Brass and Landé, already cited; B. D. Graham, 'Change in factional conflict. The case of the Uttar Pradesh Congress Party, 1964–65' (mim. University of Sussex 1968) refers to most of the relevant literature.

The most obvious example in the study of developed polities is the work which led up to A. Campbell *et al.*, *The American voter*, New York 1960.