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Area studies and comparative politics: Africa in context¹

It is frequently proclaimed, loudest and clearest in the United States, that Area Studies are finished, and this for a wide array of reasons.² They are said to have failed significantly to illuminate the events they purport to explain and, from a policy point of view, to have been ill-equipped to anticipate the major changes that have recently taken place in vast swathes of the world: globalisation is said to be sweeping away area 'particularisms'. Furthermore, it is argued that their analytical weakness is congenital since it is rooted in their very 'multi-disciplinarity': area specialists are seen as not properly trained in the social sciences and thus not equipped to develop their work within a proper comparative perspective. Whether these charges are justified or not, it is well to remember that the present swing against Area Studies is also, and perhaps primarily, due to the reluctance of governments and funding agencies to continue to support what are often seen as relatively expensive 'minority' programmes.

My concern here, however, is not so much to seek to justify continued funding for Area Studies on the basis of their 'usefulness' – however that may be defined. It is to examine the very question of area specialisation, chiefly in respect of Africa, within the context of the ongoing debate about comparative politics. The issue at stake, then, is whether an Area Studies approach can help us understand better the evolution of Africa since independence. It is not a question that we can, or seek to, avoid. Indeed, present debates about the future of the continent show clearly that the assessment of current trends in Africa is largely predicated on our approach to this question. The view that Africa is now, finally, following a 'universal' path – meaning democratisation and free market economics – derives from the assumption that all societies develop historically in similar ways regardless of local conditions. On the other hand, the notion that Africa is moving in a direction that separates it further from the rest of the world rests on the belief that its very special cir-

¹ A preliminary version of this article was presented in the Department of Politics, University of Lund on 20 March 2002.

² For a comprehensive discussion of Area Studies in the United States, see Ruguet, V. 2004: 'La politique comparée aux Etats-Unis et la question des *area studies*'. In Thiriot, C. / Marty, M. & Nadal, E. (eds.), *Penser la politique comparée: un état des savoirs théoriques et méthodologiques*. Paris: Karthala.

cumstances, which require 'expert' analysis, are preventing it from developing 'satisfactorily'.

It can thus be seen that the debate about the usefulness of Area Studies is of the utmost relevance to the understanding of Africa. It is not merely academic. Although the discussion that follows is centred on the relationship between Area Studies and comparative politics in general, rather than on Africa *per se*, it is a reflection that is critical to the future of African Studies. My argument is that the analysis of contemporary Africa is constrained not so much by a lack of data but by methodological weaknesses – weaknesses that are in many ways linked to the uncertain dialogue between comparative politics and Area Studies. That dialogue has been vitiated in the recent past by a general trend away from area specialisation, first in the United States and now in Europe. All told, this is probably not a healthy development from the point of view of making sense of what is happening in Africa, as this article will try to show. The point, then, is not to defend Area Studies on principle but to show how the political analysis of Africa would benefit from an approach that made intellectually coherent use of its specialist knowledge.

Although the question of Area Studies is habitually confined to the margins of the social sciences, it ought in fact to be at the heart of our reflection on comparative politics. Indeed, I believe the loss of confidence about Area Studies is due in part to the current 'conceptual' crisis in comparative politics. The publication of Lichbach and Zuckerman's volume on the subject is clearly a call for greater theoretical consistency in the field of comparative politics and a plea for academic debate between its three main areas: rational choice, structural, and cultural approaches.³ Typically, however, this volume says nothing about Area Studies, which remains a black hole at its centre, since even the chapters on cultural perspectives do not properly address the issue.

Yet, the question of how best to 'compare' politics across the globe cannot ultimately be disassociated from that of Area Studies. How can one 'do' comparative politics without comparing 'areas' – in one way or another? Conversely, how can one understand 'areas' without 'making' comparisons? Comparative politics is, virtually by definition, an attempt to draw generalisations from the examination of various parts of the world, whether they are labelled 'areas' or not. Even comparisons between individual countries in widely different regions must perforce be built on assumptions relating to the ways in which they may, or may not, display political attributes similar to those of the countries with which they are most readily associated. Hence, the notion that one can compare political elites in Brazil, the United States, Sweden and Italy, without reference to how the politics of elites within those countries fits in within, say, a regional context is very largely illusory.

3 Lichbach, M. & Zuckerman, A. (eds.) 1997: *Comparative Politics: rationality, culture and structure*. Cambridge: Cambridge University Press.

Of course, one of the main reasons why there is often a reluctance to acknowledge the link between Area Studies and comparative politics is that the very notion of 'area' raises the issue of culture and the issue of culture is often profoundly unsettling to political scientists.⁴ But since the heuristic value of comparative politics ought only to be judged according to its capacity to make sense of what is happening in the real world, it is necessary to consider how comparative politics handles matters of culture. Thus, whatever the approach chosen, it is hardly possible to advance hypotheses about comparative political behaviour without making assumptions about the role of culture in politics.

Rational choice theory, in its vain quest to emulate economics, tries to factor culture out by narrowing the realm of political action to its lowest common denominator: individual self-interest. Other approaches, whether institutional or structural, try to 'tame' culture into more recognisably categories of political behaviour that are amenable to quantitative or qualitative comparisons. Cultural theories posit that an explanation of what is happening must be rooted in the understanding of the relevant culture. But none of them finds it especially appropriate to rely on Area Studies for comparative purposes. Why?

There are two types of objections to Area Studies: one is concerned with their 'scientific' weakness, the other with their lack of perspective. On the first point, it is indeed true that the flowering of 'area' studies since the sixties has given rise to a large number of activities, which may seem to contribute very little to the advancement of scientific knowledge. A genuine desire to focus attention on the specificities of certain groups of people, or countries, or regions, has often become a self-referential industry in which 'identity specialists' with little serious disciplinary training debate points of political correctness. The development of undergraduate degrees in Area Studies, bringing together students interested in subjects as diverse as literature, psychology and history, for instance, has done little to contribute to a body of 'scientific' scholarly work. Quite understandably, social scientists trained in a recognised discipline (anthropology, economics, politics, sociology) have often criticised Area Studies for being a theoretical or conceptual potpourri of very little relevance to the more serious work of comparative analysis.⁵

4 On the question of culture and politics, see Chabal, P. / Daloz, J.-P. 2005: *Culture Troubles: politics and the interpretation of meaning*. London: Hurst and Chicago: Chicago University Press. Much of the argument presented in this article derives from *Culture Troubles*, which offers a systematic theoretical and methodological discussion of what culture means and how a cultural approach to comparative politics makes it possible to compare distinct polities.

5 Here the example of African-American Studies programmes purporting to study Africa comes readily to mind.

Equally, Area Studies have undoubtedly suffered from the consequences of excessive specialisation. Students trained in a particular 'area', with little or no schooling in one of the social sciences, and whose research has been entirely devoted to that 'area' not infrequently begin to lose perspective. Entrapped in the intricacies of their specialty, they 'go native'. The cultures or peoples on which they focus their attention acquire an incomparable status – that is, special characteristics which only the initiated can divine. Culture is seen only in its uniqueness. Such approaches, driven as they are to demonstrate singularity, can easily fall prey to what I call the 'tautology of exceptionalism' or, even worse, to essentialism – meaning the argument that people behave as they do solely because of who they are. Groupings are thus reduced to what is deemed to be their 'essence'. The risk then is that, eventually, the 'area' under study (whether ethnic, regional or continental) is explained entirely in terms of the attributes that are supposed to characterise it.

Another charge levelled against Area Studies is that they have frequently been too closely associated with foreign policy, particularly in the United States.⁶ Government funding of area specialists, asked to provide policy advice on demand, has made it inevitable that students and academics should have tailored their research to those fields of expertise that are most relevant to policy makers. The Cold War required Soviet specialists, so they were produced in droves. The wars in Southern Africa threatened regional stability, so Africanists were asked to give answers. The European Union expanded eastward, so Eastern Europeanists were quickly summoned. The scenario is familiar. Yet, scholarly activities that are dependent on targeted government funding is liable to be both shallow and ephemeral. Policy concerns change and the focus of analysis with it. Specialists are forced to simplify what they know in order to answer short-term policy driven questions. When they are wrong or when interest for their area wanes, they are left exposed to the cold winds of derision and forced to engage in an endless quest for further instrumentally justified funding. The reputation of Area Studies suffers as a consequence.

Apposite as these critiques are – and they do expose the vanity of the zealots who claim 'unique' expertise – they do not address the more serious issue of the ways in which an area focus may be relevant to comparative politics. However, before I tackle this crucial issue, I want to touch on some questions of definition. Although I have no wish to engage in a sustained deliberation about the demarcation of our field of study, it is useful to remind ourselves of some of the important assumptions that underpin our discipline.

6 Since 11 September 2001, there has been even more intense pressure on Area Studies specialists to address the needs of foreign policy and security issues. Although such may appear today like a novel feature, it is well to remember that during the Cold War there were several periods when Area Studies were 'directed' to be more policy relevant. Therefore, this is a well-worn pattern in the relationship between government and academic research in the United States.

The chief tension at the heart of comparative politics lies between the theoretical demands of an ostensibly scientific scholarly activity and the need to provide an analysis that accounts for political processes in historical perspective. The argument today is between those who stress the theoretical imperative – that is, the requirement to develop better, more compelling, ‘models’ of politics allowing systemic comparisons across the contemporary world – and those who favour analytical insights – that is, an approach tailored both to the questions asked and the areas concerned. Of course, all would claim that they want the best of both worlds but in reality there is, as there always has been, a deep-seated difference of emphasis between the two. One seeks theoretical elegance, the other interpretative depth.

That debate is made all the starker because the theoretical banner is currently being flown in the United States by the rational choice advocates, whose call for rigour brooks no dissent. Their claims for conceptual clarity leave little space for those who do not subscribe to the economist fallacy that insights into human activities are best revealed mathematically. The attraction of rational choice theory is that it provides well-worn quantitative instruments for comparison across the globe. The contention that political analysis must be contextually plausible is dismissed on the grounds that such an approach can never be ‘scientific’.⁷ We have here an attitude bordering on bigotry, which the Lichbach & Zuckerman volume seeks to overcome, pleading as it does for more conceptual ecumenism.⁸

But to return to the discussion that concerns us here, the growing intolerance of rational choice theorists has indisputably contributed to the onslaught on Area Studies – that approach par excellence that is seen as being guilty of trying to explain the general by means of the (over-)particular. In assessing Area Studies, however, we ought not be constrained by the diktat of one particular school of thought. We need instead to go back to the roots of comparative politics. Although the founding fathers of our discipline believed they were providing a framework of analysis that made it easier to understand political differences between countries, regions, or continents, they laboured under the assumption that there was but one model of political change. Their approach was predicated on an unilinear notion of political development. Such a postulate is still held by a number, although now perhaps no longer a majority, of our colleagues but the time has come to challenge it.

7 In truth, there is little possibility of dialogue with rational choice theorists because their allegation to advance the understanding of comparative politics rests on an axiomatic bias in favour of quantifiable methods and a reductionist definition of the ‘individual’ – not to mention an utter reluctance to confront the fact that the very paradigmatic development of the social sciences (including rational choice theory) is itself also historically bound.

8 For a systematic discussion of various approaches in comparative politics and an appreciation of the Lichbach & Zuckerman volume, see *Culture Troubles*.

Is it credible today to argue that all parts of the world are evolving politically along similar lines? Is globalisation (whatever it may mean) such as to induce an irreversible march towards a single political model, marking, as it were, not the 'end of history' but the 'end of politics'. The future is bright; the future is democratic? Although this is indeed the view taken by a number of comparativists, a moment's attention to the present complexities of world politics would suggest a little more caution. Are the current experiences of the Democratic Republic of Congo, Afghanistan or Indonesia, for instance, mere hiccups in the movement of these three countries towards a cheerful 'democratic' future or are they indications that their political evolution is unlikely to follow the single model of development that has resulted in Western parliamentary democracy, as we know it? Posed in this way the question leads to an answer that should be obvious to all but the theoretical 'fundamentalists'.

The discipline of comparative politics needs a means to account for unanticipated or un-orthodox change, or else its heuristic value will simply wither. The social sciences must not aspire to the mathematical rigour of the physical sciences and should not, therefore, seek to emulate (and in so doing become bound by) their methods. Instead, they should aim to provide the conceptual framework that makes possible enlightening analysis and relevant contrasts. And for this reason alone, comparative politics needs to make place for Area Studies. The question, therefore, is not whether Area Studies are dead but under what conditions they may best contribute to comparative political understanding.

Any discussion of the relevance of Area Studies to comparative politics must tackle three main questions: (1) how does one define an area of study; (2) how can one set up comparative questions which illuminate processes rather than reify differences; (3) how can one best account for dynamics and change over time. I propose to discuss these questions in general terms and then, briefly, to illustrate my argument by showing how some of my recent work on Africa has applied in practice the ideas developed in this paper.

What is an 'area'?

Although a simple question, it is critically important. Comparative politics requires heuristically meaningful units of analysis and not every criterion is equally valid in this respect. Geography, for example, may or may not be a relevant consideration. In and of itself, it might not be useful to compare neighbouring regions or countries. It is not clear, for instance, that the currently fashionable notion of a 'Mediterranean basin' is necessarily relevant. The first point to make, therefore, is that the choice of area is dependent on

the question(s) being asked. Some questions, such as 'democratisation' are so general that they lend themselves to comparisons between fairly large and relatively undifferentiated areas: how do democratic 'transitions' in Asia and Latin America compare? Others are very much more specific and require more precisely targeted 'areas': how does Islam affect political representation in West Africa and the Middle East?

From this point of view it is clear that the standard groupings of countries into aggregated regions such as Europe, North America, Latin America, Africa, the Middle East, South or East Asia are not always the most relevant boundaries for Area Studies. It is, of course, sensible to acquire expertise in such regions but an enquiry bounded in this way may not always readily lend itself to an elucidation of the political questions under examination. Whilst there are good historical reasons why Area Centres have been created under these headings, we should not allow institutional weight to determine the most appropriate units of analysis. A study of corruption in Europe in the twentieth century would more sensibly group countries into 'northern' and 'southern', or catholic and protestant, or industrial and rural areas. Equally, an enquiry into state intervention in the economy might more insightfully bring together countries of relatively similar GDP per capita throughout the world – Japan, Sweden, Austria, and Canada – than a more geographically focused comparison between, say, East and South Asian countries.

The point is plain: comparative politics must conceive of Area Studies in a creative way. Although often a specialisation in one of the obvious geographical regions of the world will serve as a useful platform for comparative analysis, in many instances the notion of area will have to be redefined according to the questions being posed. By the same token, the questions being asked cannot simply be chosen without consideration of the 'areas' to which they are meant to apply. For example, it would be unhelpful to study the relative political influence of Christian Democratic parties in Western and Eastern Europe. If the study aims to compare *either* party politics across the board in these two halves of Europe *or* the influence of religion on politics, then a focus on Christian Democracy is obviously not the most appropriate framework and might even be singularly unhelpful.

Presented in this way, the argument is obvious but it is well to remind ourselves that a very large proportion of what passes for applied comparative politics sets out to make sweeping comparisons across different parts of the world regardless of how instructive such enterprises are likely to be – other than in compiling yet another volume of country studies. An investigation into the role of city mayors in Europe and North America, for instance, would be relatively unenlightening since there is no common constitutionally agreed definition of the office in these countries. A study of the relationship between local and national political elites, on the other hand, would result in an infinitely more revealing comparative exercise.

Whether one approaches the analysis of different countries/regions from the perspective of comparative politics or Area Studies, the value of the study undertaken will depend entirely on the purpose, range and scope of the questions asked.

How to ask questions?

The key issue here is how can one set up comparative questions that illuminate processes rather than reify differences. The tension we have identified at the heart of our discipline between the theoretical imperative and the quest for interpretative depth is in this respect particularly acute. Theorists of comparative politics want above all to elaborate the instruments that will allow a discussion of the conceptually pregnant political questions across the globe. Area specialists believe that answering comparative questions demands an understanding of a particular region. The former want to investigate what matters to political 'theory'; the latter want to make sense of what they witness.

This is an issue that cannot be resolved *a priori*, and even less by fiat. It is the creative opposition that lies at the heart of our field of study and which makes it both fascinating and potentially fruitful, for the need to reconcile these two demands ought to encourage us to produce work that elucidates the events taking place. And in this respect, asking the 'right' questions is more important than giving the 'right' answers – *pace* the policy makers! This, I believe, entails a certain approach, a method, which it is important to discuss.

First, it is essential to seek to address 'real' questions: questions that arise from what is happening, concretely, in one or several parts of the world, and not questions that appear interesting from a pre-conceived theoretical, or even ideological perspective. For example, it is more useful to ask why young Muslims in Saudi Arabia, Yemen and Algeria resort to fundamentalist 'violence' rather than why there is 'terrorism' in the world? Or, to investigate why multiparty elections in Africa do not appear to enhance parliamentary democracy rather than whether 'democratisation' is spreading throughout the world. Or, to analyse why the effects of corruption in East Asia and Africa have differing economic consequences, rather than whether corruption is an indication of 'underdevelopment'.

Second, there should in our approach be no presumed notion of causality, by which I simply mean that we should keep an open mind as to what the answers to the questions might be. Of course, we need to have working hypotheses about the nature of the processes we observe. But we should not pre-judge how the outcome of our analysis will derive from the factors we deem useful to examine. We know, for example, that the quest to identify social

classes in the Third World has long been at the forefront of much comparative work. But to presume that the existence, or absence, of certain classes will provide a convincing explanation of political processes is to impose an *a priori* ideological causality that is likely to hinder useful interpretation. Or, we may enquire as to whether urbanisation loosens ascriptive bonds such as ethnicity, but we should not ask questions that presume only to elicit answers confirming that living in a city automatically strengthens processes of individualisation. Or, we may test whether societies that eschew ostentation – such as, for instance, Sweden – are less corrupt than those that favour it, but we should not necessarily ignore the possibility that what passes for corruption is culturally bound and that it may have other, more subterranean, causes.

Third, the conceptual framework that we employ should be relevant to the comparative questions we ask. It is a founding myth of our discipline that the theoretical models we construct are equally relevant to all polities. But the truth is that ours is not a physical science and, much as some of us keep dreaming of ‘grand theories’, there is in fact no single ‘model’ that is equally applicable to all areas, for all times. Yet, the quest for a general model goes on in certain quarters and there is always pressure on comparativists to evolve inclusive, rather than exclusive, paradigms. The measure of the relevance of what we do is still often measured in terms of the extension of its application: the more universal our ambition, the better. But it is obvious now that such a quest is illusory and it is here that Area Studies can help bring a healthy dose of pragmatism to a discipline that sometimes loses touch with reality. To give but one example, a model of development that assumes modernisation goes hand in hand with the greater secularisation of society would not help us usefully to compare the nature of political representation in the former Soviet States of Central Asia and the European Union.

Fourth, and following from the previous point, paradigms should be constructed by induction rather than deduction: that is, models should arise from the particulars of comparative analysis rather than from (grand) theories. This may irk the purists amongst us who yearn to approach problems armed with a cast-iron framework but it will not surprise those who are more concerned to make sense of what happens. The defect of over-generalised models, as is repeatedly demonstrated by the failure of political scientists turned pundits to speak sensibly about the great crises of the contemporary period, is that it appears to give them an ‘expertise’, which their knowledge of the area under discussion scarcely merits. As such, they frequently fail to ask the simple, or obvious, questions.

Examples abound. The demise of communist parties in the former Soviet Union and Eastern Europe was in principle to be applauded because comparative theory seemed to suggest that it would unleash ‘democracy’. In practice, it has done so in some settings and not in others – an appreciation that any area specialist could have offered in an instant. Similarly, and also accord-

ing to current theories, democracy is taken to be more conducive than state authoritarianism to economic development. Unfortunately, the case of the erstwhile Asian Tigers demonstrates precisely the opposite – as any specialist of East Asia would demonstrate in a matter of seconds.

In brief, it will be obvious that asking the right questions requires aknowledge of Area Studies. Comparativists cannot achieve their ambitions unless they know how to deploy the right framework for the right ‘area’ of enquiry. It is simply vacuous to pretend that any student of comparative politics can apply any given model to any collection of case studies in the hope thereby to advance knowledge, either of comparative politics or of the countries selected for examination. To be interested in political parties, or in civil servants, or in revolutions, or in corruption, is not enough to generate research that will produce comparative insights. It is only possible to set up interesting comparative investigations on the basis of a serious prior knowledge of the historical, social, cultural, economic and political context of the areas to be studied in depth. I will illustrate this point in respect of Africa below.

How to explain change?

Ultimately, the true test of comparative analysis is the insight it provides in terms of explaining processes over time. Since by now it ought to be clear that theories in the social sciences also evolve over time, the question arises as to how such an evolution takes place. On paper, of course, the answer is straightforward. Hypotheses are formulated, tested in the field, and theory reformed accordingly. In reality, it is not nearly so straightforward. Theories continue to flourish long after it has become plain that they no longer apply: witness, for example, the time lag between our changing perception of the role of the state in Africa and the changes in approach made by the Bretton Woods Institutions. Furthermore, realpolitik and foreign policy imperatives greatly influence frameworks of analysis, regardless of what is happening in the real world: consider the current emphasis on the constructive role of ‘civil society’, a concept which few understand concretely.

Here too I want to suggest that our discipline can only fruitfully evolve in tandem with Area Studies, and this for two main reasons. The first is that the knowledge required to assess whether particular paradigms are useful in terms of explaining the evolution of various polities can only come from an in-depth study of the historical changes taking place in those particular regions. In other words, the only way to find out whether conclusions reached by means of synchronic comparative analysis stand the test of time is to observe what happens in the field diachronically. For this reason alone, it is unrealistic for comparativists, even those of a theoretical disposition, not to

possess and cultivate an area expertise. It is Area Studies that feed the conceptual reflection from which paradigmatic shifts in our discipline will derive. Making sense of what happens on the ground over time is the material that drives forward the refinement of the analytical frameworks we employ.

The second is that the debate about theories and concepts should logically arise from the puzzles generated by new and, at times, unexpected events in different parts of the world. One of the great weaknesses of our discipline is that it has been relentlessly driven by our perceptions of the evolution of the West. Our areas of interest, our hypotheses, our paradigms almost always originate in the attempt we have made to make sense of the comparative differences between relatively similar political processes in the so-called advanced world. Naturally, we then seek to find elsewhere confirmation for the theories advanced in respect of change in the West. But this will no longer do. The limitations of such an approach are all too evident, as my discussion below on Africa will show.

I want, however, to stress one aspect of this shortcoming that is well nigh lethal to our theoretical ambitions. The inability to acknowledge that the models we employ are very largely derived from the experience of our own societies masks the extent to which our conceptual frameworks are a historical. Because we are aware of the changes taking place in our own part of the world, we lose sight of the ways in which we automatically adjust what are largely synchronic comparisons to historical changes, which we know by dint of our living in that particular area. For instance, it is generally understood that political parties in the West are less and less dependent on identifiable social classes – not because our conceptualisation of the party has changed radically but because we have instinctively fine-tuned our analysis of party politics to this important historical evolution, which we have come to know, as it were, by osmosis.

However, when it comes to other geographical settings we fail to take into account the extent to which our theories are a-historical and why, therefore, they may be unable plausibly to account for change over time. Witness the conventional, and (it has to be said) relatively superficial, way in which most comparative volumes come to study political parties in Asia or Africa – as though these groupings were intrinsically similar to those in, say, the United States. Here too, then, only the information derived from an Area Studies expertise can provide the historical knowledge needed meaningfully to apply a comparative analytical framework.

The point, therefore, is that comparative politics can most fruitfully be used to explain the real, as opposed to the imagined, world if it is nourished by history. The only way realistically to appreciate historical change over time is to acquire an area expertise. And the only way political scientists can deploy historical knowledge creatively is to test their hypotheses against the conclusions that are reached by the specialist historian (and even anthropolo-

gist). As is obvious to any scholar who works long enough on any particular area, the changes that take place in the present always have deep, usually infinitely complex, root systems in its long history. All comparativists thus need to be area historians. There is no place for a-historical comparative politics.

An illustration: 'democratisation' in Black Africa

I end with a brief discussion of one particular example of a thriving field of comparative analysis: the study of 'democratisation' as it is applied to Black Africa. On the face of it this is a case of an appropriate, well-defined and theoretically coherent study. The area is clearly demarcated: Black (or formerly colonised) Africa. The question is justified by events on the grounds: virtually all such countries since the end of the eighties have evolved from being one-party states to holding multiparty elections. The issue is conceptually clear: have these elections advanced the consolidation of a democratic order? The minimal definition of democracy is relatively uncontentious and includes, *inter alia*, regular elections, free and fair polls, peaceful regime changes and some form of parliamentary system. The approach involves a model of change over time from multiparty elections to the establishment, and later consolidation, of democratic rule (as adjudged by the regularity of subsequent polls).

Nevertheless, I would argue here that the study of 'democratisation' in Black Africa as it is presently being carried out is largely misconceived. Not because an interest in this subject is inappropriate, since there is evidently an international context in which more and more countries are holding multiparty elections. It is because in most instances the aim of such research is to establish whether the political transitions taking place – dubbed all too easily 'democratisation' – are bringing about enhanced political accountability, facilitating economic development and reducing poverty. But a close study of African politics would show that the answer to these questions is not best served by the examination of multiparty voting. In fact, concentrating attention on such elections is likely to obscure the analysis of the very political processes that matter most for the nature of accountability and the prospect for growth – as many Africa specialists never tire of explaining. The hypothesis that multiparty polls *ipso facto* bring about greater political choice and freedom, and thereby improves representation as well as political responsibility cannot be taken for granted. It must be examined in actual case studies. Readers of *Africa Works* will know that the book attempts to explain how, on the continent, power is exercised by means of the straddling of formal and

informal political processes.⁹ Studying Africa over time, from the pre- to the post-colonial period, it becomes obvious that the measure of accountability is determined by the norms of what is often called the neo-patrimonial system. Within such a context, the legitimacy of political leaders is determined not primarily by their electoral strength but according to their ability to distribute resources to their clients, often on an ethnic basis. Thus, multiparty elections are seen as, and in turn lead to, intra-elite competition for control of state resources. Therefore, such electoral contests are unlikely to lead to greater political accountability in the sense in which it is usually understood in the West – that is, greater dedication to the promotion of the common weal.

Equally, the notion of power extant – neo-patrimonialism – is inimical to economic development as it took place in the West, or later in Asia. This is because it fails to foster, and in many ways totally undermines, economic growth – which is, and remains, the prime basis for sustainable development. In African political systems, political representation and legitimacy are based on the maintenance of a situation in which patrons must simultaneously uphold the image of substance their station requires and feed the networks on which their position depends. Thus, they can scarcely defer consumption and expenditure for the longer-term purpose of ‘national’ economic growth. That is why African states as well as entrepreneurs rarely invest in economically productive activities.

To conclude, then, this briefest of examples illustrates the extent to which meaningful comparative analysis is dependent on Area Studies expertise. Clearly, Africa specialists would be aware of the limitations of a study of ‘democratisation’ and would account differently for the political processes that have marked the continent since independence. At the very least, their contribution would be steeped in the knowledge that elections in Africa have more often than not resulted in divisions and violence. Comparativists attempting to study Africa from the perspective a more general theory of democratisation are likely to reach misleading conclusions about the putative trajectory of the continent in the years ahead. Whether greater democracy will come to Africa or not is uncertain, regardless of the holding of multiparty elections. If or when it does, however, it is unlikely to be because of outside pressure. More importantly, it is unlikely to be in the Western garb with which we are familiar.

9 Chabal, P. / Daloz, J.-P. 1999: *Africa Works: disorder as political instrument*. Oxford: James Currey.

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