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Anthropology: The Study of Social and Cultural Originality

Prologue

I was invited to reflect on the theoretical foundations of anthropology, as practiced in Africa 'over the last century', but found intellectual artisanship to be a more compelling topic. As Gupta and Ferguson (1997) point out, adherence to a particular logic of enquiry in anthropology has changed less than theory, so by focusing on the craft of argumentation I can combine the summary of the past that was requested of me with a discussion of current work. I write as an advocate of this craft as a key component of the social science repertoire, and will argue that it can constitute a major resource for research on the present – by African scholars as well as Europeans and Americans – if its strengths and weaknesses become topics of debate, application and improvement.

In discussing the first draft of the paper I discovered that it has more audiences than I was thinking about at the time of the conference. Unexpectedly, anthropologists read it as a contribution to current debates in the discipline in Europe and America, and encouraged me to draw out the points relevant to those issues. I had intended it, however, for colleagues and students in other social sciences, especially those based in Africa, for many of whom anthropology is now a more or less unknown discipline. Even more specifically, I had written it at a time when the colleague who was slated to comment on it at the conference would be Archie Mafeje. In fact, I rushed him a copy in advance, so that we could pick up a new theme in the conversation that he alludes to in a recent article in this journal (1998: 98). The publication of the paper in the same journal confirms my preference for retaining the version as it was first written because it addresses, from a different direction, the problem that Mafeje has referred to as anthropology's 'epistemology of alterity'(1997:5).

I do not take issue with all of Mafeje's points, but I do substitute the concept of originality for the concept of alterity (or difference) and endorse it rather than rejecting it as the bedrock of a characteristically anthropological epistemology. Originality is a phenomenon at the frontiers of commonality; it is partial and emergent. Alterity rests on a foundational proposition of difference; it is complete and eternal. However difficult the conceptual challenge, we have to address partial and emergent phenomena because that is the reality of the world, and – in my own view – an aspiration *in* the world. It has been said (perhaps apocryphally) that there are two ways of losing oneself: in the universal and in the particular. Anthropology is the only social science to have wrestled consistently with the spaces between universality and particularity, with no

facile assumption of either. Alterity certainly had its moments in the discipline, and the intellectual artisanship of the search for understanding of originality has not necessarily always been consummate. But, like all examples of engagement with difficult problems, the discipline's characteristic methods are enormously instructive, especially when we need to develop thought and debate of our own, for the originalities of our own era. Mafeje's argument would quarantine the anthropology of Africa, where I would like to see an African anthropology returned to the center of social scientific study: debated, refined and augmented. The rest of the paper is more or less as written for the conference.

Introduction: Anthropology, Africa and the Disciplines

Post-colonial Africa turned away from anthropology as an academic discipline, because it was seen as entailing a deadly combination of political complicity and cultural nostalgia, in an era when sights were set on an autonomous future as nation-states. More than thirty years later Archie Mafeje (1997; 1998) has declared the discipline moribund, if not already dead: intellectually aimless since it became deprived of its colonial object in primitive societies; unconnected to key debates within Africa about its own future; and methodologically vapid in its passionate insistence on the privileged viewpoint provided by an otherwise vague process called 'fieldwork'. Its continued operation he sees as posing a profound question about how knowledge is produced and for whom. Given this history, and the unlikelihood that formal training in anthropology will be considered a priority for African academic futures in an impecunious present, what was a paper on anthropology doing at a meeting charting the future of social science in Africa?

There is one very simple argument in favor of at least addressing the discipline, and this argument can lead us, eventually, into deeper considerations. The fact is: anthropology hasn't died. The discipline is still producing distinctive work and recourse to sources and methods from the anthropological corpus is increasingly incorporated into a wide variety of other disciplines: psychology, sociology, geography, environmental studies and so on. Questions posed and methods developed in anthropology are now applied to the study of all kinds of organisations, from drug-runners to mushroom-collectors, and anthropologists are employed in corporations and business consultancies. Far from seeming a patronising and exoticising method, ethnography has recently been advocated by a well-known sociologist, Michael Burawoy, as 'the paradigmatic way of studying the social world, and from this point of view anthropology becomes the paradigmatic social science... (in) that it best exemplifies what is distinctive about the practice of all social science... (combining) both understanding and explanation.' (1991:3) So while African social science struggles with the colonial legacy of the discipline (see CODESRIA 1992), Burawoy seeks 'to unchain ethnography from its confinement as a quaint technique at the margins of social science'. (1991:3).

We are now in the seemingly odd situation where much – perhaps most – anthropological writing is about the west, leaving relatively little attention to the rest of the world. It would be a tragic irony if we faced an era in social science where ethnography was considered part of a rich repertoire of approaches which could only be afforded in the west itself, while Africans – as one of the key populations whose societies and cultures inspired the methods in the first place – found themselves almost entirely at the mercy of rapid surveys and off-the-shelf methods devised elsewhere. It would be an intellectual version of the African resource situation: primary production and what Nigerians call *tokunbo* – that is, second hand imports – for consumption and use.

My own experience with interdisciplinary collaboration (at various times, in various ways, with historians, geographers, economists, political scientists and demographers) is that the debate about how to study the African present is not yet fully engaged. The quantitative sciences still approach Africa as new terrain (see, for example Paul Collier's enthusiastic invitation to economists in 1993), although the early confidence is now seriously jaded by the clear limitations of their ability to predict successfully the on-going dynamics of structural adjustment. Our interchanges about both enthusiasms and failures are not yet fully sustained, or embedded in research that aims not only at empirical knowledge but at conceptual and theoretical development. Anthropologists tend to take a critical stance, but to advocate a unique and identifiable contribution for anthropology – especially in an African context where it is unlikely to constitute a separate academic department – demands expressing more succinctly than we have done so far the positive nature of its intellectual mission.

The evidence is very strong that it does still have one. The discipline as a whole – across a broad swathe of difference and debate – engages with the world and its interlocutors from a largely uncodified but very specific cumulative experience of trying to come to terms with its improbable mandate from the past. Reviewing the division of intellectual labor in the social sciences as it was established in the nineteenth century in his presidential address to the U.S. African Studies Association in 1965, Joseph Greenberg described how anthropology inherited 'all the remaining societies on earth and all their aspects', by a process of elimination, after the west had been assigned to several 'topical disciplines' (political science, economics) and the ancient literate civilisations to area specialism (Sinology, Islamic Studies). Ethnocentrism not only ensured this residual definition of the domain of anthropology, but also that – even with its massive responsibility – it would be the smallest. 'With pitifully weak forces it sought to include in its subject matter the majority of the world's societies in all aspects of their social life'. (1971 (1965): 231). The task was overwhelming. And this, I think, has had lasting effects, on which we can now build again in a different way.

What was done with the mandate to address all of non-literate human social and cultural history in all its complexity? It is inaccurate to generalise the entire

history of the discipline from research in Africa in the decade between World War II and Independence. In the flush of social engineering after the war, and facing the nationalist challenge, the concerns of science and administration were brought closer together than they had ever been. As Mudimbe showed (1988), the terms of engagement betwen Europe and the non-literate world before the rise of institutionalised science was through the missions. Between 1900 and the beginning of development and welfare movements in the late 1930s, the view of anthropology that prevailed was as a science patterned on natural history (Kuklick 1997) and engaging with questions of human psychology. Colonial and applied concerns were addressed, but they did not explicitly drive the scientific agenda. There were very few professional anthropologists and financial resources were even thinner on the ground. Some of the research and publications we think of as classics owe their very existence to private philanthropy from wealthy people who were simply convinced of its worth. For example, without the personal (inherited) fortune of Elsie Clews Parsons to support the field research of Franz Boas's students, American anthropology might look quite different, and we might not have such seminal works such as Melville Herskovits's voluminous sources on the cultures of the peoples of African descent in the Americas.

This lack of a critical mass of scholars and resources was made up for by intellectual ambition and in some cases enormous personal dedication. Without a doubt there was also serious over-reaching, as is true whenever there are not enough voices in the conversation to exert the critical controls on which science ideally depends. But what also grew out of this disproportion between the challenge and the means was a whole series of acute intellectual strategies designed to apprehend, triangulate, describe and appreciate the specific originalities of society and culture outside the west. The purpose was to identify shared and original features; and, with cognisance of the sheer enormity of the task stretching out before them, anthropologists concentrated most closely on appreciation and documentation of novelty (or originality). Rather than presuming commonality until originality was proven, anthropology worked on the presumption of originality and set very high standards for the proof of commonality in social and cultural life. (Racial theory as a basis for understanding difference persisted, but did not define the mainstream of university-based theory). Since one's working concepts necessarily come from the repertoire of known cases, anthropology cultivated techniques to prove itself wrong: not about this or that hypothesis but about the entire formulation of the categories of observation and the relations among phenomena.

Originality is both centrally ir   ortant and hard to define exactly because it was a moving and changing quality, as the corpus of knowledge grew and as debates and challenges opened up new domains of research. The common accusation of a concentration on exoticism does not capture it accurately. In fact, many topics that had sensation value in western culture are quite scantily

addressed by empirical anthropology, so focussed was the profession on the structures of the daily life-world. Neither is the concept of 'difference' an adequate synonym because it implies too global and too static a classificatory ambition. Rather, originality exists at the point where the skillful use of disciplinary technique meets the capacity of empirical reality to contradict, evade and otherwise scramble one's expectations. Originality is simply a name for that capacity. Choice of topic, methods of study, attitudes of mind, traditions of self-critique within the discipline: all these guiding principles of intellectual artisanship, at their best, bear the indelible mark of the experience of the practitioners of a very small discipline aspiring to address problems that were beyond them.

Let me jump ahead in the argument to indicate where it is going. The encompassing historic mandate of anthropology was to describe and explain the originality of the cultures and societies of the world in a classificatory mode. This is how it became identified with the study of difference; societies and cultures differed by broad criteria based on attributes assumed to be more or less persistent. Although some texts and other summaries (such as the Human Relations Area Files) certainly presented categorical distinctions between, for example, matrilineal and patrilineal descent, states and stateless societies, the theoretical debates in the discipline were less about developing a universal grid than about how to an enlarge and refine a conceptual vocabulary to illuminate social and cultural life in general. As Paul Bohannan writes throughout his classic study *Justice and Judgment among the Tiv* (to which I return later), and summarises quite defiantly in the final sentence: 'both sets of "technical" (English and Tiv legal) concepts can be compared, each elucidating the other... Only so have I been able to see "law" not as something universal but as the tremendous cultural achievement that it is' (Bohannan 1989 (1957): 214. My insertion). The legal vocabularies are not mutually exclusive, as in a classificatory scheme, but neither are they completely mutually translatable. They overlap. And at the borders, each offers insights into qualities that may, in fact, exist elsewhere but without the same cultural emphasis or elaboration.

This orientation has been far too robust, pervasive and creative a theme in disciplinary history to collapse with its classificatory version in the 1960s and 1970s. Several decades ago the intellectual goal shifted to understanding forms of originality that were historically emergent in our very midst, rather than – as was assumed in the past – embedded in cultural premises laid down so long ago as to be validly considered (to quote Meyer Fortes) as *sui generis*. Originality was then seen as embedded in peoplehood (lifeways, traditions) and revealed to scholarship by 'decoding operations' (Bourdieu 1977). The faster the world changes, however, the more clearly does originality become novelty, created in the here-and-now, in a reflexive, recursive fashion. The area that lies beyond the familiar is not different in any categorical sense because it remains connected to well-known aspects of social life. But it *is* remarkable enough in qual-

ity, in magnitude and in its potential for growth and elaboration to be treated as original. So, contrary to some assumptions about anthropology's equation with a fast-disappearing 'tradition', its styles and standards of study and argumentation actually lend themselves highly appositely to conditions in the present. This, I believe, is what explains anthropology's sudden enchantment with the west. The west is currently producing novelty very rapidly and surprisingly in many areas that profoundly affect intimate realities and large structures, areas such as reproductive technologies, the management of knowledge and concepts of personhood in relation to illness, work, aging and sexuality. But present day Africa is also original, and within the same world order. Pivoted around to address the recreation of new orders and disorders in the contemporary Africa of the turn of the millennium, the discipline of self-critique that was imposed on anthropology by the assumption of empirical originality can serve both as a resource for doing justice to Africa, for Africa, and for contributing from African experience to a more general knowledge of the world.

What conditions in contemporary Africa seem to demand this classic scepticism? Can we not 'get by' entirely with the model-building and hypothesis testing of the other disciplines? As a scholar who uses the other disciplines I certainly favor combinations of approaches. But the 'big picture' of Africa (and indeed, the world) suggests some unprecedented dynamics. Much of Africa is deeply influenced by, but not fully subjected to, the imperatives of national government, global economy and spreading religious fervour. For many places the contingent relationship of people and places to global dynamics is the simple result of a mineral economy: the revenues of the state and the wealth of the nation in an international context are overwhelmingly provided by a minute proportion of the total population and the resource base. The rest is structurally irrelevant, and its peoples and environments are subjected, at worst to the random carelessness of irresponsible power and at best to a losing battle to overcome the distortions that single-source national revenue eventually imposes (see Karl 1997). And yet people live, create social orders, try to access larger resource pools and create cultural understandings of their situation: in ways that cannot be clearly derived either from past relatively autonomous conditions of resource control or a colonial or national state modeled after the diverse economies and tax-paying dynamics of established nation-states. How then do we – and people themselves – understand and account for economic orders that continue to provision and employ large numbers of people in regional economies in spite of ruinous policy episodes? How are localised social and cultural orders refashioned in the wake of calamity? How does each successive cohort of youth attempt to build occupational, residential and reproductive careers in the face of radical uncertainty? What are the ideologies and practices of national government and corporate business in a context with so few and such weak institutions of accountability, or even of literal account-keeping? And what is the participation of us intellectuals and researchers in the shaping of

interpretations of all these phenomena that play into their ongoing logics? Economic theory affected structural adjustment policy; demographic theory affects population projects. All these processes have the quality of unexpectedness with which classic anthropology prepared itself to study, for example, Hopi ritual life or Trobriand exchange. And – just a footnote – when I use the collective 'we' here I don't mean 'we the West', but 'we scholars' of whatever national provenance, whose job it is in the great division of intellectual labour to represent the social and cultural world to itself in terms of evidential arguments that follow canons of logic.

What is original about present-day Africa is the radical configuration of religious, economic and political life in a *contingent* (i.e. non-autonomous but non-incorporated) relationship to global religions, global markets and global political dynamics. A population that is growing and urbanising at historically unprecedented rates, without concomitant industrialisation, is literally recreating society and culture on a new model (or plural models). As Olukoshi writes of his studies of society under structural adjustment: 'Although there is increasing recognition that beneath the surface of chaos or stagnation that prevails in Africa, the continent is undergoing profound changes, there is still considerable uncertainty as to the precise nature of these changes and still less about the problems and prospects which they pose for our conceptual/analytic frames as well as for the long-term development of the continent... The theme of the decomposition and recomposition of structures, institutions and identities in Africa is, therefore, one which needs to be researched more closely.' (1996:77). The day that we are convinced that there is nothing to these processes but minor variations on global themes, and that the other disciplines are adequate for studying those themes, is the day that anthropology will lose its specific intellectual advantages for a general social science of modern Africa.

My emphasis here on anthropology as a practice rather than a set of topics and findings may make it seem a rather fragile enterprise, with no solid philosophical justification. The philosophy certainly needs work to make it more explicit, but much of it is embedded in habits of mind and modes of exploration that have been fostered by our concentration on variety, difference and unanticipated realities. Across wide differences of theoretical persuasion, anthropological thinking is clearly recognisable, as if representing a set of fixed centers of gravity whose relationship to one another is reshaped but not fundamentally changed by the topological processes of theoretical debate. The conditions of the present are unprecedented enough to render that legacy particularly salient now.

We need, however, to identify its characteristic steps and components, to formalise and augment them, and to debate how they can be applied to the societies and economies that people are creating, dismantling and rehabilitating now. The other disciplines need our approach to originality. What anthropology needs to do is to continue and extend its experimentation in how to apply

the legacy that was developed to meet a classificatory and generalising theoretical ambition, to new demands that arise in the study of historical emergence. In the post-structuralist mode, the problem resides most acutely in conceptualising different levels of social and cultural reality and in relating them to each other, thereby putting a strong logical foundation under aggregative arguments about the larger significance of detailed studies of particular arenas. The arenas we study are no longer necessarily 'micro' in the sense of the classic 'village'. But whether describing diasporas, commodity chains, government offices or other social forms we still build in a distinctive step-wise fashion towards a configurational argument. Whereas under structuralism (of various sorts: functional, Levi-Straussian, Marxist) the connections underlying configurational arguments were derived from theory, at present we follow out the connections amongst otherwise analytically separated domains of society and culture through *literal juxtaposition or relation in the world.* When anthropologists take leave of both tight theory and reality as the basis for connection (because there may seem to be no other way of addressing large issues) their work takes on the quality of a literary essay, and instinctively they make up for the lower persuasiveness of the argument on scientific grounds with the persuasiveness of expositional elegance. Usually they are very interesting, because a great deal of disciplinary history and individual expertise goes into the insights they bring out. But it is doubtful that this can continue as a disciplinary practice, that we teach and that others – outside the discipline and subject to the expositional genres of their own – will replicate.

By contrast, of course, the quantitative sciences study large phenomena and aggregate small studies to major significance through statistical logic, working backwards to the propositions one needs about the units and categories of analysis in order to do that. Their practitioners know full well that these units are often fictions and the categories over-standardised, but modification would often reduce the subsequent amenability of the data to aggregative and comparative analyses. It may be utopian to think that one could explicitly address these problems as logical rather than politicial-ideological ones (or at least to cut down the ideological quotient and thus raise the level of accountability to commonly-held criteria for the results). But if ever a situation demanded that one try to do so, present-day Africa is it because so much is at stake.

To illustrate what I think anthropology has done, could do more of, and needs to transcend I look briefly at law and governance, starting with Paul Bohannan's ethnography of Tiv justice and judgment, carried out between 1949 and 1953, and published in 1957. I first extrapolate some characteristic intellectual strategies of the discipline as they were applied in the 1950s, and then for each one I look at current and possible future applications, and the adaptive changes that we need to make. To choose Bohannan's book is in one way rather arbitrary. It is simply one useful, accessible and (in my own view) inspirational classic to illustrate themes for a context such as this, where most

people are not trained in the discipline. It also, however, seems to me a moving document on the crucially important topic of locally created orders. Nigeria's local governance has been so thoroughly muddled under military rule that it is unclear how – if at all – the manifold small problems of coexistence and large problems of legitimacy that Bohannan described are now to be addressed. Any building of a viable new future, in Nigeria and in many other African countries, must depend on the capacity of people to engage creatively with one another in a multitude of potentially disputatious contexts. National life will have to combine large numbers of new grass-roots processes that have grown up in the shadow of states whose wealth and capacity to intervene in social life have been inversely related to their accountability. For an understanding of how this presently works, or might work, the logics of exploration exemplified by Bohannan still have a great deal to offer.

Examples and Extrapolations: Intellectual strategies in Bohannan's Justice and Judgment among the Tiv

Paul Bohannan set out to describe the legal ideas and processes in Tiv culture and society as features of 'another culture', that is, as part of the study of 'originality sui generis'. His work could certainly be seen as colonial ethnography. He was supported by the British Colonial Office, published one of his most detailed works with Her Majesty's Stationery Office and – in this work on law – studied an aspect of life that had been and continued to be of critical importance to government.[1]

The book demonstrates particularly clearly certain features of the intellectual artisanship of anthropological method that I believe have been brought forward across the theoretical threshold from concentration on originality as a property of structure to a focus on its historical emergence in multiple forms and contexts. In this ethnography Bohannan works systematically into the spaces between known/standard concepts such as 'law', 'court', 'witness', 'oath' and so on and the unknown/original ideas and practices of the Tiv, in order to clarify the latter's particular nature, meaning and power. His most important means were:

1) the grounding of all description in analysis of local concepts and case studies of their use in practice;
2) the pursuit of 'redundant' information, that is, the continual maintenance of vigilance – through exploring sidetracks and applying multiple methods – that one was not missing important facets of the phenomenon under study;
3) the extension of description along lines that emanate directly from these concepts and cases;
4) the creation of a configurational model of key premises and institutions so that

5) the final understanding can address the logics of collective action: can render original behaviour comprehensible.

This is not an exhaustive list but it serves to illustrate particular intellectual orientations in anthropology from which are derived the multitude of specific methods for documenting and analysing everything from symbols to residence patterns. I look at each strategy in turn, as it is applied in practice to particular empirical sources from his book. For each intellectual strategy I then show how it has been pivoted around by more recent anthropologists, from application under the *sui generis* assumption about cultural originality to application under an assumption of emergent originality. The examples are simply indications; they are covered briefly and are supplemented by a few suggestions of my own. Their purpose is simply to show the continuing possibilities for ethnographic method in the present.

1. Grounding

a) Linguistic concepts

Bohannan never brings up any topic without addressing the translation problem. The exact sphere of applicability of key concepts in the Tiv language is established before any other description begins, and if the concept is recalcitrant to accurate rendition in English it is explained but not translated. The central concept of 'jir' is just one of many he treats in the same way: a *jir* is both a tribunal and a case; it is the entire action of hearing a case. Since this concept defines the most basic unit of analysis its 'essential ambiguity' (p.8) has to be retained through all stages in the ethnographic process; it may be translated for comparative and generalising purposes, but not in the immediate context of description.[1] He keeps pointing out that on this and all other topics, unless the concepts are clearly anchored in local usage we literally do not know what we are describing and we have no sound basis on which to build subsequent arguments.

This kind of systematic linguistic skepticism could profitably be applied to any and all activities of modern governance. People's own terminologies and discourses about their use are not completely sufficient for understanding action but they are absolutely necessary. One could make this claim on theoretical grounds, but here I make it on methodological grounds alone: language is probably the best, fastest and most delicately differentiated way into the life-world of social actors. In Africa, as elsewhere, the workings of 'modern' governance are far from being generic and therefore transparently obvious enough to exempt them from ethnographic analysis. Do we know yet how different officials, or governmental processes and offices are categorised by local populations, or conversely how they define themselves relative to national and local arenas? Basil Davidson's idea of government-as-foreign – the state as the Black Man's Burden – has a linguistic dimension. For example, in Southern

Cameroon in the 1970s the local government office was still referred to as the house of *ntanan*: conventionally a white person, although literally etymologically, someone who habitually spoke loudly and pompously. A Nigerian colleague told how his sister used to greet him back from Lagos to the countryside by asking 'How was Nigeria?'. How are such terminologies invented, applied and discussed? Using this method, Peter Ekeh's (1978) concept of the 'two publics', with their different moral orders, could be put to ethnographic test by detailed examination of people's uses of moral terms in varying contexts. They may, or may not, break down into a neat dualism: as concepts and/or as practices. It is striking that so little of this work – unlike African scholarship in the humanities – incorporates African concepts in their home language.

Two papers by anthropologist Mariane Ferme (1998, 1999) explore Sierra Leonian thought and practise with respect to national democratic elections (in the era before the war). She examines the ideas and processes people call *politisi* (the competitive, winner-take-all mode of national electoral politics) with what they call *ngu hite* ('hanging heads'), which refers to 'a public sphere based, in the local imagination, on a dialogics of compromise, of consensus forged through both overt and covert consultation, of communal and sectarian interest, of civility' (1999: 185). The latter includes recourse to the occult, to rumors and to secret meetings through whcih dangerous forces are contained. Idioms and practises from the familiar dynamics of 'hanging heads' were applied to the elections, to avoid the dangers of *politisi*, which was seen as courting violence. That the enumerations involved in modern governance also have this connotation of violence is explored in another paper (1998). Her analysis is exemplary of the anthropological method of starting with local concepts and seeing how they are mobilised in specific instances or cases, applied to emergent novelty, in this case democratic elections.

As far as I know there has been no systematic attempt yet to analyse the terminologies for modern government that have emerged, not just in mono-lingual contexts but on the multi-linguistic frontiers where soldier meets farmer, area-boy (armed robber, hoodlum) meets police, political candidate meets the masses (or local hierarchies), and so on. Ferme notes how the concept of 'making sacrifices' in the context of price controls and international loans was translated into Mende using the terms for ritual sacrifice of food and animals (1999: 166). Mbembe shows how delicate was the task of translating the political message of the UPC nationalist party in late colonial Cameroon from French into local languages (1996). In volatile polities, with voluble populations, old and new terms are being tried out all the time, along with their moral entailments. For example, over the years of navigating increasing numbers of roadblocks in rural Nigeria, I sense that people have even begun to incorporate the young soldiers and officials who work the inspection points into the category of generic youth rather than government official. Knowing that salaries are paid only intermittently, drivers give up the expected dash with the observa-

tion that these soldiers, inspectors and customs officials are just 'children' and they have to eat too. This idea is one among the many that must now constitute a rich repertoire; for one specific but very frequent mundane situation it literally inverts the concept of *government* as paternal or patrimonial, to return that role to the *people*.

So from the point of view of anthropology, a terminological analysis would go part way towards a practical research agenda on the puzzling processes that political science rushed to assimilate – in the event inexactly and unhelpfully – to the concept of neo-patrimonialism. As Lentz (1998) argues in a paper I discuss in more detail later, the political science focus on two sets of power idioms – consumption and paternalism – with respect to African dynamics of legitimacy prevents us from seeing the full range of ideas at play. Her position reflects classic anthropological method: If the construction of any ideal type of 'power in Africa' or of a specifically African "post-colonial mode of governance" is at all a useful exercise (and there are good grounds to doubt that it is), it must proceed much more cautiously and on a much broader empirical basis than has become fashionable lately'. (1998:47).

b) Case Studies

In the skeptical anthropological mind, one does not know how anything works in action without witnessing cases. Insistence on the empirical touchstone here is not only a reflection of a theory of knowledge (seeing is believing), but the result of criticism within the discipline about the relationship between ideas and actions. In a relatively short book of about 200 pages, Bohannan covered 84 cases. His 'day in court' introductory chapter outlines the major features of 10 cases; his discussion of the legal procedure discusses 16; marriage, 22; debt, 8: 'criminal' cases, 17; the market *jir*, 5; the age-set judicial process, 1; and 5 moots (*jir* at home).

Each case study performs two functions in the argument: (i) it builds the dense centre of overlapping characteristics that ultimately will support the claim that there are patterns and central tendencies (norms) in the meaning-in-action of concepts, and (ii) it establishes the criteria for judging variation. At this stage the cases will never be aggregated numerically because it is precisely their sameness that is in question. Rather, they are layered over one another to establish correspondence and differences.[3]

The collection of numerous cases in action, to identify novelty, is a continuing goal of recent anthropology. For example, Fisiy and Geschiere (1991) and Geschiere (1995, 1998) examine the differing named types of witchcraft in Cameroon and their enactments, showing how new forms take on specific new characteristics by comparison with those already existing. For example, a new form termed *ekong* addresses wealth on a large scale and refers to much wider geographies than older forms. The deconstruction of the category of 'witchcraft' into its named components and then the examination of the range of over-

lapping characteristics of many case enactments follow a classic ethnographic strategy.

The collection of many case instances, in full detail without prejudging central features, can be applied to any topic, however mundane. By happenstance in 1997 I spent several hours lining up in a petrol station during the recent Nigerian fuel shortage. The maintenance of a tenuous order was remarkable. There seemed to be well-understood rubrics for precedence in line, length of wait, ultimate price and the amount one was allowed to buy, and including dashes to the young soldiers who were 'maintaining the peace'. There were some ominous moments but only a few drops of blood were spilled (as far as I know). I did a short analysis of this case (1997), working with basic data that were limited by the circumstances of the occasion: my own lack of preparation for it, my total conspicuousness, the fragility of the mood and the armed presence of the authorities.

After the event I was struck with how much one might understand about the effects of the inevitable fluctuations of price and availability of goods in a life that is contingent to global processes, if I had worked on the assumption that these mass queuing events represent a new form of social life, and had set out to gather detailed case material on as many instances of them as possible. Following the classic logic of case analysis, variability and central tendencies in people's understandings and behaviours would emerge from the layering of particularities, and full space would be accorded for noting original features, whether these were ultimately established through internal comparisons (for example, between queues under different political conditions, in different social or ethnic contexts, for different goods, or by different genders) or through comparison with standard social science models.

2.   The Pursuit of Redundancy.

Building up case material in anthropology is therefore only partially coincident with the logic of statistical sampling. The study of more cases and instances includes the possibility of taking new perspectives. This is because anthropology – at its best – doubts that its premises, assumptions, definitions and operating procedures are truly adequate to the task. The characteristic way of dealing with provisionality and iterative thinking is not necessarily to keep changing the instruments of research; a survey, census or map does not have to be changed mid-stream. It has been far more a question of augmenting given methods with others, that illuminate from another angle some facet of the reality that had never been envisaged at the outset. This is the rationale behind multi-method approaches; one should always be ready to try another tack. Because novelty has to be apprehended from quite limited data anthropologists aim to collect not just quantitatively increased amounts, but different kinds, some of which may end up simply affirming the initial position. The edifice of argument may eventually stand up without buttresses but there have to be some

buttresses in the research design. Confidence depends on a considerable measure of redundancy.

Sometimes these are reported in the eventual text, giving the impression of anecdotage. Bohannan does report his peripheral explorations, with explanation of their purpose: for example, to clarify the dubious nature of a category of court cases as officially – rather than indigenously – defined. In the process he provides information to support his view of the full range of types of delict and case: the relative absence of one kind of delict (arson), the widespread nature but innocuous moral status of another (gambling) and the questionable classification of a third (fraud). These short forays into minor cases give added density to the support for what he argues to be a quite ambiguous correspondence between type of court, type of case and legal outcome.[4]

His work illustrates a specific form of the general case about redundancy. Outliers beyond the known categories, or beyond the central tendency in statistical distribution, are taken very seriously in anthropology, and not because the minor case or the low-key phenomenon may have a certain 'exotic' interest, but because such topics may provide a new key to the whole study, if our initial premises turn out to be mistaken. Distributions of cases are thought of as illustrating a repertoire, and it may – or may not – be the mean or the modal case that turns out to be conceptually central.

Finally, many classic works explicitly note the limits of the author's understanding. Note Bohannan as an example: 'The issues in this moot were never as clear to me as those in moots described earlier. I believe they were aloso less clear to the Tiv, though it may be that there are values and categories on thought in my own culture which made it difficult for me to understand the matters they were explaining.' (p.187) Mudimbe (1991) has critiqued anthropology's low standards of cultural knowledge and brief exposure by comparison with the missionaries of the past. Cautions may be more in order in anthropology than elsewhere because the dangers of misunderstanding altogether are higher when language is a barrier. But other social sciences hardly possess this convention of identifying frontiers of ignorance at all.

Lentz's (1998) recent paper on power in Ghana can be an illustration of creative redundancy. On the assumption that the contexts in which power is wielded may not be discretely distinguished in Ghana in the same way as they are in fully bureaucratised polities, she observes her key figures – a chief, a mine captain and a politician – in a wide variety of contexts, including her own interviews with them. She follows closely their strategies of action and speech in each one, and finds several co-existing but only partially consonant elements: (i) a well-accepted definitional difference between positions that are 'traditional' and those that are 'modern', (ii) a constant shifting across the moral registers of power associated with those positions by individuals who typically operate in both, and combine the two by skillful situational foregrounding, and (iii) a certain '"grammar" to which the powerful and their crit-

ics both subscribe at least to a certain degree', which downplays the linkages across the domains (1998:63). The tension between conceptual distinction and operational linkage generates a constant and varied moral debate about power. Without breadth of documentation, all the way to anecdotage, one would have only shaky confidence that one understood the full range of ideas and action that fall within the domain of political legitimation for these Ghanaians. Rather than start with the model, classically one works from the local to the western to the encompassing general human concept.

3. The Extension of Cases

Bohannan himself takes the extended case method only so far in its extension. The method was pioneered by the Rhodes-Livingstone/Manchester school in what was then Northern Rhodesia. But almost every ethnography, of whatever persuasion, does a more detailed analysis of some cases (or events) than others. The extended case method is one empirical strategy of moving into the configurational thinking I bring up next because the 'extension' is the means of linking this case to others and to the wider context. Classic extension takes three forms: back in time through the recreation of the history of the event or the biography of the person, outwards in social space by the inclusion of parties who were more peripherally involved, and deeper into the domains of meaning and significance by tracing out the operation of the same elements in other contexts. In all three instances the intellectual strategy is to move outwards along as concrete and identifiable links as possible: the same actual people and the same issue at a different point in time, the same people in another social or spatial context, the same elements used differently. This disciplines the leaps of one's own imagination, to allow the connections between different domains and contexts to emerge empirically.

Different theoretical traditions emphasise the three directions of extension differently. Paul Bohannan was working in the era when anthropologists were trying to work out the politics and administration of kinship, so the extended case studies largely deal with extensions in social space. In five of the moot cases he explores the entire range of participation: all those named in discussions of the causes of illnesses, all those present at the moots, and all the possible social dynamics they addressed. Many points are clarified by interviewing afterwards, in case first impressions and the hasty descriptions of the moment end up being biased or incomplete. Key people are re-interviewed. Alternative interpretations are elicited and recorded: 'My clerk and I were both dissatisfied with the course which this *jir* had taken. It seemed to me that the whole thing had bogged down... Iyorkosu, however, interpreted it differently...' (p.186).

None of Bohannan's cases is as extended as those undertaken by the Manchester School. Their own strategies extended more radically, and in other directions. Max Gluckman (1940) incorporated colonial personnel and missionaries into his analyis of a 'situation'. Victor Turner (1967) addressed the

breadth of contexts and instances in which specific ritual symbols were evoked. Sally Falk Moore (1985) looked into the deep historical antecedents of specific events.

The extended case method offers possibilities for the study of current governance. Both Sara Berry (1993) and John Peel (1983) have discussed the emergence and reiteration of contentions – the negotiability – of Yoruba social life, especially under the regime of customary law. Custom could never be stated unambiguously, so it could be implemented and appealed to in the light of current issues and interests. Only by starting with a case and extending it outwards and back in time can one see the changing import of negotiations about what is, on the surface, the 'same' recurrent issue. Extension may need to encompass several levels of hierarchy to yield a satisfying understanding. During a sudden flare up of a dormant dispute in rural Nigeria, one could see that it had been brought to court in a particular political moment, when (people claimed) government had invited them to submit unsettled chieftaincy and land claims for adjudication. There could be no surer way of disorganising all levels of local governance in Western Nigeria, and wasting people's time and financial resources, than to encourage them to bring old land and chieftaincy cases to an adjudication process that would be bound to drag on for a long time. Was this part of Abacha's politics of confusion? We do know of many manipulations visited on the country in general and (perhaps) the Yoruba in particular in the aftermath of Abiola's victory in the June 12th 1993 election. So when this small local dispute was raised in the mid-1990s, after being non-negotiable for decades (if not a century), the extension of the case analysis may trace out, through the levels of government, a direct connection between the tactics of misrule and the micro-politics of negotiation in local arenas. Different analysts might stop the extension at different points; this is a matter of theoretical conviction and debate. But the step by step tracing of tangible empirical extensions from a central phenomenon is a shared method. One works through literal linkages, to avoid a too-hasty judgment about circumstantial connections that might not be warranted, and to reveal linkages that might never occur to a researcher armed with standard methodologies. Extension then begins the process of working towards configuring systemic characteristics, cautiously, in order to make maximum space for the emergence of the unexpected.

By now, the comparative and theoretical library is fully at play, since knowledge of other places, other times and other theoretical arguments offers a guide – and a goad – to identify the pathways to explore in most detail. Here, for example, Mamdani's critique (1990, see also Mamdani and Wamba-dia-Wamba 1995) that non-African scholarship fails to engage with social movements should enforce attention to that connection. The Nigerian dispute is presented as a chieftaincy dispute; it is exacerbated by a national policy; but it is also enacted within a long history of political mobilisation and

party organisation that has roots in precolonial regional politics. Theoretical critique demands that this history be included in the case extension.

4. Configuration

At this stage the leap to linking empirically unrelated phenomena can be envisaged. This range of case material and kinds of data are argued into consonance through several complementary methods: the discovery of internal logics to seemingly unrelated cultural propositions, systematic application of particular institutional resources in seemingly unrelated situations, mutual reference of different domains of life as well as statistical correlation where this is relevant. One follows out logics: if people are doing this or believing that, then one might expect this or that implication or corollary elsewhere in their lives. Is it there?

For a full appreciation of configurational argumentation in *Justice and Judgment* has to be read with other works of Bohannan's Tiv corpus, but the outlines are indicated here. The meaning and operation of justice come together around the untranslated Tiv concepts of *tsav* and *swem*, both of which have to be looked at in all their internal complexity, separate meanings and mutual reference[5].

Problems of adapting classic anthropological method to new forms of emergent originality really set in at this point, as tangible extension opens up to full scale configuration of elements whose connections are argued circumstantially. Under structuralism there were models of aggregation for relationships, institutions and basic cultural propositions. Without a workable theory of structure, we have weaker guidelines for aggregation. How do these concepts and cases of different sorts 'add up' step-wise, up through levels of changing valence in a local life that is far more complexly determined now than was assumed in the classificatory mode?

It is not an accident that some of the most successful configurational arguments in the study of larger arenas stay very close to *actual* connection. The state in present-day Africa is a topic that simply begs for an approach that draws heavily on the anthropological tradition, to complement the comparative structuralism of – for example – Terry Karl (1997) writing on oil economies (but with conclusions that can extend to all mineral economies). Geschiere (1995) and Stoller (1995) both argue that there is a matrix of ideas about legitimacy that emanate from religion: not exclusively and not pervasively but nevertheless powerfully (a point that Mudimbe also makes, in a novel). Their evidence is classic in its caution; they can show empirically how occult forces are consulted and mobilised by the actual persons in power. They can also show how a discourse about occult power is mobilised by people. It is more difficult to extrapolate to how important this might be in the political process and affairs of state, and the authors remain suggestive rather than definitive on this topic. By contrast, since he juxtaposes phenomena and creates circumstantial argument with less caution, Bayart's (1993) work on 'the politics of the belly' is

recognisably not an ethnographic argument, even though it mobilises cultural material.

Anthropology does need, however, a new set of acceptable ways of making arguments about configurations at an aggregated level. Otherwise, the next stage of making projections about their implications – their effectivity – becomes even more problematic. Whole domains of reality or levels of social inclusion can be relegated to peripheral vision for lack of either principled ways of addressing them or even – as Mamdani's example (1990) implies – they simply haven't been considered important until they are pointed out. Here, and in the next section, a disciplinary version of this paper or an extended version of the present one would be much more detailed.

5. Identification of Effectivity

These standard logics of enquiry should create a sense of people's logic-in-the-world and hence the power of action. In the past that logic was conceived deterministically as the logic of culture or social structure. Now, it is more often seen as the logic of situational action (including action that invokes or expresses identity, as well as action in the more mundane sense). This latter is a thinner, more tentative logic because it does not claim predictive power in the strict sense; but analysis should be able to suggest scenarios of how repertoires might be used and how options might be assessed by people themselves. Explicit purposes, entailed consequences, ramifying implications and the scholar's accountability for their argument before the test of what actually transpires and how people interpret it: all these are comprised under the idea of effectivity. It answers the question: What does this configuration of reality *do*?

The rigours of Bohannan's analysis support an interpretation of the *jir* as mediating breaches in social relations and restoring a particular social order. The fact that the argument is theoretically recognisable (as supporting structural-functionalism) does not mean that it lacks evidential support. Regardless of the specific theory at stake, the technique here is to work out the implications and effects for on-going social life – or in more recent terminology, the agency and unintended consequences – of the configuration and mobilisation of cultural and social resources. The most extensive description and interpretation of effectivity in the book refers to two cases in which *swem* is used to punish. After detailed examination of the diagnostic arguments:

> The oldest man of the lineage then dressed up a new *swem* in a large potsherd. He stood in the clear space, surrounded by all the participants, and held it high above his head. 'If we have not discussed this *jir* properly.' he said in a loud voice, 'and if we are wrong, then *swem* will catch anyone who has done evil deeds (*ishor I bo*) in this matter.' He dashed *swem* to the ground; the ashes in it raised a small cloud of dust which was carried away in the breeze. (p. 203).

The poetics of this passage underscore Bohannan's main inference. The moot and its conclusion 'removed the suspicion of evil from themselves and restored

smooth relationships to the entire lineage' (p.195). In general 'Moots settle disputes between persons in relationships that can never be broken or ignored. The function of the moot is only incidentally the settlement of particular grievances; its main function is to make it possible for people who must live together to do so harmoniously' (p. 171).

In recent work, making this judgmental step has become the most difficult of the intellectual steps I have discussed. For one thing, it has become a far more complex *series of steps* to identify purposes *at several levels* of nested orders, to develop a theory of their aggregation and to support a judgment about what they augur for the future. But these are necessary steps, that some recent work has neglected in favour of suggestive juxtapositions of a 'multiplicity' of ideas and actions, whose linkage is not made explicit. I tend to agree with the seering critique by Zeleza that a scholarship that rests content with simply demonstating 'difference, hybridity, and multiplicity' is itself part of, rather than a disciplined approach to, a global crisis in ideologies of the future. (1997: 16).

There are three separable intellectual processes here, which were merged in the classic works: the documentation of people's own collective purposive logics in the recreation of social and cultural life; the scholar's aggregation of these purposes across levels of social order to make general inferences about systemic dynamics; and the ultimate accountability process for our work, the question of how and to whom we as scholars address these representations and projections. Again, these issues need a different and longer discussion. The one problem I want to pose here, because it affects interdisciplinary debates, is how we anthropologists now think about how the local and possibly ephemeral efforts that we describe in such detail aggregate into collective futures. The quantitative social sciences aggregate routinely. We do it painfully and doubtfully: because we mistrust the methodological individualism on which much survey work depends; because we doubt very much that we yet know how to define relevant levels of spatial and cultural incorporation (see Meyer and Geschiere 1998); and because we lack the statistical virtuosity to be as nimble and imaginative with numbers as we aspire to be with case material. And yet we cannot claim to make a vital contribution to social science in general without looking more closely again at this issue.

III: Originality and Aggregation

Anthropological methods are being reapplied as part of the social science of the present turbulent and unpredictable world. Our biggest problem is not relevance but getting beyond what other social scientists would see as mere 'insights'. If we do not, we fail to make a dent in conventionalised approaches that may be quite wrong and potentially dangerous, as Fairhead and Leach (1996) have shown so starkly with respect to persisting ecological assumptions about deforestation.

Aggregation was always a problem in anthropology because one aimed for heterogeneity of data. Hence the importance of extended cases, which show some connections in action, and configuration, which builds towards an argument of effectivity. The decline of structuralism leaves a variety of theoretical positions about which extensions, which configurative dynamics and which projections onto a larger scale should be made. None of them grasp the nettle of aggregation as a general challenge, not only in theoretical terms but for intellectual artisanship. In the case of here-and-now emergent originalities we have no clear guidelines for arguing that some have particular staying power over time, particular penetration across domains of social and cultural life and particular cumulative magnitude or power.

In practice, some of the logic of enquiry now works backwards: from effectivity in the world to the cultural and social constitutive processes that underly them. Something manifestly serious and new happens – the Truth and Reconciliation Commission, youth involvement in war, AIDS, ethnic hostilities, the miracle of continuous food supply to Southern Nigerian cities in the era of severe fuel shortage and so on – and we work back to its dynamics in society, history and culture. Some of us are also trying to engage with the quantitative social sciences, since documentation of the sheer size and pervasiveness of a phenomenon can help focus research, and possibly eventually help to produce complementarities of method (see Bledsoe et al. 1999). But Mafeje is right to point out that the larger theoretical picture is less clear than it used to be. The shift that I have suggested to be relatively elegant at the level of epistemology – from the study of originality as structure to originality as emergent novelty – is presently still clumsy in its articulation as a larger theoretical or practical project.

The challenge of describing, understanding and working *for* a new African reality in a globalised world is insistent enough to keep us at the grindstone. This is where a critical mass of scholarship and strategically focused attention really matter. Anthropology is still a very small discipline, and even smaller in Africa than in Europe and the United States. The interdisciplinary debate needs to be more demanding and more productive than it has been in the recent past. In the past we have been hobbled by the sheer complexity of the critique of anthropology, which has made it difficult for us to convey our methods with any confidence to others. Others, meanwhile, have little or no other systematic exposure to the legacy and aspirations of the discipline. The chasm is hard to bridge: between diffidence and self-critique on the one side and ignorance on the other. I am sure the version I have put together here begs many questions. But it offers a positive starting point for re-incorporating the anthropological library into the social science of turn-of-the-millenium Africa: not as *the* authoritative voice but as an intellectual resource that offers unique strengths and, like others, should be critiqued: but also used.

Acknowledgments

Notes

1. It is worth a digression just to show how he addressed the colonial context of his work. He does not assume that the law he is witnessing is 'traditional' in all its aspects, but he does aim to elucidate what he expects to be still-recognisable general principles that were developed in a culturally autonomous context. He describes how some kinds of cases and some sorts of adjudicative processes from the past had been assimilated to the formal legal system, and other procedures, such as administration of the poison ordeal, had been banned. He also shows in various ways that the result is not a complete dominance of local justice by colonial customary law. 'Tiv do not see the courts from the same point of view as their administrators'; in fact they distinguish between *jir* (courts) of the D.0. (District Officer), or *jir* of the N.A. (Native Authority), and '*jir* at home' or '*jir* of the lineage', which have diverging features. In fact, this book could be used as an extended example relative to Mamdani's (1996) recent depiction of colonial rule in the rural areas as 'decentralized despotism'. Older and parallel practices continued to be enacted, so that there was always a plurality of contexts and terms in which fundamental ideas about justice and human life were under legitimate discussion. So *Justice and Judgment* still bears reading as a nuanced source on colonial legal conditions as well as an example of a colonial era ethnography.
2. The ethnography will cover a court and its cases, the legal procedures, the substantive law to be invoked (in the case of marriage and debt disputes), and cases of wrongs for which settlement may include a *jir* but generally goes beyond it in some way. Smaller arenas than the major courts, that are also referred to as *jir*, are also included: the market *jir*, the age-set *jir* and the *jir* at home.
3. The way in which Bohannan uses each case to check which features account for central tendencies and which vary could be illustrated with almost any one. Market *jir* # 4 serves well (p. 155-6). It concerned a stolen duck, which the market seller claimed to have bought from another person he didn't know and the original owner claimed to recognise when it was offered for sale. A *jir* was called before the market chief and an eventual exchange of a small amount of money settled the matter without anyone having to admit wrong-doing. In his short comment Bohannan can indicate how this case illustrates a) the breadth of the concept of *jir* to include a no-fault outcome, b) the process of a market *jir*, and c) the logic of compromise here in relation other venues, under other authorities. He infers: 'The difference between a market jir and regular *jir* can be seen very quickly-the market *jir* are almost always compromises in disputes which do not really involve a custom (*inja*). Was this Kwamande's duck?... No-one knew, and they realized that it didn't matter. Tiv say that fights start easily at markets, and that the market judges

must settle every small dispute quickly to reduce the likelihood of fighting...
Sometimes, however, a case of this sort is said to be "bigger than" or "to beat"
(hemba) the market chief, and is heard at the *jir* of the *mbatarev*. (lineage elders)'
(p.156)

4. Bohannan tabulates the cases heard before the MbaDuku Grade D Court over five
    years: a total of 509 (p.114). This turns up kinds of accusation he has never seen
    through his attendance at *jir*s and through his discussion of concepts and princi-
    ples. 'I have grouped together into Class IV cases of a sort that I have never seen or
    about which my material is wholly inadequate. I have never been present at a case
    concerning arson...' (p114). He then rummages through his own memory of this
    and other places, the D.O.'s memory, and many discussions to come up with cir-
    cumstantial evidence for the nature of these delicts, even though there are so few of
    them. The presence of 'gambling' as a charge in three cases leads him into a short
    discussion of the presence and morality of gambling games (which turn out to be
    widespread, though with no attempts to close them down.) He then takes the six
    cases defined as 'fraud', tries to figure out how they might be terminologically dif-
    ferent from theft by invoking another incident in which goods were stolen by trick-
    ery rather than by force or stealth. At no point does he dismiss these cases as
    mistakes or irrelevancies; they have to be addressed and accounted for, even when
    the numbers are so small (20 altogether, over the five years, or about 4%).

5. *Tsav* is a personal quality of talent or power necessary for leadership, which can be
    mobilized either for good or for evil. Leadership is clearly morally ambiguous:
    'The *mbatsav* (the elders of the community who have *tsav*) are "dangerous"
    (*kwaghbo*); they are feared; they are also trusted. In times of political upheaval...
    the *mbatsav* begin to use their talent to kill for personal gain... In peaceful times the
    mbatsav of the community protect its members from evil... (They) have the license
    to destroy its individual members for purposes considered legitimate and for the
    good of all... Tiv see all leaders in two lights: as their protectors and as their even-
    tual vanquishers' (p.163). So a full understanding clearly goes beyond legal mat-
    ters *per se*, and beyond what might be seen as religion or spiritual life; it requires
    exploration of the nature of personhood and power in all domains of life. In the
    case of evil, *tsav* has to be counteracted or punished, generally by mystical means
    invoked by the oath on *swem*. Physically, *swem* is a small pot of ashes and medi-
    cines that is set down in the midst of a *jir* and invoked when the participants swear
    to tell the truth. It is 'the name of the religious and political fetish which contains
    the idea of justice' (p. 217). Once sworn on *swem*, a litigant or witness is fully
    expected to suffer automatic reprisals if they lie. Any transparent effort to lie under
    oath during a *jir* is greeted with anger and scorn by the owner of the *swem*, who
    considers it spoiled. Undetected lying and the secret mobilization of evil *tsav* will
    result in being 'caught' by *swem*, even if it takes several years; the body swells up
    and death is highly likely (p.42).

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